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Richard Feynman - Session III

June 27, 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 crystalize; 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:

You haven't gotten to Cornell yet?

Feynman:

I haven't even gotten to Cornell yet.

Weiner:

Let me just establish one thing, that it's June 27, 1966, and we are resuming, after an intermission of a few months, our tape-recorded session with Professor Richard Feynman, and with Charles —

Feynman:

That's me. I'm here.

Weiner:

Hey, Ma! — and with Charles Weiner occasionally interrupting. We were just saying that last time, we left off at Los Alamos, where you were cracking safes, and we had agreed last time that the next thing to do was to get you to Cornell. One of the things I'd like to talk about is this transition—how the idea of going to Cornell came up, what you had thought at Los Alamos about what you would do after the war, when you knew that the war's end was approaching. I'd like these details. Then we'll cover the transition period. Then we'll talk about what happened after you got to Cornell.

Feynman:

Well, the details I don't remember. Bethe was a man I worked under. I gradually got so I would say I loved him. I liked him so much. He's such a nice fellow. I would say I loved the guy, you know? He was from Cornell. As the war was nearing the end, after the explosion at Alamogordo when we began to see the end — or maybe it was earlier than that—the question was: what are we going to do? Or maybe it was VE Day in Europe or something. You know, your mind began to work on it. Somewhere along the line, I got an offer of a job from Cornell, through Bethe's influence there. And that's what I wanted to do. I never considered anything else. I got other offers from other places, but I just didn't consider them because I wanted to be with Hans Bethe. I don't understand now why. I liked him very much, and I never regretted that decision, but I did get other offers.

Weiner:

From where?

Feynman:

All I remember is something from California.

Weiner:

Berkeley?

Feynman:

Probably. I don't remember very well. It was simple. I decided to go to Cornell. They offered me what then was a fair salary, but they didn't realize at Cornell what was going on in the world, what was happening right after the war when the salaries offered were very much higher all over. What happened as a result was only that every once in a while I would get a letter from Cornell telling me that I'd get a raise. See, I hadn't gone there yet, but I got a succession of raises while I was still at Los Alamos. I believe that the raises were the result of Bethe's knowledge of the various offers that were being made to me, which I was refusing. But being rather idealistic, I was refusing them irrespective of salary, simply on the basis that I wanted to go to Cornell because Bethe was going to be there and he sounded like a good guy to work with. So I believed. That's what I think happened. That's why I paid no attention. I can't remember who offered me what. All I remember was the, to me, amusing thing, that I got a series of at least three raises, if not four, before I arrived at Cornell, without any effort on my part whatsoever to have done a thing.

Weiner:

You gave me last time a letter about one of the raises. That was sent to you at Los Alamos?

Feynman:

That was a letter from somebody suggesting to somebody else at Cornell that they raise my salary.

Weiner:

That was probably when you were there. I think it was probably '46.

Feynman:

Well, I wasn't there then, no. This was one of the letters asking to raise my salary even though I hadn't been there yet. That was some internal problem at Cornell, as to why this guy gets raises when he hasn't done anything yet. But they were moving more in line with the prevailing wages of the day.

Weiner:

Where were the other guys going? Do you remember what the feeling was?

Feynman:

Oh, people were going all over. They were looking for their home bases. There was a lot of jockeying around, people looking for guys, a lot of guys getting jobs.

Weiner:

Did you sense a competition between, say, Chicago and Berkeley?

Feynman:

Oh, I think there was probably competition. I was not sensitive. I had decided to go to Cornell. Oh, I remember. Later, the question of who else would go to Cornell became interesting, and there was competition for certain guys that Bethe would like or I would like very much as a good friend. I think Bill Woodward, and probably Bob Wilson, who I worked with.

Weiner:

At Princeton?

Feynman:

I'd worked with him at Princeton. Other people I liked had been offered jobs at Cornell too, and some of them were more doubtful than others as to whether they would go. And I would help to try to convince them that it would be great to go. I remember vaguely feeling that it would be great if these guys would be there, and making some effort to get a few of them. But I don't remember know which ones were in doubt. But there were doubts and considerations and discussions as to who would be where, so we would try to think ahead. Not think ahead, but one felt the interest as to who would be there, all these friends. We were a lot of friends by that time, and you want to know if your pal is going to be at the same place that you're going to be at later. But I don't remember ever thinking not to go to Cornell. All I remember is trying to do my best to

convince anybody else that I wanted to be with to come to Cornell.

Weiner:

Wilson didn't go direct. He went to Harvard, I think, for a year.

Feynman:

Well, perhaps. Perhaps Wilson was one of the ones that I was worried about the most, who didn't go. I don't know. There were some we lost, but I think we won in the end or something like that.

Weiner:

When did you leave Los Alamos?

Feynman:

I left a little earlier than most. I left in time to begin the school year in Ithaca. My memory says November, but this is incredible. I mean, something's the matter, because I don't think the school year starts in November. However, I got there before the school year began by some period of time. I described that to you also.

Weiner:

You went as an associate professor?

Feynman:

Probably an assistant professor. I just don't know. I think I would be the lowest rank.

Weiner:

I thought so too, but biographical references and so forth seem to show —

Feynman:

— but I was getting raises rapidly. At the beginning, you see, the world of the old university, the men who were left behind, didn't know what was happening. As soon as the wild scramble began, the demands were such that raises in salary and rank probably came in rapid succession. I can't tell you what form I was in when I got there.

Weiner:

But you said you can describe your reaction when you got there.

Feynman:

Yeah. The first thing is, I was invited on the way to give a talk in Iowa, on something — Iowa State University — and I stopped there. I can't remember why I decided to do that, but I did that. This I just remember, but I don't know why. Then I went on the train. I went on the train all the way to Ithaca. Now they had asked me to come at a certain date, and all during the war, when anything was on a certain date there was a lot of pressure. Like, when I would go to Oak Ridge, I had to figure out on the airplane during the time and so on, and you got this idea that everything is done for a certain day. They would like me to begin on such and such a day. I thought that that was the first day of classes. So, on the train I prepared my course to teach. I was supposed to teach mathematical methods of physics, which is sort of a mathematics course, applications in physics, for graduate students. So, on the train I prepared the outlines and worked out the whole course. I'd never taught a course before. I had it all figured out on the train. I can tell an amusing story — you can always later throw it away, you know? I finally arrived in Ithaca at 2 a. m. or 12 something, in the night. I got off the train, and I slung my suitcase onto my shoulder as I always used to. Then I said: "Wait a minute now. You're a professor, and you have to try and behave like one." A porter asked me: "Can I carry your suitcase?" "No, I carry my own." Then I realized: I've got to start living in a dignified way. So I let him carry it to a taxi, and I sat rather elegantly in the back of the taxi, and the guy says, "Where to?" I say, "Biggest hotel in town, please." He said, "That would be the Hotel Ithaca." On the way he says, "Do you have a reservation?" I say, "No." "Well, the hotel situation's tough. I'll take you there, but I'll wait for you. They probably haven't got room." So I went and sure enough, they didn't have room. I went to another hotel and they didn't have room. I left the suitcase there, and I dropped the taxi — it was too expensive. I started to walk around the town to find some place to stay and took quite a while. I didn't find anything. Then I found another man wandering about, and he didn't find anything either. So the two of us started out together, and we wandered. We wandered in various directions, and we thought to stop at someone's home and ask if they knew any place that you could stay in this town, and so on. Just about that time, we noticed a building. We could see through the window there were a lot of beds, like several double-decker beds, you know, and so on. Obviously it was some kind of dormitory for students or something. And we went to that to ask them if maybe they had an extra bed or something. We went to the door and there was nobody there. It was completely empty. I never did figure out exactly what it was, but we went in, we went upstairs, we saw the beds, and we figured this would be a good place to stay. This friend of mine said, "All right, I think we should stay here." "Great idea." Then I suddenly realized: "I'm a professor" — you see, and that wouldn't be so good. See, I never had been a professor, and I was a very lively character, and everybody had been teasing me about Cornell back at the other end — "Wait until they find out what they've got." So I

was trying to avoid it should it be too bad — you know? So then I realized this wouldn't be good. I'd wake up in the morning, the guys come in, they say, "Who the hell are you?" — kick me out — it turns out it was a professor. You know, it's not so good. So I told him I couldn't do that without permission from somewhere. He said, "Oh, all right. Come on." So we went out. We started to wander around some more. Then we found ourselves evidently on the campus, because there were buildings of a school, and there was a great pile of leaves raked up — a great big thing. It would be good to sleep in. We had to sleep somewhere. Unfortunately it was under a light, a street light. We decided to find another pile of leaves. Just then we found a building with a light in it. We went in and there were some couches in a big lobby, like leather couches. So I went and found a janitor who was working there at 2 a. m. "Is it okay to sleep up there?" "Okay," he says, so we slept on these couches. In the morning I went into a kind of men's room that was there and washed up and so on. This was for my first day. I'd prepared myself then, and I ran to the physics office by 9 o'clock, figuring this was the first class. I didn't know whether my first class was at 9, 10, or 11. See, I thought I'd be ready. When I got there I was shocked to discover they'd asked me to come a week early, because they'd figured I'd need time to get used to the place, settle down, we would get used to you, and all this slow stuff. See, I was going like a firecracker trying to find anybody in the office: "Where's the boss? Where's the — what's the? — Where's my class?" and all this stuff. It was completely — like the nervousness of working during the war. And this university in the backwoods of New York State was going at the typical university rate: "Well, he should come and get used to us..." So I sat in the man's office all excited. "When do I give my class?" and he's talking so slowly and batting the breeze about the weather; I couldn't get used to it. No businesslike, you know no beep-beep-beep. At any rate, he told me it was a week ahead, or five or six days ahead at any rate, before my class began. I was all prepared to walk into it within the next five minutes. Then I said to him, "Well I have a little trouble with a room. Where can I sleep?" He says, "You go down to a place called Willard Strait Hall, a communal thing for the students, and there's an office there. They'll give you a place to sleep, see?" So he told me the directions and I wandered down to Willard Strait Hall and went into the center there, saw this booth or something, went up to it and said to the fellow — remember, I looked rather young — I said to him, "I wonder if you could tell me where I could stay, you know, get a room to live in?" He says, "Listen, Buddy, the room situation is tough. In fact, it's so tough that believe it or not, a professor had to sleep in the lobby here last night." So I say, "Look, Buddy — I'm that professor. Now, do something for me, will you?" The thing that amused me and bothered me already was that I had tried to come to Cornell without making any kind of a noise that I'm peculiar. I'm not there more than one half a day, and not only is there a rumor about me, but it's so extent that I hear it myself right away. So that was my beginning at Cornell. I don't know if you want that kind of amusing —?

Weiner:

Fine.

Feynman:

But then I did find a room, not a good one, and I stayed in it all the time. You know how a young man is. A fool. He should find a very good place to live, and not just any old place to live. I lived in any old place for a long time. I've learned since that you should unpack your suitcase, settle down, and live nicely. It takes a little longer to find a place, but it's worth it. Anyhow, I can't remember the first days, but then I started to teach this class. The thing that I had prepared was, I realize now, as I think about it for the first time, much like those lectures I prepared in physics.

Weiner:

You mean those known as the Feynman Lectures?

Feynman:

Right. What I did was, I tried to describe the entire subject with some kind of completeness, and tried to explain to the boys that these pieces all fit together. They certainly couldn't learn quickly; it's too complicated. But I wanted to mention them so they knew what the words meant and learned what they were filling out, you see. And to my great surprise they learned everything. So my course was really a complete course in mathematical physics, very complete, and they learned everything. I was very surprised.

