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Richard Feynman - Session IV

June 28, 1966

Interviewed by: Charles Weiner
Location: Altadena, California

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Abstract:

Interview covers the development of several branches of theoretical physics from the 1930s through the 1960s; the most extensive discussions deal with topics in quantum electrodynamics, nuclear physics as it relates to fission technology, meson field theory, superfluidity and other properties of liquid helium, beta decay and the Universal Fermi Interaction, with particular emphasis on Feynman's work in the reformulation of quantum electrodynamic field equations. Early life in Brooklyn, New York; high school; undergraduate studies at Massachusetts Institute of Technology; learning the theory of relativity and quantum mechanics on his own. To Princeton University (John A. Wheeler), 1939; serious preoccupation with problem of self-energy of electron and other problems of quantum field theory; work on uranium isotope separation; Ph.D., 1942. Atomic bomb project, Los Alamos (Hans Bethe, Niels Bohr, Enrico Fermi); test explosion at Alamagordo. After World War II teaches mathematical physics at Cornell University; fundamental ideas in quantum electrodynamics crystallize; publishes "A Space-Time View," 1948; Shelter Island Conference (Lamb shift); Poconos Conferences; relations with Julian Schwinger and Shin'ichiro Tomonaga; nature and quality of scientific education in Latin America; industry and science policies. To California Institute of Technology, 1951; problems associated with the nature of superfluid helium; work on the Lamb shift (Bethe, Michel Baranger); work on the law of beta decay and violation of parity (Murray Gell-Mann); biological studies; philosophy of scientific discovery; Geneva Conference on the Peaceful Uses of Atomic Energy; masers (Robert Hellwarth, Frank Lee Vernon, Jr.), 1957; Solvay Conference, 1961. Appraisal of current state of quantum electrodynamics; opinion of the National Academy of Science; Nobel Prize, 1965.

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Weiner:

We're resuming now on the morning of June 28th, 1966. We talked, off tape, about getting back to Cornell, to fill in some of the background, some of the incidents that you recall.

Feynman:

We've been remembering some things that we left out. And it seems to me still that we should do the things that we left out also chronologically. One of the things I left out was the relation of Bohr — with Bohr and with Fermi at Los Alamos. So that takes us even further back. So let's start there. Let's start first, for example, with Fermi. When I was at Los Alamos, I knew many of these great guys. Fermi was at Chicago all the time, but about half way through, somewhere, I don't know exactly, he came to consult from time to time, to help out by consulting, at Los Alamos. One of the early times, perhaps the first time, I don't know, he was in a room, and we were supposed to be discussing some problem involving mixing uranium and hydrogen which I had been working on with my group. He wanted the results of it. Now, for everybody else in the lab I was particularly good at, or I seemed to be good at, understanding the results of a calculation. So when somebody would make a calculation, I could see why it ought to be more or less like it was without actually calculating it by some physical reasoning. I was the expert, the guy they'd usually try to ask if he could see why it came out that way, and I was fairly successful. As far as my own calculation for this hydrogen business, it was rather complicated for me to see why it came out the way it did. It was only to me the result of calculations I could give, rather than a real understanding. So, Fermi was in this conference, this little group. It was in a small room, and there must have been eight or ten people or something like that, and he asked what about this stuff with hydrogen. He wanted me to report so he could think about it. So I thought it out. I told the problem and he said, "Ah, let me see what I think might happen." Then he started to give the kind of physical argument I'm always giving, and he went along; it would be this way and then it would be that way. I said, "No, you left out a feature. You see, there's this extra complication." "Oh, yes," says he, and then he went a little bit further, and he worked it out and explained the results of my calculation, which I hadn't previously understood. It was very impressive to me, because he was able to do to me what I was able to do to somebody else. So he was just that much beyond me as I was beyond a lot of other people. So I remember that very distinctly, as a very clear thinking, clear physical thinking man. On another occasion, perhaps during the same visit — in other words, I didn't know him well at all — or perhaps the next visit —

Weiner:

By the way, was this the first time you'd seen him?

Feynman:

Very close to the first time I'd seen him. I can't remember.

Weiner:

You hadn't really had any exchanges.

Feynman:

Well, I believe it was either the first or second, or very close to the beginning of my first contact with him. And this other story is also close to the first. I don't know which was first. I think this second one is second. I had been doing some calculations on water boilers, so called, which is slow reactors in which you have uranium — I mean U-235 enhanced in water. He had done a lot of work with piles, naturally, in Chicago. We were discussing something, I can't remember exactly what, and I had a way of looking at it by which I could see a certain conclusion for my water boilers. He didn't believe the conclusion, and he tried to explain to me how to look at it, that it wasn't true. But I had this way of looking that I knew damn well it had to be, because I had a physical argument that had to be right. So I knew I was right, and I tried to explain it to him. But he had this other way of looking at it and tried to explain to me that it couldn't be true. It's a kind of a famous story, because you see one thing that was my characteristic, which I see time and time again in those days, is, once I got into physics I forgot who I was talking to. So it didn't mean a damn thing who I was talking to, it just was that it was really right and I had to explain it to this guy. The thing that I wasn't doing was listening to his explanation, to find the flaw in it or something. Anyway, it's a famous story; that we argued something like 45 minutes or an hour and a quarter, something like that, and finally he saw that it was right. That was famous because I bested him in an argument, but I didn't feel it that way while I was doing it, or anything of the kind. It was just kind of, after we were finished, people made these comments.

Weiner:

Was this a group?

Feynman:

There were a lot of people — not that particular group. This was another time, when we were arguing in a room somewhere, and there were people around listening. But the reason for the difficulty was that he was — like I was in a similar position — so used to talking to people who don't understand easily that you don't listen much to the other guy, you know? It's a habit you get from experience. It doesn't pay. And so you try to

explain, as clearly as you can, and so that's what he was trying to do. First, he had worked on these things which had certain proportions different. Because of the very large dimension, the rate of leakage is easy to analyze a different way, and these were relatively small dimensions. There were different things of different importance, so that the way of looking at it was essential, better than the other way, because of the differences in the character of the problem. But he thought he could get away with the other way of looking at it, which was useful for piles. Anyway, this is some relation, yes, with Fermi. Well, sometime later — we were friends, of course, through all —

Weiner:

Are you going to leave Fermi now?

Feynman:

Just another little story, if you want to know something about Fermi.

Weiner:

I was afraid you were going to leave this other one out.

Feynman:

Well, it was always very friendly. I mean, there were jokes and everything else — these conversations and arguments. Another story of this kind is that I wrote some kind of report on something, and when he was visiting — this was later, I know this was later — he was visiting, and he calls up on the telephone. He says, "Hey, Feynman, I've been reading LADC 162" — whatever the number was — "this article of yours," and he says, "I wonder why you bother to print it, because couldn't even a child see that this result has to come out this way?" So I say, "Yes, if that child's name was Enrico Fermi." "No," he says, "even an ordinary child." It turned out at the end it wasn't as obvious as he thought, but anyway, that's the way we talked to each other. He's a very nice fellow. I was often at his home, and his wife made us dinner — you know we had parties at his home and everything. I liked him very much.

Weiner:

When did you have the opportunity of being in Chicago at his home?

Feynman:

I visited Chicago later, much later.

Weiner:

Yes, I have letters about that —

Feynman:

I was in Chicago to give some lectures about helium, and I saw him there then. Also he visited Caltech sometimes. If you want other things about him, related to that, we should put it in here.

Weiner:

Yes, I think it's appropriate. Just put in your reaction to him.

Feynman:

Well, in the lectures on helium, he wanted me to come to tell about the helium, and he listened to all the lectures to try to learn it, and he would ask appropriate questions, very penetrating and so on. I don't remember more. I was at his home, and there's a lot of pleasant stuff but I can't remember. Then, when he came to Caltech, I do remember one little incident. We were discussing something. He had a theory at the time about how when two things hit each other at very high speed in cosmic rays, there was some kind of a hot box made where everything got to thermal equilibrium and then it began to explode, a statistical theory of how many pions should come out in high energy reactions and so on. And we were discussing the criteria for its being right, and so on. Somewhere along the line he said, "Let's see, what is the criterion for the WKB approximation?" That's a technical point. I said, "You should know." He said, "But I don't remember," and he hands me the chalk — "Professor, so let's see —" And I couldn't get it straightened out, so I said, "The chalk, Professor." He took it back, "I think I know now," and he starts to explain, and he couldn't get it straightened out and he gives me back the chalk. And this went on for fifteen minutes. Now, that's a very elementary preposition that we always expect all the students to know right away, and we were both confused. Finally we caught on, and then of course it was obvious and we both felt very silly. The reason that this was important, to me, was something interesting. Whenever Fermi lectured about any subject whatever, and I've heard many, or talked about any subject whatever that he'd thought about before, the clarity of the exposition, the perfection with which everything was put together to make everything look so obvious and beautifully simple, gave me the impression that he did not suffer from a disease of the mind which I suffer from, which is CONFUSION! When I think about something, I go along in a certain way, and then I get balled up, and then I go back, and I think — I get mixed up easily. I easily get into confusion, which is the horror of the whole business when you're thinking. It's like building a pile of cards and the whole thing collapses, and you keep going and it collapses. But when he'd give a lecture, or when he'd talk about anything, up to that time — I had heard him, like I'd heard him give something new

about the hydrogen business I told you about, and he comes out clear — even when I was arguing with him about the piles, in what he was saying he was not confused. He was just not understanding what I was talking about. There was never any sign of confusion. And so I asked him about this afterwards. I said, “I’d got the impression that you don’t get confused.” He said, “That’s impossible!” I always get —” It’s just that I hadn’t realized. I thought he was so perfect that he didn’t have this difficulty of getting confused as he went along. But apparently everybody does. That’s what stops you from proceeding. You get balled up and forget and get mixed up. So anyway, I found him mixed up, just like me, at this same time. That’s another story—that the great Fermi can make a mistake, or can be confused about a simple idea.

Weiner:

Did you regard him as a theoretician, as an experimentalist, or what? How did you classify him in physics?

Feynman:

Well, of course, he was both, but I thought of him as a theorist all the time. That’s right.

Weiner:

How did you feel about him as to his relative position in physics? By this time, or even in Los Alamos, you began to know a lot of people. How did you regard him as to his relative standing in the world of physics?

Feynman:

One of the very great physicists. Yes. You must appreciate that I don’t put them in order. There’s no order, because their qualities are different, you know. I mean, each one has a way, and his was clarity of physical reasoning, which was his expertness, you see. So I don’t make an order. For instance, I couldn’t say which is the better physicist, Bethe or Fermi. That is to me an impossible assignment; Oppenheimer or Bethe, Pauli versus someone. They’re all very great guys. And Fermi to me was someone I loved to talk to, that I thought was marvelous, a very great physicist. Very great.

Weiner:

Was he in the group that occasionally walked together at Los Alamos, the group you mentioned yesterday off the tape?

Feynman:

Yes, that's right. I think maybe we met sometimes. We went on several trips, this and that. It's hard to remember, because, you know, different people would come in and out.

Weiner:

What did you do, hike in mountains there?

Feynman:

Yes, we'd hike in the mountains or we'd walk in the back woods. We'd take a special trip in the car to some mountain and hike in the mountains. Or else we'd just walk back around there in the canyons. It was a very delightful place. We'd just go off and walk in the canyons and mesas and everything else.

Weiner:

You relieved the pressures somewhat. You had time to do this.

Feynman:

Well, yes, you didn't work all the time. After supper, say.

Weiner:

OK. Now, what about Bohr? You started to talk about him.

Feynman:

I also met him when I was at Los Alamos, and as you know, he came under the name of Baker. I had heard of him—naturally, I had heard of him, and it was exciting when he was going to come. I did talk about Von Neumann, I'm sure, before.

Weiner:

At Los Alamos? Well, while you're talking about Bohr —

Feynman:

All right. Bohr came, and visited, and asked some questions and so on. Now, to be absolutely frank with you, you see, Bohr was an old man, and had come from the deep past of quantum mechanics. He was a great physicist and a deep thinker, difficult to understand because of his language. His English pronunciation was handled as though it were Danish or something like that, and not really, in my mind, so terribly profound,

possibly because I couldn't understand him. But when it came to the physics thing, it was not impressive, because these things were quite modern and quite out of his ken. So he wasn't too good, it seemed to me, about it. He would take me aside. Well, this is later. The first time he visited, I can't remember exactly, but there was some kind of conference. He was making remarks, other people were making remarks, and I made some remarks. And, as the usual phenomenon, I must have said something disrespectful or something, if you know what I mean. I just said, "That's not right." But anyhow, the next time he was due to come to Los Alamos, I got a telephone call early in the morning, 8 o'clock or something, getting me out of bed in the dormitory. "This is Jim Baker. My father and I have arrived last night at Los Alamos, and we'd like to talk to you." So I met them in the office that was given to him, up at the technical area, and I was the only guy there — just the two — me and Jim and his father. Jim helped to interpret for his father, because he was more modern-minded, but the father was pretty good.

Weiner:

Jim — you mean Aage Bohr?

Feynman:

Aage Bohr, yes. So it went something like this. During the several months that they were away, and having learned a lot of stuff at Los Alamos, they began to think of some problems. "Now, why can't we make the bomb, say, this way? Wouldn't that be a good idea?" I explained that that had been thought of first — I don't remember the way, exactly — and had the following trouble with it. It wasn't any good because of this and that. "Oh, then why don't we fix it by doing such and such?" In other words, they had a lot of ideas they wanted to discuss, and they just put one idea after the other, and I commented, "This is no good. This may be possible if you did this. This one's no good, that's this, that, and so on," until about 10:30. Finally the Old Man said, "Well, I guess we can call in the big shots now." And I talked to the son, who explained to me what happened. His father said — now, you have to check with the son, but this is what I remember — his father said, when they left Los Alamos the first time, "There's a young man there who doesn't hesitate to criticize me. Everyone else thinks I'm a Grand Old Man and they're afraid to say anything. Next time we go, if we want criticism, we must discuss our ideas first with him." So the second time they came they picked me out to criticize the ideas. They couldn't get criticism, you see — Niels Bohr couldn't get an honest critique of his ideas and he needed it, and he saw that there was some jerk there who was such a fool that he'd tell him exactly what he thought. Because of my disease, once I thought of physics I forgot whom I was speaking to. I didn't pay any attention to it. So that's the story, at any rate, and it would be nice to check with Aage. I haven't checked recently to see if I got the thing exactly right. It happened. I was of course quite surprised — "Why do you want me? What happened?" And I asked him, you see, afterwards, after they called in the big shots, "Why did you do this? Why me? And so he told me the story of his father. And his father and I, sometimes — once or twice, his

father would say to me, after supper when he was up there, "Let's go for a walk." And we'd go for a walk on the mesa top, and he would discuss some philosophical questions. The misery of this is, I didn't understand him, because of language. His language was so difficult. And so there might have been some very deep wonderful thinking. I might have learned a great deal, you know, and it was rather sad. I had to sort of mumble as if I understood, yes and no, and I couldn't act intelligent because I couldn't comprehend his speech very well. He had his pipe in his mouth, and Danish is hard to understand anyway.

Weiner:

I heard he had a speech problem in general, no matter what language.

Feynman:

Maybe. At any rate, I felt very uncomfortable on these talks, because I did not understand what he was talking about, and it was not because the subject was too difficult, it was because I couldn't understand what he'd say. Now, it turned out that I was one of the best understanders of him, generally, around, because I had talked so many times with him, and he had done this thing that I just mentioned, had these talks and so on. When he would give a lecture, which everybody would attend — and he did lecture on some of the history of quantum mechanics and other things to entertain — I would sit, and there would be a group of people around me, and they would ask me, "What does that mean, 'keston'?" I would say, "Keston mean Question." And so on. And I would translate. Most of the lectures I could understand, because they were more formal, but his talking directly to me about philosophy on the mesa top, I just couldn't get enough to make clear sense of it. So I felt uncomfortable about that.

Weiner:

Did you see him after Los Alamos?

Feynman:

Oh, yes. I've seen him at meetings and so on. He died recently, didn't he?

Weiner:

Oh, sure, in '62.

Feynman:

Yeah. Well, I saw him just before that, at some meeting in '61, in fact, at the Solvay

Conference.

Weiner:

You mentioned Von Neumann. I don't seem to find anything on that in what we covered before. If you start telling me, I can interrupt you if it sounds familiar.

Feynman:

All right. Well, it was nothing particular. You asked me about different personalities that were there. Von Neumann was another man that I met, that I had met before at Princeton, because he had been at Princeton. But he came to Los Alamos and he was very useful in giving advice and so on. I guess there's nothing more to say. I remember, sometimes when we went for walks, he would come. He was with us because he seemed to be a good friend of Bethe and Bacher too.

Weiner:

The walks we talked about — Bethe, Bacher, Smith sometimes when he would come down, and you.

Feynman:

Yes. And I just remembered Von Neumann was sometimes there. There was much personal life, and many things which I have completely forgotten, which is really bad, because there were so many kindnesses and so many wonderful people, like the wives of these men. They would make parties. They would try to keep us happy and entertained, and they would do nice things for the poor guys that were living in the dormitories, by inviting them to their homes for dinner, very often, and so on and so on. And I would play with the children of the various families. It was very good, and I appreciate it very much, but it's all in one great mass of confusion. I can't remember the individual people. I know — I don't remember people individually very well.

Weiner:

Well, this was a difficult period for you, because of your wife's illness and everything.

Feynman:

Well, that only happened near the very end. The illness wasn't difficult.

Weiner:

No, I meant the culmination. Well, let's — if you think it's appropriate — get back to some of the details on Cornell.

Feynman:

Things that I forgot to mention about Cornell. When you talk about different people, I would like to say that when we talked about Cornell, I said it was kind of an intellectual vacuum and this and that. I forgot to mention that there were these mathematicians, Mark Kac and a man named Feller, that I did have great conversations with, and we talked a lot. Mark Kac got very interested in the path integral business, and he interested me in a number of problems in statistical mechanics, like so-called “Onsager” problems and so on. So we had many conversations with him, and I didn't mention that. I've just looked over, thought about these things.

Weiner:

How about Feller?

Feynman:

Feller, too, but I don't remember too well. The other thing about Cornell, when you asked me why I left — it was because, besides the various things mentioned, it's isolated in a small town, and there isn't anything very wild, or — you can't quite get away, exactly. I used to go to some small place called The Traveler Hotel, or something, because in the evening they had dancing, and some students and other people danced to the jukebox. And I used to sit in a corner and get fried egg sandwiches and coke — not coke, it couldn't be coke — fried egg sandwiches and something, and sit there and figure, calculate, and watch them dance and calculate, and so on, and maybe dance with somebody, then sit down and do some more figuring, because I liked to do that. That's what I used to do at Las Vegas, too. I'd work for a while, then I'd play for a while, then I'd work for a while. But I developed, at the time when I was at Cornell, a desire to see the wilder aspects of life — that is, like what goes on at bars, drinking, and all that kind of stuff which I had never been really in contact with before. Then, Cornell got a laboratory called the Aeronautical Laboratory in Buffalo — some Aeronautical Laboratory that they'd bought or something. And then, probably as part of the contract, they said that they would give some lectures in physics to teach some of them, like I give lectures in Hughes now. They sent somebody to do it, but the guy was too mathematical minded and abstract, and they were complaining, so they asked me if I would take it over instead of this other guy. I didn't want to do it. It's all the way in Buffalo. You have to go by airplane one night a week. It didn't look like fun. But you have some loyalty to the university, if you're asked. So I said, “Ok,” and they said they'd pay me \$35 a night, besides the expenses, to do that. I had never spent any money before. I was born in the Depression and had never had enough money. I was always saving; I never thought to spend it wildly. But I had this great brilliant thought: I don't want to go to Buffalo.

They're paying me \$35 to compensate me for going to Buffalo. That's \$35 I wouldn't have ordinarily had. So what I'm going to do is, when I'm in Buffalo I'm going to spend the \$35 in the one night that I come there, one night extra, and try to see if I can't amuse myself in such a way that I'll enjoy going instead. So I'm not going to save this money for a change; I'm going to spend it.

Weiner:

This was a conscious part of your plan?

Feynman:

Absolutely, because — well, I was made that I had to go, and I thought of the \$35, and I suddenly realized that this was something I wouldn't have had anyway, and it was just directly to compensate, so why shouldn't I use it? It was the first time I thought to spend money for nonsense, to waste money, you might say, because I never had done it before. I was always very — what is it, parsimonious, or something. So I decided just to have a wild and wonderful time. I remember, the first time I got there. I gave my lecture, and I got a taxi to take me from the lecture place into Buffalo and to the hotel. I talked to the taxi driver, and I asked him where's a good bar that people are very friendly and it's kind of interesting, and he gave me the name of a place. Then he took me over there, and before he let me out of the car I said, "Listen — I don't drink. What is the name of a good drink to have? How do you order it and so on?" He said, "Whiskey. Ask for whiskey." I said, "Well —" He said, "You usually say how you want it, on the rocks, or with water and so on." I'd seen a lot of Western movies, so I said, "Suppose I want it straight?" He said, "Well, you can have it straight, but usually people have something on the side in case it's too much for them, like a glass of water. So you can say whiskey and water on the side." I said, "But usually you name the brand." This is the level I was on. "What's a good brand?" He says, "Black and White." So from that time to the end of my drinking career, I always asked, "Black and White, water on the side."

Weiner:

Black and white, that's scotch?

Feynman:

Scotch, yeah. Water on the side. So I used to take the straight scotch and then I'd take a little water.

Weiner:

Was he amused by this?

Feynman:

I don't know. He gave me his name. I still remember it, to show you how impressed I was — Markusso, his car number's 169 — in case I go again in Buffalo. He said next week when I come, to ask for that car and he'd take me around to other things, and so on. He got me a prostitute at one time, and so on. It was quite a time. So I had a very good and interesting time there. I went to this bar, and I found it very interesting, but I didn't understand it very well. There were very fancy looking women and men, and so on, and some of the girls had furs. So I sat down on a bench, and in a few minutes, a very few minutes (luckily), a very good-looking girl happened to come over and sit down next to me. I bought her some drinks and so and so and so on, lots of things. I got drinks. I got drunk. And when the bar closed up she just went away, and so on. Of course, it was one of these B-girls, but I didn't know what the hell was coming off. So I learned, slowly. That whole subject has interested me as a hobby, that is, how everything works in such a place. And it's always been a hobby, and it still is a hobby. That must have been about one semester. The last time I went — luckily it was the last time I went — I got into a fight with a man in the men's room, and we gave each other a black eye. So when I got back to Cornell the next day, I had a great big beautiful shiner. Of course they had fun with that.

Weiner:

Yes, I imagine there was a little talk on campus, probably.

Feynman:

Yeah. It was amusing.

Weiner:

Well, back to Cornell. You mentioned an encounter with the draft board.

Feynman:

Oh, yeah. Well, I went to General Electric the first summer, I believe, with Bethe, and we did some work — maybe it was the second summer. Yeah, it was probably the second summer after the war, if you want dates. The first summer I went back, I believe, to Los Alamos to finish some papers and stuff that I had not finished. I had run off too quick. The second summer I think I worked at GE, which is a little interesting. I'll describe the General Electric business. We worked there, and they told us we could do whatever we wanted, so we were doing some work in cosmic rays, some research in cosmic rays, Bethe and I together. They said you could do whatever you wanted. It was quite exciting. They had brought together a large number of fairly good people from

different places. But they had some problems, like to design a detector for the synchrotron that they had made. They were trying to seal synchrotrons at the time. It never worked out.

Weiner:

Was there a big enough market for them?

Feynman:

Well, the market, see, was expanding. I mean, the next synchrotron would be more energy than the one they designed. They didn't realize. They made a mistake in trying to get into the synchrotron business. You can't make a unit and sell it all around, apparently. At least, that's what they thought they could do, but it didn't work out, because people would re-design; want something different, and so on. Something happened. It didn't work. Anyway, they were doing some research there, and they were trying to become leaders in experimental research in physics, by having the highest energy synchrotrons and so on, see. I'll explain to you why in a minute, in my opinion. Well, I'll tell you right now. It's my opinion that the plan of the company was this — that they were going to get into atomic energy. In order to get into atomic energy they had to have knowledge and good scientists there to do this engineering work and to give advice when necessary. So they wanted to have a number of good scientists who had been at Los Alamos, say, and they realized that one way they might do it would be to have a good laboratory in high energy research, to attract such men. In addition, I believe, they also expected to sell synchrotrons. Anyway, they had built some synchrotrons, and they were talking about counters for it, and with this plan they could get a lot of guys to come in the summer time to this laboratory. We would worry about the design of a counter that would be independent of the energy of the gamma ray, would count the total energy in the gamma rays independent of the energy of each individual one, or something like that. So we worried. The people would come worrying us with counter design and other problems, in addition to which I was asked to give a series — I don't remember if it was a series or a few — lectures on the methods of calculating how much uranium you need for a pile, how a pile works, and so on, because I knew a lot about it at Los Alamos. So I would lecture. They were going to build nuclear reactors, so they had to know. So this is the way they found out. They got these scientists there, and they gave their engineers lectures and so on. So that's what we were doing mostly. We were doing cosmic ray research. We did a lot of extracurricular stuff for the General Electric Co. At the end of the time that I was there, I went into the office of the head, who was a man named Soups, head of the research laboratory, and he said — "I have talked to Professor Bethe about it —" and he talked about how he would like to have people at GE who were good, and what great opportunities they would have in this big laboratory, and everything was going to be duck soup and stuff, see. He said, "There are a few difficulties, because Professor Bethe made me promise not to offer you a position and a salary which, with good sense, you could not refuse." You see? He had

talked to Bethe and Bethe told him not to do it, but he was doing it, he was telling me that he was going to offer me a position which with good sense I couldn't refuse, but he couldn't tell me the details. So I simply told him that, "Inasmuch as I don't know what — you don't give me any details, I know nothing about it — it's very easy for me to refuse." But I wouldn't have gone there anyway, because I gradually caught on to this kind of thing, which happens in many industrial laboratories, or at least used to happen. A lot of laboratories tried this, which was: They had real engineering and technical problems of their own; they would woo the scientists there on the grounds that they can do whatever they want, you don't have to bother with anything, you've got your own laboratory, your own assistants, you do whatever you want. While you're working there and you're doing whatever you want, little technical problems come up that somebody asks you about. Now, most scientists have a feeling — or most guys under these circumstances have a feeling of a certain amount of loyalty to the company that pays their salary. They feel they're not doing any good to the company. It's true that the company, when they were hired, told them, that's our business, you do whatever you want. But they can't — for some reason, they don't believe that, or they feel they have some responsibility to do something useful for the company, not just fool around with their own problems. So they feel a responsibility to do these things, and in this way the company gets them to do it. But what happens is, the company can't resist, and they ask them more and more, until these fellows are completely involved, or nearly completely involved in company work, and they can't do their own work. And of course, it's their own fault, and the company says, "We told you, you can do whatever you want," and so on. And the guy doesn't know what to do, and he just gets upset and quits. I would guess that was going to happen at GE. That's what did happen, after several years, to many of the men who decided to stay there. They had to quit, and the reason they quit was, I believe, this sequence. I mention this because I think that this is a thing that has happened in many industries and many places. The Bell Telephone Co., for example, is not of this kind. They did have a laboratory that you could do whatever you want, and they did have a policy of research, to exclude that you should get involved with anything. There wasn't any force of this kind. Whereas the other lab, the GE I think, had a plan, purposely to do what they were doing. The Bell Telephone Co. tried to avoid that. And so in various research laboratories, it depends on what the policy is, and if the policy is really that you're getting these guys to do the other thing by enticing them with freedom, it doesn't really work. I mean, guys just leave after a while. I've always had a negative attitude to certain industrial laboratories, as a result of this. But laboratories have changed since 1950. That's experience.

Weiner:

With your Hughes lecturing now, and between now and the GE summer experience, have you done any work in connection with industry?

Feynman:

No, none. But I talked a lot to people who are in it.

Weiner:

I understand that part, but I mean, yourself.

Feynman:

No, I haven't, because of various experiences, and what people tell me.

Weiner:

Is it true that when people get involved in a company, in the work, that they get interested in it?

Feynman:

I know, but then they understand that that's not what they're really interested in.

Weiner:

They have a conflict.

Feynman:

They have a conflict, and tensions, and usually they either give up completely, or there's another tension that happens. If they get 100 percent involved in the company work, then another trouble happens. There's always some guy on top of them, the boss, who is dumber technically than they are, and who makes the decision as to what they ought to do, and whether what they do is worthwhile. And they see that the guy's making mistakes, because of his technical shortcomings. So they think (again, loyalty to the company, or the government or whatever there is) that it would be better if they were the head, because they could make the decisions better. And so they go into that position. And then they're in an administrative position. They started out wanting to do research, and then they're doing company research, and next they're doing administration. And they just get terribly upset, unhappy and frustrated. See, it's an interesting phenomenon. It's an interesting phenomenon because it's a sort of a one way street. You gradually, step by step, get enticed into this.

Feynman:

Is it recording properly? Is the needle moving? I can see it from here. You asked me about consulting. I'll tell about all the various consulting positions, in succession. Not

just this particular time. I said I didn't do any, but I remember some. The first one was, I was consulted on a problem of safety for nuclear energy peacetime plants. I was thinking of designing one at the General Electric Co. The reason that I was involved, of course, was because I was involved in the safety at Los Alamos. And so I felt, you ought to help out if you know about these things for peacetime use in business. So I was on a committee to worry about these problems. But after some time, the problems got of a kind that I didn't feel I could make a contribution to. You see, the questions would come. I remember a specific question — for example, if you put the plant further away from a population center (even if it's calculated safe something can happen — there's a small chance) what would happen would be less serious, of course, then if it's near a lot of people. That's perfectly clear. I'm just setting the problem up. The question was: was the difference in safety worth the fact that the people who had to go to it, had to commute so far? And that the people who were doing management would be further away from the plant, and this and that and this. Well, that's not anything I know a damned thing about. I mean, I don't know how hard it is to consult, to travel so far, how many men they have working, what they have. To heck with it. So, I mean, I can figure out what the neutrons are going to do, but I don't care about the other problems. I mean, that's for someone who can put 2 and 2 together. I didn't know how to answer the question. So the questions became questions at this level, for which, although they were important questions and somebody has to decide them, I don't want to waste my time worrying about such things. They're important things, but they're something I don't like to worry about. So I quit. Now, another time — the date I'll have to get for you — I got a letter from the Army, asking me to be on a new committee that they were forming, or a revamped committee or something, Weapons Evaluation Group? No. Not Weapons Evaluation Group. I'll have to find the letters and find out exactly what it is called. But it was something where scientists were to advise the Army, a scientific advisory group for the Army, for their problems. And they said that they had a lot of problems, problems in organization as well as hardware problems and so on, problems on how to administer and organize research in the Army, and would I help? I know that I know nothing about organizing and administering research.

Weiner:

Well, you'd had experience at Los Alamos in —

Feynman:

I didn't organize it. Well, fifteen guys. If you asked me to do a job, I'd do it in my own way, but I ain't going to tell some — no. So I figured that I forgot that, I had no business, I didn't know how to organize research, except by doing it myself. I couldn't tell somebody else how to organize it. So I wrote back that I'm a theoretical physicist and I don't know anything about these matters, and I didn't want to do it. So they wrote back, it is their experience nevertheless that theoretical physicists have as a learned — something — well, anyway, that they're smart, so they can do stuff like this. I don't know

how they put it, but they said that their experience was that theoretical physicists were useful to them, and they did have a way of thinking, and so on — flattering letter in addition to which, would I please come? And I wrote, "No." Maybe it was only two letters back and forth, but anyway. Then, somewhere along the line, I got a letter from the Secretary of the Army, which said that they would very much like to have me and it seems to be very important work that I would do, valuable for the country, and so on, and that they would make it this way, that I could come to the first meeting which was three days or something like that. Then I would really see how things were, and I could see whether or not I could make a contribution. Then I could decide in the end whether I wanted to continue this or not. Well, what can you say? So, ok. So I went to the first meeting for several days, and it was very very entertaining. The Army told me everything, all the weapons they had, and all the gadgets, everything and they asked me all kinds of questions. Then they had cocktail parties, meeting generals and so on; informal things. A general would tell me that what they need is a tank that uses sand for fuel, because it would be great if you could just scoop up the sand and turn it into power, because they could keep on going. The problem is refueling the damn things when they go too far, and so on and so on. They apparently thought that science could do anything. There probably is a way of solving this tank problem, but not by scooping up sand and making energy out of the sand. Anyhow, we then had these meetings. We would discuss a number of things, and every once in a while I noticed that the problems would come up, and I'd find myself talking about something like this — whether the research should be under the Army Intelligence division or under the Ordnance and Supply Department, or something like that. And I heard myself saying, "Well, it seems to me it would be this way, or that way and that way —" and then I realized this is absolutely crazy. You get into the feeling of it. You hear the discussions, and you get half-baked ideas, and you start to talk. So I knew, when I heard myself saying this, this is the end, because I can't do this. Furthermore, it was decided at the end of the meeting that the main problems that they would discuss — there were also technical problems, like how can we make a tank that would last without fuel, other questions, and others where I thought I could figure out what kind of research to do, and how to go about it, but that's all — I could do something about. But they said that it seemed to them that the problems, the technical problems, would be best left to the laboratories and so on. But the problem of general organization, like what department, would be more what the committee should worry about. When I heard that, I figured, that's the one part I can't do anything about, so I told them, "No." They were absolutely flabbergasted, because during the time, it was clear to me, they had put next to me a lieutenant or a general or somebody, next to me at dinner — the same guy, you know, all the time. We would go to lunch after the meeting, and he'd sit there and he'd say to me, "Wonderful remarks that you made. Very important contribution." And when I listened to what I said, I realized that for the problems that they were talking about, the man who ran Macy's would be the right man to do it, because he has the problem of logistics. When Christmas comes, how many should he order? They don't know how many they're going to sell. How do they get in? How do they get out? These problems, you know. I figured there's lots of people in the world that have much better talent for this business than I. Then I hear myself talking

about it, and then after, this guy was telling me that what I said was very important, very interesting, and so on. And I knew that he was just put there to flatter me. The more I did it, I laughed at him. I said, "You're crazy. If you think that's worthwhile, you have no sense." And I didn't go. But they were very surprised, because they thought all this time that they were convincing me. But I was convinced quite the opposite. From these experiences, I've therefore almost never accepted any kind of proposition like that, because I don't like the non-technical problems. They're not to my liking. I just don't feel good about them. So I don't have much to do with it and have never, therefore, done much consulting.

Weiner:

Well, let's go back to Cornell, then. We're still in the Cornell period.

Feynman:

Yes. There's one other story about Cornell that I would like to mention. Somewhere — and you have to look at history again — during these years, Schein discovered or claimed to have discovered the existence of artificially produced mesotrons, pimesons; probably pi-mesons, maybe mu-mesons. I don't know whether they knew the difference in those days.

Weiner:

No, pi-mesons was during this period; mu-mesons was the same that was later given to what Anderson and Neddermeyer discovered.

Feynman:

I know what they are. But I don't know at this time whether the mesons that Schein supposedly created at GE were supposed to be pi-mesons or mu-mesons, or whether, when he discovered them, in fact pi-mesons were known. Anyway, Schein discovered mesons (one or the other kind), made by the GE synchrotron, in cloud chamber tracks. The energy of the synchrotron was not enough to create a particle of this mass, so there was something confusing about the situation. The thing got in all the papers, and the General Electric Co. was delighted. And it was even in their advertisements. Bethe who likes to worry about such things, began to analyze the situation, and worked out formulas for the probability of seeing a curve — you see, the momentum was measured by the curvature of the tracks in a magnetic field — having the wrong curvature due to scatterings accumulating, by statistics, accidentally. He showed, by statistical analysis, that Schein was underestimating severely the errors in curvature due to scattering. In fact, Bethe worked out the important thing for cloud chamber work, then, which was the probability theory of the errors in curvature measurements due to scattering. He argued with Schein about the mesons and, one day, he went to General Electric to have the final

argument with Schein. He had all these formulas prepared and everything else. And he took me with him. I was there with him, and this was a wonderful experience, because I was just a young guy. It must have been rather early. We went into a room. In those days, the key instrument for looking at plates was some kind of projected light that would come up from below the plate. You'd look at the plates that they would take, pictures in the cloud chamber, and you'd have a light underneath so you could see the pictures. You'd stand over them and look at the plate. I remember this room. It would make a great scene in a movie, because the light comes up from below, you see, and all these faces, and the two great brains looking down, Bethe and Schein looking at this. And the smaller ones, Schein's assistants and so on, with their heads no so clear because they're further from the light, looking worried at the plate. And I was looking also at the plate, looking, and the smoke was rising in the room; you could see it in the light, and some on. Schein said, "We have lots of plates, and some of them are very good." Bethe said, "Let me see them." So Schein puts one plate on there, and Bethe says, "But look, the gas seems to be swirling. You see these other tracks over here? They're curved too, so that this extra curvature may have been swirling of the gas." "Well, we have another one." So he shows another one. This is a track due to something else, you see, and so on. On each plate, he noticed something the matter with it. Then he said, "And then there's the statistical thing." Schein said, "Yes, but the chance that this would be statistics, even according to your own formula, is one in five." "But we have already looked at five plates," said Bethe. And so on. This kept going. Then they said, "We have lots and lots of them." When they went to look for the lots and lots, none of them were really any good. They were all tendencies in the direction. What happens to it — this is interesting, this is the way science works, I found out — what happens to a person, when they believe something, is that they see a few that look like it, which are due to something else. Then they believe in the existence of the thing. Then all the rest of the things which are not good become corroborative evidence, no one bit of which is very strong, but seems to be in large amount. But the moment that you propose that it isn't true, all the corroborative evidence just disappears. I mean, it's just a very small business — it's just selection of a special plate, that looks like it's in the right direction. It is not strong. And so one can build up an argument and believe something, and think it has a large amount of weight, when actually if you go and look carefully at it the weight is very weak, and each item is weak and it doesn't add up to much. So, anyhow, Schein's objection finally was, "But on my plates, each one of the good plates, each one of the good pictures, you explain by a different theory, whereas I have one hypothesis that explains all the plates, that they are mesons." "The sole difference," Bethe says, "between your and my explanations is that yours is wrong and all of mine are right. Your single explanation is wrong, and all of my multiple explanations are right."

Weiner:

Was this a friendly type of exchange?

Feynman:

Yes. But anyway, that was the demolition of the Schein meson.

Weiner:

This is Marcel Schein?

Feynman:

Yes. That was the end of it. There were no mesons. The reason I tell you the story is the following. Very soon after that I was invited to Berkeley, California, by Mr. Lawrence, to come and look and visit the laboratory. So I ran off and did it. It was at the end of the year, the school year, and I had forgotten to hand in my grades, so I got really admonished (is that the right word?) by the authorities at Cornell for this. But I rushed off to California — and nobody knew where the hell I was because I forgot to tell anybody — to visit Lawrence, who wanted to show me his laboratory and so on. Anyway, he showed me the laboratory, and he took me down and I saw Caltech, and he took me to Laguna, to his home —

Weiner:

You mean you saw Berkeley?

Feynman:

I saw Berkeley. And he took me down south to show me Caltech. And then we went to Laguna Beach down here, to his beach house, where we went swimming and boating and so on. I knew Lawrence very well. We had quite a time with him and his family. I stayed with him for a while — some period, I don't know — and I saw Berkeley.

Weiner:

What was your impression of him as a person?

Feynman:

I liked him as a person. He's a nice man, a very good fellow. I didn't want to go to Berkeley ultimately, because I felt that what they would do is they would build more machines, bigger and bigger machines. He said, "No, no, we're going to stop building bigger machines." It's incredible. "And just do research on them." But actually they did both, which is the right combination.

Weiner:

What would have been your objection to the machines?

Feynman:

I don't want to build machines, I want to do experiments. Or think about experiments. I don't just want to build the machines. In those days, what they used to do is, they'd build a machine and then they wouldn't do very many experiments. They'd get involved in building another machine. Other people would build a machine like theirs, from their experience, and make use of it. That was those days. Nowadays they've finally become a leader, because they always had the most energetic machines but the sloppiest experiments. Somebody else would always have to build another machine of the same energy to do good experiments. But more recently, they've built the big machines — even though somebody else may do so also — and they do very, very fine work. But in those days, it was very quick experiments. In other words, what they hadn't learned yet was that when they build a machine, they have to build the instruments to go with it, instruments that are just as sophisticated as the machine, to make good measurements. They used to build a machine, a great machine, and then put a penny in front in the beam. You know, that kind of clever, quick, and dirty so-called experiments, but they were always quick and dirty.

Weiner:

When did all this change?

Feynman:

Gradually, with the recent guys with the newest machines.

Weiner:

In the 50s — since '55, do you think?

Feynman:

Anyway, I thought that that would continue, and I didn't want to stay there. I liked it where I was, but anyway I didn't go.

Weiner:

Who did you meet when you were there?

Feynman:

I wanted to tell — oh, I met people, I don't know — but I wanted to tell a story, an amusing story. They told me that they had discovered an anti-proton. So, I know it's impossible because it's a 384 MEV cyclotron with which they say they discovered the negative proton, and that's not enough energy. Same difficulty as with Schein. And you know, if you go to a magic show, and you have a half belief that magic is possible, then you don't understand a number of the phenomena that you see. But if you know it's impossible, then you keep working until you find the explanation. The same way, I knew it was impossible, whereas they thought it might be possible because we don't know enough. So I said, "All right, let me see." So they got their underlings. It wasn't Lawrence, it was somebody lower level. I don't remember who — like me, I was low level too — and it was the same situation as Schein and Bethe but, say, another notch below. It was very amusing; it was the same situation exactly. In order to show me the plates that they had, they had to use the same kind of instruments, same dark room, same group of people looking, only this time it was smaller fry, you know. So it was the same game, and I had learned exactly what to do from Bethe, on each track to find out what was the matter. And they had a very very similar situation. They said, "We have a whole lot of them," and then they couldn't find the lot when they were looking for them. They had good ones. Anyway, they had one. Finally. It focused on the one. It was the most beautiful track for a proton, for an anti-proton, you have ever seen — clean, clear, it curved the wrong way. But I know there are no anti-protons. It was one of these great victories; it was wonderful. See, I deduced that the only way that this track curved the way it did was because it must be a proton; therefore, it must be going the other way. You know what the negative looked like? The wrong curvature. It's obvious. I know it's a proton, so it must be coming from the other side. It is obvious that it has to be coming backwards through the chamber. So I say, "You must have some matter around this somewhere." "No, none whatsoever. This chamber is just a very thin glass wall chamber with nothing around it." "Well," I said, "I haven't seen the design, but it usually is necessary to hold the upper and lower plates together on such a chamber, to keep the pressure, you know." They said, "Oh, yes, we have four thin bolts, only so and so much in diameter, that hold the two plates, you see." So they had a plate there, and I said, "Well, right here" — I put my pencil down outside the picture — "there must be one of those bolts." So they got the drawing out, of the cloud chamber, and they put it over the picture, to fit, and I had my pencil right on a bolt. What happened was, the proton hit the bolt that holds the top chamber, and scattered backward. So I demolished the anti-proton before it was published, before any rumors had got very far. You see, they'd said, "We have others," and so on, but the whole thing just collapsed in exactly the same matter. They had been completely convinced by the one perfect track, and the others, they were hopeful-thinking, and they weren't really good evidence. And so it was easy to demolish the other ones, easier than it was to demolish this one.

Weiner:

It was about nine years before they finally came up with one.

Feynman:

Of course, because a negative proton can't be made with that energy. It requires 6 billion volts. But that was fun. I had fun because I'd learned — I'd learned, you see, from watching Bethe, how to think about these things. And I had become fairly good at judging experiments as a result of that.

Weiner:

Berkeley, then, was no attraction for you.

Feynman:

No, it didn't work. I didn't decide to go there.

Weiner:

On that trip, you did see Caltech.

Feynman:

Only in passing.

Weiner:

Just to look at the campus.

Feynman:

That's right, yeah.

Weiner:

But you really saw it later, when you went out in '50.

Feynman:

Yeah.

Weiner:

I see, for the six weeks —

Feynman:

That's right, yeah.

Weiner:

You went to Paris somewhere in that Cornell period, didn't you?

Feynman:

Yes. There was a meeting in Paris on physics, high energy physics or something.

Weiner:

Was this one of a series of international symposiums?

Feynman:

No. They hadn't begun yet, I don't think. I'm not sure. They had these things called the Rochester Conferences, but they were always in Rochester up to that time. This was another meeting, I think. It was not too long after the war. However, it was after I had worked out my theories of electrodynamics, to some extent, because people wanted to know what they were, and so I was given some time to explain them, which I did not successfully do. It's too complicated. I had too much stuff and I tried to talk too fast, and I talked English too fast for people who didn't speak English, and so on. Of course, I had a wonderful time in Paris. It turned out I had found out before I left that I knew one of the girls who was dancing at the Lido. I had met her at Las Vegas. And Paris is a great place. So, I watched rehearsals at the Lido, went backstage — you know, all kinds of fun. But aside from that, I was at this meeting, and there were discussions of different things and I tried to explain my work, but I don't feel I did very well. At that meeting, Ashkin had come I think from Berkeley. He was the other American, and he came to report on experimental results at Berkeley. He wasn't at Berkeley, but I think he was given the information that they had discovered a neutral pion at Berkeley, and that the mass of the neutral pion was somewhat lower than the mass of the charged pion. The difference in the mass could be electromagnetic, maybe, because of the charge. And so I made a very quick calculation on the back of an envelope, while he was talking, as to how much the difference in energy might be expected to be. By this time, I could do a calculation in two minutes — while he was talking about it — and it was the right order of magnitude. The electrodynamics, which stopped at a million volts or something, was changed. So when he got finished talking, someone said that this was too much difference to be electromagnetic. I got up and said, "No, the formula for this thing is —" boop boop, and "the cutoff of the mass of the proton would be just about right to give

this much difference in electromagnetic mass. It's quite feasible." I remember that particular thing. It's the only contribution I made that was worthwhile.

Weiner:

Did you get anything out of the meeting? You know, learn anything?

Feynman:

No. I can't remember. I don't get much out of meetings anyway. Well, I don't know — no, it wasn't the kind. I don't remember. Maybe I talked to somebody about some problem and it got me started worrying about it — you know these things. Like the pimeson, for instance, which I happen to remember. This was a new problem, and I knew something to do about it. Or some other problem would come up. I can't really remember.

Weiner:

Was this in the period when you were formulating the final stages of your electrodynamics?

Feynman:

Yes, so it was partly some of that, yes. But I don't know, exactly. Dirac was there, Pauli talked, they all talked. Then Pauli invited me to Zurich, so I went to visit him there for some short time, gave a lecture or something for a day or so. It was interesting to me. All this was interesting to me. I had never been in Europe before, but we don't need to go into that personal aspect, the fun I had in Europe.

Weiner:

Pauli you had met at Princeton, right?

Feynman:

Yes, I had met him originally at Princeton.

Weiner:

As a student.

Feynman:

Right.

Weiner:

I'll bring you back from Paris. Did you go to the Rochester Conferences in the 40s when you were at Cornell?

Feynman:

Yes.

Weiner:

What was it like? What do you think they accomplished? Let me just say something — in this period you had, it seems, the Rochester Conferences, which were larger groups, and then you had this small group of theoreticians assembling at Shelter Island.

Feynman:

In my mind they get confused. It's possible that one simply grew into the other, or grew to be so similar to the other that they got mixed up. But, of course, I know that at these other conferences, the information that was coming out of Berkeley, which was the place that had the highest energy, was this sloppy information that they were measuring. I remember one example of it, just for fun—it's just amusing. You see, we were always getting a telegram from Berkeley. It was always very amusing and characteristic to get a telegram with the latest data, you see. Anyway, Serber was giving a talk on the results from Berkeley, and all these things about pi-mesons and what they did and the cross-sections for doing this and that, and so on. This always seemed to me, and to most others, I think, as a big mass of stuff which we didn't know how to handle, and didn't know what to do with. It was just a lot of data, you know. It was — at least to me it was — just a lot of data, and I didn't know what to do with it. The cross-section of pi's on carbon, and so on. Any number was all right, if the guy was talking — any number. So we were all sitting around, and we were kind of tired, hearing all this stuff, and I was feeling sleepy, and so on, and everybody seemed to be sleepy. It was getting near lunch time. And Serber says that they bombarded carbon with something, and they made mesons, and that they made five times as many positive mesons as negative mesons. So Wenzel, who was completely awake, yells, "What?" And then everybody starts discussing. There should be a symmetry, there were as many protons and neutrons, and there ought to be some kind of symmetry. Plus or minus should be more or less the same, with some slight differences, because it's bombarded with a proton. But it should be more or less the same. I was so sleepy I didn't notice. But anyway, Wenzel says, "What?" And we got discussing. At this meeting everything, even the worst data, was discussed very seriously, you know. So lots of things are discussed, and this was another one on top of it. We'd been discussing all morning, and we were sick and tired about

understanding anything, and this was the last straw — here this thing was not understood either, you see. So we were discussing for a while, trying to see how that could happen. But everybody had different ideas of how it could be, and so forth and so on. I remember going to lunch, and Wenzel said to me, “Quite a morning. All kinds of data.” He says, “Of all the things, however, that we heard today, there’s one phenomenon whose explanation I do understand.” I said, “What’s that?” He said, “That there are five times as many positive as negative mesons produced.” That’s the thing we had the most trouble with. I said, “What do you mean? How do you understand?” “That’s simple,” he said. “It’s experimental error.” It turned out to be. It was, oh, I don’t know, maybe 10 or 20 percent extra, but not five times, it turned out later. That was the kind of stuff that was covered, you see. And we were worrying about all these little troubles, and parts of them were just errors.

Weiner:

At this time Cornell also started, I guess under Wilson at the time, a machine of some type.

Feynman:

Yes, we had a synchrotron at Cornell, but nothing much was coming out of it at that time. It was too low an energy at that time.

Weiner:

When did experimental results really start making a big difference in your work? In this period you were doing quantum electrodynamics, and this is not going to upset you too much.

Feynman:

No, it doesn’t bother me. I was trying to do mesons — trying to understand mesons — so I had work in trying to understand mesons, much of which is not published, and so on. So I was worrying about these matters to some extent. I had done a lot of stuff after the electrodynamics on meson theory, to try to avoid the perturbation approximation and so on, and I gradually became of the opinion that the meson theory, if you avoided the perturbation theory, wasn’t making any good sense.

Weiner:

This was in —

Feynman:

No, that was during a period of time, as time went on. I invented a number of methods to avoid perturbation theory, using the path integrals and operator calculus and what not and I got the impression, although I couldn't work anything out very rigorously, that if coupling was large the phenomena were very different than would be expected from the theory, and different from experiment. So I didn't believe it very strongly.

Weiner:

The only other correspondence I have here, the Fermi thing, was on the same general subject. I found another letter, but I see it's just another part of the same correspondence. So there's nothing I want to ask you on that.

Feynman:

I think that takes care of everything.

Weiner:

Just one thing, about the draft board thing — you know, that encounter with it.

Feynman:

You want the details? It's just a personal thing. I mean, I don't mind that it's personal; it just has nothing to do with my work.

Weiner:

Well, you say it's a well-known story told about you.

Feynman:

All right, I'll tell you the story, in its detail, and you can just throw it out if it's too — all right? OK. Well, this was after the war was over. They were still looking for people for the draft because of the Occupation forces and so on in Germany, and they changed some rule or something, that you had to take an examination even if maybe you would be deferred for occupational reasons or something like that. So I went to take an examination in Albany. I was working in GE at the time. In these exams you go from one booth to another. I don't know, you may know how these exams — look, you're in your BVDs. The doctors are all dressed nicely, and you go from one booth to the other. They suck blood in one booth and they do something else in another booth, and so on. Finally we get to Booth No. 13, Psychiatrist. In order to understand what happened, you have to understand my attitude to psychiatrists at the time. I thought they were kind of like witch doctors and that they were a lot of baloney and further, that they ask a lot of

personal questions that were nobody's business. On such an examination it's nobody's business — you know, I don't have to answer — and they're kind of fakers, and so on. Furthermore, I had just seen two moving pictures which had to do with psychiatrists that had made me very angry, you see. There was one of them, I think called "Spellbound" or something, in which a woman's hand is stuck and she can't play the piano. I think that was the story, or maybe it was the other one. She can't play the piano. She used to be a great pianist, but her hands are frozen. She can't touch the piano — it goes on through the whole movie. It's boring as hell, and at the end of the plot, she goes upstairs with the psychiatrist into a room and they close the door. You don't know what happens in there. Then, her family's talking downstairs, and finally she comes out, comes down the stairs dramatically — hands still stuck, hands still stuck, you know? She sits finally at the piano, lifts these hands up — still stuck — and it's very dramatic. Everybody's quiet. What's going to happen? She puts them down on the piano, and of course — latatatalatada! Everything's fine! Well, this kind of baloney, you know, I can't stand it. So I'm very anti. Ok? That's necessary to understand. Well, the Psychiatry Booth 13, had four psychiatrists behind four desks, set parallel to each other, one next to the other, with the psychiatrist behind the desk and a chair at the side of the desk for one to sit in, in his BVDs. In spite of the fact that they had four of them, there was a sort of a backlog, so they had benches in front for the waiters to wait. So we waited. While we were waiting, I look at these fellows, and I see more or less what's happening. A guy will sit down, and he has these papers with him on which everything is written — all this information, his name, address, and so on, on the front, plus all the other junk that the doctors have found out. And he hands it to the psychiatrist, and the guy looks up at him with a very pleasant nice little smile and a happy look, and then the fellow answers some pleasantries with another pleasantry, and they go back and forth a few minutes this way. That's all. Well, I decided, I don't care about these guys. I ain't getting friendly with them. I just don't want to be friendly. It's none of their business. I'm not going to be friendly, that's all. I mean, I just don't like it. So that was the attitude, see. So it's my turn. I get up there, sit down, and the fellow looks through my papers, and he turns to me and he says, "Hello, Dick! Where do you work?" Well, what the hell is he calling me "Dick" for? You know, he don't know me that well. You understand what I mean? So I just said to him, "Schenectady" — in a tone, in a sense, "what's it to you?" You know? So he says, "Where do you work at Schenectady, Dick?" I say, "GE." "You like your work, Dick?" I say, "So, so." You know, not a smile. I mean, I couldn't like him less, you know. Like a guy bothering you in a bar when you don't want him to. You're trying to shut him up. So the fourth question is a complete change, complete transformation. The attitude — the smile disappears — it's like a formula, you know. He says to me, "Do you think people talk about you?" So I say, "Yeah," and I tried to explain. I wasn't trying to fake it. I said, "Yes." I meant in the sense that my mother talks to her friends, because sometimes I meet the friends and they say, "Your mother told me that you were doing very well," and so and so, and I tried to explain — honest — you know? Then he writes something down. Then he says, "Do you think people stare at you?" And I'm all ready to answer "No" when he says, "For example, do you think that any of the fellows sitting at the benches are looking at us now?" So I figure, this fourth thing — there are about 12 guys

in the thing and about 3 of them are looking. Well, that's all they've got to do. So I say, to be conservative, "Yeah, maybe two of them are looking at us." He says, "Well, just turn around and look." So I turn around, and sure enough, two guys are looking. I say, "Yeah, him and him." But by having turned around and pointing, it was a little different from the other fellows, and other guys start to look. I say, "Now a couple of other fellows are looking. Now the whole bunch of them is looking at me." And this nincompoop — this smart, sprain of a nut, doesn't bother to turn around and find out if it's true or not. He simply writes something else down. He doesn't even look to see if it's the fact of the matter. So then he asks me if I talk to myself, and I admitted that I do. I don't know if it's characteristic of theoretical physicists — I doubt — but I do talk to myself when looking in the mirror and thinking, see. Incidentally, I didn't tell him something which I can tell you, which is I find myself sometimes talking to myself in quite an elaborate fashion. It goes something like this: "The integral will be larger than this sum of the terms, so that would make the pressure higher, you see? No, you're crazy. No, I'm not, no, I'm not!" I say. I argue with myself — "You're crazy. No I'm not." And so on. I have two voices that work back and forth. Anyhow, aside from that, he writes that down, and then he says, "I see you lost a wife recently. Do you talk to her?" I said, "Yeah, when I'm on a mountain all alone, sometimes I talk to her." "And what do you say to her?" I said, "I tell her I love her, if it's all right with you." So then he asked me other questions. He says, "Do you ever hear voices in your head?" I say, "No, very rarely." He says, "What do you mean, very rarely?" I said, "Well — rarely. Two or three times in my life." He said, "What do you hear?" I said, "Well, I have a situation —" I was very interested in it, and I told him a little bit. I said, "If I hear somebody talk in an accent very hard for a long time, when I'm falling asleep I hear that accent even when I can't reproduce it. So I pay attention because it's a peculiar phenomenon." It happened, incidentally, when I went to Chicago to find out how the atomic bomb worked for the people at Princeton. And for two days Teller was explaining to me about the atomic bomb in his Hungarian accent. Well, as I'd fall asleep, of course, I'd hear the Hungarian accent perfect. So that was an example that I had in mind. So he writes something else. Then he asks me if anybody in the family had any difficulties, mental difficulties or something. I say, "Yes, my mother's sister is in an insane asylum." He says, "Why do you call it an insane asylum? Why don't you call it a mental institution?" I said, "Because I thought they were the same thing." He said, "Just what do you think insanity is?" I said, "I thought it was a strange and peculiar disease of human beings." "It's no more strange," says he, "than appendicitis." So then we got off on an argument that with appendicitis, the details of the causes at least can be more or less elucidated, whereas the other thing is —. It turned out our argument was on this: that I meant by "peculiar" an interesting natural phenomenon, not well understood; that what he meant by "peculiar" was, it shouldn't be considered socially odd or unacceptable. And that was where we were arguing for a while, until I realized what the debate was about — that he meant it was like appendicitis; a person is just sick. Ok. So this went on for a while. Then he said to me, "How much do you value life?" I said, "64." He said, "Why do you say 64?" I said, "I thought it was a kind of a dumb question, and I tried to think, I don't know any way to measure how much I value life, so I kind of imagined that I was giving an

answer.” “No,” he says, “but why 64?” I said, “I just explained to you. It was just an arbitrary number.” “No, why didn’t you say 73?” I said, “If I said 73 you’d ask me the same thing. You’d ask me the same question. It’s hopeless.” I couldn’t get out from under that. He asked me lots of questions about that answer. He bothered me about that answer because I couldn’t explain it to him, because he couldn’t imagine that I should imagine that the question was stupid. That was too hard for him. And why should I think the question was stupid? And so on. And we had a long tussle with that one. It was a bad answer. It didn’t work. Then, I don’t remember all the questions, but I’ll give you some more, if you like. Is this all right?

Weiner:

It’s interesting to me personally.

Feynman:

Then he went through the business of hitting you on the knee, you know, and the jerk, and the eye, and the pupil goes down, and so on. And then he asked me to put out my hands. And this was the first time that I really did something purposely to make trouble, because in the blood sucking line, in some earlier booth, some guy had — just for a joke, kids were talking, guys were talking — said, “Do you know what to do when the psychiatrist tells you to put out your hands?” And he showed us a trick that was so damn funny it was wonderful. When he asked me to put out my hands, I knew I was so far under water by this time that it was hopeless, and this was the only opportunity that a human being would really have to do this to a psychiatrist, see. So when he asked me to put out my hands, I put out my hands — with one palm up and the other one down. You see? So he says, “Turn them over.” So I turned them over, both of them over, so still one palm was down and the other up. This part nobody believes when I tell them. It’s hard for people to believe when I tell them — but he did not notice that. No. Because he was looking very closely at one hand, to see if the fingers shook or something like that. As far as I can make out, he was peering very hard at one hand, I guess looking for sweat in the palm or something, you see. And he told me to turn it over. I turned it over, but he didn’t notice that the other hand was the opposite of that one. So that didn’t go over so good. Then it went on like this, and I don’t remember more questions except, at the end of the interview, there was a sudden shift again, and he looks at the papers. “Well, Dick,” he says, “I see you have a PhD. Where did you study?” I said, “MIT and Princeton. Where did you study?” He said, “Yale and London. And what did you study, Dick?” I said, “Physics. And what did you study?” He said, “Medicine.” I said, “And this is medicine?” He said, “Yeah, what do you think it is? You go over there and sit down!” So I went over and I sat down on the bench. He took his papers over to another, to the next psychiatrist, see. Another man, older, more sensible and so on. This fellow had a culprit there he was talking to. So he nodded, one minute, and it was obvious I had to wait for this other guy. So sure enough, when the first fellow was done, I was called over and the second man starts in. He went through exactly the

same pattern. He started out, "Hello, Dick. I see you were at Los Alamos. There used to be a boys' school there, wasn't there?" "Yeah." "Well, Dick, were there any of the old buildings from the school there?" "Well, there were some buildings from the school, but the government built a lot of stuff too." Then something "Dick" again, third question, I can't remember, but that was the pattern. Fourth question — change of voice, same idea, see — exactly the same pattern — and the fourth question was something I can't remember, but among the questions later was the question, "Do you believe in the supernormal?" So I said I don't know what the supernormal is. He said, "What, you, a PhD in physics, don't know what the supernormal is?" "That's right." "It's the stuff Sir Oliver Lodge and those people believe in!" I said, "Oh you mean the supernatural?" He said, "You can call it that if you will." I said, "All right, I will, but I don't believe in it." He said, "Do you believe in mental telepathy?" I said, "No, do you?" He said, "I'm keeping an open mind." So I said, "What, you, a psychiatrist, keeping an open mind?" I had fun with him. Then he asked me details about the voices that I hear as I fall asleep, and I try to give in much more detail that it was very, very rare, and that because I'm scientifically inclined, I noticed it. And I went through all this stuff, and that it was only when the accent was very strong and for a long period of time. And I said, "Doesn't everybody have something like that once in a while?" And he put his fingers over his mouth, you see, and you could see the smile through the holes between his fingers, this superior smile. These guys just get — they're so damn — what do you call it? Pat. I mean, they're so convinced of themselves. They don't have to consider the possibility that they could be wrong.

Weiner:

Smug.

Feynman:

Smug, yeah exactly. I'll tell you what that was about, so you'll understand those questions. The other fellow had written that I talked to my deceased wife, so he was trying to find out whether I believed in the supernatural. I mean, this is my interpretation of it. Or in mental telepathy, because if I believed in those two things, it wasn't insane to talk to my deceased wife. If I don't believe in them, I'm really a nut. See? Anyway, this fellow starts in, and I couldn't help but tease him at the end. I loved to tease him, because he said to me — one of the questions was, "Do you consider yourself different or peculiar in any way? Different from other people?" I said — and I had to tease him, because I just couldn't stand it — so I said, "Oh, I don't consider myself different from anybody —" just opened another hole, you know. "Well," he said, "in any way do you consider yourself, somehow or other that you don't behave like others," and so on. "Well, I wouldn't —" and so on, he dragging it out of me and me holding it back. Finally I said, "Well, yes, when I go to parties, I get wild like I'm drunk, and have a very good time, as if I have a lot of — when I don't drink very much." So he says, "Does anybody ever tease you about this?" I said, "Well, I wouldn't say they tease me," and so on. You

know, this same thing, this kind of a game—I drew it out a long time, you know, and finally admitted that they call me “Two Beer Feynman,” because it only takes me two beers to get drunk. So he wrote something else down. And then he gave me the papers. Then I went off to the next booth, where you jump up and down to see if your ankle bones are OK or something. I had fun. I had a lot of side things. I looked at the list, though. On the front it had “D”, for psychiatric, and “N” for everything else. “D” was deficient, “N” is normal. I looked on the inside to see what this fellow wrote, and if I’d seen it written and not known the situation, I would have believed it myself. It starts out: “Thinks people talk about him. Thinks people stare at him. Talks to self. Talks to deceased wife, died June, 1946. Hypnagogic hallucinations.” That means generated by sleep, I presume. Something like “peculiar stare.” I think it was probably when I said, “And this is medicine?” I don’t know. The other fellow I couldn’t read — oh, “Maternal sister in mental institution,” you know. It looked good. When written in a technical jargon, it sounds so much more powerful you know, than “my mother’s sister is in an insane asylum” — “maternal sister in...” So then the other fellow writes, and he must have been more important, because I couldn’t read his writing so well — it was scrawled, not listed so neatly, and I couldn’t read it all. Probably something he said like “gets drunk on two beers,” but one thing it did say — “auditory hypnotic hallucinations confirmed,” or “auditory hypnagogic hallucinations confirmed.”