Weiner:

This was a one semester course?

Feynman:

I don't remember. Probably one year. Anyway, that course had a great influence, it turns out, and some of the students who have taken it have made corresponding courses in other schools, and so on. In fact, our Caltech course here is made by Bob Walker, patterned after that course, because he was a student of mine at Cornell. So that has had some influence, although it's not published anywhere. But you know what I mean. I did have some success in teaching. That's what I'm trying to say. It worked out all right.

Weiner:

How did you feel when you got up for the first time in front of a class?

Feynman:

I had no problem. By this time I had gotten up a number of times at Los Alamos to

explain or to give lectures on some subject, and I seemed to have discovered some knack to do it. And people told me I did OK. So I had no trouble with teaching the class. Not at all.

Weiner:

You had the Princeton seminar experience too.

Feynman:

Well, that was more of a shock. I mean, that was not enough to learn. It's just one. But many times discussing and explaining things at Los Alamos, apparently — it's the only way I can figure it out, because I have never had any trouble lecturing in classes after the first couple of times that I did it.

Weiner:

When you got there, was Bethe there too?

Feynman:

No, Bethe hadn't come back yet. He came back some short time after.

Weiner:

Who else was there?

Feynman:

I don't remember. Parrit was there, Lyman Parrit, and the head of the department, whose name was Gibbs, R. C. Gibbs, was there. And there was another important man whose name at the moment escapes me. It's just crazy. It's one of these blocks.

Weiner:

Was Bacher back there yet?

Feynman:

No. I can't remember the exact timing. There weren't very many people that I know. They were not people I knew. But they came rather rapidly, within a month or two. I was just a month or two early in getting out of Los Alamos. I was one of the first rats to run from the ship. But the others came. In fact, there was an arrangement, because

Bethe felt that although we both couldn't get out, it would be good to help them get started to have at least one guy there. But he got out pretty soon after that. My first year at Cornell was quite interesting in many ways. You see, I had lost my wife, and so I was a bachelor really — right? And I found the girls at first rather interesting in Cornell. I would go to freshman mixers and so on. At that time, there were many students who had come back from the Army who were old-looking, and it was kind of mixed up. It was difficult to tell who was what. So while I was a professor I could act very much like a student, even a freshman. I could be mistaken for a freshman in a perfectly legitimate way. I remember the first dance I went to. See, I wasn't sure of myself. I'd danced with my previous wife and so on, but I hadn't danced with a girl to try to get a girl to like me in so many years that it was kind of experimental. And I remember the first dance that I went to which was a freshman mixer of some sort. The freshman girls were there and I was there and so on. I remember I would dance with one, and we'd dance along — we were dancing all right — everything going all right. Then she would start to talk to me and ask me some questions and converse, and then at the end of the first dance she'd say, "Excuse me, I've got to powder my nose," or something. And this went on with three or four girls. I didn't know what the hell the matter with me was. But one girl, fortunately, had the nerve to tell me. We danced a while, and she would ask — you know, they'd ask who you are? I'd say, "Well, I'm in physics." "What are you?" "Well, I'm a professor of physics," and so on. And she'd ask more questions, and then she'd say, "What did you do during the war? I'd say, "I worked at Los Alamos on the atomic bomb," and then she said, "I suppose you saw the atomic explosion in New Mexico?" and so on. I'd say, "Yeah." "You're a damned liar" — and she walked off. They all assumed that this guy was, you know, a big blow-off, a liar. So I discovered that, and after that I always concealed my background, and then was much more successful. The next time, I didn't tell her the truth, and the girl didn't walk away after the first dance.

Weiner:

Isn't that funny. I'd think it was very glamorous, then, in the mind of a freshman.

Feynman:

It was too crazy. It was out of proportion. The girls were too smart to believe all that baloney. It was much easier — obviously it was the kind of thought that some faker would make up, you know? Then I taught my classes. Either I taught one class or two classes or else this went on longer, because I prepared mathematical methods of physics, and I also prepared a course in electricity and magnetism, whether at the same time or the next year I'm not sure, because those early years are confused. There's one or two years involved. But I was busy working on the courses and doing really nothing about my — See, I had hoped to run back and do all this work on this physics that we were talking about. But I decided, when I first got there, I'll take a little rest. I'll do my courses, but I'll rest. And I used to go to the library and read. In fact, I read the Arabian Nights all the time in the library. I tried to meet girls. I simply didn't do anything but

prepare the courses. I now realize that preparing a good course is a good full time job, but at that time I didn't think that was hardly anything. I thought I should be doing research. So I got deeper and deeper into a kind of — I wouldn't say depression, because I wasn't depressed. I'm a lively and happy fellow. But behind it all I was always worrying about the fact that I wasn't doing anything. I became conscious of the possibility that I was burned out, and I wouldn't accomplish anything. It happens to people. You see, this was at first a vacation, but the vacation kept going and all I was doing was preparing classes, and I couldn't get to work. I would fool around, go to a dance, or I'd sit on the grass and look at the sun, or I'd read the Arabian Nights, or play drums or something — in the room or something like that—and always never get down to work. I couldn't get to work. And I began to think that this was the end. At the same time I was perpetually getting, as I explained to you, raises because they were still attacking from the outside, trying to get this guy out of there. And I would pay no attention to it. In fact, I would write a letter "no" without telling anybody, but apparently the secretary would tell, or somebody on the other end would ask or talk so that I was getting these raises all the time. If they'd offer me more money from somewhere else I would say no, and I'd get these raises — and they helped my psychology not one bit. Here I think: I can't do what they think. You see? And their opinion is going up and up, and I'm feeling less and less adequate to the situation. And I feel guilty.

Weiner:

Where do you think this reputation came from? From Los Alamos? From the people who knew you there?

Feynman:

I think it's probably from the work at Los Alamos, yes. It's partly from something from Princeton. The guys must have known the thesis; they might have thought it was good. But I think Bethe knows what I was able to do at Los Alamos and all these little jobs — he had probably a considerable feeling that it was... I don't know exactly. That's from the outside. I didn't understand it at that time, and I'm not going to try to understand it. At that time I thought, they're cockeyed. See — they're wrong. I thought, they don't know. I'm no good. So I kept doing the class OK, but nothing else. This went on for nearly two years or over. I still remember, everyone in a while somebody would get a problem, something about gamma rays, that I'd start to get interested in — and then I wouldn't do anything. And then there would be some discussion — I'm not doing anything. I mean, other students would talk, why I don't do anything, and so on. So I didn't do any physics of the old kind. Little tiny things — but nothing else. You know, discuss with guys and all this. And teach the classes. So I had this psychological thing, see. I tried a number of things, like getting up every morning at 8:30, try to work hard. Nothing worked very well. So one day I got an offer from the Princeton Institute of Advanced Studies, which was where Einstein was. I'm looking at it from my own point of view — you know: there are great men out there, Einstein, other guys. This was the

height of intellectual super-something. An offer from the Institute, which wasn't only an offer, but they explained to me that they would let me be professor of physics at Princeton University half time, and at the Institute the other half of the time, because they knew of my feeling that there's a little too much thinking at the Institute of Advanced Studies, not enough contact with the fundamental world. You know what I mean? So they would give me a special kind of a thing, even one notch better than Einstein in the sense that I would have liked to have contact with students. But then I would have the freedom of time to work in the Institute, so I wouldn't have as much of a load. It was just perfect, and the salary much higher than I was getting, and so on. OK? But to me, in the psychological condition I was in, I concluded: "They are absolutely crazy!" So absurd was this proposition to me, so mistaken was it, so obviously wrong, that I was worth that — you know? I was shaving that morning. I knew I would refuse it. I was refusing many things. I was refusing them on the grounds that, they don't know I'm no good. But this is so crazy, you see! While I'm shaving I think, to myself, you know — "I can't be responsible for dumbness like this. I can't live up to their idiot impression, right?" In other words, I got the brilliant thought that I had no responsibility whatever to live up to somebody else's impression of how good I am. That was a thought. And so I shouldn't feel uncomfortable that I'm not as good as they think I ought to be. I never said I was. I never claimed a god darned thing. See, this was like a shock. It was so crazy from the point of view of where I was that it clearly showed that it's impossible. I can't live up to such a view. It's impossible — therefore I shouldn't try at all. And this gave me a new way of looking at the whole thing, and I was released from the guilt feeling. Coincidentally, about the same time, within a day, Bob Wilson, who was head of the nuclear lab and had something to do with paying my salary or something, called me into his office. I don't know why. It would be interesting to ask him why. He gave me a talk of which the sole content was that when we hire a professor we take a risk, and it's our risk — namely, the organization's; that the chance that a guy comes out to really accomplish something is not high, and there are many professors that just do the work of teaching the class, and that's perfectly all right, it's perfectly OK — you know, this attitude, that we're not responsible to the university. Now, why he gave me such a lecture at the time when I needed it I don't know, but it was right, at just the right moment. I had just gotten the idea, and this came on top of it, and that just turned it around. So I decided that, when I was a kid, I used to enjoy the subject for the fun of it. I used to like nature and do it for the fun of it. So what I ought to do is play games with it, just whatever was curious and interesting to me — I should just play. You see? Just like I was when I was a kid — try to find a relation between things, do this, do that, whatever I felt like. I don't have to do this problem because it's important or that problem because it's important, or everybody expects me to do something.

Weiner:

Because you weren't trying to get your PhD, or working for the war.

Feynman:

That's right. I had nothing. And now I wasn't even trying to live up to the reputation that they had — you see, I had nothing left to live up to. OK? I had no strain. So that day, or the day after, within days — it's very quick — this psychological thing worked like a charm. I was in the cafeteria eating as usual. I used to eat in the students' cafeteria because I liked to look at the girls. And some kid throws a plate up into the air. You know how kids are always — plate goes up in the air and comes down. Now the plates at Cornell had a blue seal at one side of the rim, and he threw his plate up in the air, and it was sort of flat and wobbled, almost horizontal but with a slight wobble. At the same time, the blue mark on it, the insignia on the plate, went around the plate. And it looked to me interesting. The wobble and the motion seemed to be related. So I wondered, what is the relation? How many wobbles per rotation is it? So, after fiddling around with the equations of this thing — see, this is a new thing, to play, just to play — I found out that if the wobble isn't very high, if it's almost horizontal and just slightly wobbly, it goes around (if I remember rightly) twice. The insignia goes around twice, while the wobble motion goes around exactly once. It's cute, and it's a nice relationship, two to one. But then, because I always liked to do this, I wanted to understand, not from the equations, but from Newton's laws alone why, if a plate is spinning at a certain rate and it starts to wobble, it'll wobble at exactly half the speed, you see, because there's a nice ratio and a simple proposition. I want to see the forces, not just set up the Lagrangian and differentiate all these equations—no, but how it worked for a disk. So after considerable effort on that afternoon, fiddling and drawing diagrams and forces and so on, I saw an easy way to see how the motion would mean that all the acceleration was normally balanced and that was the right motion, by hand, you know. So I ran into Bethe's office and I said, "Hey, I saw something funny about a disk," and this and that, and I show him all this stuff, see. And he says to me, "But what's the importance of that?" I remember saying, "Hans, it doesn't have any importance. I don't care whether a thing has importance. They haven't got importance. Isn't it fun?" He says, "It's fun." "Well, that's all I'm going to do from now on." Within a week, altogether, this question of the rotation started me worrying about rotations, and then old questions about the spinning electron, and how to represent it in the path integrals and in the quantum mechanics came back, and I was in my work again. It just opened the gate. It worked, getting at all the junk I used to play with before in the same old spirit, and so on. So I got free at that moment.

Weiner:

That, you think, was in the second year.

Feynman:

That's in the second year.

Weiner:

The academic year 1946-47.

Feynman:

Yeah. It can't be very much before I did the work for which I got the prize. It wasn't very long before.

Weiner:

Let me try to fix a date in your mind. I came across this: "The Future of Nuclear Science." It was at Princeton University Bicentennial Conference.

Feynman:

While you're talking, I'm thinking.

Weiner:

OK. That's what I'm trying to do.

Feynman:

I get a certain amount of confusion, because I can remember in one room—see, I always remember where I did the work. Yes. OK. I had a year or half a year of working hard before I got something. I played with a lot of things. I got started with the path integral. I did an awful lot of work then, trying to make my path integrals work with spinning electrons. There was a long period of that.

Weiner:

That was '46 some time?

Feynman:

No, because '46 is when I got to Princeton — rather, Cornell. So it's at least a year and a half or two years later. The best way to find out is when the hell I got the offer from Princeton.

Weiner:

You left Los Alamos in '45.

Feynman:

Yeah, you're right, in '45.

Weiner:

So if you had months or even a year of getting used to the place, it still puts you —

Feynman:

— something like a year, and a year and a half.

Weiner:

Now, there was a conference at Princeton sometime in the fall of '46.

Feynman:

Right.

Weiner:

And you were there.

Feynman:

Right.

Weiner:

And you said a few things — I'm trying to help you fix the date, but I also want to know about this conference, because —

Feynman:

I'll tell you about the conference.

Weiner:

All right, tell me about how come you were invited, what it represented in your mind. It sounds like a —

Feynman:

— all right. Good. Conference had a bicentennial or something?

Weiner:

Yes, bicentennial.

Feynman:

OK. They had a bicentennial. And I was invited by Professor Wigner — presumably because he thought my work was good when I was at Princeton, as a Princeton boy — to come to the bicentennial. Also Dirac was going to give a paper, and would I please introduce it? OK? I think I was in the same psychological condition, because I remember feeling at that point like a ward-heeler in the 53rd district introducing the President of the United States — you know what I mean? It's about as important as an introduction. Who knows me, that I should introduce Dirac? That's clear. It could have been in any period, but I would guess that this was before my release, my psychological release. I'm just guessing. Anyway, I went to that, because it was an interesting conference, and I had a lot of trouble there of a certain kind which is interesting. I went to the meeting, and the meeting was very peculiar. It was of scientists as well as high school teachers. So there was a mixture of: how to educate the young, discussions of atomic energy, and the atomic bomb and technical sort of discussions, what is theoretical physics doing these days, in which there was Dirac's paper, etc. Now, Dirac gave his paper. He had given me a copy. By the way, it was handwritten.

Weiner:

You don't have that, do you?

Feynman:

No. No. He gave me a copy ahead of time so I could read it, so I could prepare my remarks. Now, I'm trying to remember the best I can what happened. The first thing that struck me when I read it was that this was on the wrong track. I had been doing this work on electrodynamics, and I felt that he was going backwards and working more and more in Hamiltonian and it's just not going to get anywhere, and that really the paper was not important. That's what I thought, that it didn't seem to me to get anywhere. Furthermore, it was highly technical, very technical, and the audience was a mixed audience. I have a feeling for the guys in the audience, and I felt bad for all these teachers that wouldn't understand what in the hell is this all about. After I introduced him, I was supposed to make some remarks afterwards, you see. So after he gave his lecture I made some remarks. I tried to explain what Dirac was talking about in the simplest possible language, so that the teachers and other people could understand me

and understand what the hell this was about. See, he was telling about the problems of quantum electrodynamics which were not solved, and he was working on this problem. So I tried to explain to the audience, but my audience was not my technical friends but the teachers and other people. Historians were there, see — in physics, scientifically oriented, but still. So I tried to explain the position of electrodynamics, say what Dirac had been talking about, and then make a criticism of Dirac on the grounds that he wasn't coming to the central problems, and explain what the problems were and so on. When I got through — of course, in my usual way I made jokes as I talked along and so on — when I got through my good friend Weisskopf said to me, "It's a shame. It makes me feel sad to see a good friend of mine make such a poor presentation." That hurt me a little bit, because I wasn't confident. See, it was a poor presentation, if I were really criticizing at the same technical level. So he wasn't even thinking about the other guys, and I was thinking about the other guys. Then Bohr got up and said, "Feynman makes very many jokes," and so on, "but aside from the jokes we have some important problems here to discuss." You see, my level was aimed at somebody else, and these guys all thought in their private, provincial way they should worry about their own technical problems and discuss technically what Dirac had done. So I had done something not right. I was criticized by Bohr also for talking in such a light-hearted fashion.

Weiner:

He did this publicly in his comment?

Feynman:

Yeah, yeah, with some joke about Feynman and his jokes.

Weiner:

By the way, you had met Bohr earlier?

Feynman:

Yes. I'd met Bohr earlier. I haven't told you anything about my relations with Bohr, but I should. Are you sure I haven't talked about Bohr coming to Los Alamos? All right, put that down because that's interesting. At any rate, he was there. And then he made some commentary which I thought was absurd, which was that the infinities that we were getting in these various theories were all going to balance out, that there would be more particles — there's protons, then there's mesons, there's this, there's plus signs and minus signs and plus signs and minus signs, plus infinities, positive energies and negative energies, C's and positive energies, background, and they're all going to add up, so there was no problem. He therefore deduced there must be one more particle of such and such a kind, in order for this to work. That sounded crazy to me, just instinctively. It

was, of course. So I didn't like that theory. And they're all sitting around worrying, and they're talking, and I look out the window, and through all this Mr. Dirac, paying no attention to anybody, had walked out and was sitting on the grass, lying on the grass with his elbow against his head looking up at the sky. I thought, that's interesting, and I went out too. I went out to him and I said, "I guess you don't care what they're saying — I don't remember, something like that, because I didn't believe what they were saying either. I felt a kindred spirit. I talked to him a minute. But the main thing I remember was, I'd wanted always to ask him a question, and I asked it. I said, "Do you know, in your book you make the relationship that the action in classical mechanics is analogous to e^i over h times the action, times the Lagrangian, is analogous to the operator carrier for an infinitesimal times from one position to another, from one wave function to the next, really from one position to another." I said, "Did you know that they're not only analogous but they're equal? Or rather proportional?" He said, "No, are they? Are they proportional?" I said, "Yes." So at least I found out that the discovery which I had made which led me on, that I told you about, that they were really proportional, was really a new thing. He himself hadn't noticed this, but I didn't know. For all I knew, he always thought they were proportional. See, I still had the belief that he thought they were proportional and simply was explaining that. But anyway he said that he didn't know they were. He said, "Oh, are they?" That's what he said. I said, "Yes they are." "That's very interesting." That was the end of the conversation, I think. But anyway, that's what happened at the Princeton Bicentennial.

Weiner:

I found the brochure —

Feynman:

— now you're going to prove something.

Weiner:

No. No, it's different, a different matter. They're talking about the discussion of particle theory after Dirac's talk, and this is a summary, see, and I spoke to the guy who ran the show. I asked him about a month ago at Princeton, when I was putting this together, did he have a Proceedings, the paper?

Feynman:

It's interesting that you get me to tell all this before you read it, because it's fun to see the difference between what's remembered and —

Weiner:

— well, it says here, “Typical were the comments and queries made by R. P. Feynman of Cornell University and Gregory Breit of the University of Wisconsin.” Then they have a big quote, and I think it’s from Breit, and then they say, “But perhaps all this may best have been summed by the comment of Dr.

Feynman:

“We need an intuitive leap at the mathematical formalism, such as we had in the Dirac electron theory.” Editor’s note: “The theory which predicted the positron discovered in 1932.” Then to go back to your quote: “We need a stroke of genius.” This they categorized —

Feynman:

— let me see if the quotation’s from Breit or from me.

Weiner:

No, that’s from you. The earlier one, I’m not sure. They don’t give any credit.

Feynman:

Why don’t you turn that off, save tape, while I read this? It’s certainly possible I said that, this part that you’re talking about, the part you didn’t read. It’s possible. It sounds like I might have. What I was trying to do was to explain the character of the problem, and I think that may have been a quotation from me, but I can’t tell you for sure.

Weiner:

It has your picture on the back page; it has a whole group picture. See what you look like.

Feynman:

Yeah? It looks kind of young.

Weiner:

You’re behind Wheeler.

Feynman:

I see myself. Wait — is that? I can’t see good now. I’m too old. I’ll look at it again. Isn’t

it — yes, “the Future of Nuclear Science?” See here — “Scholarship in the Secondary School.” That was part of the conference. All these poor guys were sitting in the audience while these guys are talking all this technical stuff. So I — I screwed it up, according to my friends... Yes?

Weiner:

Did you get a feeling after the war, and was this conference a symbol of it, that the whole field of nuclear physics was now in a new stage, that there was a whole new excitement brewing? What was the effect of the war in nuclear physics, other than the fission question? Was it just a question of marking time until the job was done and getting back to the old problems?

Feynman:

Yes. Yes. With one great advantage. There were a large number of new tools available because of the neutron piles and so on, which produced very large numbers of neutrons, new kinds of experiments, and so on.

Weiner:

What was the effect of bringing theoreticians and others into contact in a single place, in Los Alamos?

Feynman:

That kind of a thing, like social effect in science, would be outside my interest. They obviously had effects, but I wouldn't be interested in them at that time. My impression at that time was only that we had marked time. Of course it had a big influence. We not only marked time. Obviously I met somebody; I did some work. Everybody did something. There was a change. But regarding the feeling, as far as I was concerned we just were getting back to something. There were three problems of importance, three differences, I would say. One was the technical, experimental tools that had been developed. Well, two at least, that I can think of. I can't see what I said “three” for. Another was a question in people's minds having to do with secrecy associated with science, that the nuclear work and all this stuff was in secrecy. What kind of influence was that going to have? How were we going to keep going doing nuclear research when there were secrets or regions that were secret in the physics? You know, it was a problem that was in our minds. It never amounted to any difficulty as far as I know, anywhere. They worried about it then.

Weiner:

There were a lot of talks on it.

Feynman:

Right. But it turned out that somehow or other that problem was averted. As far as I know — maybe other people don't agree — there was in my experience no serious difficulty produced because information was maintained as secret that was essential to a more or less fundamental understanding, or was kept secret too long. There were important things which were released gradually — but in time, so to speak.

Weiner:

Now, this conference, as far as its effect on your own work goes, you seem to think that it was before you had gotten into this new psychological state.

Feynman:

Yes. See, I only remember the part of it having to do with Dirac. The rest of the conference didn't mean a damn thing — can't be remembered. I don't remember anything, except vaguely the problems being discussed. But I do remember another thing, which is just consistent. The usual feeling I had in the years before was reinforced: the feeling that the big guys don't know what they're doing. They're not getting to the problem. See, I felt that Dirac's paper was not on the ball, that Bohr's commentary was cockeyed, and they hadn't gotten at it. They were in the wrong direction. I don't know what the right direction is, but the problem still isn't solved completely. But I still felt that they were on the wrong track, and that was a feeling of a certain amount of confidence. Whether that instigated me to do more work or not, I can only tell by looking at what date I did different things, and I don't date my papers so I can't tell you. That'll take some work, to find out if that had an influence.

Weiner:

Well, you can date your published papers.

Feynman:

Oh, yes, but they take time to write.