Weiner:

You hear voices.

Feynman:

That’s what it means, auditory — hypnagogic, at the time you’re going to sleep — hallucinations, voices that aren’t there. He confirmed it. Well, ok. Well, anyway, any guy with that disease confirmed is really in trouble. But I still thought that these guys are kind of, you know — I mean, nobody believes in this crazy joke, and good practical men don’t pay much attention to it. And at the end of the thing, there was a good practical man. There was a military officer who was hard as nails, and was trying to drag the bottom of the barrel, because they just needed them for occupation forces and it wasn’t so important any more. He’s the guy who decides that you’re not in or you are in. The ear doctor says, he can’t hear out of one ear, and this guy decides, therefore, he shouldn’t be in the Army, or it’s not enough to make it serious. So he was the final arbiter. And he was very careful. He talked a long time to everybody. They guy ahead of me had bumps in the back of his neck. Something’s the matter with his bones sticking out. The fellow doesn’t believe it. He’s got to feel the bones, ask lots of questions, make sure how serious is this thing, you know. So I figured, ok, with him I’ll explain, I just didn’t want to get friendly, and this is what happened. So I hand the stuff to him. He opens up the paper. He puts his head down to read it. He doesn’t look up. As he reads it, he puts his hand out for the rejection stamp without looking up at all, stamps the thing “Rejected,” and hands me the paper still looking straight down, and does not say a word. That’s all.

Not a question, nothing. People are afraid of that, you know.

Weiner:

He didn't look at you?

Feynman:

Didn't look at me, didn't talk to me, didn't ask anything, didn't say a word, and didn't try to find out if maybe it's wrong. Like if the doctor says, "This guy's got bumps sticking out," you do something. But he says, "He's a little bit nuts," you're afraid to ask questions. Very amusing. The only thing that bothered me after that was that during the war, my draft board was getting letters saying that this guy's important. He's doing research in physics; we need him, we need him, we need him. Now they're getting letters saying, he's teaching scientists at the university. It's important. He's teaching these scientists. This is very valuable, and so on. Now all of a sudden they get a thing; he's off his rocker. One natural conclusion might be that he tried to fool the draft board because he got scared. You know? So I was worried that I would get into some kind of difficulty. At least, I was worried about it. So I wrote my draft board a letter which ran more or less as follows: "Local Board No. 1: Dear Sirs: I do not think I should be drafted because I am teaching future scientists, and it is partly on the strength of her future scientists that the national welfare lies. If you do not consider this sufficient reason to defer me, you may still wish to defer me because of my medical examination, in which I was found to be psychiatrically unfit. I do not believe that any weight whatsoever should be attached to this examination as I consider it to be a gross error. I am calling this error to your attention because I am insane enough not to wish to take advantage of it." Actually, it wasn't quite that beautifully done. That's the outline. I also included in the letter an explanation of why I thought they made an error or how they made an error. Just so it wasn't just that clever. But that was my original intention, just to write that. But then I thought, to be honest I should write a P.S. explaining why I thought it was in error.

Weiner:

You thought you needed to set the record straight.

Feynman:

Yeah, I did. I told them why I thought it must be an error. But their response to that was to send me a card marked "4F." No questions asked. So that's what happened. So that's how it looks to society when the scientist meets with society. And they're always talking nowadays of wanting to show that — you know I'm human like anybody else. If I were human like anybody else I would have passed the medical examination!

Weiner:

Throw that in their faces.

Feynman:

Yeah.

Weiner:

Well, do you want to stop the tape for a minute?

Feynman:

Yeah, I do, because I'm getting tired.

Weiner:

All right, we're on again, after a brief break for lunch.

Feynman:

You'll make the people hungry who are transcribing the tape.

Weiner:

You should tell them what we had, and then they'll have a prejudice, now. Before we were sort of backing up and filling in the chronology of events. We had gotten up to accepting the offer at Caltech, and in your own mind making it clear, during the Brazilian stay, that you definitely would follow through on your acceptance. After 10 months in Brazil, where did you go; directly to Caltech, or to New York?

Feynman:

I'm mixed up. Yes — Brazil must have been 10 months. But I'm mixed up, I don't know.

Weiner:

Well, let me tell you what you said.

Feynman:

I know what I said, but you straighten it out because I've got another fact which confuses me. Somehow along the line, I'd been traveling west all the time, trying to get to see the west. I always got stuck at Las Vegas. And I wanted to see Los Angeles. So one summer I arranged to get myself a job working at the Institute for Numerical Analysis at UCLA. And after I got the job, I think, I got the job at Caltech for the winter after. So it was unnecessary to go to Los Angeles for that summer. But I had already made the arrangements, so I went. I took my mother that summer on a trip to Mexico, and then ended up at this Institute of Numerical Analysis for the rest of the summer. She stayed in Westwood Village. I'm just trying to think, because I can't figure out which summer that was. At the same time, I thought that I was in Brazil for the summer, for 10 months. But 10 months isn't an entire year, so there may be some way of figuring out from that. I don't know. I worked at the Institute of Numerical Analysis, and then I started working at Caltech.

Weiner:

Possibly it was before you went to Brazil for the 10 months. It may have been, at the time you went you had decided — Caltech said, "We'll send you for a year" — you weren't going to do anything with that money till after the year was out, to determine whether you'd come back to Caltech. And that was after you'd been here for six weeks?

Feynman:

Maybe the Institute was a job I took, and then got this opportunity to be here for six weeks.

Weiner:

Yes, I think so.

Feynman:

So therefore I was disappointing my — yeah, that's very likely. That's possible.

Weiner:

And then the following year you were in Brazil. Now my question is, if it's at all important, did you go from Brazil directly — from Rio directly to Caltech, or did you come back East?

Feynman:

I don't know. Sooner or later I moved from the East to Caltech and started regular

work, living here, working here.

Weiner:

This was in '51, I gather. When you got here, did you have a teaching load the first year? How did this compare to the Cornell teaching?

Feynman:

Well, it wasn't much of a load in either place. There's usually one or two courses, I think two courses.

Weiner:

These were graduate courses?

Feynman:

Yeah. So it was no particular problem. I don't remember what I taught.

Weiner:

Whom did you get acquainted with on the faculty?

Feynman:

Everybody. Somehow or other it isn't very interesting. I don't remember any excitement associated with the first few years or anything. It seems to me I've got nothing else to say. I just did my work from then on, and everything has been quiet and pleasant. I'm trying to remember.

Weiner:

Where did you live?

Feynman:

I lived in a house behind Mr. Ward, a professor of mathematics at Caltech. He had a small house behind his home that they had made for some grandparent who had died or something like that. I lived in that little house. It was a nice little place, very near to the campus, within walking distance. I used to live there and walk to the campus. But I guess I did nothing during that first year, as far as I can remember.

Weiner:

There were papers published, but they may have been follow-ups of the preceding period. One was a paper from the second Berkeley Symposium on Mathematical Statistics and Probability in 1950. It was published in 1951, "The Concept of Probability in Quantum Mechanics."

Feynman:

Well, I think that I went from Caltech to that Berkeley symposium.

Weiner:

Yes, that helps fix some dates.

Feynman:

I was just invited to that by, I think, Cox. It was just a symposium on probability, and they said they would like some discussion on Quantum Mechanics. When I had lived in the Telluride House, the boys had some kind of a thing on Wednesday night, in which each person gave a lecture on some subject to the others. This was a sort of party, and they asked a faculty member also to do it. That was me. They would suggest a topic that they were interested in, and one topic was, "What is all this stuff about waves and particles?" So I invented a half an hour or 40 minute speech or something, which was the condition of the thing, to explain this puzzle of waves and particles to intelligent creatures that didn't have physics background. And I just developed that same thing, that same speech, a little further at the Berkeley Symposium. You'll find that's Lecture 37, fundamentally, in the Lecture Notes on Physics that I later gave. Really, way back then, I gradually understood what the fundamental character of quantum mechanics was. It starts from way back then. As a matter of fact, I'll just take this small opportunity to say — if I forget to say it later — that I've often been challenged about speeches. I'm often challenged to give a speech to intelligent but not very highly trained people, on one or another subject in science. For example, my first wife asked me what relativity was, why is the time shifted. And I invented a way to explain why the time was shifted in a moving space ship, rather than still, that even an untrained person could understand. That appears in a lecture on relativity — the moving clock business — in a lecture on relativity in these lectures on physics. This other thing I'm talking about was on quantum mechanics, explaining the principle. I liked that kind of problem, and I worked very hard. Then, when I first came out here, there was a series of lectures called Friday Evening Demonstration Lectures. Professor Watson was in charge of it. He told me that people had written in a number of times suggesting topics, and one suggestion was, the relation of the mechanics of Einstein to the mechanics of Newton, and would I like to give that lecture? So I prepared a lecture on relativity for that purpose, which is one of the lectures, essentially, in that Lectures on Physics. After I gave that lecture, someone

else wrote in that this man seemed to be able to explain more or less direct things like that, but how would he deal with a thing that's abstract, like the conservation of energy? How do you explain that, describe that? So I was challenged to give a lecture on the conservation of energy, and that appears in those things. I'm trying to explain that the Lectures on Physics which are given here were really the result of an accumulation of challenges, which I tried to answer in popular lectures in different places.

Weiner:

This was the first opportunity you had to give them continuously?

Feynman:

Well, I used them. It was the other way around. I didn't want an opportunity to put them in continuous form. I tried to teach, and I had the good fortune of having a number of them already prepared. You must remember that those lectures were given two a week, and to prepare two lectures a week of this character was hard work. Anyhow, the Berkeley Symposium is not an important paper of any kind. It's no research whatever. It's just a discussion of physics — quantum mechanics. It's just one of a large number of lectures of this kind that I gave that happened to have been recorded.

Weiner:

I see. Well, then, in '52, there's no publications except the one with Laurie Brown. We talked about that before.

Feynman:

That was some Cornell work.

Weiner:

The next publication is in '53, "The Lambda Transition in Liquid Helium." That apparently begins a new series of papers.

Feynman:

Yes.

Weiner:

What got you interested in that?

Feynman:

Professor Cox. I remember doing some unpublished research which I would like to mention. They had found these strange particles, these peculiar "Hooks," they called them here and V particles and so on, in those days. The cosmic ray people had found them — Leighton and Anderson and so on. And there was much discussion of them — they were discovered in other places, too — and what it was all about. And I had a number of experiences with them on these things. One of the more interesting ones was that Leighton asked me some questions about the particles or something, and I said, "Listen, Bob, we don't understand these particles, why they should exist or anything about them. So theory is not really able to say anything. Now, you might find people saying things, but on no basis, because they're completely unexpected and there's no way to say anything about them." It was only about that night or the day after that I began to think about it again, and I realized that there was a paradox of some kind, unless these things were made in pairs. In other words, when one strange particle was made, another had to be made along with it. So the next Thursday or something Leighton was giving a seminar, research conference talk, and I told them this fact, and he laughed at me and said, "I thought that theoretical physicists couldn't say anything about it." I said, "Well, I was wrong." And after he had given his talk, I was given 15 minutes to explain this thing. I remember distinctly saying, "I am sticking my neck out. These things are produced — when one is produced, another strange particle of some kind, maybe the same thing or something else, is produced along with it." They had no evidence of this. They claimed that if that were the case, they would see more plates with two such things on it that they didn't see, so they were arguing against it. And at that meeting, Willie Fowler suggested that maybe it's like radioactive decay. He resolved the paradox another way — because the energy was different, the rate of decay was different. It's technical, rather. There may be some kind of barrier to disintegration, like there is in radioactive decay. I thought about that for a while and I realized that it might be possible. When Fermi came, I discussed that with him, and he suggested the barrier of centrifugal force or something. We thought about it. I don't know whether he suggested it or he liked that idea or something. And we gradually realized that another possibility was that, instead of their being made in pairs, it might be that they were objects of high angular momentum. That's another alternative. But in fact, they were made in pairs, as was discovered later. I remember that was very early. Just before Fermi visited, I guess. I don't remember this in terms of dates. But that was interesting because I remember predicting, claiming this, and they said that they didn't see it in the plates. It's too bad, because they did have plates with more than one on them, about that time or later, that could have shown they were on the right track.

Weiner:

This was unpublished. How did the explanation, then, become effective?

Feynman:

Others thought of it.

Weiner:

And published it?

Feynman:

Yeah.

Weiner:

That's curious. Is there any record of your paper on this?

Feynman:

No. I didn't make a paper. No, I just talked. I just said so, in the colloquium. Then, another stage in this game — I'm just saying this to show the kind of thing I was doing at the time, all right? Another stage of this game, they had a lot of data accumulated for the disintegration of what was called a V meson, which is now called a Lambda meson. And other people thought that they saw that in the disintegration, the energy was always unique; the sum of the energies of the disintegrated fragment, which is now known to be a proton or pion, always added to the same amount. Leighton and his group, however, found that it was distributed, that it wasn't always the same amount. So there was some discussion about the question. So I went down to Leighton's office and I looked at the plates, and said, "How do you measure? How do you do this? How do you do that?" And he explained how you measure ionization, and how you estimate the accuracy of it, and all this other stuff. So he went out, and I stayed there in the laboratory and measured all his plates by myself. I compared the ionization to the ionization of other protons on the plate. I realized, by looking at different tracks, that the illumination intensity was not equal all over the plate, on certain pictures, because of the lighting, and therefore they would underestimate the darkness of the track, and so on. I went through all the things, over an evening, and concluded that the smallness of the errors was exaggerated by the other people. The errors were, as far as I could see, bigger than they thought. I wrote a letter to Bethe, which I probably never mailed but I have a copy of, in which I told him that in my opinion, it is still possible that they disintegrate only into one energy, and that the arguments of Caltech to the contrary are not good. I felt kind of proud. See, Bethe had taught me how to evaluate experiments, and of course it turned out in fact that the energy was unique and that the statements from Caltech that they were not unique were erroneous, and due just to the things that I had noticed. I tell you this because of pride in doing something that was a little out of my usual line.

Weiner:

This was the first time that you really did the technical work?

Feynman:

It wasn't so technical. It was easy. First, they said this was twice normal ionization, or once normal, and I would estimate it and compare it and look carefully at the plates. I just went through the plates to make sure. And I'm usually pretty fair at judging the experiment, whether it's really OK or it's exaggerated accuracy and so on.

Weiner:

The cosmic ray group here, Leighton, Anderson —

Feynman:

I talked to them a lot, and, you know, kind of gave half advices and so on.

Weiner:

Were you considered sort of theorist-in-residence?

Feynman:

So to speak, but not only that. I mean I would do it once in a while rather intensively — just look at everything in detail, then go away and not do anything for a while, you see.

Weiner:

Does this continue, by the way — the idea of being called in by different physics groups?

Feynman:

Oh, yeah. Often people would talk to me about all kinds of things, you know. It was perpetual. I don't know where all the time goes. You can't ever find it, but I'm always yakking to somebody about something. I can only remember the times that came out pretty good, you know. So I have this letter, I remember, to Bethe, because I kind of felt that it was important. He was worried about it. And I don't think I mailed it.

Weiner:

Why didn't you —

Feynman:

Oh, I don't know, I don't get around to it — writing is for me — you know, I didn't get around to it. I don't know.

Weiner:

I should think that after you went to all the trouble of writing it —

Feynman:

Well, I went to the trouble for my own interest, really.

Weiner:

To express your ideas, you mean.

Feynman:

No. Not to write it — to write it, I don't know why, maybe — I don't know, I can't tell you, I don't know. But mostly I did this for my own interest, and the question, is it or isn't it? And they say it isn't, while the other guy says it is. There's a puzzle, I've got to find out, are they wrong or are we wrong? I decided we were wrong.

Weiner:

There's a lot on this generally, and probably still is, in the background, that will not show up in published data.

Feynman:

Oh, yeah, there's a lot of it. I do an awful lot of that kind of thing.

Weiner:

Was it done in an organized way, in terms of colloquia, too, as well as just sitting in or being called in when they had a problem?

Feynman:

Well, they don't just call me in when they have a problem. I mean, they tell me about something, and I ask them questions, you know. I hear about something.

Weiner:

You involve yourself.

Feynman:

Yeah, yeah, sure.

Weiner:

Were there any weekly colloquia that you would attend?

Feynman:

Yes. Every Thursday we have a colloquium, a general thing on all different aspects of physics, depending on who's talking about that. Outsiders come, insiders talk, and so on. But that was another one. There was another amusing one of the same kind. This was fund, too. A student of Jesse DuMond from the X-ray department was talking about researches he'd done on a very new instrument that Jesse had designed, for very very accurate gamma ray measure. It measures energies of gamma rays by crystal reflection, which was a different way and very accurate. And he was describing the gamma rays which came from some lead nucleus. And they were quite complicated, many levels and so on. Then he showed the energy level system that this nucleus would have to have to produce these lines. Well, I looked at it, and it was kind of nuts. It had levels like 100 kilovolts apart, which is all right for a nucleus, but then each of those levels was split very fine, like — well, I can't remember now, but maybe 1000 volts apart. See, like three levels up there a thousand volts apart, and three levels at the bottom, or four, at a thousand. And that's just crazy. I mean, it's too small energy differences, too many levels, for a nucleus. So I said to the guy, "Do you mean those are really the energy levels of the lead nucleus?" So he says, "Yeah." And I say, "That's can't be. There must be something else. It's something to do with x-rays, its electrons something, it's x-rays," and so on. See, Bacher had just said, "Our colloquia are too stiff. We should have more arguments." So I raised in kind of a humorous fashion, "Don't be absurd," you know, and all this kind of thing, to do what Bacher said for the fun of it. And Jesse DuMond was there when Bacher said this too, so for the fun of it he livened it up and said, "Those are not x-rays! I know x-rays!" he says, "and if those are x-rays I'll eat my hat." I said, "All right, I'll say it — I know nuclear physics, and if those are nuclear levels, I'll eat two hats!" We had lots of fun, everybody laughing, it was great. But that night, I went into Jesse's lab. He gave me all the graphs, the curves and the data. I looked over all the data carefully and finally decided what it was. Each line was multiple because there was some scattering; it turned out, from the jaws of the slit. I could identify L-lines and K-lines and so on of lead from the jaws of the slit — a complication. What he thought would be a line was a multiple image, because of reflections and scattering in the jaw of the slit, and he was interpreting the reflected images of other lines, and it wasn't. So I

figured it out by looking at all this stuff, and measuring and figuring and calculating and noticing some coincidences of the splittings, until I unraveled the puzzle. So that was another one where I had fun interpreting an experiment. I've had some experiences in interpreting experimental things. But that's not very important. I just mention those things, because you want to know what I'm like and what I enjoy doing. And you know, these little successes are fun. You beat the other guy out, you know what I mean? It's fun. And so on.

Weiner:

Let me just —

Feynman:

Not just to beat the other guy out, but to find out what the hell's going on.

Weiner:

In '53, sometime in '53, you went to a conference at Tokyo. Was that anything of any special significance?

Feynman:

Before that I had worked on the helium. I had started the helium. The way I got started on the helium is, I think — in fact, I'm pretty sure — that Cox had come here to give some lectures, and was lecturing as usual on the Onsager problem. Now, I must say that I had invented a number of methods, the path integral method and also the operator calculus, and what I would almost always do, when I heard a new problem of complexity in quantum mechanics or another field, I would wheel up these two machines, so to speak, and try to see if I couldn't apply them to these problems. So I was working on the Onsager problem with some crazy business with the path integrals, and was fiddling around. Incidentally, I got married in the meantime, to my second wife, yeah. I was fiddling around with it, and got kind of stuck and confused, and found out that I had made a mistake. I thought I was making progress, but I was mixed up. There was an error, and the method wasn't going to work. But, I said to myself; this method doesn't work here, but it probably could be useful on the helium problem, the question of how the helium 4 transition comes about.

Weiner:

Had you been concerned with the helium problem before?

Feynman:

Well, not directly, but it was one of the well-known puzzles of man. And I understood the puzzle wrong. When I was at Cornell, some guy, Kirshner or somebody, a young man, gave a lecture on the helium problem, the helium 4, and we were told then that there were two theories. One was that it was a consequence of quantum hydrodynamics or something, and the other was that it was analogous to the Bose condensation of gas. London's idea had to do with the statistics of the helium, that the wave function must be symmetrical. The other was just that it was some general quantum mechanical thing of any liquid. The one was due to Tisza and London, and the other was due to Landau. Landau's was the view that it was a general property of liquid. That's what I understood. I didn't know that through the years people had resolved themselves, and Landau, too, and people had changed their minds. They were convinced that it was due to the Bose transition, whereas at the time that the lecture was given by this guy, I got the impression that people believed it was due to general quantum hydrodynamics and Landau, and I thought, that's nuts. It seemed to me perfectly obvious that if a gas has a transition, a liquid's going to have a transition. It's got to do with the statistics. So I thought that Landau's hydrodynamics was not worth much, and that that was the problem. And that the problem was to show that in the liquid there would be a transition analogous to the transition in the gas. So I worked out and proved that there would be — to myself. And I think others. But it was my physical argument, of a kind which is not popular and which is not understood because it takes too much work and is not rigorous mathematics and so on. I showed that transition will occur in the liquid in an analogous way to that which occurs in the gas, although perhaps in a different form for the curves of the transition and the specific heat. Therefore I had understood the transition in helium. I didn't know that in the meantime people were convinced on the statistics, partly because they'd even looked at helium 3 and didn't find the transition, and so on. So I was working on a problem ten years — no, not ten years, but seven years — older, and in the meantime people's opinions had changed. So it didn't look like much to them, but I thought it was a big deal. I made some success. So of course I was in the problem now. You always want to get more out. The liquid helium had a number of strange properties, a flow and everything else, and now that I knew why the transition occurred, I ought to be able to see why the various properties were different. At that time, someone came into my office and told me that Landau had a theory that there were these rotons and that the formula for the energy of a roton was a constant plus p minus p_0 squared, over 2μ , where p_0 was a constant. And he at first proposed the same formula with p_0 equal to zero. I thought, when I looked at that thing, that that's crazy, that p is a vector and to subtract from the vector — I said, "You mean the magnitude of the p minus p ?" Unsymmetrical, lopsided looking, crazy formula — Landau is nutty. So I didn't even pay attention to that. But then I read a paper by Tisza and then a paper by Dingle. You know, the only papers that I read. The paper by Dingle gave a summary of Landau's theory, not in terms of the theory, but if the theory were right what would be the consequences, to show certain connections between the statistics and the model of the excitation. So there were certain things I didn't have to work out. I just had to find the theory of the excitations, what the energies of the excitations were in the liquid, and then the rest of it would come out. I had also read a

few little reviews, to know, for example, that the film problem was probably not serious, and the climbing on the wall was probably not an especially serious problem. So I concentrated on the right end of the problem. I felt this way — that I don't have to read anything about experimental results, or any more than all this, because I wanted to demonstrate that Schrodinger's equation predicts these phenomena. If I'm going to demonstrate it, I should only have to know Schrodinger's equation, hm? It would be unfair to read all the experimental results and try to get an intermediate model. That's not what I'm trying to do. What I was trying to do was see how the principles of quantum mechanics and the equation of Schrodinger could lead to phenomena of some kind like this, and what would they lead to, and predict as much as possible while knowing as little as possible. That's why I paid very little attention to people who would come and tell me things about Landau's spectrum or something like that. I wasn't worrying about it at the moment. So then the problem was to understand the next stage of the helium. I gradually realized that the next step was, at low temperatures only phonons are involved and not anything else. There were things called rotons which are excited later, but I didn't know what they were. So all I knew that I had to explain was, there were no other excitations at low energy than phonons. Rotons were higher energy. So I concentrated on that, and I gradually saw the reason for that, by using the path — now, changing the method, not by using the path integral method any more. You use different methods. Then, the next problem after that was to try to understand roughly what energies would the next excitations be above phonons? And I worked on that quite a while. I was in Brazil, finally, working on this, and explaining. I had explained to myself why the lowest excitation was phonons, and why the next excitation had a finite energy, but I couldn't quite identify the excitation. It might be like oscillation of an atom in a cage. It might be like a rotation of a pair of atoms around each other, or a little ring of atoms around each other. And it might be like a single atom moving through the liquid at a certain speed, and it might be a number of other possibilities. I didn't know what it was like. I was explaining and I was struggling to find it, and I was getting more and more clear what it was like. So I was explaining it to Lopez, my position, and trying to explain what I thought the state might be like, like a rotating group of atoms. But that group may be either here or here or here and the wave functions of such a thing would be something like this. I kind of gradually realized that the wave functions that I would propose for these different models were all essentially the same. Mathematically the form is essentially the same. And then I got a big moment. I can't remember exactly how. I was walking along the street — and zing, I understand: The form has to look like this. Well, I was in the street, after talking to Lopez, and I went home to my hotel, and I put that form in, and calculated the energy expected to see if I got anything sensible. And I got a form for the energy, and I computed the specific heat from it, and I got the wrong behavior. Instead of rising more rapidly than you get for the phonons, it rises less rapidly. In order to get that formula, however, I had — in one place, there's a function that I didn't know, which is a little complicated to explain, but it's an interesting story. It has to do with what's called the structure factor of the liquid for x-ray scattering, and it really is a Fourier transform of another function, which is the probability that if an atom is at point 0, the origin, there's another atom at distance R. It's the Fourier transform.

It's the thing that determines how intense x-ray scattering will be as a function of angle from a liquid. The structure factor, it's called, in the liquid. Well, that function came in, in my calculations, in the formula, and I didn't know that function. But I did know, by thinking about it, that at very low values, it's a function of momentum, it's a linear curve with a known slope. At very high values of momentum it approaches a constant. I had learned from Hans Bethe that if you know the ends of a curve, you just take the smoothest thing between and you're always all right, you know. So I made an artificial formula which had those two end properties, which was smooth between. It just rose linearly and the asymptotically petered out to a constant. And I had put that in to compute the specific heat. And it was wrong. So then I went back over my logic about the helium, and this was good exercise. I concluded, after going back over that for a week or so, there was absolutely nothing wrong with the logic. It was absolutely a consequence of Schrodinger's equation, damn it, and there's no escape. In other words, it wasn't that I got the right answer and therefore I was happy. I got the wrong answer. So I'm the only one that knows for sure that my argument is right, because I went through that thing back and forth with a fine tooth comb, and was jammed up against the end. I was just squeezed by the fact that the experiment gave one answer, and my formulas, my deduction, must be right. And the only thing I could think of was — well let me look at it backwards. The formula must be right. What kind of a function must I have in order to get that kind of a specific heat curve? What kind of structure factor formula? And I looked to see, that kind of a specific heat curve that to have a structure factor which rose first before it came down to the asymptote. The moment I saw that, I said, "But of course that's just what liquids do. If you're measuring x-rays as a diffraction ring, because of the partial structure (almost like a solid) of a liquid, there's a maximum, and that's the maximum, that's the diffraction ring of the x-ray pattern." It was a terrific moment, you see. It was interesting. I just tell you because you're interested in how discoveries are made. In a terrific flash of a very few seconds, I saw: a) this was a diffraction ring; b) that when the formula was in the reciprocal, this high peak would make a notch in the curve of energy of excitations so that the curve of energy of excitations would be linear for low momentum, which is phonon, and then at high momentum would be in parabola around p_{naught} . I vaguely remembered that this guy had told me that Landau had discovered that the formula for the energy of rotons was a constant plus p minus p_{naught} squared over 2μ , which is the behavior of a parabola at the bottom. And I realized that that was right, and that I understood this thing that Landau was talking about. And I also saw that the two things were on the same curve. They're not just two different kinds of things; they're all part of the same curve. All in a very few seconds. But it was a terrific excitement, because I knew that I had understood everything all of a sudden. I found out later that Landau had proposed that they were both the same curve, and had made an explanation of why it might be such, p minus p_{naught} , but I hadn't paid any attention or learned anything about it, so I didn't know that. That's not a question of priorities. I'm just telling you; I try always to work with the least knowledge possible, with a little knowledge of what other people are doing, because I feel more happy being more individual, if I am not following the line and getting confused by what they say. So this was an example, and I felt that particularly with this

example, because it must come from the Schrodinger equations, to be honest, I should need to know nothing else. So I was convinced then, that I understood the fundamental aspects of the helium, and of course then it was just a question of cleaning it up, estimating the energy. It turned out to be somewhat higher than the actual energy by a factor I guess of nearly 2. But this didn't bother me, because with the accuracy for such kinds of calculations, that was all right. So qualitatively it was right, and everything was ok. It was just a question then of doing a lot of figuring and seeing the consequences, cleaning up and polishing this thing again. The same business — you're over the top, you know. You work out deductions and you check various things, and everything works all right. You get the formula of Dingle all over again by a different way, and you check a number of items. Then you see if maybe something is due to something else. You see certain things don't fit quite right, like right near the transition, and you struggle to figure that out but you can't, and so on. About that time I went to the meeting in Japan.

Weiner:

This Brazilian interlude here was another trip to Brazil?

Feynman:

Yeah.

Weiner:

Was that for a summer?

Feynman:

Yeah, that was in the summer. I was with my wife then. My second wife.

Weiner:

I see. And the paper itself was published — well, there were a series of papers —

Feynman:

That's right, a series of papers.

Weiner:

So the work that you describe culminated in a series, four different papers —

Feynman:

No, three.

Weiner:

Three, and one of them, that I thought was the fourth, appears to be the report at the Tokyo Conference.

Feynman:

No, that's just a report of something that's covered by the other papers.

Weiner:

Yeah, that's what I meant.

Feynman:

No. In fact, I had written a paper understanding why the lowest energy excitation is the phonons, and there's no other excitation at low energies. I had written a paper explaining that, which I had sent in. But by the time it came back, you know, for printing, I added in the proof the statement, "I now understand what the state is, what the function is of the other excitations, and I will tell about that in the next paper." Then I went to Japan. I was at the Japan meeting, and there was some discussion of helium. There was a solid state group there, a group of people talking about it, Onsager, Feurlich, and others, and somebody said something about liquid helium. So I got up to remark that I have another view that I think is right, and that unfortunately, however, I wasn't able to get the transition exactly right. This is one feature that I don't understand very well, just exactly how you get the transition. I still don't, and nobody else does, but that's incidental. It's a nice puzzle that I couldn't solve. I tried to explain, I know all about it except — you know? I must explain something — the day before, I had sat next to Onsager at dinner. He's a great man in statistical mechanics. I have great respect for him, very brilliant man. He sat by me and said, "I understand you think you have liquid helium understood?" I said, "Yes, I do." He said, "Oh, ho..." So then when I made this remark, about not fitting, Professor Onsager got up and said, "Professor Feynman is new to the problems of statistical mechanics and liquid helium, and I think it is up to us to tell him something that he doesn't seem to know." Boy, am I really going to get it! He says, "The fact that he doesn't get the transition exactly right is of no importance and significance at this time, because no one has ever gotten the transition correct for any transition by theoretical method yet. And he shouldn't criticize himself so severely because he doesn't get the transition." It was just opposite to what I thought he was going to say.