Weiner:

Let's get back to Cornell, then, if that's the important place. So you got into this new frame of mind, and you decided to play and do things because they interested you, and not to worry about —

Feynman:

Right. Very rapidly, within a week or so from that time, the things which interested me were the old things which interested me, like path integrals and how to do spinning electrons with path integrals, and I spent an awful lot of time on that, a tremendous amount of time. I can't remember what it adds up to. I at that time had met Robert Frank and his wife, and I was often invited to their home for dinner and so on. I remember many times working over there — see, I can remember what I'm doing in terms of where I'm getting the ideas or where I'm working — and talking the problems over with Bob Frank. That would help me date it, if I could talk to him. In other words, I was working on this stuff.

Weiner:

You talked over problems with him. Anyone else at Cornell you discussed this with?

Feynman:

Possibly, but I don't know. They were very private problems to me — this special problem of making a math integral to do spin. And I studied quaternions in the books in the math library and read all about Hamilton, who was a great ideal of mine. I like Hamilton. Hamilton, also Maxwell. So I learned about quaternions and thought that was a very important thing, and partly solved some of the problems of getting this. In one dimension I did a very pretty relativistic theory of an electron by path integrals. It was so exciting that I was sure that I could gradually work it out in four — in two dimensions, space and time, one space and one time. But I never did figure out how to date the analogue in four dimensions in a happy way.

Weiner:

This is the period, too, that you went back to the earlier paper. We've discussed that before. The paper that you had done with Wheeler. Just too sort of set the record straight. Remember this? It was a 1945 paper that you published.

Feynman:

Oh, no. No, that was not going back. That was done by Wheeler. If the paper says Wheeler and Feynman on it, and it's published in 1945 — it was written by Wheeler.

Weiner:

Then you picked it up. You had something to say on it in '48.

Feynman:

Oh, '48, yes. I haven't got there yet.

Weiner:

That's right, of course. We're still '46-'47. Anyway we covered that particular paper last time.

Feynman:

Somewhere along the line, I visited a good friend, Mr. Burt Corbin, whom I met at Princeton somewhere, at some time in the past. But I visited him and stayed at his house for several weeks on end in Pittsburgh, and we discussed much in physics. Now, I don't know exactly when this is, but when we're there, he convinces me or someone convinces me or I convince myself that this is a great opportunity to write up for publication my work that I did for my thesis, a piece of it anyway having to do with how to do quantum mechanics with path integrals. And so he or somebody convinced me to work it out, write it up, and we worked together. We didn't work exactly, but I talked to him while I was trying to write it, and reformulate, reformulate, until we got an axiomatic way of describing it. And then I wrote it up and handed it in, sent it in to the Physical Review, and they suggested I print it in Reviews of Modern Physics. They sent it back first, and Bethe taught me the trick. They sent it back and said it was too long and this and that, and this stuff is old hat or something like that. Not exactly, but they said that this first part was well known to everybody and you could leave it out. Bethe said you should write back a note which says that you realize it's well known, but in order to show how from the well-known, with just a slight change, you go into the other things, you have to emphasize that this part is known, this part is not so known, and it takes a few paragraphs. "However, I will shorten it for you" — and then take out one sentence. He says if you just make a small effort in the direction indicated you don't have to take the whole thing out. And that worked. They published it.

Weiner:

Reviews of Modern Physics?

Feynman:

Yes. And that's the article called "Space Time View of Relativistic Quantum Mechanics," published in Reviews of Modern Physics. OK? That's the first article.

Weiner:

Yeah, that was published in 1948.

Feynman:

They'll probably tell you when it was sent in.

Weiner:

Yeah, I think they even have the article here, and while you're talking I'll look for that.

Feynman:

Yeah. Because I'm trying to remember.

Weiner:

Corbin — you mentioned Burt. In the paper you acknowledge H. C. Corbin.

Feynman:

That's the boy.

Weiner:

And his wife. What role did his wife play? She cooked?

Feynman:

More than cooked. She was very enthusiastic, cooperating with my writing and trying to encourage me.

Weiner:

She wasn't in physics, though.

Feynman:

Actually, she claimed to be at the time, and I half believed — I believed it. And I would explain it to her while I was doing it. I didn't realize it at the time, but I was explaining at an ever and ever more elementary level, and I would explain a great deal to her too. But to Burt I didn't have to explain on such an elementary level. So we discussed it. Anyway, it was just like a foil for the ideas. Also they supplied me with food, and it was very pleasant, and we would go on picnics and so on. They had three kids that were great fun to play with, and I'd play with them all afternoon and work at night.

Weiner:

In the summer?

Feynman:

It was in the summer time, yeah. It was great. I visited them on other occasions after that, but I remember that one. There was a guy named Al Schild around, who was a mathematician. He was someone with whom I could discuss things, too.

Weiner:

At Cornell?

Feynman:

No, he was at Pittsburgh. So that was a kind of a pleasant time — where you have nobody with letters, with telephone calls, with nothing, and you can work hard, and think all day long and write all night long, and so on. And then go on a picnic and relax, and then get back to work. It was just great. So I accomplished something in writing it up.

Weiner:

Here's another name that you mentioned in papers. I don't know if it's premature to bring it up — Ashkin.

Feynman:

That comes a little bit later on another paper. That was very simple. Ashkin was a group member, a member of a group I was in at Los Alamos. He was a member of my group. I was a leader of a group. And I respected him a great deal for his care and accuracy, and when I wrote this paper — it was rather complicated and long — asked him if he would look it over for mistakes. So he was very careful and studied it very hard and corrected the mistakes that he found, and I appreciated it very much. He's a very good man, and I knew he was, and so I sent it to him, as a friend, to look it over.

Weiner:

I can't seem to find the article in Reviews of Modern Physics which would show its date of submission.

Feynman:

OK. Now, somewhere along this time — and I think that I did write this article before this important event which I'm about to describe — somewhere along the line, there are meetings. See, timing is impossible. Leave me alone. Let me just tell you about it. I can't tell you the exact order. But another thing that was happening was that there were meetings of theoretical physicists, of relatively small groups, 20 theoretical physicists who would get together in different parts of the East. The first meeting was on Shelter Island, called the Shelter Island Conference, and there were some theoretical physicists, also Pauli. We were supposed to discuss the theoretical physics problems of the day. The Shelter Island Conference was my first conference with big men, you know, and I was invited there. Bethe was there, Oppenheimer was there, Pauli was there, Breit was there. Everybody of any importance in theoretical physics who was still alive and was around that part of the country was there. Now, where the money came from and who — Oppenheimer had a great deal to do with it, I think, in inviting the people and organizing the thing, somewhere, somehow. Money came from the National something. There was no National Science Foundation then? National something.

Weiner:

National Research Council, perhaps?

Feynman:

I don't think so. National Academy of Science, perhaps, or some private source. It's interesting. We had this conference. The first conference I remember in several ways, because I'd never been to one like that in peacetime. It wasn't like the Physical Society. It was a small group, and we were put in a very pleasant place, Shelter Island. It was a nice place. It was off season, a resort. Incidentally, it was interesting — the first night there we were invited (before we got there) to a restaurant on Long Island, just opposite Shelter Island. We would eat dinner there, and then we would go to Shelter Island. We had been invited by the restaurant owner to have dinner free. The other men were escorted on busses, paid for by the city of New York or something, whereas I for some reason came from a different direction and went by myself by car. Or I made a mistake and didn't meet the bus. When we got to the restaurant we ate free, and the man made a speech thanking us for finishing the war, and telling how it was that his son was in the war, and how he read all the things about how hard it was to take Okinawa and how difficult it was going to be to take Japan, and how he was worried about the whole thing, and how all of a sudden a miracle occurred, that some men had been working all this time with a great idea... He gave a great speech to thank us for stopping the war. And that's why he wanted to give us a dinner for this. So somebody thanked him for the dinner, and so on. I repeat this because I remind you, we were heroes. The city thought it was a good idea to put us in busses and take us out to Shelter Island as a gift. And the man thought it was a good... It's an interesting era. It didn't last very long.

Weiner:

It's new to me. I didn't know that.

Feynman:

Yes. But it's an interesting era; that the physicists will be in the city became an important and exciting thing. I just remembered this, see. So it's an interesting side issue. Then we get to Shelter Island, and we discuss many problems. Now, again, I will have some difficulty in remembering which conference was which. We had altogether at least three, possibly four, separate conferences. This was definitely the first. In this conference we were discussing the various problems, and at this conference (I believe it was at this first conference, possibly at a second conference — the confusion in my mind is very great about the history, but it's easy to find out, so just let me keep going) —

Weiner:

I was going to mention the names of a few of the conferences, if that would help.

Feynman:

I know the names of the conferences.

Weiner:

Was it Ram Island?

Feynman:

That's the Shelter Island Conference. That's the same one. Then there's Oldstone, on the Hudson, Conference, and there's the Pokono Conference, which is another place in the Pocono Mountains, unless Oldstone and Pocono are the same. There were three conferences, I remember.

Weiner:

Pocono is '48.

Feynman:

OK. Anyhow, at one of the conferences, probably the first conference — in fact, almost definitely the first conference — they discussed many of the problems. Ah yes, it gradually comes back to me. I can't figure out which conference! Many of the problems

of the day were discussed: puzzles about what the mu meson is, a suggestion by Marshak of an intermediate meson, a meson produced that disintegrates into mu, which was the pi-meson. A lot of exciting things were suggested and talked about. However, in spite of all this, they ran out of ideas. And they asked me if I wouldn't explain my path integral way of doing quantum mechanics. So I did. Now, I must have been preparing my manuscript, or finishing writing the manuscript, because it was all organized, and if it had been before this time of working, I would have been so disintegrated (I hadn't looked at anything) that I couldn't have done it. So this gives us a certain amount of timing. All right. And I did OK. I explained it. Of course, it's hard to pay attention to some new ideas, and they didn't pay much attention I suppose. Then, at this conference or at a later one (and it's an historical question, it's easy to figure out) some questions about the Lamb shift business were suggested or measured or indicated, or somebody said they were going to measure it, or it had been indicated that there was some shift. And Schwinger claims to have said that he thought it was due to this electromagnetic energy shift that had been coming out infinity up to now. And I tried to estimate it by how much our damped oscillator shifted in its frequency, but I didn't understand the real problem. Schwinger understood it better than I, and got too low a value by very many thousands. But Schwinger said that wasn't the right way to figure it out. I remember this. See, this was up in the meeting that we were talking about it. I would estimate and say, "What's the matter with this? It comes out too small." He says, "No, no" — and so on, and so on. So there was some conference at which we discussed this question. And also there was a conference, but I think it was one later, in which it was indicated that the magnetic moment of the electron was not right, that the magnetic moment of the electron was not exactly the Dirac moment but was slightly corrected, and that this possibly was also an electromagnetic correction. Probably at the first conference, but possibly at the next conference, this went on. OK? So we were aware of the problems, and we were beginning to get, from Rabi's group, some evidence that the magnetic moment was cockeyed, and from somewhere else some evidence that the Lamb shift existed. So that's interesting and important to me, of course. These three conferences — as far as I can remember there were three conferences — were to me of very great interest and importance, and I was very unhappy when they ended. I asked Oppenheimer later why they ended when they ended, and it was, he said, because they were getting big. It was very difficult, after you invited somebody, not to invite him again. But people always had to be new ones because they were doing something experimental, they had reports, something — and it just began to grow. And there were too many insults, everybody was insulted, and everybody was writing, "Why didn't you invite me? Why didn't you invite me?" And he was sick and tired of it, and so he quit this thing. But this was a very important conference. There have been many conferences in the world since, but I've never felt any to be as important as this.

Weiner:

Oppenheimer discussed these conferences in January at the Physical Society meeting. I heard a talk on the meson theory after 30 years. And I asked him if I could tape record

it. I did, and his secretary transcribed it, and he's working with the transcript. Someday we'll get it. He mentions it in sequence, you see. It's a little clarified. He should know.

Feynman:

Yes, he'll know which was which.

Weiner:

There was one — the Pocono Conference —

Feynman:

That was a later conference.

Weiner:

'48, I know that.

Feynman:

Yeah, but there's more that happens in between. OK?

Weiner:

I just wanted to use that as a cut-off.

Feynman:

I'll get there, but I've still got important things to say in between — things that happened in between.

Weiner:

Yeah, I know, it's the most important part.

Feynman:

By the way, by now I'm living in a much more comfortable place. I had been invited to live at the Telluride House at Cornell, which is a group of boys that have been specially selected because of their scholarship, because of their cleverness or whatever it is, to be given free board and lodging and so on, because of their brains. They live at this house, and they like to have a faculty member live there too each year, and they select different

ones, and I was living there. And this was very convenient. The meals were available and everything was available, and you didn't have to worry about all that. I could work in my room, or play with the guys, or work on the place; so it was very good. It's there that I did the fundamental work. At any rate, around this time the Lamb shift measurement came out, probably at one of the conferences, maybe at the Oldstone Conference, maybe at the Shelter Island Conference — 1000 megacycles, roughly, of shift. And people were worrying about it, and I was doing my old-fashioned quantum electromagnetics, backward, upside down, with path integrals and so on. And Bethe began to work on it. (This is like another lecture, but I'll repeat myself.) Bethe's characteristic is to try to get a number. You try to understand any number that's made — or at least, it was his method in those days. At these conferences we also discussed meson problems, but I'm not going to talk about that now.

Weiner:

Not at the moment, but I'd like to ask you about it later. I'm sorry, I don't understand what you said about Bethe.

Feynman:

Bethe characteristically was always very practical. If a number is measured that the theory can't get at, that's the challenge of the theory. He doesn't think like I used to. I think a little bit more philosophically, more like Einstein, about the general problem, you know? How are we going to solve the general problem? What concepts are necessary? Do we need to understand new concepts of space? Tat-tat-tat-tat. With Bethe it's the other way around, from the other end. You've got a measurement, you've got a number — where does that number come from? See? Very practical, down to earth, from the other end really. And so, when he got the number for the Lamb shift, he began to try to figure a way of getting it. And I was in his home at a party, and he was not there. He had been suddenly called away for a while to do some consulting work for GE, when he had already arranged a party. I was at his home, and at his home I got a telephone call from him—he called me at his house — and he said to me that he understands the Lamb shift. He explained the idea about the mass renormalization or something—I can't remember what he explained. I didn't follow it very well. And he said he got 1000 megacycles, and he was very excited, wanted to talk about it. I didn't understand it too well, but I realized from his excitement it was something important, so I said the appropriate things that somebody says when they don't understand what you said but they know that what you just told them is important. Then he came back. And he gave a lecture at Cornell in which he explained in detail how to compute the Lamb shift. He invented the idea, or he worked out the idea of calculating the shift in the level, noticing several things: for instance, that a good deal of the shift would be due to the fact that an electron, even if it's free, has its energy changed because its mass is changed by its self-energy. We certainly don't want to include that in the shift. We must consider the difference of the S, the level in the S state and in the P state. And the energy of the

electron is the same in both. You have to subtract that, so he got rid of that. Then there's a kinetic energy difference because the mass has changed in the formula for kinetic energy, momentum squared over $2M$, and if the mass is changed by the self-energy, that term will be changed. But that was the same in both levels, and he could subtract that. Then finally he gets to a thing which was not the same, which was not the same in the two levels. Then taking the difference, he found that the thing diverged logarithmically when you did integral over all the frequencies of the virtual photons involved. You got a logarithm integral from some minimum energy which is of the order of the binding energy of hydrogen, about one Rydberg up to infinity. And he guessed that, just like the self-energy of the electron which diverges quadratically if you do it non-relativistically, but converges logarithmically if you do it relativistically—that his thing, which was diverging logarithmically when he did it non-relativistically would, when he did it relativistically, converge. Therefore the upper limit should be about Mc^2 , because he was only calculating non-relativistically. So when he put Mc^2 in the numerator and Rydberg in the denominator, he got about 1000 megacycles. So he knew he was on the right track. The only problem remained as to how to deal with the relativistic end exactly — exactly what you do with the upper limit, not just cut it off arbitrarily — you get a rough estimate. So it was a relativistic problem. In this lecture he said that you have to make so many subtractions of such large terms, really infinite terms, that it's very confusing at times exactly what to subtract from what. And he thought that if there were any way whatever to make the theory finite, even though it didn't agree with the experiments, some artificial way of cutting off electrodynamics which was relativistically invariant, then we could cut all these things that were integrately finite, you could subtract them exactly, and it would be very much simpler. Then you could do the relativistic end without ambiguity. Otherwise it's very confusing. So I came down after the lecture and said, "I can do that. I can make a correction in electrodynamics that makes it finite, even if it doesn't agree with the experiment. But I can do that, so we can do all the subtracting." He said, "All right. Fine. How?" I said, "I'll show you in the morning." So I went home, and I looked over the things I had. See, I had this tremendous package of different ways of doing things and cutting off and everything, all aimed at half advanced and half retarded method.

Weiner:

This dated from the Princeton —?

Feynman:

Right. I decided, for this problem that I'll just use the standard electrodynamics to retard it, but put some of my inventions in, various methods of cutting off electrodynamics, representing everything by path integrals, and then cutting the functions in a relativistically invariant way. I knew how to do things to electrodynamics that would leave it relativistically invariant, because I wasn't using a Hamiltonian way but the path integral way of expressing it, and then it was easy to correct it so you didn't destroy

relativity. So I made the corrections. I tried to translate it back into the language that other people use. And then I went in, the next day. I said, "Tell me how to compute the self-energy of the electron and I'll show you how to correct it, so you'll get a finite answer." It's interesting to notice that at this time I still didn't know how to compute the self-energy of an electron. There's a point to doing too much esoteric work. I didn't know how people computed the self-energy of an electron, which is quite stupid. It's a simple second note of perturbation theory. This really shows you that I had gone too far on my own. I'd gotten so far I hadn't looked at simple problems. He showed me how you calculate the self-energy of an electron, and I showed him what the correction ought to be. I tried to translate my principles into the other language that he was explaining this thing in. And we computed it, and it diverged now not logarithmically but as a 6th power of the upper limit, which is much worse, and I was sure it was supposed to converge, you see, so I was quite offended. Then I went home and I worried about it and I worried about it — I don't know how long, days, a couple of days, a week or whatever it was. And I couldn't see anything wrong physically with what I was doing. It must converge; it just had to converge — at least physically. There was something wrong with it. So then I taught myself how to compute the self-energy, and I did it over again, without him, on a piece of paper, and it did converge. And neither of us has ever figured it out. In fact, he even forgets the situation. But I never could figure out what we did on the blackboard, or what happened that made it diverge, because I changed nothing. I didn't change anything; I just adhered to the same rule and the same idea. But it converged. OK. Now I was on the track. Now I could calculate.

Weiner:

You felt sure enough of it to try it over again, to make it come out somehow?

Feynman:

Well, I wasn't sure at the moment, but I went back and thought about it and thought about it, and couldn't get out. I mean, you think about it — you say, "Why doesn't it work?" And you can't see any reason why it doesn't. You keep logically arguing this way, that way, physically. I can't remember the reasoning. But the more I thought about it, the more clearly it got that it had to work. And so, then I finally did teach myself how to calculate the self-energy, what you're supposed to do, and when I did it making the new modification I was proposing it worked. I mean the result was a converging integral, as I had expected. So that's what the situation was. Well, then I was launched. I mean, that was all I had to do. Now I could calculate the difference in energy in a hydrogen atom, the effects of hydrogen atoms, all the effects of electrodynamics, by the method of first cutting it off, and then making the subtractions, as suggested by Bethe — that is, re-expressing everything in terms of the experimental mass of an electron, seeing that the mass of an electron is theoretical mass plus a correction to electrodynamics, the correction being finite in my false electrodynamics. Right? So the experimental mass is written, and the theoretical mass puts a correction. The theoretical mass is not observed,

but the experimental mass is. Then when you calculate anything else, like the energy levels of hydrogen, you express all the answers in the experimental mass of an electron. The mass of an electron is its energy when it's not in an atom, in empty space, and you compute the energy in the atom, and express it in terms of the mass, that you experimentally would measure, which is the way you always do express the answers. Then, when you do it that way, as Bethe expected, the answers were finite. That is to say, depending on my cut-off — I had some kind of a cut-off limit, small distance somewhere, to keep things finite. But when I expressed the answers in terms of the experimental mass, if I kept the experimental mass fixed as I took the limit, as the cut-off went to zero size, the answers would have a definite limit. And that definite limit was the Lamb shift, for instance, for the Lamb experiment, and for other experiments. With one complication, having to do with polarization of a vacuum, which I'll come to, but you seem to be nervous about the tape, which is making me nervous.

Weiner:

It's all right now.

Feynman:

Anyway, I got all the formulas. I got the formulas that Bethe got, in the non-relativistic end, and I got the Lamb shift figured out, and I found out that the magnetic moment of an electron was shifted. Schwinger had already done this. I noticed it was shifted by e^2 over 2π , in proportion. E^2 over hc , and 1 over 2π . And I got the same result. Then, I went to a meeting in New York. Schwinger was there, and he was a great hero, because he'd done all this stuff. He was giving a paper on the subject and said that he had some difficulties — that with the electron free, he got e^2 over 2π for the shift, but with an electron in the atom, it seemed that the answer was two-thirds as much, or one-third as much. I can't remember what. The experiments of Lamb and so on showed that it was e^2 over 2π and, furthermore, he can't understand why it should be two-thirds as much in an atom. So he was having some difficulty as he mentioned in his talk. So I got up and said, "I would like to say that I computed the same thing, and I agree with Professor Schwinger in all his results, but that the magnetic moment of the electron in the atom is also e^2 over 2π , and there is really no difficulty. If you go back and fiddle around some more, you'll see that it's the same magnetic moment in the atom and out of the atom." I really wasn't trying to show off, I was trying to tell him that there's no problem there, that it'll come out right, because I had done the same things that he had done. So I was trying to say that it would come out right. Many people who had never heard of me before had heard of Schwinger before but as I try to remind you (I don't know if I told you this, because I forgot who I talk to) Schwinger was well known. He had done many great things before the War in deuteron theory, with polarized helium, with scattering of neutrons from helium to produce polarized neutrons, and other work. I knew of Schwinger, he didn't know of me. Many people knew of Schwinger that didn't

know of me. So I've heard from people that it sounded very funny to them. The great Schwinger was talking and some little squirt gets up, "I did it too, Daddy, and I don't think you're in trouble at all. Everything's going to be all right." You know?

Weiner:

Was this the first you had heard of his work?

Feynman:

No, not of his work in electrons.

Weiner:

I mean this paper.

Feynman:

No, I had heard that he got e^2 over 2π . When I got to the meeting, I was surprised that he got another value in the atom. I was trying to tell him he's right, it's OK; it's 2π , because I got the same result. That's really what I was doing: "See, I caught up with you, but I see it in the atom and it's OK." What I was trying to tell him was that there's no difficulty.

Weiner:

That was a Physical Society meeting in New York?