Weiner:

That's very nice. What else was discussed at that meeting? Of course that was just one of the many topics.

Feynman:

Yeah. They were discussing questions like, is there a transition in the solid — in a gas of rigid atoms? And so on. You can look up the Proceedings.

Weiner:

The Proceedings are published. But this wasn't on solid state, this particular —

Feynman:

Transitions, statistical mechanics — there were all kinds of stuff. This is theoretical physics. And I was going crazy, because they, as usual, had scheduled things that I was interested in from two slides, like high energy physics was going on at the same time as

Weiner:

On meeting (I don't have it with me) you're scheduled on a program over here on something, and then they're talking about electrodynamics, and you're not on that program.

Feynman:

Yeah, but then I was stuck. I could only go to one or the other, and I went to the helium. I went to the helium one, you see.

Weiner:

I see, because that was your current work.

Feynman:

Right.

Weiner:

Did you meet Tomonaga on that trip?

Feynman:

Undoubtedly, yes. I had met Tomonaga I believe before.

Weiner:

Oh, when was that?

Feynman:

In fact, even earlier, I think, when I was at Princeton. I believe I met Tomonaga somewhere along the line. What I was doing in Princeton, I don't know, whether it was because I was going to school there, I don't know. I was too young. No, somewhere I was visiting Princeton — I don't know when I visited Princeton, but I remember walking with Tomonaga at Princeton. I don't know how.

Weiner:

You discussed your work — you discussed your reaction to his work and so forth —

Feynman:

Well, I had met Tomonaga and had the pleasure of talking to him, and of course I met him in Japan. I met Yukawa. I also met Tomonaga and others, and discussed many things with many people. I liked Japan very much and the Japanese scientists and everything else. Somebody made a toast that they hoped we can treat the Japanese the same again, or something, or they said we hope to someday return and so on. Nobody paid much attention to me, but I vowed that they would see me, I would return to Japan because I cannot stay away. And I did. I returned for several months, three months.

Weiner:

When, several years later?

Feynman:

Not very long later. About two years later. I've been there three times now, if I'm not mistaken.

Weiner:

When you went, did you lecture at particular institutions there?

Feynman:

Which time?

Weiner:

Later — I mean, after the 1953 conference?

Feynman:

Yes.

Weiner:

Here, in 1955, you spent three months in Japan.

Feynman:

OK, '55.

Weiner:

And you gave certain lectures, and visited Kyoto universities, conferred with Yukawa —

Feynman:

Yeah. Incidentally, by going to Japan the first time for the meeting, it was very amusing. You see, we'd had a war and so on, and Japan was trying to come back — right?

Weiner:

Yes, do you remember what part of '53 that was? Just curious.

Feynman:

No. Well, no, I don't. It must have been in August and so on. So they had this idea for an international conference on theoretical physics. Everybody went, and it was good for Japan. It gave a good many people there, physicists there, a lift, you know. They were recognized as being part of the world again in spite of everything. So that was an important thing. However, Johnny Wheeler sent to each, I presume — at least he sent to me — a letter saying that we were going to Japan, and it would be a good idea to know the language. It was a nice thing to do. And he sent me and I think everybody, because that's the way he is, a small booklet, the Army booklet, for very simple phrases in Japanese. So I was very excited to go to Japan. I'd heard a lot about Japan. I knew something of the cultural aspects and this and that. So what I did was, I studied this

book very hard and tried to learn the phrases. I found a Japanese woman, some friend of some friend who was there, who helped me with some of the phrases. I went to her house for dinner and I asked her a few, like how you say "Thank you." And she corrected the pronunciation, and we got started a little bit that way. Then I took chopsticks and I practiced lifting pieces of paper and so on and so on, with the chopsticks, so I could eat when I was there. Then when I got there, I stayed in a Japanese hotel. It was with great difficulty that I got into the Japanese hotel. They didn't want me to do that, because they didn't think I would be comfortable. I have very amusing stories about that, but that's outside the — Well, it's not entirely outside. They put me in a beautiful Japanese inn.

Weiner:

In Tokyo?

Feynman:

Yes, and the first time I went to the bathroom — not the men's room but the bathroom, to wash in the morning, to take a bath — I didn't realize that the maid was going to come and tell me when I could. I said I wanted to take a bath. She said, "All right." So she went out and didn't come back. So I got mixed up, and so I went to the bathroom and started to wash up. Actually what I should have done was wait for her to scrub me there, because she'd wait till it was empty, see, and they had it all figured out. But I went on washing. While I'm washing, I hear noises — a guy in the bathtub, with the door — like a shower door — closed. You know, it was a big Japanese bathtub. And he comes out, all nude and so on. It was Professor Yukawa. I hadn't seen him since I'd come to Japan. But imagine all the luck in the whole world, the only Japanese guy I knew, for crying out loud, was the guy in the bathroom. He was pleased I was in. He told me it was quite wrong of me to come in. He explained to me that that wasn't right, and that I should wait for the maid, and he laughed and so on. Then we talked. I went to his room, and I met lots of Japanese men, and we sat on the floor wearing gowns. It was very, very interesting altogether, very pleasant, very nice.

Weiner:

You found Yukawa easy to talk to.

Feynman:

Oh, yes. I had known Yukawa, I think, before — I'm not sure, I forget — but I'd always found him an interesting nice man to talk to, very pleasant. He took me to shows with his wife and all kinds of things.

Weiner:

You know, one time he wrote a little article in Japanese which was translated, sort of an autobiography, very brief, giving an account of his family and background and how he did his work.

Feynman:

Oh, the other thing I wanted to say about the trip is, this was early in the business of the government supporting science. So I was offered to get my fare paid to Japan by something, MATS or BLATS or SPLATS; by the Army. And I said, no, I'll pay my own way. I just say this; I don't want to make a big public deal out of it, I'm just telling you, because I felt at that time that the Army should not be interested in science. If it's only interested in it for the use of making something, bombs and so on, the theoretical physics we were talking about is not that. Their interest is something like industry's interest, which is that they want to get the scientist happy so that the scientist will be ready in case —. You feel like a whole, see? Waiting for the customer. So I didn't like the idea, and I went on my own money — which was crazy, but I did that — to that meeting. I was probably the only guy who paid his own way. It turned out that being able to speak Japanese of course helped, a little bit, a little tiny bit, when I was in this Japanese inn and so on. I had lots of fun. Oh, here's a story; this is amusing. When we got to Kyoto—by this time I had talked Pais into this idea of staying at a Japanese Inn instead of a Western style hotel — there was a hotel which had both kinds of rooms, the Miopo Hotel. So we asked for the Japanese-style room, the two of us — we would share it. So we shared a Japanese-style room. There are various stories about that, but one story which has to do with work and so on is the following. While I was in the room, I got a telephone call. "This is Time, the correspondent from Time in Tokyo." He wanted to talk to me about my work, and what I was doing, and do I have a copy of my work, that he could see, of any kind? "Well, it's rather technical," and so on. We talked quite a long time about the copy of the work and what it was because I had done this helium stuff, see. And when we got all finished, he said, "Will you send it to such and such address? We need it very quick because we're going to write an article," and so on. I said, "Yes." And he said, "Well, thank you, Mr. Pais." And it was most amusing. He thought he was talking to Pais, see.

Weiner:

It didn't make any difference what you were saying to him.

Feynman:

No, it did. I didn't say anything about the helium. I said I would send him the paper. No, we didn't discuss it. I'd send him the paper. And when did I do the work? You know. I did it last year while I was in Brazil. It was all right, it was all consistent. "Thank you, Mr.

Pais." I said, "Excuse me, you made a mistake — it's not Mr. Pais. Then when Pais came in, I told him, "Hey, Time —" (I was all excited, because I'd never had anything like that happen before, see). "Hey, a guy from Time Magazine called, wants you to call him back!" "Aw, the hell with him," he says. "Publicity is a whore." There's the difference, you see. I was anxious for it, and he — I learned he's right. He's right, and I shouldn't have been anxious, but it was quite a thing — one of those amusing shocks.

Weiner:

After that, when you came back, you started receiving some recognition, which I gather is the first real public very high level recognition, with the Einstein Award. Is that right? Is that the first?

Feynman:

Yes. Well, I don't know. God damn, I don't know.

Weiner:

Well, I'm saying, the only record I have —

Feynman:

Yeah, but those things don't mean anything to me. I mean, I have received recognition when, for instance, Ashkin used my stuff. And then as I see more and more people using the stuff, and I see in the Physics Review these idiot diagrams I cooked up, that's all there was to it.

Weiner:

Yes, well that's different. You're differentiating between the receipt of a prize and —

Feynman:

No, I'm not differentiating. You're differentiating.

Weiner:

What I'm asking you is —

Feynman:

What you said is the first recognition. I said, no, it's not the first recognition. No, I

consider the other as recognition, and the other is consequence. If Time were interested in writing it up, that's because somebody thought it was important. You know. For example, my backwards-moving electrons appeared in one of the science fiction magazines, in a science article. You know, stuff like that. There were all kinds of little crazy stuff, see, gradually increasing. Then, one day I got a telephone call — this is interesting — I got a telephone call at home, and the operator said, "Mr. Lewis Strauss wants to talk to you." The name sounded familiar. This shows you how dumb I was. I turned to my wife — "Hey, some guy named Lewis Strauss from Washington wants to talk to me." She says, "That's the head of the Atomic Energy Commission." I said, "Oh, boy, I guess he wants me to do something." So he told me his name and said that this was what he wanted to talk to me about — nothing to do with the work of the AEC or anything. And then he started in and he described the existence of this prize, that every so many years, which I forget, there's a prize given called the Einstein Award, in honor of his parents, and it amounts to so much money, and so on. While we're talking I am completely convinced that I know what he's going to say, which is he wants me to act as a judge, to help judge who should win the prize — and the reason is, it happened to me more than once. See, by this time, because of this Time Magazine and a number of other things, I'm always asked to be a judge or something, you see. So I was sure what he was going to say. Therefore it was a surprise to me, in spite of all the introduction which he was trying to make so I'd catch on. It was a complete surprise to me when he said, "I wanted to tell you that you won the prize." "Won?" I said, "hot dog!" You see? So he says, "It's interesting to hear a serious scientist saying something like, hot dog." I said, "Listen, you call up any serious scientist and tell him he won \$15,000, he'll say hot dog." And so on. So I had to go and collect that money, at some later time.

Weiner:

That was in March, 1954.

Feynman:

Yes. My experiences talking to Mr. Strauss concerned Mr. Oppenheimer, because that was the time when the Oppenheimer case had just been finished, and the stuff had been just published. Here's a story, but you may not be interested in that.

Weiner:

Why not?

Feynman:

Because it's nothing to do with my work, and if we start going into all these things we'll go on forever — I mean, I don't mind, but it's up to you. I tell you that there exists a conversation between me and Mr. Strauss about Mr. Oppenheimer and the published

documents of the Atomic Energy Commission. I had read the documents in the day or two (after they were published), because my cousin is a reporter for the Associated Press, and I had been visiting my sister when I came east to get the prize. I stayed with her a couple of days first. My cousin came over and had this transcript that had just come out, and I read the whole thing. So I was well prepared (and knew everything that was in it) to talk to Mr. Strauss when he met me. But anyhow, that is the prize. Oh, when I received the prize — I think these things were very personal, but I'm saying them to you anyhow. I don't know what to do. See, there's lots of bragging in here, and there's lots of things in here, and you understand that this is the situation. So I keep right on going, but I remind you, this is another sensitive point. When I received the prize, I knew what had happened to Oppenheimer, and that Strauss had something to do with it, and I didn't like it. And I didn't like Strauss. Ok? And I thought: I'm going to fix him. I mean, I was not nice. I don't want to take it from him. The hell with it. And I thought: Maybe I won't take the prize. All right? And I worried about it, because in a certain sense I felt that was unfair. The guy is offering the money — you know, he's trying to do something nice — and it isn't that he just did it because of this, because he's done it before. There were previous Einstein Awards, as far as I know, or something. And therefore, it wasn't just for this reason. I was kind of confused, and I talked to Professor Rabi, who was visiting Caltech about it. Rabi said, "You should never turn a man's generosity as a sword against him. Any virtue that a man has, even if he has many vices, should not be used as a tool against him." That's the way he put it. You shouldn't use a man's virtue as a weakness, to take advantage. I saw that that's what I would be doing if I refused him and made publicity about "I won't accept this because he's such a stinker." That would be a terrible thing to do. So I took the \$15,000.

Weiner:

That's very interesting about Rabi. That's the second time he's played a role as a senior advisor to you. Do you have any sort of special relationship with him?

Feynman:

No, it's just the way he acts, behaves, walks around and so on. He likes to feel himself as — you know what I mean? — an elder statesman or advisor. And so, for example — I don't know whether I told this one — when I was at Los Alamos, once he came by. We were talking about something, and he said to me, "You know," in this serious voice, "contemplate the neutron." He says, "Contemplate the neutron." So that night I was baby-sitting for the Bethes or something, and had nothing to do. I was sitting in the room, and I said, "Well, I'll contemplate the neutron." And I began to think all about neutrons, and of course I had to think about everything. I mean from neutrons you get into protons, then pi mesons — you can't think about the neutron all by itself. I kind of understood what he meant. It doesn't make any difference where you start. You start with these things and you get involved in all the problems. I started thinking about all the problems. But I remember — he has an influence on me, yes. If he says to me,

"Contemplate the neutron," I contemplate the neutron. You know what I mean? Yeah, he's a sort of older advisor, like a great father of the young scientist business. You know, I like him very much. Yes, we have been good friends.

Weiner:

In 1954 you were also elected to the National Academy of Sciences.

Feynman:

Yeah, that's another one of those things. Now, between us, again, these things bothered the hell out of me, all these things. I had trouble with the National Academy of Sciences. Let me just put this down for the record, because I had never heard of the National Academy of Sciences. Never! I didn't know what it was.

Weiner:

And you didn't know that some of these men you admired so much were members?

Feynman:

No. I'd never heard of the National Academy of Sciences. I received this thing, and I didn't know what it was. Somebody told me Epstein would know, because he was the only guy around at the time. Bacher was out or something. I said, "I don't want to join it, Professor, because as far as I know, they don't do a damned thing." He said, "But they have this National Academy of Sciences thing that they publish. They have meetings." I said, "Yeah, but I don't read the National Academy whatever it is." I don't even remember what the name of the publication is now. It's a publication, but the articles in physics are not impressive there. I never had to refer to it. Never knew it was there, and I never knew anything that they did. "Well, it's an honorary society." I had already made, when I was a kid in high school, a principle, see. Like a nut, like children do, you make ideal principles, and then later you make yourself miserable in life by having to change the principle. See, I had become a member of what was called the Arista, which was an honorary thing for the students, and the only thing we did in the Arista was to select other students who might become members of this thing. So it was a mutual patting on the back society, and I looked at what we were doing, and I thought, "That's not right. All we do, we get into this thing, and we give the honor to the next guy, the great honor. What a position to be in! Kingpin Joe, he's going to permit somebody else to have this marvelous honor that is so wonderful to have attained," you see. So I made my decision, not to be a member of an honorary society, if it's an honorary society only. If this damned thing did anything, it would be all right. So I made my objection to Epstein, "It's only an honorary society and they spend their time electing other guys. I don't like the idea — I don't think I'll — I mean I don't want it because I didn't know anything about it." I didn't want it. So he said that this honor is supposed to be pretty important,

that I had many friends in the organization who had probably worked very hard to get me elected into the organization, and I would disappoint my friends, and so on and so on, if I said no; it would make a big noise and was not necessary. So I said, ok, and I quietly accepted it. But I didn't pay any dues, and I told them not to send the journal, and I tried to forget it. I went to the first meeting. Oh, wait — I did give them a chance, I always do. I went to the first meeting. It was as I'd proposed. It was a lot of talk about getting members. They were discussing, "We have to stick together in the physics group, because we have only so and so many votes, and the chemists have so and so many votes. We have to kind of agree on it among ourselves, on which physicists we're going to vote for, because if we don't we're not going to have votes to counter the number of votes for the chemists," and so on and so on and so on. This is, to me, for the birds. I'll vote for the chemist, maybe he's better. You know? It's for the birds. I couldn't stand it. Then the meeting itself, the scientific meeting itself, was a shock to me, because many men who have gotten in may be old by this time or something, but the caliber of the thing was extremely variable. There were some very good talks. There was a great talk by the man in weather, who was very interesting. But then there was a talk by some guy who got up and told about experiments which were supposed to be the effect of stress on rats, on their desire to survive or something — on rats. What he had done was — I mean this is what he'd done — he described it in terms of a cylindrical glass tube, but it was a bottle. He took a jar and he put rats in the jar in water, and he screwed the lid on so that they couldn't quite come up high enough when they were swimming to get a good breath, and they were drowning — hm? And he watched how long it took them to drown, under some kind of circumstance. Now, if that ain't a kid fiddling around! There was nothing scientifically measured, in the sense that there was some sense to it, except that the rats got very nervous, you see, and they swam faster, or something. It was the most cruel, unnecessary, stupid thing. It was very much — except it wasn't human beings — like the stupid kind of Nazi experiments in which they don't exactly know what they're doing; they're just experimenting. Huh? It was just like a kid would experiment, God damn it, in drowning rats. It had no real scientific virtue of any kind. I was all ready to jump up and complain about this, but I expected somebody else to do it, to say, "This is scientifically very poor; this hasn't any sense to it." I kind of was a little bit reluctant to make a big stink as a new member, so I didn't say anything — which I regret, because nobody else said anything. And I feel, if it's a scientific organization, aside from the cruelty to the things, scientifically it was —. Even with machines it wouldn't make any difference. Scientifically, nothing was measured. It was misinterpreted activity. It was perfectly clear that the thing got frightened. I mean, it was just crazy, see. It was so poor. And I didn't criticize it, because I expected somebody else to, because I thought it was obvious. But that was a mistake. I went to another big talk by some man who was in Mexico, a great doctor who had been working with Norbert Wiener down there on some business. He had done something with the heart, in some heart institute, and now he was doing something with nerves. And he had found some kind of a thing which goes something like this. In the nerve response to a pulse, there's a place where the third derivative of the curve is discontinuous. He measured the thing, and it varies with this and that, and so on. The third derivative of a curve is one hell of a thing to get at.

Anyway, they found that when they wanted to fit these curves, the curve of the response, with empirical formulae — you know, exponentials summed up together — that they couldn't get one set of exponentials to fit the whole curve. So they had to do it in two sections. Then the third derivative didn't match. But this is evidently an artifact, and evidently Wiener doesn't appreciate what's involved in physical curves; i.e., that you can't pick the formula, and that the second derivative or the third derivative, whichever was discontinuous, when it was physically really discontinuous, required very careful measurement of the curve. It's extremely hard even to get the slope off a curve well, and then to get the slope of the slope — you could make errors. For example, I already knew that if you took a curve and fitted it with a French curve, even if you made a very big scale and very very carefully, and then took differences, slopes, and kept going to the second derivative, you see tremendous irregularities exactly at the places where you change the curves. It's impossible to beat that. So after the thing, I went up to the man and I asked him what was the way in which he got the curves. I mean, you see the curves on the scope, but how do you get them on the paper and how do you compare them to the formulae? So he said, "Do you mean you want me to tell you how we take a photograph, and how we test the photograph, and all this?" I said, "Yes, I do, to understand the cause." I said, "It's very hard to get the second derivative out, you see." He said, "I was working with Professor Norbert Wiener." I said, "I still want to know how you did it." "I haven't got time to talk to you!" So I say, I came to the conclusion that the National Academy of Science is not enough critical of its own science, and is an honorary society, and I don't want to have anything to do with it, hm? Ok. So I never did anything for it. I don't want to have anything to do with it, and every time there's an election, I never vote. And I get all kinds of pleading letters from my colleagues that we need every vote and all this stuff. I won't have anything to do with it. And I wrote once to Adrian Bronk, the president, at the time —

Weiner:

Detlev Bronk?

Feynman:

Detlev Bronk, excuse me. Adrian and Bronk worked together on neural things. So I wrote once to Detlev Bronk and told him, "Is there any way I can get out of this society quietly? I don't want to be associated with it, but I don't want to make any objection. I don't want to make a big public something. I just want it to be quietly forgotten that I'm a member of this thing. Is there any way to do it?" I said, of course, "You understand that if you don't want me to do it, if it can't be done, that I won't do it, but you'll have one reluctant member, one unhappy member of your organization." So he wrote back: "You'll have to be an unhappy member. I would rather you would stay." What I'm going to do now that we have another president is some time write another letter and try again. I don't want to be a member of that organization. It's scientifically poor, and it's honorary, and I don't like the combination. I'll tell you more that I don't like — the

history of it. It was invented by, I believe, Lincoln, in the Civil War, to help scientifically, to give advice, to assist the Union in winning the Civil War. It had therefore a real purpose, hm? Ok. By the time the next war came along — I don't know which; say the First World War — it's ineffective, so they can't use it. Instead of that, they appoint another thing whose name I don't — oh, the National Research Council, hm? — in order to give advice to help win the First World War. We have these two things now — hm? Ok? And they don't buy, it doesn't buy. It has its building in Washington. It's paid for by the taxpayers. It doesn't buy, because it can't do its job, and they knew all this. Second World War comes around — neither of the two organizations works now. They need another thing — OSRD, or something like that, hm? And they put that on top of the thing. I don't remember all how it goes, but this stuff just doesn't make sense. If it doesn't work, forget it. And if it works, use it. But not just pile one on top of the other, because it'll just get like barnacles. So they have now four or five organizations. They all exist, from back in history. Well, I don't like it. So altogether, you can take the National Academy of Sciences and — I don't care what happens to it.

Weiner:

Someday I'll remember to send you some documents that talk about the origins of the Academy and the resignations that occurred at the time because of someone's disagreement with the way the members were selected, which has nothing to do with your feelings in your case, but there is a precedent...

Feynman:

Well, I don't object to any specific way that any specific member was selected. I'm just saying, I don't object, and I'm not jealous of somebody getting in or not getting in. I don't know who's in and who's not. I don't know who's a member and who's not a member. I don't give a God damn.

Weiner:

Well, that's the National Academy, and that was '54. That takes care of that.

Feynman:

Yes, but you see, I told you, every item is going to involve a story.

Weiner:

But this is quite a revealing story.

Feynman:

That's a revealing story, so I shouldn't hide it. Ok.

Weiner:

Yes, because that's fascinating. 1954 — I'm skipping around —

Feynman:

I wish I could get rid of it. I'm going to try again. There must be a way to quietly disappear from that organization. What I did is, I wrote to Who's Who and told them not to include that sentence in there, but they still do. I can't get out of it. You see, you touched this weak point. I shouldn't have accepted it.

Weiner:

In '54, you went to the physics conference in Rochester. I think those conferences were pretty good.

Feynman:

Yeah.

Weiner:

I know that Bethe and Teller and Oppenheimer were there. Is this a special conference of significance in your mind?

Feynman:

Well, you give them by dates, and I can't —

Weiner:

Things were beginning to happen, that's why.

Feynman:

Yes, things were happening at all these conferences, and all these conferences were important.

Weiner:

In this period of time, '54, '55 for example, they discovered the anti-proton, and the next

year the fall of parity.

Feynman:

Yes, we always had this stuff going on. We had these meetings, and these meetings were important, but I can't remember any — the meetings were important. They tell you what the problem is, what the situation is, and I had many discussions and arguments in the meetings, of various kinds. Yes — oh — can I tell you about the relationship to the press? Would you be interested in my relationship to the press?

Weiner:

Yes.

Feynman:

Right after the war, since I was trying to explain about the atomic bomb (and I usually can explain it in a relatively elementary way), and since I had tried to tell Lawrence of the New York Times when he had visited Los Alamos how the thing worked, he afterwards asked me questions at these Rochester meetings. I would try to explain as best I could what Oppenheimer meant by his statement that we now understand everything. I would tell him what he meant, and I would tell him I don't agree with it, and so on. You gradually gather around you an enormous number of reporters from different organizations, and they would invite me to dinner after the meetings, say, and I would try to explain things and answer all their questions. I tried to explain. I made a real effort because it seemed to me that my colleagues were not paying any attention, and they shouldn't criticize the crap which was coming out unless they made an effort to try to explain things to these people. So I made an effort. And the effort wasn't too useful. It didn't come out too good. Some guys were very good, some were only fair, and some were obviously not good. And so on. But I kept doing this all the time. One time, at one of the meetings, they asked me if I would do it again, as usual, and I said, "Listen. Every meeting I do this. This time, no. I don't want to do it." I was tired. "You can get somebody else." They said, "Well, we're having a conference of all the reporters" — 20 of them — "in the news room or something, and we'd like an expert to answer questions on something." I said, "No. Get other guys to answer the questions." At that meeting, Teller had come. That was about the time of Oppenheimer's trouble, and people thinking that Teller and Oppenheimer were opposite to each other, and so on. And Teller had some theory of the nucleus in which he claimed that the forces between nuclear particles were highly velocity dependent, because of the difference in force. It was a little balance: there was a repulsive force from scalar mesons and an attractive force from vector mesons, and they were almost exactly the same, and only the velocity dependence was different. Instinctively I know that such things don't work. I mean, it's very rare, unless by accident. But it's hard. You can't expect an accident really to explain it. It's not fair. I mean, it's instinct. So I didn't think it was right, and I also had other

reasons not to think it was right, in that I believed that if I had scalar forces to a high enough, to arbitrary power, you could show that you'd get infinite numbers of pairs. He also thought that his theory was convergent, that everything was going to be all right, and it would explain the facts. And I objected, for two reasons: one, that it was an accident that he was invoking, and two, this other thing. I made an objection. So after I did this, they asked me again, would I please come up and answer questions. You know, they were very interested in Teller's stuff and so on. So I went up there, somewhat reluctantly, and they started asking questions. And it was very amusing. For me it was a surprise. I didn't know how to handle it because I didn't expect it. They started to ask me questions about this thing, what it means, and what do I think of Teller's theory. And I explained as best I could, in simple language, elementary language, what I thought was the matter with Teller's theory. And they said, "Is it possible that you don't think Teller's theory is right because you don't like Professor Teller?" I said, "No, it is not possible." "Well, do you like Professor Teller?" I said, "It's irrelevant. You know, like this — I was sort of surprised. And because I was surprised, I didn't know how to handle myself on these things, and I got more and more angry. It's wrong, dopey — but I got angry. And they kept badgering me, like it was a political meeting and they were trying to badger a guy to admit something. And they'd go, "Brrrpp" from one side, "Brrppp" from the other side. "Well, perhaps you don't have this feeling, but don't you think that the reason that nobody pays much attention to the professor is because —" "No," I said. "I believe that at that meeting, they were all thinking almost completely scientifically about the value of the thing." "But you criticized —" "Yes," I said, "Because I criticized scientifically," and so on. "Why," I said, "can you counter my arguments?" You know what I mean. And so on — and it got worse and worse. I got more and more upset. I was sort of yelling, you know. And I came out of that place shaking. So that was rather interesting, relations with the press.

Weiner:

What have you done with the press since?

Feynman:

I don't care for the press too well, for several reasons, but that's just one added on. Of course, that doesn't make all the difference. I have tried to explain things, but it's almost hopeless, to a large fraction of the press. There are certain particular members that I still will try to explain things to. For example, there was a man from Fortune Magazine, Mr. Frank Bellow, who now works for the Scientific American.

Weiner:

Francis Bellow.

Feynman:

Francis Bellow, yeah. Now, he's a guy who, if you take patience and explain it to him, makes it come out the other end better than you explained it to him. That's great! Because I explained to him the principles of quantum mechanics, and he wrote an article in Fortune, which may be incomprehensible to the reader — that doesn't make any difference — but it was better than my explanation. It was better than I could have written, whereas usually what comes out is such a garbled thing that, although it's now comprehensible, it's meaningless.

Weiner:

Yes. He has a little book, I think —

Feynman:

For instance, that's just one example, another guy from the Herald Tribune —

Weiner:

Earl Ubell?

Feynman:

Yeah, right. He's good.

Weiner:

There's no longer any Herald Tribune, though.

Feynman:

Yeah, but there are good ones. You see, my reaction is not just to the press in general. I gradually began to see that there were good guys who were useful to try to explain to, and there were useless ones. For example, the guy from the local newspaper, Star News — he came and bothered Pelham and I, and asked a lot of questions; we explained a lot of things. He gets all through and tells that we're trying to attain absolute zero and all this kind of stuff. He just made it up. He made up quotations — "Professor Pelham said," — quote — Then he says something that's so ridiculous that Pelham seems like some kind of a jackass — which he isn't. The quotation was ridiculous — "I am going to attain absolute zero within the next two years." Well, Pelham could not possibly have said such a thing! It's meaningless — it doesn't mean anything to attain absolute zero. So he was livid mad, Pelham. But that kind of stuff happens, and it's kind of useless.

Weiner:

In '54 it seems to me there's a whole bunch of things — for example, on liquid helium, and we talked about that —

Feynman:

Right. But there was one piece of liquid helium in addition that I have to do yet, besides the ones I mentioned.

Weiner:

When was that?

Feynman:

Well, I just kept going. I mean, I had to understand. After I got finished, I realized that the critical velocity which my theory gave was much higher than the real critical velocity, and that the perfect flow of superfluid must break down in some manner. But I could only predict that it would be vortex — free circulation. I didn't know how it worked when it was circulating, when it had rotation in it. And so I used to think about it, when I was lying awake at night, and I saw how it had rotation. I described finally the vortex lines. I don't know that it's worthwhile saying how I thought the thing out, but maybe it is.

Weiner:

Yeah, I think so.