Feynman:

Yes, it was a Physical Society meeting. OK. Then I did more problems and more problems, and kept working with it, and so on. There was a problem which kept bothering me, which is called polarization of the vacuum, where a field produces a pair, and a pair annihilates again producing a new field, which in my diagrams would have meant the closed loop. Oh, I had tried to figure out. See, all my theories were non-relativistic theories of matter, because I was using path integrals. But I just had to translate them, operator by operator, quantity by quantity, by trial and error. Everything I did was by trial and error. I would guess from the forms of the expressions. Like there would be a momentum, and I would guess momentum over mass as a velocity; I'd guess that's the velocity operator alpha (Dirac, see?). And I would rewrite expressions by a kind of guesswork. Then I would compare them to what I would get by the more tedious methods and old fashioned methods, or look them up, you see. And so I gradually developed a way of knowing what the right formulas were, relativistically. In

the process, however, the problem of what to do with the pairs always bothered me. I never did understand that negative energy stuff. And I had some trouble. So I began to say, I can't do the pairs this way, it's too confusing to me. And I remembered Wheeler's old idea about the electrons going backwards. So I simply made a project — imagine what would happen if my space time trajectories would be like the letter N in time; they would back up for a while, and then go forward again, which is famously described in one of the papers later. And I found that I got the right formula for the positron end of the Dirac cases. You see, when you have to sum over some intermediate state, some with just electrons, some with pair productions, the ones with pair productions seemed to come right if I let this backwards path go, within a sign. Then I made empirical rules about the sign by doing more and more complicated problems. You must use a minus sign for each reversal or something, you see. So I finally found it wasn't the number of corners that determined the sign, but the number of back sections, you see, or some such thing. And I gradually developed empirical systems for computing everything, so that I knew the rules for myself. And I would always get in the end what everybody else was getting, and I knew what I was doing. What I was doing was presumably OK.

Weiner:

Did you write these down, these rules?

Feynman:

No, I was working with them, and I had them — yes. I was trying to write them down. I would get to a point and say, "With the sign plus or minus...," and then later, "The sign is equal to the number of something or other." Then it wouldn't work. Then I would try again. So essentially I was discovering the right rules by a kind of cut and try scheme, which I've used ever since. At any rate, though, in this view I had about electrons going backwards in time, the idea of a pair production followed by annihilation was a closed loop. It was a ring. The electron went around, forwards and backwards in time. And I felt that it may not exist. See, in Wheeler's original idea about electrons and positrons being back sections of the world lines, the question of whether there was a world line all by itself in a ring in space was opened. I was confused by it. One possibility was, it didn't exist. It gave me infinity. I didn't know how to get rid of it. So that's a certain stage I was in. I could do everything else except the polarization of the vacuum was infinite. That bothered me. I hadn't got that under control when I went to the meeting in Pocono. So that tells you how I was at the meeting in Pocono. I remember the exact situation here. That was historically, to me, of some interest, and I can describe that if you want.

Weiner:

Yes, please.

Feynman:

This meeting at Pocono was very exciting, because there Schwinger was going to tell how he did his things, and I was supposed to tell how I did mine. I was very nervous and didn't sleep well at all. I mean, at many of these meetings I was nervous, I don't know why, but I was very nervous at this meeting, couldn't sleep. It was very exciting. I would talk to Schwinger at the meeting, and we would compare notes, like what do you get for the radiation with scattering, compared—? We agreed, you know. This term comes from here and that term comes from there. It was very interesting about that meeting: nobody really understood either of us, and we didn't understand what the other guy did exactly — how he did it, rather, how he did it. But we would agree on the answer. We could talk to each other. "What did you get for this? What kind of term is that?" "Like this," and you'd start to describe the physical idea that's involved in producing this term. "Oh, well, I got this — did you include that?" And so on, you know. We could talk back and forth.

Weiner:

Was this private, two man?

Feynman:

Private, two man stuff, was great, we were fine. When he went to explain his stuff, he had great difficulty. Every once in a while he would say, "Well, let's look at it physically." As soon as he would say that, the wolves were out, and it was terrible. He had great difficulty explaining, and people didn't understand it, and they were getting more and more tired. Also, the thing that I would best characterize this by — just by how it looks to a young man in front of all these great minds — is a thing that I do also now, I suppose, when I'm in this opposite position: You don't trust that the young fellow really knows what he's doing. So you want to make sure that he does. He has to explain everything to you, and in your own language.

Weiner:

You've got to test them.

Feynman:

Huh? You don't trust them; you've got to test them. He's got to explain everything to you, and in your language, not in his. You don't have the energy to follow the other way, see. And therefore, the difference — the reason why people didn't understand him, and the reason they didn't understand me, and the reason we could talk to each other and make a great thing of it, was that I simply assumed that he knew what he was doing, and if he said that this was where it came and this was what came out, this is what came out. When I would say this was that, he didn't assume that this jerk maybe doesn't know

what he's doing, he would simply say he believed, when I'd say this comes from here, this comes from here. If we don't agree with each other, then we'd try to figure out why, and not assume the other guy is making some kind of a grand error. OK. In other words, we don't assume the other man is making some kind of a gross mistake. Yes? OK.

Anyhow, as a result of this, Bethe said to me, "You'd better explain this thing mathematically and not physically" — he said in conversation, "because look how much trouble *Schwinger* has every time he says anything physical." One of the troubles was that all my thinking was physical, and as I told you, I did everything by cut and try. So I didn't have a mathematical scheme. But I had recently discovered a single mathematical expression, which, if analyzed, all the diagrams and all the rules would come out of. This expression involved, however, a new branch of mathematics that I'd had to invent, which was called ordered operators. And the only way I knew that the formula was right was, when I worked it out it gave the right answers. OK? So I did have a mathematical scheme. It was purely mathematical. But I couldn't substantiate it from the other mathematical schemes known, you know? I couldn't deduce it. But I knew it was right. So I tried mistakenly — I mean it was bad pedagogy so it's nobody's fault but my own — to say: "This is a mathematical formula which I will now show you produces all the results of quantum electrodynamics." "Where does the formula come from?" "Never mind where the formula came from, it works. It's the right formula." "How do you know it's right?" "Because it gives all the right answers." "How do we know it gives the right answers?" "Well, I'll show you. I'll do one problem after another. I'll show you how the formula works." "All right." So I'd start to explain the meaning of the symbols by doing an example, like the self-energy of an electron. They would get bored, because when you go into detail on any particular problem, you're involved in little details, and Bethe said, "Never mind that, explain to us how it works," and so on. OK? "What made you think the formula was right in the first place?" OK. So then I had to go with physical ideas. And then I was much deeper in the soup, because I'd come at them in the wrong order, and everything was chaotic. I started to explain about path integrals and all this stuff. I had discovered a number of tricks. For example, there's a small technical trick — there's a thing called the exclusion principle, which says that you can't have two particles in the same state and all this kind of stuff. It turns out you don't have to pay any attention to that in the perturbation theory in the intermediate states. I had discovered that by empirical rules. If you don't pay any attention, you get the right answer anyway, and it's much easier, and if you pay attention you've got so much thinking about the cases and so on. So I'd try to do something, and they would say, "What about the exclusion principle?" And I would say — like Teller would say, "What about the exclusion principle?" And I would say, "It doesn't make any difference in intermediate states." Teller would say, "How do you know?" I'd say, "I know, I worked from a —" He'd say, "How could it be?" You see? I'd say, I know, and I'd get a lot of things they don't believe. "How could it be?" They would argue against it as fundamentally wrong because of this and this example. I would try to argue, and then we would finally settle down, and people would say, "Well, we'll discuss it later" — you know that kind of trouble. Also, at the beginning when I said I would deal with the electrons, the single electrons — I was thinking of this backwards in time business, like a

single electron, the Schrodinger equation, goes backwards in time — Dirac said, “Is it unitary?” I said, “I’ll explain it to you, you can see how it works, and then you can tell me if it’s unitary.” I didn’t even know what that meant (is it “unitary”). So I went a little further. You know, we’d get into these arguments. Then Dirac would come up, you know, “Is it unitary?” So finally I said: “Is what unitary?” He said, “The matrix which carries you from the present position to the future position.” I said, “I haven’t got any matrix which carries me from the present position to the future position, I go forwards and backwards and forwards in time. So I don’t know.” You see, each one had a kind of a thing in his head, like I know now what it is. Dirac had proven somewhere that since quantum mechanics must be a unitary operator, in a certain sense, there is no unitary way of dealing with an electron, with a single electron. But, because he had been thinking about progressing only forward in time, he couldn’t think of going forward and backward, and here’s this theorem, and he wants to know what’s the matter with this theorem, see? And each guy had an axe of this kind, something they’d proved is impossible, and I’m saying you can go ahead and do it. But there was always one gimmick in my stuff, one extra complication that they weren’t noticing or something like that. So it was very difficult.

Weiner:

And Bohr was at this meeting?

Feynman:

Bohr was at this meeting and somewhere, after I’d tried and tried and I talked about trajectories, then I’d swing back — I was forced back all the time to explain. Finally I go back to the idea of an amplitude for each path; that quantum mechanics can be described by the amplitude for each path, and after that Bohr got up and he said, “Already in 19” — something, 1924, ‘25, or something — “we know that the classical idea of the trajectory in a path is not a legitimate idea in quantum mechanics” and so on. In other words, he’s telling me about the uncertainty principle, you see, and so on. And when I hear this, this was the least discouraging of the criticisms, because it was patently clear that there was no communication, as you like to say. Because he’d tell me that I don’t know the uncertainty principle, and I’m not doing quantum mechanics right. Well, I know I’m doing quantum mechanics right, so there wasn’t any fear or anything. I mean, it was no trouble. It’s just that he didn’t understand at all. And I simply got a feeling of resignation. It’s very simple, I’ll have to publish this and so on, let them read it and study it, because it’s right. I wasn’t unhappy from that, you understand me? From Bohr’s criticism.

Weiner:

Was there antagonism in this criticism?

Feynman:

No. No, only the usual personalities. I mean, Teller, full of excitement, and Dirac mumbling "Is it unitary?" No, there was no trouble. It wasn't antagonism. But to tell a guy that he doesn't know quantum mechanics is to say, you know — It didn't make me angry; it just made me realize he didn't know what I was talking about. And it was hopeless to try to explain it further. And I said so. I gave up. I gave up completely, and I simply decided to publish it, because see, I knew it was OK. First of all, I had the confirmation with Schwinger. We'd sit there and we'd get the right answers, you know. So that was just before the meeting broke up for a little temporary rest, and Bohr came up to me —

Weiner:

How long did the meeting last, by the way?

Feynman:

I don't know maybe an afternoon. And Bohr came up and apologized. His son had told him that he didn't understand it, that I really was consonant with the principles of quantum mechanics. But I said, "It's not necessary to apologize," — you know, something like that. After that, I don't know what I did. I didn't do any more, but just decided to publish it. There is one little thing, though, that's interesting, that also added to the complications. When I got up to talk, I started out by saying, "I can do everything but I can't do the closed loops, the self-energy of the electron, I mean the vacuum polarization." Schwinger got up and said, "I can do everything, including the vacuum polarization." And he worked something out, and he got a term which looked like vacuum polarization. He had to subtract, and it left the vacuum polarization. It later came out that he had not done the vacuum polarization, but he had left it out — he didn't even notice the term — and he had another term that he'd been doing, and was doing it wrong. And it looked like a vacuum polarization correction, the error, which you could get rid of by saying he had vacuum polarization. He got rid of it. Well, I was doing it more right, and didn't have any vacuum polarization term at all, and knew it was missing, and said it was missing — whereas, he thought he had it and included it and got it right. But neither of us knew how to do it. But we didn't know it. He said he did. And I said I didn't do it. So one of the criticisms they gave was, "Why should we bother with this, you haven't done the vacuum polarization yet. And the other thing is all done." So you see, that was another, a small thing. I'm just saying it wasn't something that bothered me. It didn't bother me. I'm just telling the difficulties that people have in paying attention to me. They thought I hadn't as much as he had — actually, I happened to have more but I didn't know it — and so on. I could describe the specific terms, but one time a few weeks later, when I was visiting MIT, Schwinger called me up and said, "According to what I understood from what we were discussing, the terms which you have included give no vacuum polarization term, and that you have this extra thing. Well,

now I found this extra thing. But now what bothers me is that the terms which I thought I had, which were the same as yours — I have a correction, it looks like a charge correction from those terms, and you said you had none. How did you handle them?" So I had to discuss terms on the telephone. We could do it. And I explained to him which terms would cancel what, and he hadn't noticed those. "Oh," he said, "I forgot to put those in." So he put them in, corrected the thing, and got the same result. So, you see, we understood each other. We corrected each other. You know, we each fixed the other up, by pointing things out to each other at the time. So we were cooperating very well. But it was hard for us to know exactly what we were doing, and we would sometimes misrepresent the situation a little bit.

Weiner:

You could even talk about this on the phone?

Feynman:

Even on the phone we could identify the terms, I remember, because we understood what we were doing. We could visualize. I would say, "The term I'm talking about is canceled by a term which comes from a photon which is first emitted before interaction with the nuclear potential, is first emitted and then absorbed before the interaction with the nuclear potential." And he'd say, "But that's just a mass correction." I'd say, "No, because of the fact, the mass correction is when there's a free particle, and because of the fact that a potential is going to act soon, there's a slight correction near the end point of the integral." "Oh, yeah!" You know? So it would go something like that. We could talk on the telephone to each other. We understood each other very well.

Weiner:

This was after the Pocono Conference, this particular phone conversation?

Feynman:

A little bit, yet. Yeah. You want more about the Pocono Conference, or shall I just continue this whole subject a little further?

Weiner:

Whatever you feel is logical.

Feynman:

All right, I'll just finish the subject. I made calculations — had one error in it. A

calculation I made of the Lamb shift had an error in it, and I didn't notice it. It was very subtle, having to do with longitudinal waves and so on. At the same time, in a much more pedestrian, hard-working way, following the logic of Bethe and Weisskopf, who also suggested the same thing as Bethe somehow — I don't know what the Bethe-Weisskopf historical relation is (who did what first) — Bethe would tell me everything and then say that Weisskopf did something, too, of the same logic as Bethe, and I don't know the situation. But anyway, Weisskopf also computed the Lamb shift by following the logic of Bethe, or Bethe and Weisskopf, and calculating also in a much more pedestrian, careful, old-fashioned way, but with very accurate thinking. He did that with a man named French, and he got a different answer than I did, and he called me up on the telephone to tell me about the difference, and the formula he got compared to my formula. I didn't see the difference. I was so sure that his was so complicated and that he must have made a mistake, that I didn't pay enough attention to the possibility that I was making a mistake. And so for a long time, they hesitated to publish their results, because my method was so much more efficient, and it was so much easier to get to the answer, that they were sure that they had made an error. They kept checking and checking and checking, and delayed their publication, which made me unhappy, because they were right.

Weiner:

Meanwhile, had you published?

Feynman:

Now I'm publishing, during this period. Yes, because I know I'm on the right track. I first have to explain the method of cutting off the electrodynamics. So I wrote a paper to explain a method of cutting off electrodynamics in classical electrodynamics, so that the physics of the method was clear.

Weiner:

Just the relativistic cut-off —

Feynman:

In classical electrodynamics, right. Then it was suggested by Rabi, somewhere in one of these meetings, that the thing for me to do was not wait until I got everything perfect, but to put out something explaining what I'm doing. So that's why I put out the classical and quantum — I really put them out almost together. They were within a few — they can't be far apart. They mean to be together. It's just a technical thing of a typist or something that separates them.

Weiner:

One of them ended up at page 946 of Physical Review; the next one starts at page 1430.
(crosstalk)

Feynman:

OK. Anyway, “A Relativistic Cut-off for Quantum Electrodynamics” came next, and that described my idea of how to make the calculation, but with more pedestrian methods, none of the clever tricks of the backwards moving electron — I mean, I don’t think that’s in there yet. I don’t remember. No, it was in there. But not everything, not the smooth methods of doing the integrals, the better notation for everything, and so on. Since I’m doing so many problems, I invent notations; I invent improved techniques and so on. Then I publish this big paper called “Space-Time Approach to Quantum Electrodynamics.” The other paper’s already had the information, but this paper had it much better organized. No, I did that one together with a thing called “The Positron Theory,” “Theory of Positrons,” because that’s part of it, in order to talk about the backwards moving tracks. Then the “Space-Time Approach” — those really went together. I published those, with the most efficient methods of calculation that I knew by this time, which include this business of very much improved techniques for writing and calculating quantities involved. All the rest was just improvement in technique, you see.

Weiner:

Now, in between, I think, you have a paper with Wheeler, Reviews of Modern Physics; the paper is “Classical Electrodynamics in Terms of Direct Interparticle Action.”

Feynman:

That was written by Wheeler, and was done essentially independently. We worked together.

Weiner:

Oh, that’s the one where you talked about work started by you earlier and so forth.

Feynman:

Yes. I think I told you — if I didn’t, I’ll say it again —

Weiner:

Please do, that way it will be clearer.

Feynman:

Wheeler suggested that I write up our work. Did I tell you this stuff? When we did the work together, way back in Princeton, I wrote about the half advanced and half retarded potentials in classical electrodynamics. He asked me to give this lecture, you remember. He also asked me to write up the work, and said, “It shouldn’t take more than 20 pages. Write it nice and neat.” I couldn’t do it in less than 27 pages. I wrote a 27 page exposition. He looked at it, and thought about it, and didn’t like the way it was written, and said the work was much more important, and should be written in much more detail. And he started to work on the writing. It got bigger and bigger and bigger. (I think I said this.) And then he had these sorts of semi-grandiose ideas. He’s going to write a series of five papers, of which this is No. 4, a critique of all of classical and quantum physics, or some such thing. And I was not in philosophical favor with the spirit of this, you see? So when I was writing my relativistic cut-offs and so on, in the “Relativistic Cut-off for Classical Electrodynamics,” I inserted the theory of half advanced and half retarded potentials in there, written in as neat and short a way as I could, as the first publication. I mean, my publication of it is in there, hidden in that paper in which I expound the theory. And that’s all that’s necessary. It’s all been condensed by this time. Of course, over the years I’ve thought of the neatest way of expressing it. So I had it reduced to a very small kernel, which I shoveled into there. And I feel that that’s my publication of that theory, whereas he was in the meantime writing this long thing. It would be interesting, I think I could find somewhere the manuscript of 27 pages, of the original article that I would have presented, if he hadn’t said no to. It’s kind of interesting.

Weiner:

It would be good to compare —

Feynman:

It would be interesting to see how it was written. Anyway, so he was publishing that, and I was just putting my OK to it, or hardly knew it was happening. So I was not involved in that any more.

Weiner:

But these other two, the pair of papers you mentioned, “The Theory of Positrons” and the big paper, “Space-Time Approach to Quantum Electrodynamics” — these were published in 1949?

Feynman:

Yes. There are some delays. See, it took me a long time to write them. It took me an awfully long time to prepare them. They were prepared a considerable time after I knew the technical methods.

Weiner:

Whom were you discussing it with? Dyson mentions a private communication with you. No, he doesn't call it back. He says "of work by Feynman as yet unpublished."

Feynman:

Yeah, I was sending things to Dyson.

Weiner:

How did that come about?

Feynman:

Well, Dyson was at Cornell, and we had had many discussions, and I had been proposing these methods to everybody who was doing any problem in electrodynamics. There were several of Bethe's students trying to work things out, and I was showing them how to do this technique and this method. By this time I had developed the technique. So Dyson was there and he knew what I was doing. But at that time, I had not proved in any way its relationship to the standard electrodynamics — in other words, what was wanted by many people would have been this. Start with the standard formulation of quantum electrodynamics, with operators, creation and annihilation operators and all kinds of theoretic things, and you show that as a consequence of that, these rules and these formulas of mine were right. Then you could be perfectly satisfied. There was only one trouble. The author of the formulas, namely myself, didn't know anything about creation and annihilation operators and all this correct formulation of electrodynamics, because he never learned it. He was therefore considerably too impatient to learn all that stuff which he'd done all this work to avoid, you see, in order to prove to people who happened to have learned that, that they don't need it, that the rules are easier, and I felt very strongly that everything was simpler than the regular formulation, because even Schwinger was working things out and would write a whole lot of stuff before he got to a point that I could write down immediately with my diagrams.

Weiner:

Now, I'm sorry to interrupt, but the diagrams — somewhere along the line we got you into the midst of using them without telling when you first wrote them, or if you did I don't remember.

Feynman:

I can't tell you when I first wrote them. I can't tell you when I first wrote them, because I had been doing it — I'd have to look through papers to find the piece of paper which has one on. It would be a very interesting and amusing problem. I had been doing these path integral ways of doing electrodynamics for so long, and thinking about it so long, I was visualizing in space and time these things, and had worked out perturbation expressions many times, on many pieces of paper. And I probably made diagrams to help me think about it. And I finally didn't do half advanced and half retarded, but the full retarded and all that stuff. It was probably not any specific invention but just a sort of shorthand with which I was helping myself think, which gradually developed into specific rules for some diagrams. I think. I can't tell you.

Weiner:

For helping you think physically? In other words, you were seeing in physical —

Feynman:

No, mathematical expressions. Mathematical expressions. A diagram to help write down the mathematical expressions.

Weiner:

And by making a line on a piece of paper, you could write the mathematical —

Feynman:

— Yes — well, I was seeing something in space and time.

Weiner:

That's what I was getting at.

Feynman:

Yes. I was seeing something in space and time. There were quantities associated with points in space and time, and I would see electrons going along, scattered at this point, then it goes over here, scatters at this point, so I'd make little pictures of it going. That's what those things were. Emits a photon, the photon goes over here —

Weiner:

— That's what I was getting at, you'd say something physical in —

Feynman:

— yeah, semi-physical, associated with the mathematics, yeah. (crosstalk)

Weiner:

Wasn't this sort of a natural —

Feynman:

It was natural to me. Now, I can't tell — we'd have to look back over it — when such drawings began. I think they gradually evolved. I think you'll find very old pictures, similar but not as clean as the final diagrams, and they gradually evolved into a kind of shorthand of diagrams. I think.

Weiner:

I think the important thing here might be, when they began to become very important to the work, rather than just an accompaniment —

Feynman:

Well, when I was doing more and more problems. It was just when I was doing these many problems. Instead of thinking so abstractly, then I would use them more and more. And there was a time, definitely, when I was at this Telluride House at Cornell, and I was working on the self-energy of the electron, and I was making a lot of these pictures to visualize the various terms, and thinking about the various terms, that a moment occurred — I remember distinctly — when I looked at these, and they looked very funny to me. They were funny-looking pictures. And I did think consciously: "Wouldn't it be funny if this really turns out to be useful, and the Physical Review would be all full of these funny-looking pictures? It would look very amusing." And it turned out that in fact, it came out. But I was conscious at the moment that it might be useful, and if it were it would be very amusing to see these funny-looking pictures in the Physical Review.