Feynman:

Ok. The problem was to get a situation where the helium had rotational flow. That means, if you go around a line, in the fluid, and take the average velocity in the direction of the line as you go around, the average is not zero; there's a circulation. So I figured and I worried, how could it circulate? My theory says it didn't. My equations were only for states that didn't circulate. I knew there must be states that did. The question is, what are they like and what is their energy, and so on. How are they formed and all because that had to do with the critical velocity? So, in order to force that the liquid would have circulation, I was lying in bed thinking in my mind, visualizing the following situation, which I kind of forced. I had the liquid on one side of an impenetrable membrane, infinitely thin, made of some material that doesn't exist, because it has to be made of atoms. All right, you can think it anyway. On one side, the liquid is standing still. On the other side, the liquid is moving. But there's this layer in between. Now, I know the

states, the wave function of both halves, of course, because I knew what to do. And each part of the liquid is OK, see. One is just drifting; the other's standing still. So I knew the wave function system. Now, I pull out the sheet between them, and I ask: What is the character of the state now going to be like — in which I had the wave function one way above, and the other way below. How are they going to fit together? What are the conditions? I was talking about how I got the idea about what are now called vortex lines. I was lying in bed, trying to figure out what would be circulatory flow, and I forced myself to have circulatory flow by imagining this liquid flowing above a plate at a uniform velocity V , and below the plate not moving, and I knew the wave functions of each. The one at the top differed in the way that if you move an atom a distance X , the phase of a wave function would change by E to the I over H time MV times X . By a certain phase proportional to X , and the V measured this, the speed measured the phase, rate in which the phase changed with X . That was the difference. First I thought, when I pull out the sheet the two liquids, the two sections being in really different stages and different kinds of motion, may behave like two independent liquids, and that there's just like a surface tension between the two liquids, which would have energy proportional to the surface area between two regions. But (this is the way the mind works) I realized right away that that isn't the case, because if the velocity of the upper one would grow smaller and smaller and smaller, certainly when the velocity of the upper one was zero, they would not be that energy. That's a very considerable energy, as a matter of fact, that surface energy. And certainly, when the velocity of the upper section is zero, it's not needed. They could just mesh together. But if it were infinitesimal, is it suddenly going to be at infinite energy? And I didn't think that was intuitively likely. So I tried to think a little more how maybe they could be together. Now, I realized then that the phase which is varying across the top is varying sinusoidally and that, say at X equals zero, the phase at the top and the bottom is the same. Say, at X equals zero. So an atom could move from the top surface to the bottom surface without changing anything in the wave function. Then if I went 2π , a certain distance corresponding to 2π further on, again the phase was zero which would mean that the bottom wave function and the top wave function were the same at that point, that atoms could move up and down freely. There would be no surface tension energy between the two liquids, at little patches, every so often. So now I had the view that there would be sort of glue, I mean a continuity of fluid, and then a little strip where there would be this surface tension, when they couldn't mix, when the atoms couldn't go from above to below — then a glued strip and then another, a glued area, short area, and then another — and so on. Then I began to think: How long is the strip of the surface tension, and how long is the glued area? How wide? I mean, how much difference in phase can I tolerate? You say you can't tolerate any difference of phase — but no, I could distort the wave function a little, take a little energy, but I don't have to have the function along this edge be precisely E to the I V X , but vary a little bit, make it nearly constant for a while, while it touches, and then vary over the strip part, and so on. Well, that just kept going. I mean, I kept thinking about that, and I gradually realized that the best solution is that the strip is very short. The strip where the surface tension is may be only one atom long, and in fact it's a hole — a round hole, and not a strip. The glued portions are long as they can be, and all the phase

change is in a very narrow or one atomic distance, rather than over the long distance. And you have these lines that are spaced from one another by the distance corresponding to how far you have to go to change the phase by 2π . Now, if the velocity is lower, you have to go further. Therefore the lines are further spaced from one another, so the energy is lower. So there is continuity; i.e., as the velocity of the upper region goes to zero, this energy goes to zero, and there were these lines around which there was a circulation, and in the region in between there was free motion without circulation. This was the invention of the vortex line. I understood it — and I kind of jumped out of bed, see. Then I went on and did the usual thing in any of these researches — raising different arguments, purifying the analysis, clearing it up, making more powerful arguments, and understanding it deeper and deeper once the clue is given that this is the only way it can be, that the strips are really lines, and so on and so on and so on. And I made this theory of vortex lines. Then I found out that Onsager had proposed that there were such lines, many years back, in a reference. There was an article — it was in some Italian journal. There was a conference, and there was somebody talking about super-conductivity, and then a question period. He said that he thought that in the super fluidity of helium, this, that and the other thing would happen. He described these lines exactly. I had never seen it. What's amusing is, he has these things, and he doesn't publish them much. They're very accurate and wonderful, and nobody pays much attention.

Weiner:

How did this help in explaining experimental...?

Feynman:

Well, that was the thing. I hadn't gotten to the flow. I had two things missing. I hadn't known how to describe the flow of helium when it was above the critical velocity, and I hadn't figured out how the transition works, very near the transition. And I had at last solved the first one, but the transition I have never solved.

Weiner:

What was the name of that paper? What did it correspond to?

Feynman:

Well, now, let me just look for just a second, if you have just a second. I have the papers here, and I can tell you. It's not in any of the Physical Review papers. It came afterwards, and it's in an article in a book.

Weiner:

There's a book "Progress in Low Temperature Physics."

Feynman:

That's right, yes, exactly.

Weiner:

Published in '55.

Feynman:

That's it. There's where it is.

Weiner:

Volume I, Chapter 2. I see.

Feynman:

Hm-mm, relating to helium, there's one other thing. In the meantime, Michael Cohen was working with them. I had tried to make more accurate calculations of the energy of the excitations of the roton and had gotten some ideas, but hadn't carried them all the way through. They were too hard. And Michael Cohen found they weren't as hard as I thought. He was very clever and worked it out very carefully, and got a more accurate estimate of the energy of the excitations of liquid helium by a more complicated wave function. The fit was done very much better. It was not off by a factor of 2 but by 20, 30 percent.

Weiner:

Who is Michael Cohen?

Feynman:

A statement.

Weiner:

And did he get his Ph.D. under you?

Feynman:

Yeah, this was his Ph.D. thesis, I think.

Weiner:

Which one, the energy spectrum or an article on roton state?

Feynman:

No, the energy spectrum.

Weiner:

Then you have a Reviews of Modern Physics article on super-fluidity and superconductivity. Was this sort of a summary?

Feynman:

I don't remember that. That must be in some... Some meeting or something, where they asked me to give a summary and I made it into a paper, as far as I know. I'm not sure.

Weiner:

Now, there's one paper that intrudes in this — there's two. There's a quantum electrodynamics paper, with Beranger and Bethe, on "Relativistic Correction to the Lamb Shift."

Feynman:

Right. Well, that was in a continuation...

Weiner:

That was in 1954, right?

Feynman:

Right. Well, I tried to get more accuracy in the Lamb shift calculation. The next order of accuracy, of course, interested me from the beginning, and I tried many ways. It was quite complicated, but I invented a number of suggestions for simplifying it, and so on. But then I left Cornell. I had also started Beranger on the problem, or Bethe and I had. Bethe and Beranger finished it, using some of the things I suggested. They did a lot of independent work, but we all three have our names on the paper, because I kind of got some of the ideas in the beginning. I didn't carry it all the way through, and they carried

it all the way through to get the next order correction in there.

Weiner:

So this was earlier work.

Feynman:

Earlier work that they continued doing until they finished it.

Weiner:

Then there's an article with Speisman...

Feynman:

Speisman, yeah.

Weiner:

On "Proton-Neutron Mass Difference," in '54. Now, where did this start?

Feynman:

Yeah, that was at Caltech. The story of that one is simple and interesting. Speisman came in. He's looking for a thesis. These guys come in and they're looking for some kind of thesis job, see? So, I don't know — among the various problems, I suggested the following problem. It is well known that the neutron is heavier than the proton. We all believe that the energy due to electrical charge is positive, and that the difference in mass between neutron and proton is probably electrical. The electrodynamic theory gives a positive answer. I had looked at it, in fact, long ago, when I was first doing quantum electrodynamics. I got the sign and it was positive. The energy of the proton should have been heavier than the energy of the neutron, as I remembered it. All right? Yet the energy of the neutron was heavier than that of the proton. So I would explain to him, therefore, that there must be something wrong with the electrodynamics, or some peculiarity in the electrodynamics. There must be some strange way in which either the neutron or the proton interacts with protons at high energy so that when you do the integrals the sign gets reversed, because I can't believe it's anything but electrodynamic. Then there was data, measurements of the behavior of protons bombarding protons, making mesons and so on. Here's the project: You find out what kind of modifications you would have to make of electrodynamics to get the sign reversed, what kind of thing you could learn, what kinds of things experiment permits, and make some suggestion then of an experiment to check the proposed thing, so that we can understand this

difference. So then I said to him: "In order to learn this, the thing you should do first is to calculate the difference in the regular way, without any modifications, and you'll notice that the proton is necessarily bigger than the mass of the neutron." So he went off to do this first, see — regular way. So he did it, and he came back. There was something funny about the way he did it. Yes, he came back with the energy formula, and he had some sign reversed, because he had the proton lighter than the neutron. I pointed out where the sign was wrong on the term. He went back, and he found that the sign was not wrong on all his terms, just one, and he found that the other sign was the other way. So we found that the answer could come out the other way. I don't know what I had done wrong when I first did the calculations. I had done it relatively quickly the first time, when I was at Cornell, long ago, and I had gotten positive for the difference — whereas if you just went ahead and did it, you'd get negative. Speisman just went ahead and did it and found it was negative. He found that the regular electrodynamics predicted a negative mass difference. See, it could predict a negative mass difference. It didn't have to be positive, whereas I had thought it had to be positive because all the terms were positive. Oh, I remember, we had a little byplay of confusion, where I pointed out an error in sign and he pointed out that he made the same error therefore in the other term. So as I was correcting it, he was uncorrecting it at the same moment. Anyhow, I felt then that the puzzle, that this energy was the wrong sign, was not a serious puzzle. And we simply wrote that paper to say that the sign being reversed was not a definite proof that it wasn't electromagnetic. Some people have misunderstood the paper, and claim that we tried to make a calculation of the mass difference quantitatively. But I was only trying to show that the fact that the proton was lighter than the neutron did not mean that it was impossible that the difference was electromagnetic.

Weiner:

Did this have much effect on theoretical thinking?

Feynman:

I don't know what effect it had on the thinking in the field because, see, what I didn't know is to what extent people thought that it was impossible to get the sign reversed. I don't know to what extent they thought that. Since I thought that it was, to me it was important noticing that the sign was reversed. Then came another little game, in which Weisskopf argued that the sign was wrong by physical arguments. Anyway, we all got everything straightened out, and Weisskopf finally got a physical argument to explain where everything came from. It was very very simple, and I was ashamed of myself, because I should have been able to see the sign of this thing by ordinary argument and not by calculation. Weisskopf tried to see if by ordinary argument. And then following his argument, I was able to see the thing. Maybe he just told me how to do it. There's an error in this paper. That also helped to make it hard for me to understand.

Weiner:

In your paper?

Feynman:

In this paper with Speisman. It says that “the term for mu — zero representing the coupling of current with current is positive, as is also the term in mu squared.” But the term in mu squared was in fact negative, if we had done it right. And the whole thing has to do with this — that the energy of a magnet is in fact negative. If you compare two objects which have the same angular momentum, one charged and one uncharged, you can show that the energy of the magnetized one, which is the current going around, is negative, not positive. And that’s classical physics. But we didn’t know that. See, the energy of a charge is plus, but the energy of a magnetic field, when you compare the field with the same angular momentum, is negative. It’s a simple fact of classical physics, and it would have been easy because the magnetic moment of the proton is bigger than that of the neutron. Therefore the negative contribution of energy in the magnet could exceed the positive contribution from the charge, and there you are.

Weiner:

That’s the explanation now, but it wasn’t included in the paper?

Feynman:

We didn’t know at that time. We really made a big calculation and it came out minus, but then Weisskopf pointed out why. So it’s very simple. Very simple.

Weiner:

You know, the next thing, as far as a major piece of work goes, appears to be the Beta-decay work. Do you agree?

Feynman:

No. There’s a business about the polaron.

Weiner:

I’m sorry. “The slow electrons and the polar crystals?”

Feynman:

Yeah. That’s in between, isn’t it? Certainly it is.

Weiner:

Yeah, let's see -- wait -- '55.

Feynman:

Yeah. Now, if you'll just turn that off for a few minutes... Incidentally, I found a published article by me on a talk given to the World Affairs group that I have upstairs, that I'll let you have. All right. Turn it off so I can say hello to my boy...

Weiner:

It's working.

Feynman:

"Slow electrons in a polar crystal?"

Weiner:

Yes, there were two papers.

Feynman:

All right. I've got a letter to Feurlich on this story. You might be able to find — I'll try to find a copy. But anyhow, one day I was feeling kind of low, nothing to do, so, you know how you goof off. And there was a pretty librarian around at the time. I thought that she was working in the library, so I went into the library to — look at her. And I just picked up a book in order to have something to do, and it was *Advances in Physics*, and there was an article in there by Feurlich on slow electrons, an electron moving in a polar crystal. He described the problem, and said that if this problem is solved it will go a long way toward understanding superconductivity — a remark which I didn't understand in the slightest. I still don't and it was not in fact correct. At any rate, the problem was interesting. It was, to find the energy of an electron which is in interaction with the phonons of the crystal. And this is just like the field theory problem of a particle in interaction with a meson field, except that the complications of relativity are removed, and all of the divergent difficulties are non-existent, and so on. I know that I had developed methods of doing these kinds of things with finite coupling constants (which is what the problem was, to figure the energy out with finite coupling constants), and I thought: Well, I'll try my path integral scheme on this thing — the usual game. So I set it up in terms of path integrals, and I fooled around with it. I had gradually been getting the idea that there must be some kind of a minimum principle for the calculation of path

integrals. I had an assistant at the time called Beranger, and I never could give him anything to do. I mean, I let him just run — I just can't work well with assistants. So I thought, what the hell, I'll tell him to see if he can find this minimum principle for path integration. So I went upstairs. "You know, Beranger, I've got a good problem you might like to work on." And I said, "I think there ought to be a minimum principle for path integrals that's something like this. Try to find it." He said, "Well, which way would you turn? What makes you think that there's a minimum principle — how could you expect it — what do you think it's like?" I said I thought it would be something like this. You could set up this — and I started to write something — and then do something like this, and then the difference of these two would be less than that, of course, because of this, and you gradually — and so on and so on. When I got all finished, I had proved that I had found a minimum principle. He said, "But that seems to me to be a minimum principle already. You seem to have it all proved here." I said, "Yes, indeed I do. Thank you very much." I was so ready to get it that I thought — you know. In fact, that's an example of the reason I can't work well with students or assistants. I don't like to give a person a job unless I think it can be solved. I don't think much can be solved, most of the time. I don't want to give them a job until I think it can be solved in some way that's reasonable to do. And that means that I have to nearly do it, because the only way I can tell if a thing is possible to solve is to solve it. So I always find myself doing most or a lot of the work, and just a little extra, see. Ok. Anyway, I had invented a minimum principle for doing path integrals, and I applied it to the polaron problem rather easily and very nicely and simply. And I got, in a most direct fashion, a very accurate answer for the energy of the polaron different coupling constants of the crystal, which solved Feurlich's requested desire perfectly, ideally. If necessary, you could get more accuracy, but it wasn't at all necessary. Incidentally, I have since worked out the energy to higher accuracy, but have not published the answer. It's not necessary for anything, but it's interesting to keep on going. But it was very accurate the way it stood, from various tests. And that has interested me very much, because I had tried after that to translate what I did into the normal language of Hamiltonian and other forms of quantum mechanics, and I have not ever been able to translate it. In other words, I can't find out what to do in the normal language, without path integrals, which is equivalent to the formulas that I obtain for the energy of the polaron. And my energy for the polaron is, over the greatest range of parameters at least, the lowest energy — I mean, a more accurate energy than any other energy by any other method calculated. So I have a very powerful tool, and I can't find a way of translating it into normal language. So the path integral really has some value of importance besides the fact that I can do everything the other way. I know, Case laughed at me, and it's amusing because he said, "I remember once you were complaining that you couldn't find any problem that you could do with path integrals that you couldn't do the other way. And now you've found one, and now you're complaining that you can't find another way to do the problem except with path integrals." The reason I wanted to find another way very hard was that the path integrals are limited to cases where there're no spin operators or other operators, and the corresponding problems. The method would apply, say, to electron interaction with photons and so on, if I could only find some way to translate — and there I operated

like gamma matrices and so on. And the techniques might work, if I could figure out how to translate it into more conventional language, because I can't write those other things in the path integral form. So that's why I struggled to then find it in normal language, but I have never been able to find that in all those years — over ten years since that time — and nobody else has, as far as I know, although maybe they have. So this, I think, was a very important step. It shows that the field theory is not in a difficulty by itself. It's just the relativity and the divergences that make it impossible to make calculations. If those things were removed we could really make calculations, accurate ones, without difficulty, that do not necessarily involve expansions — perturbation expansion. It's always been criticized that all we have is perturbation expansions, but I think the reason is that the field theory isn't really wrong. It's divergent, it's incorrect and we can't calculate anything because we get silly answers. We can't find them because we haven't got the intuition to guess at the answer — because it's silly. And when the problem is physically sensible, completely sensible, in the same realm of mathematics as field theory, as this polaron problem is, it's quite trackable. So that's the result of that. Sometime later, questions came up about other properties of these polarons. People became interested in calculating the energy. I calculated also the mass of the polaron, the inertia. By experiment, people got interested in another thing — what they called the mobility, which means: you have an electron in a crystal and you put AC on it and you ask, to what extent does it respond? How much does it move? And that had a finite frequency.

Weiner:

They were interested in this because of developments in solid state?

Feynman:

And experiments and so on, yes. I wasn't really interested in the problem of fluid solid state application. I just picked up the problem as an exercise for the advanced student. You know what I mean; it was a challenge to do, and I just did it because it was easy. But then some of the boys, some of the students, Platzman and Nittings, I think — or maybe only Platzman, or whatever it is — said that they were interested in this practical problem, this problem through solid state, and could I also get other properties? Now, of course, if you can really calculate the energy accurately, you have some understanding of the thing and all its properties, and it's just a question of working some more to get the other properties. People didn't pay much attention, but they complained that all I could do is find the energy. But that isn't true; it's just that they didn't know how to do it. And so I demonstrated by finding the mobility, and I also found the correction. I also calculated that to one higher order and set that up to show that you can improve that. But I've never published that.

Weiner:

But the paper that you did publish —

Feynman:

The paper that I did publish was a very much better estimate of the mobility of a polaron than had ever been determined before. But the methods are quite difficult for people to follow, because they're not used to the path integrals, and they've always been objecting that the path integrals do everything the same as anything so why do they have to learn them? And here is a problem where you have to learn them and they don't bother because, you know no other reason to learn them, or something. They always try to find — it's perfectly natural — a way, in terms of their own way, to get the same results or equivalent. That's what I do all the time to other people's work, so — fair enough. But I do think that it was exciting to me. It still is because it shows the problem, which is beyond the usual range of mathematics, for which one of my invented mathematical forms is useful and can do something nobody else can do.

Weiner:

Rather unique —

Feynman:

Yes — for a change. Instead of finding another way of doing something that everybody can do, I found a way to do things better than they can do by any other way known. So, it still appeals to me. It's interesting. It's all been a sort of side issue for me from the beginning, a sort of exercise and a game. It's been a side issue, and a little pleasure to solve such a problem, but not a directly central challenge. It's just that I happened to notice in the library that this problem was one that was probably within the range of my tools; and it was, in fact.

Weiner:

Now, you published the first paper on that problem in 1955, and then the other one you mentioned was in 1962. What about that period, 1955? I know you went back to Japan for a couple of months and so forth, but what about the work itself? What's the next logical step for us?

Feynman:

Oh, during that period I spent an awful lot of time trying to understand superconductivity. I did an awful lot of calculations and developed a lot of methods, which I've seen gradually developed by other people for other problems. But I didn't solve the original problem that I was trying to solve, which was, where does

superconductivity come from? And so I never published anything. But I have done an enormous amount of work on it. There's a big vacuum at that time, which is my attempt to solve the superconductivity problem — which I failed to do.

Weiner:

But you did publish an article on superconductivity and solidity.

Feynman:

That was probably the result of some meeting where somebody asked me to give a summary of the situation or something.

Weiner:

It was Reviews of Modern Physics.

Feynman:

That's where it's published, but I think that's what it was — some speech given somewhere. No, it's not a work. See, there's a complete difference between these surveys and things like real research work. That paper is not research work. It's just some qualitative remarks, a result of unsuccessful research. It's not worth anything. I don't remember what's in it.

Weiner:

It's a review paper, I assume, it was published there.

Feynman:

No, I don't review. I mean, I wouldn't go over there and say what everybody's doing and what the present situation is. No, I don't write that kind of paper, where you can use it for references to the field or any such. It's not a review paper.

Weiner:

This might be a good time to ask: Have you ever had a paper rejected?

Feynman:

No. No. No. [Slowly]

Weiner:

Just a curious point — you know, it wouldn't show up on a bibliography, it's sort of negative.

Feynman:

No.

Weiner:

Have you ever had any difficulty with referees, in terms of papers?

Feynman:

No.

Weiner:

So usually you send it in and it's accepted as such.

Feynman:

Ever since the first paper, which is the Review of Modern Physics paper on path integrals, in which there was a small objection which I mentioned, there's never been anything. I mean, I send it in and it gets published, just the way it is.

Weiner:

Who would be the likely referee on your papers on quantum electrodynamics, for example? There wasn't a very large group that they could turn to.

Feynman:

I don't know how that works. I myself don't referee any papers.

Weiner:

What was your attitude on that?

Feynman:

Well, I started to try to look at the papers of other people but, you see, I have a funny

thing. To me there's an infinite amount of work involved. I would have to first understand how he's thinking about it — not just understand the problem, but what he's thinking about it. Then I'd have to go and see, is it Ok? Hm. Or what is it? I mean, it's too much work, darn it. It's like almost research: checking the ideas, seeing if it really works, and so on. It's like research, and I can't do somebody else's research. I'm not built that way. I can't think his way. I can't follow and try to go through all these steps. If I want to worry about the problem, I read the paper to get the problem, and then maybe work it out some other way. But it's too much work. Now, to read and just check steps — I can't do it. And then, if a paper comes out that's bad, that's not very good, I'd feel very uncomfortable to say that there's something the matter with it, or that it's not OK, because maybe I'm not understanding. Maybe it is OK; maybe somebody else will see that it's all right. I think it's a lot of nonsense. Finally, I think most of the papers are a lot of nonsense and not worth publishing. And so, altogether it's a miserable business, and I just say I won't review any papers in order to simplify it because if I start reviewing some and not others, then it sounds like a criticism. There are a number of other things — I have resisted the outside world on this and a number of other things. For example, I never give commentary on whether a man is loyal or not loyal. You know this kind of investigation. And I got everybody off my back on that by just saying I won't do it. And I never review papers. And one thing I would like not to have to do, but I can't avoid, is writing recommendations for students. But after all, sometimes nobody else knows them, and they're trying to get a job. So I have to do that. But I find it very distasteful. I don't like to judge other people, or their work, at all. I don't. I don't want to judge somebody else's work.

Weiner:

Ok. That's interesting —

Feynman:

But I have to. It's part of the job with regard to the students. There's no other solution.

Weiner:

And also the fact that there is no recommendation coming from you might be interpreted negatively.

Feynman:

No. That I have avoided in the other cases by making a policy statement ahead of time, before they tell me who they're asking about. I say I simply do not review papers. Or when they send me a paper for review or something, I say, "You're probably not aware of my general policy not to review papers," and send it back. So it sounds like it has nothing to do with this paper.

Weiner:

Now, there is a paper here that sounds as if it might be maybe out of the war date --
“Dispersion of the Neutron Emission in U-235 Fission.”

Feynman:

Right, it is.

Weiner:

With de Hoffman and Serber — this was a holdover?

Feynman:

Right.

Weiner:

Well, unless you think of something to say on that, let's get on to the beta decay thing. I don't want to rush you, but I do think it's the next thing to come to.

Feynman:

All right.

Weiner:

Its '57 that you publish the “Theory of the Fermi Interaction.”

Feynman:

I just want to say that during this period I had been working from time to time on two problems, neither of which I have solved. One is turbulence of fluids, the theory of turbulence, and the other is superconductivity, so that there's a lot of time spent on these things without anything on your list there. They were unsuccessful. I just wanted to mention that there's an awful lot of effort poured into things that don't come out. I did do something on the Onsager problem at one point, when Cox gave a report, or some other problem. And sometimes people say, “How is it you're suddenly working on this?” It's just that I finally got some success. I work often on a large range of things that don't work out. Then there's silence. And then people say, “Why are you suddenly doing this?” Well, yeah, I finally got somewhere on this. It's not that I suddenly did it.

Weiner:

Right — well, the visible part of your work is —

Feynman:

Yeah — comes out of the surface — Now, the next problem is the beta decay problem. Yes, let me go back all the way to the very beginning of my relation to that problem. At one of the meetings of the Rochester Conference, there was a session devoted to the puzzling fact that there seemed to be two kinds of K mesons. One would disintegrate into 2 pi's, and one would disintegrate into 3 pi's. One was called a K and the other was called a Tau. And it became apparent that the masses of these two things were practically equal, so it might have been the same particle. Then at that meeting, there was reported by somebody something which impressed me personally as the greatest. That was that the proportions of Taus and Ks that are produced by the cyclotron at different angles, and at different energies, are almost the same, no matter what the angle and energy. So the possibility was that the same particle was disintegrating in two different ways. But this was against the principle of conservation of parity. Ok. I was rooming with a man at the time — Martin Block. So Martin Block said to me when we were going to bed, after the discussion of the experimental situation on this problem — he says, "All you guys worrying all the time about this Tau-Theta puzzle." Tau-Theta, I guess it was called — there was a Theta meson and a Tau meson, now called the K meson. He said, "You know, from an experimental point of view, it's very easy. It's just the same particle. It's only that your principle of conservation of parity is cockeyed." He said to me, "What would be wrong with assuming that the conservation of parity is wrong?" So I said, "Well, let's see. That would mean you could distinguish right and left, in a fundamental way, but there's nothing the matter with that. I don't see anything wrong with it. But I haven't been involved in these things, and I'll ask the experts tomorrow, hm." So I said, "That's a very good question, and you should ask the guys tomorrow." So he said, "No, you ask them for me. They won't even listen to me." "All right," I said, "Ok." So I got up and I said, "I'm asking this question for Martin Block." I've been teased a lot about that. People tease me on the grounds that I said that because I thought it was such a ridiculous idea. But it was quite the opposite. I said that because I wanted to establish the correct priority for the idea. I swear that. I mean, it was not because I thought it was silly, but because I thought it might be possible. And it was so good an idea, and might be possible at that time, that I wanted to be sure they knew where it came from. I said, "I ask this question for Martin Block. What goes wrong if you assume —" you know, that parity is not violated. And I think Lee answered it, or something. It was a long complicated answer that I didn't understand. Then afterwards Block said, "What did he say?" And I said, "I don't know, Martin, what he said. It seems to me still possible that parity is violated." Martin tells me, but this is not my direct experience, that he went home on the train with Lee and argued with him.

Weiner:

I thought it was the plane. I've heard this story, I think. Was it the plane or the train?

Feynman:

I don't remember. Maybe it's plane. He argued with Lee about it, explaining again and again that it could be, and trying to prove that it wasn't impossible. Lee was thinking it couldn't be, I guess, although I wasn't there. Anyway, as far as I was concerned, that remained a real possibility but with a long chance, because of the prejudices that I had. A long chance — but the thing was a deep puzzle. And I appreciated the seriousness of it from the data I had just heard about the angles and the energies. It looked very much like the same particle. So I knew that it was possible. I didn't think it was likely, but I thought it was a real alternative. In fact, some time later Ramsey, Norman Ramsey, asked me about this. He says, "There might be something the matter with the parity," because it was getting in the wind now. "Would you think I should do a parity experiment on beta decay, to see if it was, you know, symmetrical or not?" I said, "Yes, you should do the experiment, but the odds are it will turn out symmetrical." He said, "Will you bet me 100 to 1 that I won't find —" I said, "No." "Will you bet me 50 to 1 I won't find it's asymmetrical?" I said, "Yes." So we made the bet as to whether parity was conserved. I mention this story because I was prejudiced against thinking that parity wasn't conserved, but I knew it might not be. In other words, I wouldn't be 100 to 1, just 50 to 1. Ramsey said, "Well, 50 to 1 is good enough for me because the goal, the yield, of course, would be so important. If you think that the chances are as good as 1 in 50 that this may be wrong, it's certainly worth doing." So he said he was probably going to do this. He didn't, unfortunately, get to do it. In the meantime, as you all know, Lee and Yang developed the idea and further proposed definite experiments which were done by Wu and other people, and the parity was found to be violated in beta decay and in mu decay.

Weiner:

This meeting in Rochester, I believe, was in 1956.

Feynman:

I think so. Right.

Weiner:

Morton Kaplan told me the story of someone — I didn't remember the name at the time — getting up on the floor and Block defending this view, either publicly or privately, and on the plane, the way I heard it, lecturing him for not understanding that he is going to violate conservation of parity, and he should know better. This is, of course, a bit of

folklore now, and so —

Feynman:

But this may be a very difficult question. You'd better check sources, because it may be that Block exaggerates the situation of the conversation on the plane and tells several different people. I got the story from Block. Couldn't it be that your other source got the story from Block?

Weiner:

Yes, Morton Kaplan did get it from Block.

Feynman:

Ok. So there you are. It has to be checked in some other way.

Weiner:

I read the proceedings of the conference.

Feynman:

Yes, but I'm talking about what happened on the plane. It has to be checked some other way. The Proceedings of the conference do say, "I'm asking this question for Mr. Block" in there. They say what I said, yeah. They also say at the end, "Well, I think it's time to close our minds again," says Oppenheimer.

Weiner:

Yes, at the end of the discussion.

Feynman:

Yes, that's right. He felt that the idea was so ridiculous. That's what it sounded like, you know.

Weiner:

He said, "We've ranged over this, you know. We've had a lot of fun with it. Now we can close our minds again."

Feynman:

Yes. That was amusing.

Weiner:

All right, then —

Feynman:

OK. I got interested in the beta decay all the time, and there were some people doing research at Caltech — Berne and Wastrow and... no Jensen, but a friend of Jensen, a third guy. I can't remember his name. Maybe I'll remember it later. Anyhow, these fellows were doing some experiments in beta decay and testing some of the — Oh, no, I've jumped a little bit. I've jumped a little bit — wait. There was a meeting in, I believe, Rochester, another meeting in Rochester, where by this time all the excitement is out — that is, that there is a violation of parity. And the question is what's the law of it? Well, I sometimes have trouble keeping up. I get behind and I get discouraged. I was telling my sister that I wasn't doing any work, that I can't do any work, and so on. And I said, "There's this paper by Lee that he's going to talk about tomorrow, with the parity violation, and I don't know anything about it myself. I can't understand it." She said, "What do you mean, you can't understand it?" "I don't understand it." She said, "Did you go down through it step by step?" I said, "No, I tried to figure the same thing out myself." She said, "Listen — for once, it will do you good, my young friend —" (my sister's a physicist) — "you sit down with the paper, like a student, and instead of guessing what the heck it is, you sit down and you do it step by step." "Ok." So I sat down and did it step by step, and it was very simple. It described the particular way that the parity might be violated, that the neutrino only spun one way. When I looked at the forms of the thing, I realized that there was another way of expressing it. It may be that the electron, muon and so on, are also coupled with a certain component of the Dirac equation. In playing with path integrals, I was forced into using a second order form for the Dirac equation, which was not obviously parity conserving. There was a wave function in the thing. It just looked parity conserving. And if I used that wave function, that was just the combination of what's called, 1 plus gamma 5 times the other wave function, which was appearing in front of the neutrino. But I thought, why not put it also in front of the electron? So I put it in front of the electron and the neutrino and the mu and so on, at that time, and concluded that I would have to have vector and axial vector coupling, and that would get the same result that those people got for the mu spectrum, but with the opposite sign on the spin. And electron disintegration in every beta decay would always have to have the same polarization, whereas there were some clues that had different directions of polarization for different elements disintegrating. Anyway, it would be nice to know what the record of that meeting was. I got up and I proposed this theory, in which I thought it was in front of the electron and the mu, and not necessarily the neutrino. And I thought that I would predict that the spin of the mu is backwards, that Lee and Yang have the sign reversed, and that when we figure it out

the direction of the spin of the mu will be the opposite of what they say. It was opposite. I mentioned some experiments, and people who were trying to measure it talked to me about it — how we could distinguish the things and so on. Then I was asked, what happens with the beta decay, with the nucleon case? I would say, “The trouble with my theory is it predicts it has to be V and A; it can’t be S and T,” and I made all kinds of complications. I got into a long complicated thing. It was only the night before that I worked this out, see, at my sister’s house, because she’s in Syracuse and Rochester’s nearby, and I stayed there when I went to the meeting. It was only the night before. I hadn’t had much time. I knew its S and T. Everybody knew it’s S and T — two different couplings, scalar and tensor. Whereas this theory would say it had to be V and A, and it wasn’t. So that was wrong. There must be some complication with the nucleons, and I told them about various ways I tried to kind of mix it up to get it to be like S and T, but I couldn’t get anywhere. It was mixed up and I wasn’t happy with it. I presented it just as another alternative. I realize now that there was something important that I hadn’t noticed — that in the Lee and Yang paper, the mu decay involved two constants, F1 and F2, I forget what the letters are. They noticed that the data fitted if F1 equals F2. So they said F1 equaled F2. But they got it from the data. I wasn’t very careful. Once I got my idea, I was careless again. In my view, the fact that F1 and F2 were equal was necessary, you see, and if I had noticed that, I would have realized I really was one step advanced. I wasn’t sure whether I was just saying it in a different way, and wasn’t saying any more than they were or not. I thought I didn’t say any more than they did. I said something different, but not more. I hadn’t realized at that time that the fact that F1 and F2 were equal was not a consequence of their theory, but was a result of the experiment. In mine it was a consequence. I would have been much more impressed by my idea, because it would contain something that hadn’t been noticed. You know, I would have realized I was a step nearer the truth. But this way I thought I was only as close as they were, with an alternative view. And then — well, nothing much happened. I went to Brazil on a trip. During this time there were lots of measurements, and there were all kinds of inconsistencies. People were measuring spin of the electrons to the right and to the left and everything. And in that theory I had had it always one way, to the left. And they were getting all kinds of answers, and they were getting inconsistencies, and they were measuring everything over again, and everything was... I came back from Brazil, and I went to Wu’s laboratory to visit. She wasn’t there, but one of the people there told me all the data that was available now, the new data. See, I wanted to work on it again. Then, in passing, I visited my sister, and I said, “I can’t do any work, I don’t do any work.” She said, “Listen — you have done it again and again. You have many times told me about an idea, like the parity idea, that you thought was right. You told me that Block might be right. And you don’t do a damn thing about it. You should write it up, for crying out loud, when you have something like this. You told me V mesons must disintegrate with another meson — didn’t even write it up. And you told me many things,” she said, “which later turn out to be true, that you don’t write up. And you say you have nothing to do. Well, last year, when you were here, you told me about the beta decay as another kind of law. What you must do,” she says, “is just go home and write that up, that’s all. Just write that up.” I say, “Ok, I’ll write that up. I’ll go home and write that up.” I went

to Wu's lab to get the data, and then I went to Caltech. I came in from the vacation and I said to the boys, "What's new with the data?" There were all kinds of experiments. And they told me there were all kinds of experiments, everything's inconsistent, everything's all mixed up. They told me about their own experiments, other people's experiments, and so on. I didn't believe other people's experiments because I didn't see their equipment (because I know how to check, evaluate experiments). I'd seen what those fellows had done, and how they measured, and I knew everything inside out. So I knew it was right, whereas I didn't know the others were right. So I only paid attention to what they said, and not to what the others said. See, I had a certain prejudice. I was therefore correct, though, because the experiments were right. I knew they were, because I had looked at the way they did it. So I knew. I had the advantage over some people, at least, in that I had a little selector that would select some experiments at least that were right. And I threw away everything I hadn't looked at. In their explaining to me how it is, they show me that for each experiment there's an opposite experiment. For everything, there's something that shows the opposite, and it's just a chaos, an abject mess. Finally — Speck was the other guy, Speck — Speck or Wastrow or one of them says to me, when explaining this whole thing (I'm sitting on a chair, and they're pouring all this data to me, and everything's inconsistent, and they're trying to show how terribly impossible it is to understand) — finally he says, "It's so mixed up that Gell-Mann says it could even be V. vector, instead of scalar." Well, the possibility that it was vector instead of scalar, which was now available, released the trigger of memory, and I flew out of the chair at that moment, and said, "Then I understand everything. I understand everything and I'll explain it to you tomorrow morning." They thought when I said that, I'm making a joke, because they had just said to me, "It's so confusing that nothing — everything is all mixed up —." But I jump up: "Then I understand it all!" Ha, ha, ha. And ran away. And they thought I was making a joke. But I didn't make a joke. The release from the tyranny of thinking it was S and T was all I needed, because I had a theory in which if V and A were possible, V and A were right, because it was a neat thing and it was pretty — except for this one trouble with nuclei which was so complicated. But if this was possible, then that must be right. So I ran home and I started to figure. And I figured out the rate of disintegration of the neutron and the mu. You've seen these papers. I noticed that they were the same rate, according to this theory, within 9 percent. I wasn't sure what that meant. I was worried about the 9 percent. I wanted them to be equal. I thought they had to be equal. Then I calculated a few other things that predicted that the spin was always one way and so on. I calculated all the things which fitted with the stuff from Caltech data, but not necessarily other data, and kind of organized and checked various things, and concluded I was on the right track with everything. I got it right. Everything was all right. One of the mysteries up to that time had been why isn't it simple, like pure S or pure T? V and A had to go together with these things. Before it was two kinds of beta decay, what they call Fermi coupling and Gamow-Teller coupling. It's one thing now, so it's very beautiful, simple — the understanding of a great mystery and so on and so on. So I was pretty sure I had everything right, except for the 9 percent. And I remember I went to a restaurant up here to eat, and some physics guy came in. And he said, "How are you doing?" I said, "I just got the formula for beta decay — only trouble is, it's off

by 9 percent.” By the next morning, I’d forgotten about this little annoyance of the 9 percent, because various other checks that I can’t remember now convinced me more and more that there can be nothing wrong with this, that this is absolutely right.

Weiner:

How long did you work on it?

Feynman:

Overnight.

Weiner:

All night long?

Feynman:

I don’t remember. I can’t remember, because there were times when I worked — it was probably nearly all night long. Very likely. Then I called my sister up, during the night, at night, and said, “Thank you for making me work on this. I really have found something. I understand beta decay, except that it’s 9 percent off, which I don’t understand. It should be equal.” So by morning, by the next morning, I forgot the 9 percent. See, so many other things agreed, everything was going nicely, the form of the mu decay spectrum, the data that they showed me about the directions of polarization, everything was just right. Everything was just locking in. It was clack, clack, clack — if I selected the right data. It required no selection. I’d already made the selection — I had the Caltech data. All this worked. So I then ran down and I went to Christy’s office. Speck was there, or somebody, maybe Wastrow too and maybe Forbirm — I don’t know, a couple of guys — and I said: “Listen, I got beta decay understood; I’d like to explain it to you.” The law, you know, the parity violating beta decay. So they said, “Ok.” I started to say, “I’ve got it right, everything fits, and the mu decay rate and the neutron decay rate check into each other.” So Speck says to me, “What beta decay constant did you use to do that?” I said, “The one in the book, by Siegman.” That’s the only thing I had. I had borrowed it, in fact, the day before, to get the number. He says, “Well, it’s too bad. We’ve got new data now, and there’s a lot of checks on it, and we know that that’s off by 7 percent.” I said, “Listen, fellows, I’m not kidding, mine was off by 9 percent — my figure. Huh?” They said, “You said it agreed.” I said, “No, no, I forgot, it’s off by 9 percent.” They thought I was kidding, you know. But it was really off by 9 percent and the question now immediately was the 7 percent, which way does it go? Does it make mine only 2 percent off, which would be brilliant — I could do already a prediction, you know, it checks — or else, I’m 16 percent off and I might as well go home and faint. Which way? If you’re ever running for an airplane in a taxi, and you suddenly discover that you’re using Daylight Saving Time instead of Standard Time or something like that,

and you don't know whether you're going to make the plane or not, and you try to figure out, which way? — you know how complicated it is. Well, this is the same kind of way. There were so many signs, you know. Christy said he'd think it out, which way. Because they said, "The constant is larger, which means something." Ok. "Now, let me think, if the rate of this is higher and the rate of this is lower, and this —" you know, which way it would go? So Christy said, "I'll think it out by myself, quietly." He runs. And I say, "I'll figure quietly." And just at this moment, the telephone rings; my sister calling from New York. "Well," she says, "very often you've told me you've got something, and the next morning it isn't there. Is it still there?" I said, "Listen, I'll call you up in two minutes. Can't stand to talk to you now. I'll call you in two minutes." She says, "What's with the 9 percent?" I say, "It may be 16, it may be 2." Clink. And it was just at that moment that I quietly did it. It only took three minutes. But the telephone call came just at the time when I couldn't possibly answer her. It was impossible. So I didn't even talk to her. Then we went very carefully over, and it turned out that the error was 2 percent, which is very close to nothing. Christy got the same result, and we went back over it and we argued it with Speck and we proved it — simpler and simpler until we were absolutely sure which way it was, and there was nothing wrong with it. It was only 2 percent off. So that was a very exciting moment. Then I called my sister back and said, "It's better than I thought. It's 2 percent off and everything's all right. We've got the formula." Then I sat down and wrote this thing up.

Weiner:

Right away?

Feynman:

Yeah, right away. I did a lot of work on it. I immediately turned my attention to the problem of the weak interactions of the strange particles, and what this would say about them, and worked out a lot of stuff to see what it said and to what degree it agreed. Sometimes the rates were several times too big or too small, and there were some puzzles. But still I knew a lot about the pattern of beta decay from this, and I wrote this thing up. Then Gell-Mann had come back from somewhere, and we talked it over. It was his original idea that V may be wrong, and he was uncomfortable. And this is, again, something not for publication. May I tell you something privately?

Weiner:

All right. Do you want to turn this off?

Feynman:

I don't know what to do about this thing. Turn it off, and you see whether. Ok. Well,

Murray had been working on a similar thing, and had developed — I think with Rosenfeld — a summary of the situation and had seen all the various alternatives, among others that it might be V, and in fact it might be V and A. So we discussed this work that I did. I discussed it with Murray. He improved a number of ways of looking at some of the stuff with the weak interactions for the strange particles, ways of expressing relations, for instance, with delta S and delta Q, and so on. So he made some comments on the paper, and so on. And since he had originally got the idea that it was V, which set me off, we decided to write the paper together.

Weiner:

I see.

Feynman:

So we put both names on it.

Weiner:

I see. So instead of your paper, as originally intended, and then perhaps a separate paper by him, you decided to collaborate.

Feynman:

Right. We put it together. So it looked like nobody was fighting with anybody. We just put it together and wrote it together.

Weiner:

Now, isn't this the discovery, as you described it before — yesterday, I think, off the tape — that gave you a certain very special kind of feeling about scientific work?

Feynman:

Yes. Right. I don't know about everybody, but what I think is most impressive is, like when I read about Dirac, for example. I also get a similar feeling about Maxwell. When, say, Dirac got the equation he knows something about nature that nobody else knows. And it is a miracle that it's possible, by doing experiments over here, to predict what's going to happen over there. It is not as much a miracle to predict something if you know the laws about it. In other words, it's enough of a miracle that there are laws at all, but what's really a miracle is to be able to find the law. It's another kind of miracle. You see, knowing a law to figure out that such and such is going to do something, and then have nature do it — OK, that's pretty good. But to look at other aspects and to guess, and to

know that there's a pattern under there, and to tell nature that in this experiment she's going to do that — no by deduction, strictly speaking, from what's known but by guessing from what's known — it seems a wonderful thing to me. And I always wanted to do that. Now, my work in electrodynamics was really using other people's formulas. My electrodynamics is not unequivalent to the electrodynamics of Pauli, Dirac, and so on, in 1929, with some technical improvements and methods of analysis and so on. It's fundamentally the same thing. Also, even the diagrams and so on only help people make calculations, and therefore makes predictions, but with a basic theory which is essentially not my own. The work with helium I got a great deal of pleasure out of also, but it still wasn't exactly that same category, because in the work on helium I had the Schrodinger equation which I thought was going to give the helium. The puzzle here is, how can that equation ever lead to that phenomenon? But that's still not exactly the same. But here, for a moment, that night, a couple of nights, I have a knowledge of a law and I can make predictions analogous to, but nowhere near as important or as vital and marvelous as, the Dirac equation or Maxwell equation. It's just a small piece, but at least I have the moment when I've a new law, and could predict nature for a while. You remember I said that I was uncomfortable and told my sister I was unable to do work anymore; I'm worn out. And she said, "At least write it up," and so on and so on. So I was uncomfortable that I wasn't doing anything. And I suddenly got this thing. And then I finally said: "Well, I've done physics now. I've finally done some physics now, and I don't care if I never do anything more." I don't mean I didn't want to do anything more, but I wasn't going to feel any more that I'll be uncomfortable and unfulfilled, in the sense that it was an aim that I'd never fulfilled. I felt now that I'd fulfilled my original dream of one day discovering a law which was unknown. It turned out that that law had been guessed by Marshak, and perhaps by Salam and I don't know who all, earlier than I perhaps. But it doesn't make any difference. That doesn't bother me. Maybe it's bothering Marshak because all the glory comes to Gell-Mann and myself, and that may not be fair for all I know, because I don't know the situation. I don't read what the others are writing and doing. And I think it's possible, although that would have to be studied by an historian, that those other guys got the bad end in this case, and the names that are associated with this thing are not in accordance with priorities. I don't know. I haven't the slightest idea. I was told this after I discovered it. So I don't know, but it's possible. But that doesn't have anything to do with it. It's not the name and the priority. It could have been that they discovered it. As long as I didn't know they discovered it, for that moment I knew something and I had found a law, and I could make predictions about nature, which is the aim that I had. And the fact that somebody else was already making the predictions, unbeknownst to me, in no way takes the pleasure away, in any way. So that was really a great moment. I would like more moments like that, but I don't have to ask the gods for everything.

Weiner:

You attach more personal satisfaction to this than to the quantum electrodynamics work?

Feynman:

Each of the jobs has its own satisfaction, of course, in its own way. The quantum electrodynamics was somehow of a wider importance. Well, I don't know, they're all different things. I got a real kick out of the helium, for a very peculiar reason. In order to do the helium problem, I had to reason about wave functions in many, many particles, and I couldn't write anything. It was not possible to write equations. I had to think the whole thing through. The hardest part of the helium problem was done by physical reasoning alone, without being able to write anything. By just standing — you know, I remember kind of leaning against the kitchen sink, you see, and looking at it, and just thinking. And it was very, very interesting to be able to push through that doggoned thing without having stuff to write. That is, the real understanding of why the other excitations were higher in energies than the low ones. When I finally wrote it up, I found my argument somewhat slightly more quantitative, but it's still a great deal of what's called arm waving — but correct. There seems to be a thing that you can't think that way, but you can think that way. Mathematics is supposed to be more accurate. But the thing that's not fair — If you take another system instead of helium, a system that's idealized, and then show perfectly, rigorously, that such and such a phenomenon will occur in the more idealized thing, you still don't know whether it's going to occur in the less idealized thing. So it's no more rigorous, therefore, than to take the less idealized thing and use a less quantitatively precise argument, and see that it's going to happen. But that's not the fashion. The fashion is that it's much better to take a model, which is to one side, and do it carefully and rigorously show the phenomenon, than to take the complex situation and, using the best reasoning you can, show the phenomenon. My style is the second one, but I don't feel it's worse than the first one, because although the first is rigorous, it's on the wrong problem. There's a famous joke about the fellow who's looking all over in the grass, under a street light, for keys. He's looking, he's looking. The policeman comes along, "What are you looking for?" "I'm looking for my keys." "I'll help you," says the policeman. They look around, they look around. So finally the policeman says, "Are you sure you lost the keys here?" "Oh, no," he says, "I didn't lose them here. I lost them halfway up the block, over there." "So why are you looking here?" "The light's better here." So the same way — the light's better with the idealized problem, and that can do something. But the question is, after I've found this, do I know about the other problem? Do I know what's really going on? Anyway, in the helium problem, I got a great pleasure out of something which is hard work. I never had to think so hard, so abstractly. I had nothing to write. You see, while you're thinking, you're usually writing some things. Or else you think for a little while, then you write some things. You're thinking mathematical expressions. But this was sheer thinking. I can't explain how I did it — visualizations and picturing and so on, with very little writing.

Weiner:

Well, you visualize a sheet, and —

Feynman:

Visualize, no, not a sheet of paper. Like that sheet — that was later. But the hardest part was to see why the excitations were higher in energy, which is the second paper, I think. And that was a sheet mental effort — and that was fun, for that reason, you see. I mean, there were various funs. Even the polaron was a big kick, because here was a problem they're all struggling with, and it was just so easy. Plink, and I invented a new scheme, solved the problem so much better than anything before. It was just like Superman, sort of, kind of fun. You come in and you solve the whole thing with no effort, you know. And so on. Each one had a pleasure. I'm not Dirac or Maxwell; the thing wasn't that important. Others have gotten it about the same time, and it wasn't necessary for me to have lived in order for it to have been discovered. But aside from all those things which are not essential, I don't feel that it makes any difference. I am satisfied that I had the pleasure of seeing how it feels to know something, to get the trick by which — I don't know how it works — you can find out what nature's going to do in another situation, one that's different from the ones that you investigated. That's not some aspect where it's just more particles of the same kind. You just have arithmetic to do. But it's different where the situation is fundamentally different than the things that you tested.

Weiner:

And this is a different type of a situation. You're not judging this by its importance in the field, whether the idea dominates the field, QED, it works etc., but here — on its impact on you personally.

Feynman:

That's right. It's my relation with nature, you might say, because it's much closer to this strange phenomenon. It's a strange phenomenon to me that by guessing the question should be simpler, you are able to predict something. It's a weird sensation.

Weiner:

Now, you didn't pursue that, did you?

Feynman:

What, weak interactions?

Weiner:

That's right, this business.

Feynman:

No. I wrote the paper, I calculated everything I could calculate. I tried very hard to understand the details of the weak interactions' disintegrations, which were puzzling in many ways. And tried to use that as a clue to some symmetries among the weak interactions with Gell-Mann. We worked and we worked. We made many many models, and finally I kind of tired of the thing and decided there wasn't any simple symmetry. Then somewhere in Switzerland I discovered it, a possible theory, which I wrote up but never published. Looking it over and thinking about it, I decided there were too many things that didn't fit per things that did fit, and it probably wasn't true after all. Then I got gradually tired of this problem and couldn't think of any more symmetries. But Gell-Mann did not tire and got the final symmetry, which is the SU3 pattern, which I didn't come close to as far as I know. I can't find in my notes anything that is like that, as far as I remember.

Weiner:

When did Switzerland come up; how did you get there? I don't want to distract you from this.

Feynman:

Well, I'm finished with that. That's the beta decay.

Weiner:

You mentioned that you were in Switzerland. I was just wondering, was it a trip?

Feynman:

No, that was a mixture of two purposes, because I was in Switzerland twice and kind of wandered back and forth — to Holland? No, yes, no — yes. No. Yes. I went to Switzerland first for two reasons. One was, there was a meeting there. I think it was the High Energy Conference, this time at Geneva. Yes. Definitely. And then later in the year, there was to be a conference called Peaceful Uses of Atomic Energy. Gell-Mann and I or perhaps just me had been invited to give a talk at the Peaceful Uses of Atomic Energy Conference. I wrote some kind of an article or paper for it. So there is another published thing that's probably not on your list, on the Peaceful Uses of Atomic Energy, Princeton, 195(?) — I don't know what — the same year that there was a Geneva conference. That year the Rochester conference was in Geneva. I wrote on the weak interactions, and someone in the State Department asked that Murray's name be on it also, in order to impress. This was very unfortunate altogether. I don't mind Murray's name on it, that's not the point, but this kind of crap. They call up — so many Russians are going to talk

about this thing. They have to have more Americans talk about something scientific. We have to have some kind of balance, and it would be better if both your names were on because it looks better. It's just crazy. Absolutely crazy. Anyway, I did give the talk to the Peaceful Uses of Atomic Energy Commission meeting, and it contains some new ideas that both Gell-Mann and I had worked out together. I wrote it, but I had Gell-Mann's ideas too, so there was nothing wrong with putting both our names on it. But there was some complication. I can't remember the details. They asked me to do something funny with the paper. They wanted to get more names on it — something fishy — this stuff about propaganda mixing up with the science, you know. It was published in the Proceedings of the International — It's very amusing that, when I was there during the first period, I went to the Geneva Conference on high energy physics. After that I was hanging around there for a while, and I thought I'd go on a tour of the Palais de Nations or something, whatever it is, the United Nations Palace. And when I was on a tour, I didn't even think, you know, for a minute that I was going to come back here and talk. It just escaped — just didn't come to me. And I walk into this, and the guy explains that this is the big symposium and so on, you see, and he shows us this room. There were great doors that opened, and there were red carpets running along the sides. There were loudspeakers, simultaneously translating things, a dais where you stand up with several layers of things, and then this big thing where the guy stands in the middle and lectures. It was really very impressive, you know, and I just thought to myself: Boy wouldn't it be something to stand in front of here and give a lecture! So crazy. You know, I thought — Boy! And then I suddenly began to remember that I was going to come here to give a lecture at the Peaceful Uses, see. So I asked the guy, I said, "Listen, there's going to be a conference on Peaceful Uses. Where is the lecture going to be? Where are they going to have things?" "Oh," he said, "the building's out there, they're going to have exhibits and so on." I said, "But the main sessions, the full sessions, where are they going to be?" He says, "In that room." And it was a big surprise. I was so taken aback by it, I said, "Oh, I'm going to talk there!" I always dress comfortably, and not just to look good, you know — dressed with no tie, just a shirt, dirty pants, you see. And I was on this tour. So when I say, "I'm going to talk there," it was incredible. "Oh, indeed?" says this fellow, "indeed? Well, we'll look forward to hearing you," you know. Then I realized, how stupid, I shouldn't have said anything, but I was so surprised — "Oh," I said, "that's where I'm going to talk, later!"

Weiner:

You did a paper with Gell-Mann. You worked with him on certain things.

Feynman:

Yeah, we did a lot of work together on various things.

Weiner:

For example — well, I'll get something pretty recently — 1964. So we'll skip that for a while. There was something —

Feynman:

I met my present wife in Geneva at that time.

Weiner:

That's interesting — on that trip? This was, what year? We can properly date that here.

Feynman:

Well, I can't tell you.

Weiner:

That's an important event.

Feynman:

I met her at the beach in Geneva.

Weiner:

When were you married?

Feynman:

Some years ago. My boy is four. It must have been five years ago, six years ago.

Weiner:

I'd expect — that fits the timing pretty well.

Feynman:

It was about two years earlier that I had met her. What else you got?

Weiner:

There's another paper — I don't know if it's of any importance or not — that's done with some students, with Vernon and with Hellwarth. Were they your students?

Feynman:

Sort of. I don't know. Maybe one of them was and the other wasn't.

Weiner:

"Geometrical Representations of Schrodinger Equation for Solving Maser Problems."

Feynman:

Yeah, well, around that time the maser was discovered, and the laser, and so on, and there were a number of technical problems. This was when quantum mechanics was entering electrical engineering, in a way. And so there were some fellows — Hellwarth, Vernon, and some man from the electrical engineering department whose name I don't remember — who wanted to know more about it, to understand the quantum mechanics as applied to these things. How did the maser work, and how did the laser work, and so on? Maser, not the laser. (It hadn't been invented.) So I gave a little series of lectures to them, in which I explained all the equations that they were finding in the various papers, and how to calculate things. I just used the model of a two state system. I used the electron as an example of two states for which we know how to work things out. Hellwarth thought that that view was a useful one and might help other people, and suggested that we publish that. So they wrote it up and published that idea. And he showed how various of the formulas that he found in other places could easily be seen, more or less, by using this model of a spinning electron. But it was really a minor, very minor point. Anybody could have thought of that. That's nothing.

Weiner:

Then, a few years later, in '63, you did work with Vernon again on theory of the general quantum system.

Feynman:

Yes — the general theory of the lasers and the masers and so on. I always do everything by path integrals, so I was giving these lectures explaining things by path integrals and proving one item after another. I hadn't followed, never did follow the literature, to find out how many of these formulas were derived by other methods, and whether this particular method has any advantages or not. But Vernon I think did a thesis with me on the theory. I outlined how to do it. In fact I, as usual, worked out most of the things independently, and he just went through again to kind of check them. I knew it was soluble this way. So we got several formulas in the general theory of the linear systems. See, the connection of quantum mechanics to classical mechanics is involved there, when the thing gets bigger and bigger. At low enough intensities, a radio cavity resonator

is a quantum system; at higher levels it's a classical system, and the relationship between the two is worked out in detail in those things, and so on. So you can get formulas that describe the behavior of these systems in any level range. At lower levels it approximates quantum theory and higher levels it approximates classical theory. Although it's low enough level that it's quantum mechanical in some sense, it's not a quantum mechanical system in the ordinary sense. It is not simple — a big cavity, with lots of electrons, with wires sticking in, with detectors tied on, with antennas hooked on it. Nevertheless, it's working so low in intensity that quantum mechanics is necessary, but not because of the quanta. Really, it's the quantum mechanics of BIG electric circuits, so to speak — microscopically big, microscopically energetic. It's interesting. I'm sure that other people have done it by density matrices or by something. But I don't know the other ways, so we worked this out. All the equations and theory of this stuff, in this particular manner were worked out and published. Then another man named Wells, using the operator calculus that I had invented, found that he could do it even much simpler. And he published another paper that's not on your list. I don't think my name is on that one, but the inspiration is very closely related to my work. So it's connected, but the same problem is solved in an even more brilliant, simple, easy to understand way. But I don't know, because this field is a little outside of my own, whether those papers are worth anything, whether people need them or need to read them, whether they contain any equation that isn't anywhere else (which I doubt), and whether the other people can't derive all this stuff much easier. The conclusions are simple. So there's nothing complicated there, and I doubt that there's anything special. I don't know. I don't know the field.

Weiner:

There's one other paper, and I think at one time we discussed this off the tape, that appears to put you in a biological field. It's in 1962: "Mapping Experiments with R Mutants and Bacteriophage." Was this a question where this group asked your cooperation on a certain aspect of a problem?

Feynman:

Oh, no. No.

Weiner:

You remember the paper?

Feynman:

Yes. I got interested. I've always been interested in biology. I'd been traveling every summer somewhere, and I thought this time, where am I going to go? And I said, the hell with it, I don't feel like traveling. Instead of traveling, I'll do biology experiments. I'll

go into a different field, instead of going into a different country. So I went down to the biology lab and told them I wanted to help them out. I had expected that I would just be like a lab assistant: you know, do routine measurements for them, and so on. I went down to Viereck's lab and they said, "No, you can't do routine things. You must learn to do things right. You do some research, and we'll treat you like a graduate student and show you how." So they taught me how to plate bacteria and such from pipettes. It didn't take very long to learn the techniques. And then they assigned me a problem. They suggested a problem, just as they would for a graduate student, having to do with phage research. I started to work on the problem, and I made some discoveries. Those discoveries are not described very well in there, because that's just a mapping. They needed some of the mutants that I had as just a mapping of where certain mutants are, and they have to mention some of the mutants and the distance they are from other mutants, which is part of this thing that I did. But I discovered some kind of special suppressive mutants which, it turned out later, was worthwhile. I mean, I didn't follow it up, and I didn't publish it, which I'm very unhappy about. I was urged by everybody to write it up because it was worthwhile, but somehow I never got around to it. And so I never wrote it up, which is too bad, because those mutants were interesting. They were unusual; they did funny things, and I had discovered them. Later it turned out that other people discovered them, and they found out that they had a different interpretation as to what they are than the interpretation that I had. They made real advances. But the mutants themselves of this character were important in future biological research. And I knew they were very interesting and unusual, but I didn't write it up. And I did a tremendous amount of work. I mean, I was very careful about all the experiments, and I did lots and lots. It was good biological research, as far as what the other guys tell me. Nothing wrong with it. It was real; I discovered something. I did a lot of work to do it.

Weiner:

Was this one summer?

Feynman:

One summer. Then I had a sabbatical leave, and I spent the year doing work (or nearly, I didn't spend that much time) on microscomes. But I never did get material under control. It involved grinding, a grinding operation, and I gradually realized that this has to be done in some other way that's a lot more under control. I couldn't get the things to repeat very well. I got tired of it because I was trying to do gravitation, the quantum theory of gravitation, and felt responsibilities to physics. So then I finally stopped.

Weiner:

Where did you do this sabbatical year?

Feynman:

Here.

Weiner:

Oh, I see, instead of a vacation.

Feynman:

Yes.

Weiner:

Do you find any basic difference or similarities between so-called biological research and physical research?

Feynman:

The work I did was with viruses and microsomes, which means taking apart the bacteria and so on. I have never done much experimental physics, except when I was a kid in a laboratory. However, if you want to make a comparison, I would say the following struck me very strongly. The general methods of criticizing experiment, and understanding when a thing is really known and when it is not really known, are almost the same. So I came with a talent of knowing how to criticize an experiment and knowing what the hell I was doing, and so on. The physics knowledge is infinitely useful. You see, everything is made out of matter, and so there's an awful lot of physics in what you're doing. I understand immediately how a centrifuge works, why this is proportional to that, and I don't have to learn that double the time is twice as much, that it goes down faster near the outside, and that the formula's so and so. I understand everything about the centrifuge and how it works and how it selects the particles, and why it may change. I understand each kind of measuring equipment. I understand completely what's involved in the ultraviolet absorption method of determining how much DNA there is in the tubs, or RNA, and so on. In other words, the equipment that does measuring — like wave length scale — I know what it means. Everything is clear. Furthermore, as far as what's probably going on molecularly and so on, it's also clear — like what the effect of temperature is going to be. I don't mean that the biology is clear, because there's a complexity involved. There's a whole lot of stuff that's hard to explain, and there's a tremendous amount of junk that I know immediately. Also, such things as how much statistics I've got to take so that the pluses and minuses are less than the uncertainties and lots of other things, are perfectly obvious to me. That's a very great advantage. So there's a great deal that goes over from the one science to the other, and the most important thing that goes over is the character of the science, the critical character. It's very very much the same. The sources of dirt and the sources of error are physically

different. But you can still get an idea: does it make sense or doesn't it make sense?

Weiner:

You felt at home, then.

Feynman:

I certainly did. Yes. I had no trouble with it.

Weiner:

There's a chance that you might be dipping into that field again?

Feynman:

I don't know. I don't know. It was fun. It was a tremendous amount of fun, and it was good. I had to learn from experience. I learned several things from experience. If I did more research, I would do much better research, more directly and more satisfactorily. Although it is characteristic in biology, because of the complexity of the situation, that one makes operations which are more or less crude, like grinding the things, and not always exactly the same. That's a weakness. It would be worthwhile if you could get the conditions under control. It's very difficult. The one thing that's harder in biology (I think, probably, I don't know because I haven't done enough physics experiments) than in physics, is to get the things to repeat; i.e., to get everything under control. To get everything under control is harder. If you look into the microscope at the bacteria, for example, that you're using, and in which the viruses are growing, you see they're all different sizes and in different stages of growth. They're wiggling different ways — it's a hell of a mess. Well, now, you can't control each bacterium, but you could control the average. You must make everything identical. It's hard to repeat the experiment.

Weiner:

Well, it was an interesting interlude.

Feynman:

Yes, it was fun.

Weiner:

I was thinking that perhaps now, if you feel up to it, might be a good time to talk about the Lectures on Physics.

Feynman:

Let's see, have we got all the physics now? I guess we've got all the physics, all the accomplishments.

Weiner:

The major things. There are books that come out of this. There's something that follows the Lectures, and that is a paper with Gell-Mann and Zweig.

Feynman:

Yeah. Well, in recent times I haven't been working too well. That is to say, when I work, I work all right, but I don't work enough at a time; just little bits and pieces of half-worked-out stuff. I worked on the stars and the collapse of stars, on the quantum theory of gravity, and so on, helping other people, usually by putting them on the right track.

Weiner:

When someone mentions a problem, you get interested in it, and —

Feynman:

Yes, and then they would be cockeyed. They have something mixed up, and I point out to them it's mixed up. If you calculate you get this answer — then they calculate, they get that answer. They publish, and they thank me for pointing out that they'd get that answer. But I had to calculate it to find that. Another example is in the high energy physics. There were a lot of discussions of the different groups and the different symmetries. Then I discussed it with Murray and with other people and, sometimes if I understood a thing well enough, I go home and try to figure it out. For example, all of a sudden Murray and Dave get an idea. It's ok; they understand the new thing, some new theory. But I go home and I figure out that it's cockeyed, that it doesn't work, that it violates some principle of conservation of probability that they didn't notice. So I point it out to them, and they thank me very much, and they don't publish it. Or we discuss some symmetry. Now, in this particular thing, Murray was developing an idea, and I had a little, but not much, to do with it. But I did discuss it with him, and in fact had suggested some aspects of it. But I really did not completely understand what he was doing. But while he was discussing it, I would suggest, "Look at this. It would be like this; if this is like that, then that's like this," and make some suggestions, and we worked together, somewhat. And Zweig was doing independently something related to it. I must say, I didn't really understand that paper very well. It was written in a very great hurry by Murray, and he asked me if I wanted to get my name on it — if it was right, you know, to put the name on it. I had discussed it with him, and much of the discussion influenced

him, and so on. So my first reaction was: Well, no, I'm perfectly willing for you to use my influence. It's all right; I don't need my name on it. On the other hand, I was getting depressed by having not done anything for so many years. So I made what I would consider now an error. I don't mean that I think the paper's bad or good. That's not the question. I still don't even know. I don't even know whether what they now know is in there. I didn't understand it very well. I didn't check everything that was written. I had a principle that everything that I wrote, I should understand inside out; that there was just a little bit less written than what I knew; and that whatever I wrote would be right. I didn't like the papers that somebody would write; suggesting an idea which in three months they find is cockeyed. And there was just a possibility that that was such a paper, because I didn't check everything — in and out, back and forth — like I did with the beta decay. But he came to me when I was eating lunch and asked me if I wanted my name on it. They were getting it out right away, because it was a big hurry.

Weiner:

It was sent to Physical Review Letters.

Feynman:

Right — it's a big hurry. They're writing it this morning, and do I want my name on it? And as I ate lunch I decided: Yeah, because I haven't done anything, and it would be nice... That was a terrible thing. That was stupid. Then I looked at the paper, and I asked a number of questions to see if it was likely to be right. But I should have done the work myself. And I decided, OK, to put my name on it. I don't know whether it's right or not, but I must say that I put my name on that, and I did so little in it that it isn't really my work.

Weiner:

Let me ask you some —

Feynman:

It isn't really my work, and I've felt uncomfortable about that, since it was through weakness, a human weakness, that I got my name on that. I did a little, a small fraction of the work, and it wasn't deserving to have my name on it. It was dumb.

Weiner:

During the period immediately preceding that, you were involved in the special series of lectures which were then published and widely circulated.

Feynman:

The Feynman Lectures on Physics. Do you want to talk about that?

Weiner:

I think it's appropriate, because this was a very major activity in this period.

Feynman:

Yeah. It's interesting, now that I think about it, that since that was a major activity in that period, I'm complaining I'm not doing any research. I'm really crazy. People have pointed out to me now that it's really quite silly of me to feel that I'm not doing anything in these years, because that thing is something. But I still don't feel it that way, because when you're young you dedicate yourself to some ideal, that you're going to discover things in physics, and if you do something else it's hard for you to rationalize that it should satisfy anybody. It's just that I was teaching a class. So anyhow, the story of those lectures is the following. There was a discussion by some group, of which I was not a member, that they ought to revamp the physics course, because many of the students who were pretty good, who were taking physics, were complaining that after studying it for a year or two, all they were doing was pith balls and inclined planes. They had heard so much when they were in high school of relativity and strange particles and wonders of the world, and they would see nothing of the wonders of the world until they were graduate students. And this was very difficult, and they were trying to revamp the physics course. So they had worked out some kind of a syllabus for it and so on, and the question was, who was to give it? I don't know how they discussed among themselves, but anyway, Sands came over here and he talked me into giving the course. However, I threw away the syllabus. You know, I decided to give it my own way, of course. But I got the general idea of what was involved. They wanted me to teach the freshman lectures. They wanted to revamp the course. It used to not have any main lectures by a main lecturer, but they used to have sections of teaching by graduate students in different sections. The only thing they ever came together for then was an optional sort of cultural lecture that was not directly related to the course, once a week on Fridays, or maybe once every two weeks on Fridays.

Weiner:

Some historical thing, perhaps?

Feynman:

Well, it would be different things. I would often be invited to talk there, and I would talk about relativity. It was not part of their course. Sometimes people would talk about something that was directly part of their course, but it wasn't organized together. Now

they're going to do a new laboratory. They were going to cook up a new lab, and they were inventing new experiments to go with the lab. They were going to re-design it, so that there would be at least two lectures a week given by a main professor, and then some recitation sections that graduate students would pay attention to. And would I give the lectures? See. They had money from the Ford Foundation for this revamping. There's a lot of money for changing the world around these days. So I said, "Ok." I accepted the challenge for one year, and I tried to make a course that required giving two lectures a week.

Weiner:

Didn't you have to drop all other work, all other teaching?

Feynman:

I did, in fact. I can hardly believe it, but my wife tells me that I was working essentially day and night, 16 hours a day, all the time. I was down here all the time, worrying about these, working on these lectures, because I not only had to prepare the material, I also had to prepare the lecture so it was a good lecture, if you know what I mean. I had the idea — I got a kind of principle, a number of principles. The first was that I wouldn't teach them anything that I had to teach over again because it was wrong unless I pointed out that it was wrong. In other words, if Newton's laws are only approximate, and they're not good in quantum mechanics and they're not good in relativity, I start out by saying that so that they know where they are. In other words, there always should be some kind of a map. In fact, I even thought of making some sort of a great map of things with their interconnections, so we see where we were. I thought that one of the troubles with all the courses in physics was that they just said: You learn all this, you learn all that, and when you come out the other end you'll understand the connections. But there's no map, "guide to the perplexes," you see. So I want to make a map. But it turns out it's not a feasible design. I mean, I just never made such a map. The other thing is, I wanted to have in it things that would be enough for a good man to chew on, and then also that the average guy should understand. So I tried to invent. Let me go over the principles. The first was, I'd never introduce anything that was not exactly right without explaining that it wasn't, and what changed next time. The second thing was — I looked at books, you see, and I began to realize great weaknesses. For example, like they were teaching in the same book $F = MA$, and a little bit later that the frictional forces, the constant of friction times the normal force... as if they were of the same caliber and the same significance. They're so different in quality and — you know — nothing is made of it. So that was what the first principle was. The second principle was: That which is supposed to be understandable, and that which is not supposed to be understandable from what you've already said, should be made clear. Because I would find in books that they would give all of a sudden, say, the formula for the frequency of an AC circuit. That was supposed to be more advanced. They can't derive it now, but they wouldn't say, "You aren't going to be able to understand this formula at this level

with the reasoning that has just proceeded, but it's an added thing." In other words, what's being added in, and what should have come from the other thing? Even if it could have come from the other, but you don't make the argument, you should say it. I always say, "This is a possible deduction, more or less as follows, but we haven't tried to deduce it from that." Or, "This is an independent idea that comes from another place, you see, and you can't deduce it, so don't worry." A few little principles like that. Then the problem was to make lectures which would be OK for the average student, and yet have stuff for the advanced student. Then I got an idea, when I was planning these lectures. I would have a cube in the front of the lecture hall which had different colored faces, so that when something was only for the fun of it, for the more advanced student to get him interested, but wasn't really an essential part of the course, it would be one color face. You see? When there was something that was so basic that it was absolutely necessary to understand for the whole of physics, and everybody should try their best to understand this thing — another color face, and so on. A color face to indicate the importance, the position, of the different subjects. Because what I was worried about was that all the students would try to learn all this junk, and if they do that, then I haven't got stuff for the advanced student. You can't do it. It's just impossible, to have stuff for the advanced student without possibly confusing the stupidest student or the less advanced student. So I had this cube idea. But I gave that up as being gimmicky and instead I would write at all lectures summaries (which are no longer extant) on the blackboard of the central items which needed to be understood. Anything else that wasn't in the summary was just for the fun of it. But those don't exist anymore. Finally, let's see — I thought of some other things while I was talking. I don't know. So, then I started to give the lectures. And at the very beginning, the first thing I wanted to do was get all the students together. At a number of lectures, people don't understand the logic at the beginning. The real logic of the beginning is, get all these kids from high school to come approximately to the same rough position. For instance, I would talk about everything being made out of atoms — not because I think they don't know that, but because I want those who don't know it to know it. I can't say that, you see, so I tell it in such a way that the ones who already know it are excited by it, because it's a new way of looking at it, while the ones who don't know it can just catch onto it, to the level that I need. And so on. So the first few lectures are to bring everybody together. Also, these lectures were lectures I had given other places, the beginning lectures especially, so that I could have time to prepare the later ones, you see. And finally — oh, another principle, a very important principle; I wanted each lecture to be able to stand by itself. I didn't think it was a good idea to have a lecture and say, "Well, the hour is up; we will continue this discussion next time," or "Last time when we left off, we were doing" this, that and the other thing. "Now let us continue." So, instead of that, I wanted to make believe to myself that each lecture was somehow or other an isolated masterpiece, you see, of lecturing, in which you had a beginning, and introduction, and you had a conclusion with some drama. So each of the lectures were like that, with some minor exceptions. There were one or two places where I couldn't do it, where I continued the two lectures together or something like that — but that was another principle. I'm just telling you the guides that made those things. Finally, my main interest is in physics, and in organizing

material. I love to organize the material, and to think about how it goes together, and to discover a new way of looking at something, and how I can explain it and so on. And I'm not the kind of a teacher who's interested really in the student as an individual. I mean, I'm not worried about: this guy's married and he's trying to get his degree, and all these complications. I tried my best to teach the student more or less as the abstract student, with imaginary properties — mixed, mixed, there were many different kinds of abstract students — but no any particular individuals. The subject is the center of my interest in all cases — the subject, not the student but the subject. So, you want to know how I feel about them [the lectures]. What else can I say about them? They're all published. But I'm trying to explain to you how I feel, myself, about them, and what I thought I was trying to do.

Weiner:

Did you get any sense of feedback while you were doing it?

Feynman:

No. None whatever because I had no way to know what was happening. Because I didn't have any recitation sections and I didn't have questions at the end of the lecture. Any questions were supposed to go into the recitation sections. So there was zero feedback, except that there were some exams in which people made up problems. They gave them problems, and they would try to write the answers, in certain exam weeks, you see. And they were so atrocious — as far as I was concerned — they were so zero that I really, in a certain sense, felt discouraged throughout the entire program. Not discouraged to the point of not keeping right on going the way I was going, but in the feeling throughout that it wasn't working, that it's useless — but never mind, I'll do it anyway. I mean, it's the only way I know how to do it, damn it. But it doesn't work.

Weiner:

How about the people who were directly in contact, with the recitations?

Feynman:

People who were directly in contact would tell me that I was underestimating them, and that it wasn't as bad as I thought. But I never believed them and still don't.

Weiner:

Don't you think that this type of presentation, the effectiveness of it, is difficult to measure in a traditional examination?

Feynman:

Of course it is. But let's just assume that you're getting somewhere. But what else do you do? I mean, you asked me what my reaction was. It may be difficult, but I expected them to do better on the simple questions than they were doing. In other words, a person who couldn't do what they apparently couldn't do was certainly not understanding what I was talking about. That's the way I felt about it.

Weiner:

How long did you do this, three years?

Feynman:

I did this for a year, and then they started to work on me for the second year. And I said, "I prefer to do the first year over again. This time I want to make up problems that go with the material, and to make some improvements, but mainly to make up problems to go with the material, so that it would really teach it." And to make some improvements of things I didn't care for. Then they worked on me, and I'm glad they did — in some way, anyway. They said, "Look, nobody's ever going to do this again. We need this second year." I didn't like to do the second year, because I didn't think I had great ideas about how to present the second year. I felt that I didn't have a good idea on how to do lectures on that. But, you see, in these challenges that had existed before about lectures, they had challenged me to explain relativity, challenged me to explain quantum mechanics, and challenged me to explain the relation of mathematics to physics, the conservation of energy. I answered every challenge. But there was one challenge which nobody asked, which I had set myself, because I didn't know how to do it. I've never succeeded yet. Now I think I know how to do it. I haven't done it, but I'll do it someday. And that is this: How would you explain Maxwell's equations? How would you explain the laws of electricity and magnetism to a layman, almost a layman, a very intelligent person, in an hour lecture? How do you do it? I've never solved it. OK, so give me two hours of lecture. But it should be done in an hour lecture, somehow — or two hours. Anyhow, I've now cooked up a much better way of presenting the electrodynamics, a much more original and much more powerful way than is in that book. But at that time I had no new way, and I complained that I had nothing extra to contribute for myself. But they said, "Do it anyway," and they talked me into it, so I did. When I planned it, I was expected to teach electrodynamics, and then to teach a subject which would really be all the different branches of physics, using the same equation — like you use a diffusion equation for diffusion, for temperature, for lots of things, or the wave equation for sound, for light, and so on. In other words, the second half would have been something like mathematical methods of physics, but with many physics examples, so I'm teaching physics at the same time as the mathematics. I would teach Fourier transform, differential equations, and so on. It wouldn't look like that, though. It wouldn't be organized the usual way. It would be in terms of subjects, the point being that the

equations are the same in so many different fields. So the moment you deal with an equation, you ought to show all the fields that it comes from, instead of just talking about the equation. So I was going to do that. But then I had another possibility. Maybe I could teach quantum mechanics to the sophomores — nobody expects that to be done, that would be a miracle. And I had a crazy upside down way of presenting quantum mechanics, absolutely inside out, in which everything that was advanced would come first, and everything that was elementary would come, in the conventional sense, last. And I told these guys about that, and they kept working on me. They said I had to do it, that the mathematical thing that I was talking about, other people may someday do, but that this thing would be so unique, and they knew that I would never go for another year. I must do this unique thing, you see — even if it kills the kids, they can't learn it, and it's no good. I don't know what the situation is, actually, whether it's worthwhile or not. I should try it. So I did. And that's volume 3 on quantum mechanics. But volumes 2 and 3 were really one year, just like volume 1 was.

Weiner:

This represents two full years that you put in.

Feynman:

That's right.

Weiner:

What years were they? Volumes 1 and 2 were published in '64. I don't have volume 1 in front of me. Well, it probably tells you in the preface.

Feynman:

Right. That's why I'm looking in the preface.

Weiner:

It was published in '65.

Feynman:

'64. '65. This is volume what, 3? Yes that's the wrong volume.

Weiner:

Volume 2 says, "These are the lectures in physics that I gave last year and the year

before.” I’d say this is volume 2.

Feynman:

Well, that’s no good. It’s volume 1 that we need the copyright on.

Weiner:

Well, it says freshman and sophomore.

Feynman:

I know what it says, but the preface was written...

Weiner:

Oh, here it is, June, ‘63.

Feynman:

Ok — that’s when the preface was written.

Weiner:

Last year and the year before.

Feynman:

Ok. Good.

Weiner:

So you started in ’61 — no, last year meant ‘62 and ‘63.

Feynman:

I started in ‘61.

Weiner:

No, last year meant last academic year. You were writing in ‘63.

Feynman:

Right. Well, one is '61-'62, and the next is '62-'63.

Weiner:

I see. That fits very nicely into this —

Feynman:

Hole.

Weiner:

Way of accounting for time.

Feynman:

Except for '63.

Weiner:

Sounds like I'm your bookkeeper, you know.

Feynman:

Right. OK. So that's what I was doing.

Weiner:

And since then, of course, as you mentioned yesterday, you have better feelings about it...

Feynman:

Somewhat.

Weiner:

Because of their use beyond Caltech.

Feynman:

Well, I haven't yet, but people have pointed out I ought to. And I may be gradually coming around to understanding that. But what I insisted that I was doing, from the beginning, was teaching this particular group of students, and that's all that I could do. I kept saying, "You cannot live beyond the grave. You teach these students, that's all it's going to be, and there won't be any way to get this to anybody else." I think it's roughly true. If I listen to the lectures that other people give, on the basis of these books, I see all kinds of flaws, errors, weaknesses, and distortion. And it is true that you can't live beyond the grave. But there must be people living who aren't listening to the lectures of some professor, who are sitting just reading the book and thinking for themselves. They must get something out of it. So if I keep some hope that that's worth something to them, maybe I can feel better about the whole thing. I think that, in regard to the particular students that I was really aiming at, which was my avowed purpose that I'd set — I wasn't caring about the books or anything, I was only caring about the students — I think that the result was nowhere near worth the effort.

Weiner:

Let's take a break for a minute now...

Weiner:

We're resuming after a dinner break. The marathon continues.

Feynman:

Well, you said you wanted to hear about my experiences in Brazil, about teaching, during the ten months I was there that time and since.

Weiner:

That's right, and your total impression of science education in South America.

Feynman:

I learned when I was there, during the ten months, very slowly, something which I found almost incredible. I found out that their teaching is entirely by rote, entirely by rote, that they don't know anything about what the physics means. When I first came there, I saw children of 11 years old and so on getting physics books, much more than in our country, and it seemed that everybody was studying physics. I was teaching a class in electricity and magnetism, sort of an intermediate class in a university, and I had a lot of trouble with the class that I couldn't understand. They made excuses that they weren't used to my methods, and this and that, and they didn't do the homework problems for one reason or another. Sometimes I'd ask them a question and they'd give me the answer immediately, very neatly, you know. Sometimes I'd ask what I thought was the same

question, and nobody knew the answer. Gradually I figured out what it was, particularly by one experience. Having talked about polarized light and polaroid and so on, and having gotten them to realize that when the polaroid's were set so as to make them opaque, their axes were at right angles — in other words parallel — I asked them if they could tell me, by any method, in which direction the electric field was passing through a particular piece of polaroid; in other words, absolute axes. And of course they couldn't think of it right away. Then I said, "Well, look. You've got the light reflected from the sea out there, from the water in the bay." That didn't do any good, and so on. Finally I said, "Have you ever heard of Brewster's angle?" And they said, "Yes, sir — light is reflected from — substance of —." I can't do it like they did it, but they quickly said the law and something about the tangent of the angle of index refraction, or something. Then I said to them, "Which way is the Polaroid; parallel to the plane of incidence?" "Perpendicular to plane of incidence, Sir," was the answer. I said, "All right, then. So the light that's coming from the sea is polarized," you know. "Light reflected from a material with an index end is 100 percent polarized, perpendicular to the plane of incidence when the index, the angle of incidence is equal to the angle of tangent of the index," or some such thing. They knew that. So I said, "Well, then, look at the water again, and look at it through the Polaroid." And they turned the Polaroid, and they said, "Gee. It gets dark." So I realized that although they had told me what Brewster's angle was, they didn't know that when they looked through polaroid at water reflecting from the surface, it would look dark. And so I gradually realized that, although they told me what Brewster's angle was, they hadn't the slightest idea what the words meant. And incredible though it may seem, I found out by further looking into this that they knew all these laws by memory, and understood nothing. They didn't even know that after they figured out the direction of a ray of light, and they put their eye where that ray was, that that's the direction they have to look. And so on. In other words, nothing was related to any observation whatever. Whatever! It's hard to believe the zero that was involved. Also, to investigate further, I looked into a lot of things. I listened to other professors who were supposedly good, like in the engineering school. I heard how they gave a lecture. And the professor gave a lecture something like this: (only it was in Portuguese) "Two bodies are considered equivalent —" and so on, with pauses in between each phrase, and the students were writing it down, exactly. When he got finished saying the sentence, slowly, with pauses, he said it all together: "Two bodies are considered equivalent, that equal torques will produce equal acceleration." He was talking about equal moments of inertia, but it wasn't at all clear, just the sentence. He said it quickly, and they checked that they'd written it down. They were taking dictation, and writing it exactly. But it was unclear. It was perfectly obvious to me that if this was an introduction to the moment of inertia, it was incomprehensible to the human mind. What kind of equivalence was not defined, or why. Then there was a formula for the moment of inertia, for no good reason. There was none of the usual talk: "Well, let's see — you have to swing the object around. You do the same thing further out, and it's harder to get it going than if it's nearer in —" Or any such discussion, in terms of any experience. I asked a student, afterwards, "What are you doing?" "I'm taking notes on the lecture." "Then what do you do with them?" "I study them," he said. "What do you study for?" "The exam." "Well, what's the exam

like?" "Well, this one's easy. You can always kind of guess what they're going to ask. For example, they're going to ask, 'When are two masses considered equivalent?'" I said, "What is this?" "I don't know." He looks, and he reads that sentence out. Now, see, it was possible, I realized, to pass the examination, and to learn and everything, and to go through the courses, without ever knowing a word of what you were talking about. I also went to the examinations for students qualifying to get into the engineering school. They were difficult examinations. And I took the best student, and after they had asked some questions which he answered satisfactorily, I asked the questions in a different language. As an example, they asked him how light is altered in going at an angle through a thick, plane sheet of material with an index? He said it was displaced from its original direction; it came out parallel to the way it went in, which was quite correct. When asked how far it was displaced, he was able to set it up and do it, which was far beyond the usual. But when I asked him later, I said, "Suppose this book is a piece of glass, and I'm looking at this other object through it, another book," and I tilt the glass, "what will I see?" He said, "You'll see the image of the other book go up at an angle twice the angle that you tilt the glass." "You don't mean the image of the other book is just displaced to one side?" "Oh no, it turns, and the further you're turning it, the more it turns" — which is, of course, completely beyond the ordinary experience. I said, "Don't you have mirrors mixed up?" "No." In other words, he didn't know that he'd already answered that question — that the light ray coming into the eye, is the direction that you'll see something in the illusion. So I began to realize and I found out by all this experience, that it was a most miraculous phenomenon, how these students could memorize enough stuff to pass all these examinations, and know so little — nothing, in fact, whatever — about nature when they're finished. It's impossible to believe, but it was 100 percent.

Weiner:

These were undergraduates and weren't necessarily preparing for careers in science?

Feynman:

Yes.

Weiner:

They were science majors?

Feynman:

The first one I told you, the student that I just mentioned, was trying to pass his incoming examination to go into the engineering school. The others were engineers, studying this course with the dictation. And I was teaching what was called the science faculty, but they were usually to be teachers in science somewhere. They needed to see, since they were to be teachers in science, what the normal course of events was. They

would teach what they learned. It's most incredible, because it was so complete. It's hard to believe that it was so complete. Then, after making all these discoveries, and confirming the various things, there were some physicists who were doing something in Brazil in that other center, the Physics Research Center. But they had gone to other countries to get educated, in one way or another. I was teaching at the University of Brazil in Rio. So I had been invited by a student group to give a lecture on my experiences in teaching in Brazil, at the end just before I was to leave. So, I gave the lecture and these students had gotten to come to the thing all the professors and the heads of the departments — a lot of important people. And I went to give my lecture. I told them before I gave it that I hoped they didn't mind; I had every right to say whatever I wanted. They said, "Of course." So when I got there, they saw that I was carrying a textbook that they were using which they were very proud of. It was their best physics text, and the man who invited me, the young student who invited me, said, "I hope you aren't going to say anything about that, because the author is in the audience." I said, "That's all right. You told me I could say whatever I want." I had found that it was impossible (I had found by experiment) for me to open that book at random with my eyes closed and put my finger down on a sentence that I couldn't criticize, that there wasn't something the matter with, something wrong with something. So I gave a lecture which was really quite something. I started out explaining what science is. Of course, it's a description of nature. I said that I had to speak Portuguese, and I usually could get one idea every 15 minutes, but because I had to speak Portuguese, I can't get so many. I wanted to tell them ahead of time what I was going to say. I said, "Of course, science is a description of nature," and so on, a method of describing nature, and that what I wanted to say was, first, that science was worth teaching; and second, that no science was being taught in Brazil. Two ideas. So I explained what the reasons to teach science are. I said, "One of the reasons is, no country can be considered civilized unless it teaches science." And they were all nodding — and I knew that. Then I turned around and said, "That's not a reason at all, what the other nation considers you. But that's the reason you're using." Then I went to give the real reason for teaching science, that the reason that other nations think no nation can be civilized. And I went through all this, to kind of tease them, because I understood them. I would make them feel comfortable by using their own view, and then show how absurd it was, you see. Then when I came to showing that no science was being taught, I went through this thing which I just referred to, and so on. Then, in a demonstration with the book, I said, "Now, I'll show you. I'll open this book at random, and put by finger down, and there's something the matter with it." I mentioned some of the things I'd found. I opened it, and put my finger in, and it happened to say — just to give them an example, which will help to explain it here — it said, "Triboluminescence is the light which is emitted..." Well, it gave a definition of Triboluminescence, light emitted when crystals are subjected to strong pressures, or something. I said, "That's nothing. It doesn't say anything about nature. It's a word. If I was talking about it, I would say it like this... Can you imagine reading that, and you're going to go home and do an experiment? What are you going to do? What is it?" I said, "I would write it the other way, you know, like this — that certain crystals give out light when they're broken or pressed. For example, if you take a lump of sugar in a dark closet

and break it, you can see a light blue flash. The origin of this is not clearly understood, but the phenomenon generally is called Triboluminescence. The least important part of the phenomenon is the name of it, and you have to give an example of it — where in nature? Something about the world —” And so on. It was a very nice lecture, altogether. After it, the head of the physical department at the University of Rio, on whose head everything fell, said he felt like a man. … He said first, “Mr. Feynman has said some very strong things, “— he made a sort of keynote remark —”But I think that you should all listen to what he has to say. We should all listen to what he has to say, because I believe that he’s sincere and that he loves science, and that he has seen something that horrifies him.” Then he went on and he said, “I feel very much like a man who has a rather uncomfortable feeling in his stomach or something, and goes to the doctor, and the doctor tells him that it’s cancer.” That started a discussion, and they had a tremendous discussion, about what to do about all this and so on. The students, of course, those who were beginning to see — had seen the light — or had come from other countries, were making suggestions.

Weiner:

The discussion followed?

Feynman:

For two hours or so, yes — two and a half hours. Well, there were some other things that happened at this thing. During the lecture, I said that I believed that it was impossible to learn anything, but in spite of that I had some evidence that I’m not 100 percent right. First, there was one student in my class who was able to do things, in spite of his education, and he got an A grade. You know, he was a student that understood something, that did show some light. And further, the other thing was that there was a professor in Brazil, a Brazilian who had never left the country, who when talking to him I felt that he had some real understanding of physics. And he was there. I mentioned his name. First the student got up and said, “Excuse me, but I think under the circumstances I have to explain that I’m from Germany. I was educated in Germany. This is my first year here at the University of Brazil.” Then the professor got up and said, “When I went to school in Sao Paulo, it was during the war, and fortunately there were no professors present, and so I taught myself everything from reading books.” The two examples that I had, that I thought made me not quite so sure, were both examples in which there was a specific careful reason why… I tried to claim that nobody could get through the system. I couldn’t understand, really, how anybody could get through the system without dying intellectually, completely. It was impossible, for a reasonable mind, to have to do all that work — it would be jammed, then, you know; it couldn’t work. So, there’s been a struggle ever since, and there was this center which tried to develop its research. In doing research in Brazil, we tried to develop a center that people could do work in, but there were all kinds of up and downs — government favor and disfavor, the drying up of funds, and then the oversupply of funds, and then the drying up of funds

— so that it was impossible to maintain a continuity. And in recent years, there's been again a drying up of funds, until the thing's practically died. There was a time, not more than two or three years ago, when the thing was going so well that it was the center of physics in all of South America, and students who wanted to learn physics in South America would come to this center to study. They'd come from all the countries of South America. And now it's practically dried up. It's the irregularities of support, government support that makes it impossible to maintain and to develop the center. Education seems to have been copied from the French system some years ago. It hasn't been modified since. Of course, the physics that was being taught was all the physics from before 1850, approximately, in every case.

Weiner:

This is even classical physics without electrodynamics, then.

Feynman:

Practically. They did teach electricity and magnetism. They did have Maxwell's equation. Perhaps I go back too far, but in the beginning of the physics, there's so much — by 1850, I mean the style of educating, and the forms and everything. It looked like the textbooks had been copied from textbooks, which had been copied from textbooks which had been copied from 1850. They couldn't go that far back; they couldn't. But it's the same idea. It was very, very unsatisfactory. The reason they called that textbook "good," I found out, was because the important phrases and laws and so on were in heavy black type; the less important ones were in lighter type. The thing was organized in the type and in the positions of things, so you'd know what it was that you had to know.

Weiner:

What about Brazilian students who come to the United States to study, and then return? Was there very much of this? This is often a positive effect.

Feynman:

Yeah, they would do all right. Many of them would do fairly well.

Weiner:

But would they go into teaching?

Feynman:

Yes. They tried to, but they wouldn't be able to get into the system, because the other

professors would be jealous of their positions. For example, when Lattes returned to Brazil, he couldn't get into the University of Rio to teach even though he was a hero because he had just discovered the artificial meson. All the Brazilians were excited about the great Cisco Lattes. And they had to start a new center, the Brazilian Center for Physical Research, outside of the university. He ended up, of course, by teaching, and guys would get time off from the university to come and learn in this center. That's where I did my better teaching. That's where I taught my Argentinean friends and some of them that I've mentioned before.

Weiner:

You were talking about the situation in Brazil, and the ups and downs of financial support.

Feynman:

I was talking about the ups and downs of financial support of the Brazilian Center for Physical Research, which isn't the same as the universities and the teaching. That was what went up and down, and which had so much difficulty. So it's sad, to me, because after all these years, from 1951 to now, the center is not any bigger. Oh, the buildings are in some ways bigger, and so on, but the size, the number of people, is smaller, and the vigor is less — at the moment — whereas a few years ago it was more, and so on. It's up and down. It's a big mess. So it's gotten essentially nowhere, in that many years.

Weiner:

You wrote an article recently about this —

Feynman:

Yes, in 1963, I was invited to give a talk. There was a Pan-American Conference on Education in Science, or something like that, or even Physics. I think it was specifically physics although I'm not sure. Anyhow, they asked me to be the keynote speaker and so, for the keynote speech, I criticized in a number of ways — or not criticized but pointed out — the problem that Latin America has, because of its governmental structure and for other reasons, and the dangers of teaching just by rote, and the fact that it was taught just by rote, and so on. Rather dangerously extrapolating my material on Brazil to all the other countries, of which I wasn't absolutely sure. I was very suspicious; I had various clues that I was right. In that respect, I was very pleased, because right after the speech, a man from Chile would come and he would tell me that he didn't realize that I had lived in Chile and knew so much about the educational system. And a guy from Venezuela would admit, would say, you know, that that was a perfect description, and so on. There was one exception — the man from British Honduras claimed that I was not representing the situation correctly. But from the way the man from British Honduras

behaved, I suspect that I really was striking correctly. But he was high in the educational system and was protecting it. He's the only one who objected. But I think I had hit it there, too.

Weiner:

Do you have any foreign students in your classes here?

Feynman:

Yes. When they get here, somehow or other they seem to be able to recover, some of them, somehow.

Weiner:

I don't mean only Brazilians; I mean, in general.

Feynman:

Oh, yes. Quite a number. They have their struggles, but they seem to do all right. It's like a dried out plant: give them a little water, and somehow or other — Of course, this trouble we have to a large extent in the United States. We have a symptom of the same disease. There's an awful lot of rote learning, and a lot of mistaking knowledge for the right technical words and so on, you know. A guy who says the right words is thought to know something. I didn't bother but I could have taught my child, after he learned to talk, to say — and I thought I would, just for the fun of it, to demonstrate this, but I didn't bother the poor boy — but it's not at all impossible to teach a child to say that pi is the ratio of the circumference to the diameter of a circle. It's just as easy to teach him that as to teach him a nursery rhyme. And then to say that pi is numerically equal to 3.14159. That way you can get fooled. You haven't the slightest idea what you're talking about, and you sound just fine. So I think that we have a lot of that in this country, too. But at any rate, the point was about Brazil, which had it so completely that it's really a pity.

Weiner:

But you've gone back there, you've been invited, so you haven't worn out your welcome.

Feynman:

Well, I haven't worn out my welcome in Brazil, and there have been some movements in the right direction, springing from this center — some revamping of textbooks, and some other minor attempts. But I believe that at the present time, within the last six

months or so, things have gone rapidly backwards. I'm not sure of that. One never knows which way it's going. But anyway, it is very disheartening, after all this time, to see so little change. There are a number of other technical things which you'll find in my article, 1963, which I didn't bother to go over, which have to do with why I think it happens. The industry doesn't support the education, and the industry doesn't need the highly trained, scientifically trained engineer because they borrow the engineering inventions from the other countries, and so on. So that's another matter. I don't want to go into that phase of it.

Weiner:

Well, let me lead you away from this subject to the Solvay Congress. I'm not sure what year it was.

Feynman:

Of course, I always loved Brazil, and I liked the music. I always liked to go down there. It's just a lot of fun, and I know people enjoy the beach and so on. But this particular aspect is rather — not so good. Solvay Conference?

Weiner:

Yes. What year was that?

Feynman:

1961, the 50th anniversary of the 1911 meeting in which Einstein, Planck and so on gave all their wonderful results, and they were discussing the quantum and all this kind of stuff. It was on the same subject — the quantum theory of light. All I did was give a summary of our present position; what we knew about electrodynamics as I could see it at the time.

Weiner:

Who else was there from this country?

Feynman:

I don't remember.

Weiner:

A large group?

Feynman:

Fairly large — well, yes, Gell-Mann was there. I think Dyson was there, Schwinger was there, Bethe was there, and Weisskopf was there, I think, if I remember right. People who did things in electrodynamics; all the people who did anything in electrodynamics.

Weiner:

That was a conference on quantum electrodynamics?

Feynman:

Essentially, that's right — the interaction of radiation with matter which was the same name as the other one, see. We met the King and Queen of Belgium, and so on. But I don't have much to report there, about that. Wigner was there and some of the other different guys. Of course, all the guys like Dirac and so on were there. I gave a report on the present situation. Other people discussed other aspects of physics for the day, and so on. There's not much to say. The only thing I do say is that when I was invited, I felt rather honored because I always — well, it had a reputation, Solvay. I remember when Bethe was invited, some time back, to the Solvay Conference. He took very seriously writing his report for the Solvay Conference. So, like son, like father, when I was invited to the Solvay Conference, and I had to write a report, I took very seriously the writing of the report, much like Bethe would have done. I imitated him, in a way, in my feeling of it, and wrote my report and went there and delivered it. So I felt that I ought to do it. But I don't know whether much came out of it.

Weiner:

Let me ask some questions about recent developments in quantum electrodynamics — current things, your feeling about it — after all these developments. For example, do you think that the basic ideas of it would be violated at high energies? Do you see such a tendency?

Feynman:

Yes. No. You asked two different questions. You said, "Do I think it will be violated?" And I said yes. "Do I see tendencies of this?" I say no.

Weiner:

At high energy.

Feynman:

Yes. I don't see tendencies, though I think it would be violated.

Weiner:

What does this mean, in terms of your total view?

Feynman:

Guessing; just guessing.

Weiner:

Yeah. You think that it's all right now.

Feynman:

No. You said do I think it will be violated at high energies? Yes.

Weiner:

But you don't see the tendencies — I see; all right, that clarifies it.

Feynman:

I don't see the tendencies. I can't say a thing is right if I think it will be violated.

Weiner:

All right, that's just a basic question. Do you see schools now in physics, in the sense of schools of thought; sort of a field theory school and a scattering matrix school, and that sort of thing?

Feynman:

I don't think that way. I mean, I don't think about it. I don't know who's doing what. I know there are people who think about field theory. There are people who do certain kinds of problems. There are people who think other ways. But I don't pay attention to how many members are there in a school; is there just one guy or is it a school?

Weiner:

How about the ideas, though?

Feynman:

Well, there's lots of different ideas. There always have been.

Weiner:

You don't categorize them into major categories. You don't see them as sitting in two camps, or anything?

Feynman:

No, just guys with different ideas. Actually, there probably are little schools because I do think, although perhaps incorrectly, that today there's a tendency for the younger men to follow a leader. So, for example, if Chus is believing that everything is going to come out of the s matrix, then he gathers a coterie, perhaps — I haven't paid enough attention to this — around him, who also believe it's going to come and they work together. That's almost theoretical. I mean, I don't know that. The only school that I know of is a school, so-called, of Professor Wheeler who is doing (I don't believe they're sensible things) gravitation and geometry and so on; i.e., quantum theory of gravitation and so on. And he has a school of students around him who seem to believe his, what I think are, sort of wild ideas, and they seem to be kind of absorbed in them. There have been schools of that kind, with special wildnesses. Like when what's his name, Eddington, got his crazy theories, near the end of his life, about physics, there were a number of students who went along and published papers on it, among which was my friend H. C. Corbin. H. C. Corbin, however, woke up and realized that it was cockeyed. So, maybe. But there is one tendency that I do see, I think, in that when a new idea comes up — like Reggie Pole's is a good one — there seems to be a whole comet tail of guys. See, somebody notices a good idea and starts working on it, and there's a whole comet tail of guys who rush in, thinking that this is the coming thing. In other words, they expect to be at the forefront of the thing by seeing what somebody is doing that looks new. He says that it looks good, and they hope that by working there they'll be at the edge. So there are these large numbers of people who rush, it seems to me, from one theoretical view to another theoretical view. They're sort of mediocre characters, and follow the leader, sort of, from one point of view to another point of view, as the ideas of the times vary. There's nothing wrong that the ideas of the time vary, but it seems to me odd that there's that much work on each of the ideas, one at a time. It does seem to me that there's always a favorite idea, an "in fashion" idea. There's a fashion, more than schools.

Weiner:

I think Dyson had an article, and he called it: "The Changing Fashions in Field Theory."

Feynman:

Yes. Maybe he did. But I get the same idea, that there are kind of fashions that people rush off from one to another of these fashions. They do an unproportional amount of work that's repetitious and not very good. I don't mean the leaders at the front, like Gell-Mann or somebody, who decide that Reggie Poles are good. They always do worthwhile things. But there's always this crew of characters who talk about it and verify things and check things, back in the back, that don't mean a damn thing. And then the whole thing isn't worth anything. It turns out not to be right. Maybe. In other words, many of these guys are unnecessary. They've got nothing else to do, and they just rush around. It's just a backwash that I don't see makes any difference.

Weiner:

And you think there's more of that today than there has been in the past?

Feynman:

Very much so. I think we have too many guys in theoretical physics who've got nothing else to do — I mean, in fundamental physics. And they just rush around, all over the place, not by independent thought. They're not critical, they're not careful, the papers are sloppy, and the work isn't very good. And the real work in the field is always by a limited number of guys. Now, of course, in this scurry somewhere there may be some guy who'll turn out to be OK later, and so on; I can't say. That happens. And it's important that these fellows exist. But it just seems to me there are too many guys running around following, in these field theoretical problems.

Weiner:

Yes, that's what we're talking about. In this article that I mentioned before, quoting Oppenheimer in Newsweek last week, it quoted him as being "very optimistic that out of this current chaos in physics fundamental insight will be obtained and that it might not be as far off as decades. It might be years." He sees some order. I just wondered —

Feynman:

Yes. I think so.

Weiner:

Why? He didn't have a chance to explain himself, so if you agree generally, in what way?

Feynman:

Well, as you noticed by looking at my papers, I mumbled that I didn't believe in the meson theory and so on, in a certain era. What I was convinced more of was that all the clues were not in. Now, in history of physics, in all cases, with very few exceptions — there are exceptions, Einstein's theory of relativity is an exception, general relativity — after the discovery of the law is made, you can say: "Gee, why didn't we think of this before?" That is, everything is lying around, and people try to put it together, and all of a sudden somebody does, and you look and you say: "Gee, that was kind of obvious!" So an interesting historical question is, on each discovery, to ask: When would it have been possible, in principle, for the human mind to have made the discovery? For example — relativity was beyond Maxwell, because there wasn't the experiment to indicate, for example, that the movement of the earth couldn't be detected — the Michelson-Morley experiment. There's no clue that you need this theory.

Weiner:

But there's no evidence that Einstein ever was influenced by this experiment.

Feynman:

He certainly was influenced — that's crazy. He may not have been influenced directly, but by the people who were worrying about the problem. Like Poincare, proposing the principle of the invariance of the laws under the transformation of Lorentz, and Lorentz worrying about the original thing, talking about the contraction and noticing what formulas lead to the Maxwell equation in varying — that's all part of history. So he was influenced by the experiment in the sense that the idea was passed along. Ok. So, I mean, he isn't sitting there watching the experiment, but the experiment produces the puzzle that is then the puzzle of the age, which is, how to put the electricity and Newton's laws together? Otherwise, no problem. You can see that it's not possible to cook this thing up. It's possible to guess it, but it's not possible to establish it, or to see that it's better than another guess, before a certain time. It's always possible to imagine that somebody just plain guesses the right law. But you can't say that they discovered the Schrodinger equation by writing it down. That's not what I'm talking about. There's no clue that it's reasonably more correct than something else. But there comes an era when it's possible to make such a discovery. For example, before the neutron was discovered, nobody is going to get anywhere in nuclear physics. Ok? They had proposed the neutron, and suggested it, and gradually understood that there's a neutron — but you have to wait for that before you can begin. Ok? So I felt at the time that the mesons had been discovered, that there's not enough clues. First, there were these strange particles, which are evidently involved somewhere, and you can't get away with it by not playing them into the thing, and so on. So I didn't pay much attention to meson theory, after I'd done my original calculations to indicate that it was no good to me. So I did other problems, like helium and gravity and lots of things, waiting. And then they picked up all these particles. Well, while they're picking them up, they can't see the pattern. But when

Gell-Mann came out with the SU-3, and when the system was checked by the discovery of the omega minus, I got convinced that — I mean, just a guess — that all the clues are now in; that the SU-3 pattern and so on is fundamentally in about the strong interaction. Now, maybe all the clues about the weak interaction are not in, but I thought all the clues about the strong interaction were in, and therefore the discovery should be made — now. It'll turn out that when we finally discover the law, I think we'll be able to say, "We should have been able to do it right after SU-3 was established."

Weiner:

The time is ripe for it now.

Feynman:

That's what I say. Any old time. So I kind of agree with Oppenheimer about that. With regard to the weak interaction, I am in quite as good a position because of this K-2 business, this K naught, and the CP violation. And just today you tell me about the publication of the asymmetry in the eta decay, which I haven't looked at in any detail, to verify one way or the other. But there you are probably telling us something about the weak interactions, maybe even more about the strong interactions, and maybe a clue that I didn't know I had. But anyway, things are pregnant, in my opinion — very pregnant.

Weiner:

I think what they're saying here is this is an intermediate interaction.

Feynman:

Well, I'll look into it. I'll have to look into it, and see what —

Weiner:

That's fine. I think that we've covered, in the last couple of days and in the session before, quite a lot of ground, and it's only after we have a chance to get some distance

—

Feynman:

We'll see something missing, probably.

Weiner:

We'll see that things are missing.

Feynman:

Yeah, you might want to hear about my reaction to the Nobel Prize. I think it's too close — I mean, I want to say this on the tape, I've said it to you before, that it's too close to the time, and I haven't really settled in my mind to get a rational reaction. I have more of an emotional reaction to it, the prize, than a rational reaction. And the emotional reaction is partially distasteful. So it isn't like the other things, where there's enough time in between, and I can just look at it as if it was somebody else. I'm still mixed up in it. I always thought — I mean, I thought that it was always a possibility that I might get a Nobel Prize, because I thought somebody might think the work in helium, or maybe the beta decay, or even the electrodynamics, might be something for the Nobel Prize but then, on what kind of considerations? Usually it's said that they don't pay much attention to theory, but if you look at the list of prize winners, it isn't true. There are a lot of theoretical discoveries of importance there, if I remember right. Anyway, when I looked at it, I was surprised afterwards that there were more than I thought. Maybe, it's just the sour grapes attitude of theorists. Each year when the Nobel Prize talking comes around, of course you half-think, maybe it's possible. But I was never surer, one way or the other, whether they would pick it out or not. And this particular year, I had forgotten that it was that season. And I was awakened at 4 o'clock in the morning by a telephone call from the American Broadcasting Company, who told me that I had won the Nobel Prize. I was awakened at 4 o'clock, and one is not too wise at 4 o'clock in the morning, having just been awakened by a telephone call, and I was angry at the man for interrupting my sleep. And I was not so excited about it as the public would have me want to be, if you know what I mean. I said, "Ok, so all right." I was upset that I'd been disturbed, and I simply told the fellow, "What are you waking me up for at 4 in the morning?" He said, "I thought you'd like to know you'd won the Prize." "Yeah, but you could have told me in the morning." So I kind of hung up. I turned to my wife and said, "I won the Nobel Prize." She laughed. She said, "I don't believe you." Because I'm always trying to play some kind of trick on her, and she's always very wise. I never get anywhere; she always knows. This is the one time in which I played a trick on her the other way that worked. She was wrong. I said, "No. It's true." She believed it when there was another call, immediately after and this time some guy says, "Have you heard —?" I said, "Yeah." You know he started out: "Hello, have you heard —?" He says, "Is this Dr. Feynman?" "Yeah." "Have you heard —?" I say, "Yeah." "Do you have any comment?" "No," and I hung up. Just don't bother me at this hour. So we both lay in bed hoping that, you know, it would go away. We didn't jump up and wildly run around or anything. It was to me a rather annoying thing, because I realized what it would mean is all this noise and all this trouble and wild business, you know? There'd be newspapermen for whom I had no respect, publicity for which I had no respect. This world is so full of hot air, and just extra propaganda junk today, that it's not real. I just don't want to get involved with all that stuff, and I didn't know how I was going to escape it. I still imagined that I could by not answering the phone. So I took the telephone off the hook. And then I went downstairs, down here — because I couldn't

sleep, naturally — and began to think about it, and I decided, you can't stop this. This is too big. You can't stop this by just not answering the telephone. It'll get to look solemn. It'll be a worse mess if you do that than if you just go along with the damn thing. So, after this little business — I couldn't stop it — I put the receiver back on the thing, and I tried to answer more politely, because immediately it rang. This was the guy from the Associated Press, and he asked me some questions, and I answered, very much more politely. "All right," he says, "now we're sending a man over to take pictures." "Oh, God" I said, you know, because I didn't want all this. See, I hadn't realized that. He says, "Well, there's no way to —" He was kind of on my side a little bit. We talked about this problem. I said, "What am I going to do?" "I guess you can't do anything." You know. We had an interesting conversation. I never found out who it was, and I never saw anything of this conversation in the newspaper. He was a very nice guy about it, you know. It was just great. We had a nice conversation, in which he told me how to behave — give up, so to speak, and ride it for the fun of it. "There's no reasonable way to do it," he said. But he never made any commentary about this. It's rather interesting. So that was a good man. I don't know who it is. A good man. Then anyway I tell him, "OK, send the photographers. Sooner or later somebody's going to take pictures." Then things began to go. I mean, once I was on the nice side, everybody that called wanted to take pictures. The guy came over from the local newspaper in a car and so on. It was 2 in the morning by this time, you know. My wife got up and made coffee for everybody. The boy woke up. We were walking around in our pajamas. Then I got dressed. You know this kind of stuff — with all the excitement. That's the way it began. Then there was a press conference at 10 o'clock, over in the Athenaeum, and all this baloney — which was kind of pleasant. But it's more of an annoyance than anything.

Weiner:

What about the student reaction? Did they get out a special thing?

Feynman:

Well, the students — yes — that was good. The students put up a great banner, up on Throop Hall, either that day or the next day. It was something like, "Go, Go Feynman" or "Go Feynman" or some such. I can't remember what it was. "Go Big" or something like that. They were very delighted. They did a good job of it. And there were other things that I didn't even see, because I didn't get a chance to walk pleasantly around on the campus. I was always busy answering telephone calls and getting my picture taken and everything. But people tell me that they set up a lot of signs, like congratulatory things of various kinds, with jokes and everything else that I missed, and I'm sorry about that.

Weiner:

Was there a special edition of the newspaper that they got out?

Feynman:

That they got out the next day. And they came over to interview me. They were the best interviewers of anybody who interviewed me, because they were really interested a little bit in what I got the prize for, and had the patience to wait for the answer. But the other guys would say, “Can you tell us in —” “Of course, we don’t know, your public isn’t going to understand a thing you say,” and so on and so on. They almost say it this way, you know. They’re in fear and trembling, you know. “Will you please tell us what you won the prize for — but don’t tell us because we’ll not understand it.” This kind of stuff.

Weiner:

“Give us a quote we can use.”

Feynman:

Yeah, “give us a quote” is really what they’re trying to say. And I couldn’t figure a way of saying it. I gradually developed a way, but it was rather too late — saying I’d worked on the interaction of radiation and matter. That sounds good and doesn’t say anything. But I couldn’t think of that simple phrase at the time. I was thinking more seriously, to try to explain what I did, which is hopeless. There was one guy who came in the office, after that news interview, from Time Magazine and was taking pictures. He couldn’t have made it at the right time, so he’s taking extra pictures of something. As he’s taking the pictures, he says, “How’s it going?” I said, “I’m doing all right except when they say to me, ‘Could you tell me in a sentence, please, what you do to win the prize?’ Or, ‘tell me in one minute.’ I don’t know how to say it.” “Aw,” he says, “you know, if I were in that position,” he says, taking pictures all this time, “and they say to me, ‘What did you do to win the Prize? Can you tell me in a minute?’ I would simply say, ‘Listen, buddy, if I could tell you in a minute what I did, it wouldn’t be worth the Nobel Prize.’” So I used that joke afterwards. It was a very good remark. Yeah.

Weiner:

That was the end of October, right? Just the very end?

Feynman:

Yes. I think so. And then we had hundreds of letters, from friends all over the world, and relatives — like a relative of mine happened to be on a ship, you know, going from Spain to somewhere and, oh gee, he practically busted a gasket, and sent a big telegram. I got telephone calls from Mexico City that I can’t hear because the telephone system was no good. I still tried to get back and tell that person I really liked it, and thank them for

the call, but I don't know the address so I'm stuck. It was hard to hear but I finally understood who it was. All kinds of crazy stuff. Very nice letters. They were all full of — kind of happy. Everybody was excited. Each letter indicated some excitement in the house, whoever it was. You know, there were all kinds: serious letters, that they had never made a better choice or some such wonderful remark, or joking letters, or pure humor, or simple things like Rinus, for example, suddenly turns up and says "Superfragilistic" — yeah, that's all. There were things like that. There were all kinds of things, serious and humorous, telegrams and letters. And in each of them I saw happiness on the part of the people who were sending it and some real feeling of affection, which kind of overwhelmed me and made me feel real love for all these people, because they all seemed to be so good-hearted and so happy about the congratulations. I didn't realize that to have everything come at once like that, it really makes you feel good. So that was the good part of the whole thing, the letters. That was the good part. And everything else, to me, was something of a chore, plus a great worry, which I would like to express to you, because it's funny. I had to go to Sweden, you see, to get this prize, and this involves formality, formal things. I don't like formal things. For example, after I got out of school, MIT, I never wore a tuxedo. I don't like the way it looks. I don't like to get dressed up formal, and I swore off the darned things. But as I told you once before, I have this silly business that when I'm young, I make principles, and as I get older, I have to break them. One of them I had to break, the tuxedo — it sounds stupid, but it's one of those things. I don't like this formal business, you see. It's not a pleasure to me. Certainly it's not a pleasure. Furthermore, the best way to express it is this. When I was young, my father used to teach me that a king is no more than an ordinary man, and to not look at the appearances but what the thing really is. So, this is all appearances — after I get the prize, I mean — all this nonsense I gotta do in Sweden. Appearances, dinner with the King, meet the King, get the Prize, tatata, all this stuff, see. And the worst of it was that I ridicule kings and things like that. I ridicule ceremony. I used to. I still do. I laugh at it. And here I have to be a party to it. It's not very consistent to laugh at it when somebody else is doing it, but when you're in it, because you're receiving a prize and so on, you're going to go right along without some kind of — You know, you used to laugh, and here you are, the Big Boy, right in the middle of it, not laughing anymore, ha ha ha! So this bothers me. I don't know how to express it very well. It bothered me. And then I also heard, somebody told me, that you shouldn't turn your back on the King. You've got to back up the steps after you receive this thing, see. I'd misunderstood a little bit, but it bothered me. And I said, "This is ridiculous, like an Oriental potentate." All of this was a strain — it's stupid, but a strain. And my wife meanwhile was very happy — "I've got to buy you a tux, you gotta buy this, you gotta buy that, you gotta buy this —" And I'd think, "No, no!" You see, each thing she buys, I'd think was only for ceremony, to be worn only once. Such money, it's crazy — I don't want a tuxedo. I bought all the junk, everything, anyway. But she's gone far... Then, this business of backing up the steps—and you know what I planned to do? I thought this is so ridiculous that I'm going to do something ridiculous back. If I have to back up steps, I'm going to invent a way to go up steps backwards that nobody's ever seen before. So I practiced on these steps here, going up steps backwards by jumping, two feet at a time,

you know -- buppbupbupbup -- to see if I could do it very quickly. So when I walked backwards to the steps, I would go brrrrrrpppp -- right up the steps, in a very peculiar manner.

Weiner:

Like an old movie.

Feynman:

Yes. Exactly; backwards in order to show the ridiculousness by other ridiculousness. See, more or less like — yes, that's the idea, like an old movie. That was my plan. I practiced a little bit. But I didn't practice very much, because I expected to go there and see what the steps looked like, when there was just a rehearsal, and to figure it, then practice it, and then do something crazy. I was going to do something crazy. In fact, the Swedish Ambassador came to talk to me, and said, "You must be looking forward to visiting our country?" And I told him, no, there were certain things I was worried about. I guess he must have wired in or something, "Watch out for this guy." I didn't tell him I was going to jump up the steps backwards. But he told me it was quite silly, this business, and I don't have to worry about it. It's easy to go up the steps. You don't have to worry about not facing the King; it isn't going to be so serious. As a matter of fact, when I got there, it turned out that there was no such rule. I don't know when they changed it, but there was no such rule. But there must have been a rule not too far ago, because Block sent me — I wrote him, "How do you go upstairs backwards?" — he sent me a mirror from an automobile — you know, a back view mirror — and so on. So they didn't simply write to me, "There's no such problem." They seemed to have known of the problem. But apparently there was no such problem when I got there. Things had been changed so you didn't have to worry about that after all. Anyway, there was this worry, and I didn't sleep well at night when I was in Sweden, because of all these worries. I was afraid. I didn't like the formalities. I didn't know how I was going to live through them, without doing something stupid, because I was in an uncomfortable position. But they were very nice there, and they're very nice people. Everything was good, everything was fine there, and so on, except its formal, and I had time to take care of all — Excuse me, that's not quite right at all. They had a first secretary of the embassy who was very good about taking care of all these things. In fact, I felt a little uncomfortable, ordering the first secretary to get me a tuxedo. It seemed out of place. He was a very highly intelligent and fine man with great talent, and here I was treating him like a flunky, to get this and do that. "Remind me tomorrow morning when I have to go somewhere." It was rather uncomfortable, because I'm not used to that either. And so on. So, for an American, it isn't so easy, really. They make jokes about it in public. But I think that other Americans must have some of the same — I don't know, I would guess — some of the same uncomfortable feelings, all the way along, with this nonsense about kings, and ceremonies. Dealings with the king, and all this fancy folderol, bowing and scraping and everything else — it's just dopey. They tell me that Sweden is a democracy like the

United States — you know, is a great democracy. In fact, in many ways it's more democratic. It's true. But nevertheless, I was brought up by my old man, so it made it hard for me. So anyhow — oh, we had a lot of good things they did. I don't know whether to tell crazy stories or not. There are all kinds of stories of the events, but that's just anecdotes that don't mean anything.

Weiner:

What about your address? Do you have anything special to say there? You delivered your address —

Feynman:

Oh, yeah. All right. They have to give a speech for the Nobel Prize, something to do with your work, and I couldn't quite figure what to do because after all, three of us won the prize together for essentially the same problem. And if I talk about field theory and Schwinger talks about field theory and Tomonaga, who it turned out couldn't make it because he was ill and couldn't get there — would also talk about field theory, it wouldn't be a very good idea. Then I went around and got this idea I would describe the personal experiences, much like this history here.

Weiner:

You talked with me about it, in your office, when I first met you, in November.

Feynman:

Yes. Right. I got this idea, to talk about the history as if — you didn't give me that idea?

Weiner:

No. You tried it out on me.

Feynman:

OK. I just wondered, because sometimes somebody gives you an idea, and you think you get the idea.

Weiner:

No, you had the idea.

Feynman:

OK, I was just curious. I had the idea.

Weiner:

You must have done it right away.

Feynman:

Well, I got it after a little time, just a little time, and I asked a lot of people, you know, what they would think of such a thing, like Anderson and so on, who knew what the prize was like; and they said it was a good idea. I looked at the other speeches. There weren't many like that, but that didn't make any difference. You can do whatever you want. The real mistake that one makes when one wins the prize is to take it all too seriously for example, this speech. I worried very hard — is it appropriate to give such a speech? It doesn't make a Goddamn bit of difference. It isn't really very serious. It doesn't make any difference what you say. After all, may I remind you that I have never in my life read the Nobel lecture of anybody? They're published, but who reads them? It turns out that Faulkner had a famous lecture, somebody told me, but otherwise, in physics, there hasn't been a famous lecture that people read, that is worthwhile, that is prepared for publishing somewhere else. So I realized that to some extent, but still you do tend to take it seriously and to try to prepare it carefully, and so on. This is a kind of a waste of time. And then when you get there, and it's time to give the lecture, since you've been taking it a little bit seriously that your work is of some importance and that you were given a prize for the work, you would think that your description of something associated with the work would be more important than all these little ceremonies. That's the thought that was in my mind. When I came there and found that the lecture room was about half full, with people who were evidently only coming to see what the guy looked like — many of them were sort of sleepy and it was more or less formal, and you were hurried along because you have to go to the next ceremony, don't forget, and so on — and nobody really cared too much about what you were saying, it was quite an amusing difference to me. I realized that the people were not understanding. They weren't the kind that might. There weren't very many who would. They weren't really interested in the subject and in the speech. So the only thing that might be thought to be real in this whole thing turned out to be least interesting to the Swedish people. So that was kind of interesting. I was invited to various universities, in particular to the University of Uppsala, to give the speech. And at the University of Uppsala, the same thing happened. The room was packed solid to the gills with people, and I was trying to tell them something I found very interesting — which was Mercereau's effect on superconductivity — but I ran a little bit over time. I was caught in the middle of explaining the important and interesting things — which are clearly beautiful and interesting things — when I got interrupted by the man on the grounds that some rector or something of the university was waiting for me for lunch. So, in that it's really right. They consider that the ceremonial things, the meeting with the rector and so on, are

important. The fact that you came to give a speech is only sort of a reason to go through all these other things, a sort of a formal invitation, and the speech giving was only so that people can see what you look like, what you sound like. Nobody was really listening to what you were saying — it seemed to me. I may exaggerate, but that's the feeling I got there — that what I was saying was nowhere near as important as me myself, the freak, to be looked at. And the ceremony, the fact that I'm in Uppsala, means the next thing I have to do is talk to the rector and go to dinner, and so on. And this all to me was just backwards to what I'm used to. I usually like to think that the subject of my speech is the important matter. So those things bothered me. But there were many things that were quite pleasant and very pleasurable. There was a student time, when the students entertained us — just marvelous. Just great, because they're not formal enough yet, you know. And they slightly ridicule formality. They're very much like Americans, and I felt very much better at home, with joking and the nonsense that they went through. Then, after that, I was invited to a beer cellar by some of the students, and we had a kind of a little party, in the beer cellar. This was very informal, very much fun. I say very informal, but as a matter of fact they did give little formal speeches. There was a man with a sword who would bang the table, "Speech," you know. "I'm going to give a speech," and it was mixed, you know, very interesting. But fun. Also there was another amusing thing that happened. There was, after the main ceremony where we received the medals, a dance that came after. Or there was first a meeting with the students and some speeches and then the dance.

Weiner:

There was a thank-you speech that you gave too.

Feynman:

Yes, I also gave a thank-you speech, which does correctly express, if read very literally, my feeling about the Nobel Prize. It was put in as polite a way as I could. It expresses the idea — I mean, the idea is expressed (you could probably get it by reading it) that — well, you read it. At the dancing afterwards — you see, I had to get some release from the formalities — I overdid it then. When I got informal, I just went wild, you see. So when the dancing began, we started, I danced with my wife; then I danced with somebody else, a sister of a Nobel Prize winner. I didn't get to dancing with the Princess, because I had a — you know, I wouldn't even try. But I danced with some nice — I don't remember who it was, families of other Nobel Prize winners — nice girls and so on. Then, I had winked a couple of times at one pretty student. So, when there was a lull, and I saw her again, I went over to her and asked her — a student anyway, kind of mixing, you know what I mean, a student of the university — I asked her if she would like to dance. She said, "Yah," and we danced. She danced very well, and we danced kind of wild, and we had a great time. I danced with her quite a lot, to the exclusion of all the formal people, the Princess, everything else. And I must say, it's very amusing, the world is — really, and in Sweden it's completely under control. When I danced with my wife,

when I danced with the daughter of a Nobel Prize winner, they were taking pictures, all the time — click, flash, flash. When I danced with this girl, which I did twice as many dances as I did with anybody else altogether — no pictures. Nothing. Not in the paper. Not a picture. Nothing. Apparently there's something wrong with this, you see, and they protect the Nobel Prize winners from their dumb idiosyncrasies. But this was my idea of relaxing, of informality. I had to do something because I had to get out from under, you know what I mean? It was fun. It was funny.

Weiner:

From there, you went to Geneva on your way home.

Feynman:

Yeah, I did. I went to Geneva on the way home.

Weiner:

And Weisskopf invited you go give the address.

Feynman:

It's the same address again. I did it much better there. I wish there were a recording of that, because that I did fine. See, I had to write the darned thing for the Nobel Prize, which I wrote from some recording or something. No, how did I write it? Yeah, I think I wrote it from the recording of the speech that I made or something. I can't remember. No, I guess I dictated into a machine.

Weiner:

After?

Feynman:

During. And then later, because I didn't make it in time, and I had terrible troubles because I hate to write, and I had to write this darned thing. I gave the speech best in front of the group in Geneva, because they're friends. They want to hear what I've got to say. They laugh at the right moment. I mean, you could see in the faces there interest in what I'm about to say, and then the smile when it comes out. You can see you're talking to somebody. You know what you're doing. There I think I did very much better on that talk than I did in the particular talk in Sweden.

Weiner:

Weisskopf commented that it was terrific; everyone there thought it was.

Feynman:

I felt good about it too. I was doing all right. I was among friends. This was completely different. They were here, they liked me, but they wanted to hear the talk, too. I mean, it was completely different. As a matter of fact, it was very amusing, because what happened at the beginning was, I had worn a very nice suit that I had got from the tailor, specially made for this, you know; specially made suit. I liked the suit. Usually, I didn't care one way or the other. I liked the suit. So when I got there I started my speech by remarking that, to show that the Nobel Prize has some kind of influence on me I'd changed — because I always liked to give a talk in shirtsleeves. But now I think I have a nice suit on, and I'm rather pleased, and I would like to give this with my coat on. You know? Everybody says, "Booooo!" And Vicky(?) got up and said, "No! You must take off your coat like me." And he tore his coat off, and I had to take my coat off — which was good for me. That was fine. I took off my coat and I gave this speech in shirtsleeves. I was home again.

Weiner:

Spontaneous reaction.

Feynman:

Yes, their spontaneous reaction was "No." Yeah, that was fun.

Weiner:

All right, what do you say, then, we bring this to a close.

Feynman:

Yeah, all right.

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