Weiner:

Talking about the important work in this period, when did you get the sense that you really had something?

Feynman:

Well, I knew when I got Schwinger's formulas and results, like e^2 over 2 pi and so on, that everything was all right. If I got the energy to come out finite, and then when I calculated the Lamb shift I got the right results, the same as Bethe, and I got the magnetic moment like Schwinger, everything was all right. All I had was the damn vacuum polarization. And finally Bethe told me what to do about that. He told me of a suggestion either made by him or by Pauli — I'm not very good at references, but Bethe told me of a suggestion, probably due to Pauli, or maybe by Bethe and Pauli — on how to straighten out the vacuum polarization thing, and I did that. And that straightened that out. And then there were no more problems. It was only technical invention. But you may ask, when did I think that my inventions were highly efficient? That was interesting, because — well, there were several other things. I have a lot of other things to tell you. There were two things I have to describe. They go together, so I can describe them together. The first is, using these diagrams and these rules, I had only done electrodynamics. In the meantime, the world is full of meson theory. Meson theory in those days was made out of analogy electrodynamics. It still is. By Yukawa. If the analogy was good, one of the troubles was that it had all the difficulties of electrodynamics. It gave infinities, you couldn't make good calculations. Now, we got electrodynamics cured, obviously the thing to do is to start figuring things out according to Yukawa's theory, with the new way of doing field theory or electrodynamics, but by analogy. But as usual, I paid no attention to the literature and never knew anything about what was going on. But I heard words — scalar meson theory, pseudo-scalar meson theory, with pseudo-vector coupling, something vector mesons, all kinds of talk. So I figured, well, it's an analogy to electrodynamics, and I kind of understood the analogy from looking at books and so on. I think I never had Wenzel's book, but I'm not sure. So I knew vaguely or pretty close to the general idea, but mind you, I didn't want to translate into field theory. But I would just guess. After all, if they make an analogy to electrodynamics, I too can make an analogy to electrodynamics. So I would invent meson theories, which, see, I had no way of proving. There was no use finding out what their field theory expression was and then convert it, because I couldn't do that with electricity. So I'd have to understand a little bit the idea of it, and go right into my own formulations and diagrams and rules. So I had to invent the rules corresponding to meson theory. It was not difficult. I did it by the same super method, which is empirical guesswork. I would guess. If they said vector mesons, it must be like light, light's a vector; the only difference is that the meson has a mass. So probably the propagator for the photon, instead of being a solution of d'Alambertian of the wave function is zero, is the solution of d'Alambertian of the wave function minus M^2 , is zero, so that would mean, no doubt, that the propagator, instead of being $1/Q^2$, would be $1/(Q^2 - M^2)$, and so on. I would guess, from the idea, I would guess if it wasn't vector but rather scalar mesons, it probably means that the coupling, instead of γ^μ to the 4 vector, is one, the scalar operator, and so on. And so I would guess at the meson theory, sometimes correctly and sometimes incorrectly. I had a slight error—it was my vector mesons. I would guess. Then I would learn about the error from some problem, and then I would fix the thing, because I'd look at somebody

else's formula and I'd say, "My God, it's more complicated," so and so, and then I would see what it was that I left out, and so on. Through backing and hauling I got everything. But I wasn't sure of anything. I would guess what was meant by the theory. Well, one time — this is fun, and I'll tell the story exactly as I remember it, although it's against certain people, and so on. But we said we weren't going to worry about that, OK? I went to a Physical Society meeting, and I came a little late or something. Somebody came running up to me: "What do you think about the discussion between Slotnik and Oppenheimer?" So I said, "What discussion, Slotnik and Oppenheimer?" It turns out Slotnik had given a paper on the interaction of electron and neutrino. Electron and the neutron, I mean. There had been some measurements of the scattering of electrons by neutrons in those days, very accurate, so that people were interested in calculating the neutron-electron interaction — the scattering of the electrons by neutrons — due to meson field corrections... So he had used pseudo-scalar mesons with two versions of the theory. Nobody knew which was right—one called pseudo-scalar coupling, and the other called pseudo-vector coupling. And in those days, everybody did everything by perturbation theory, or nearly. Anyway, this perturbation theory is the kind of theory I was doing, with pseudo-scalar coupling and pseudo-vector coupling. This is what they told me, see, and that he said he got a different answer, that one of them converged and the other one diverged. And after he gave his paper, Oppenheimer got up and said, "Well, what about Case's theorem?" So poor Slotnik said, "I never heard of Case's theorem." (This is what I heard, anyway, see.) So Oppenheimer says, "Well, Case is going to give a theorem tomorrow at this thing which proves that the two versions have to give the same result." So I think — great opportunity! I didn't say anything, but I think, "Great opportunity. I'll go home and I'll calculate the interaction of electrons and neutrons tonight with the two versions of the theory." OK? Actually, I only computed the difference between the two. We could have done the two versions of the theory and computed the difference. I found out in fact that there was a difference, that one diverged and the other converged, just like Slotnik had said according to the report. So the next day I get to the meeting, and I say, "Hey, Slotnik." I met him and I say, "I heard about the discussion yesterday, and I also worked out the two things, the interaction of electron and neutrons last night, and I get the same result. I mean, one diverges, one converges. But I want to compare with you and see if I get exactly the same result. I'm not sure that I'm using what's really the pseudo-scalar meson. I don't know what those theories are so I want to check, get an example." I used to always do this. I'd check my calculation against what somebody would do to find out if I was using the right theory, to find out if I had the right idea of what the theory was. So I asked him if he would check. So he got upset, and he says to me, "What do you mean you did it last night? It took me — I don't know, six months or something," or two years, probably six months — "working with Pauli, working for Pauli, as a thesis, six months." "Well," I said, "never mind," because I didn't want to explain — "How'd you do it overnight?" "Never mind," I said. "Let's see the result. Let's compare results." So I opened my paper, a little envelope with the two answers to the two cases or whatever it was on it, and he looks at his formulas, and he opens it. He looks at mine. He says, "What is that Q?" I had a complicated expression for the answer, you know, like "inverse tangent of Q over Q

minus one plus" — something, and so on. And all he has is a number. You know a simple expression. So he says, "What's that Q in there?" "Oh, I say, "That's the momentum transfer. That depends on how much the deflection of the electron is." "What?" he says. "You did it for every deflection of the electron? I've only done it for the case of zero deflection for forward scattering, where Q equals zero." "Well," I said, "that's nothing to that. We'll just put Q equals zero in my expression, and see whether it agrees." We put Q equals zero, and it agreed. But that is when I knew that the methods I had invented were very, very useful, because he had worked six months to do a special case, for Q equals zero, and I had a complete function. You know how much more elaborate it is to find a function than to find a value at the origin? And he could only have found a value at the origin. To find the value as a function of Q would have been so much work for him. Six months for one point — it's too complicated to find the function. I kind of made Slotnik unhappy, but it was a great moment for me, because I knew that the schemes, the calculation schemes, were good. Actually, I had a little fun out of it, too, because the next day I was at the meeting, and Case gave his paper, you see, in which he proved in a theorem that the two answers have to give the same result. Now, I knew they didn't give the same result. Well, I agreed with Slotnik, and Slotnik had done it by hand, and that's much better than any theorem. So anyway, he gives his theorem and proves that they must give the same result. Of course, you can't follow the proof in a 15 minute paper with all the operators and all. So at the end of his paper, I jump up and I say, "What about Slotnik's calculation?"

Weiner:

Just as Oppenheimer —

Feynman:

Yes, you see, because Oppenheimer — backwards. I did it the other way. Just for the hell of it. Everybody kind of laughed.

Weiner:

Do you remember when? Was it '49?

Feynman:

I don't remember. That's in the Bulletin of the Physical Society. Now, this brings us to another problem, the problem of proving the connection. So I was struggling gradually to learn. I mean, I had to learn something to prove the connection between my thing and the same thing. Dyson had done a great deal in that direction. That didn't satisfy me because I couldn't follow that. Dyson told me, when he wrote his paper, "Don't bother to read it, there's nothing in it that you don't know, except that it proves it's the same as what everybody else knows, but it doesn't say anything different or do anything different

than is in your paper. Nothing more in it," he told me.

Weiner:

How did he tell you this? Personally?

Feynman:

Personally, at lunch, in the cafeteria. He must have visited.

Weiner:

He was at Cornell —

Feynman:

Yeah, because I remember him telling me not to worry about the paper. It hadn't anything in it, you see. I said, "Why do you call it drawing a graph? Is this a mathematical term that means a lot of lines taken by points? A lot of lines taken by points is just a diagram." But they've been called Dyson graphs, as a result of the word graph which he likes to use to describe a picture. Anyhow, I teased him about the formality of his paper. But then I thought I had to understand the connection, for publication purposes and others. And I had a good opportunity, because Case sent me his theorem — the manuscript of a big paper that he was going to publish in the Physical Review, which had all the steps of the theorem. Now, I argued in the meantime with myself, in my usual physical way of arguing, and concluded for several physical reasons, by some examples and other things — simpler examples that weren't so elaborate as the calculations I made — that it couldn't be true that the two methods would give the same result. In first order of perturbation they would give the same result. That was clear to everybody, and I proved it in a second. But a higher order, they don't. And this neutron problem was already second order, so they didn't. So I gave several physical arguments to myself that they couldn't be the same. So I wrote Case a letter back. I prepared a letter in which I wrote the physical arguments. Then I decided, that isn't going to convince him. Nobody pays any attention to physical arguments, no matter how good they are. I've got to find a mistake in the proof. But the proof has creation and annihilation operators and all kinds of stuff. So I went to some students, in particular Mr. Scalator who was only fair, but he understood. He had learned in a pedestrian way what it all meant, and he explained to me what the symbols meant. So I learned like a little child what all this was about, so I understood what the symbols that he was using in the paper meant, and I tried to follow the proof, and I learned enough to be able to do that kind of mathematics, see — for the first time. So I followed the whole thing through, and I found a mistake, a very simple algebraic error, in the proof. He commuted some things that didn't commute and so on.

Weiner:

This was in the manuscript —

Feynman:

In the manuscript of Case's paper. So I wrote him a letter back, explained the physical arguments, and finally said (super-modest) that I don't know this stuff, I have just learned this business about the operators, but I think (I knew damn well) that this thing that's supposed to commute doesn't commute, and that's probably where the error is, see? And I sent it back. The result of that was that I now could find a way of proving my own stuff in a more conventional manner.

Weiner:

Did Case publish this with a modification or anything?

Feynman:

Yeah, he published the — Well, this isn't very nice of me to say, but it's true as far as I know and I don't think it was very good of him — he published the paper with the correction. But now the theorem didn't prove what he had claimed to prove in a published letter of his own. Understand? He gave a paper in the Physical Society. There had been discussion about it, as to whether it's right. And it's published as a little item in which he claims to prove something. Now he doesn't prove it anymore, because of the mistake. So they say, "What a good paper, only proves it in the first order, that the two things are right," something which could be done in one line by my new methods, and by other people also in 3 lines or 10 lines. An elaborate paper that proves that these two things are right only in the first order, and says so, but doesn't say "this is not what I claimed to have proved in the previous paper." You understand what I mean? I felt that it was wrong not to point out that the thing that had previously been discussed in the paper at the meeting was cockeyed. (You might check whether I remember right or I'm just prejudiced.)

Weiner:

This was published in the Physical Review?

Feynman:

Yes. You can find out whether or not. It's curious, because maybe in prejudice my mind has got it distorted. He also wrote at the end that he thanks Professor Feynman for correcting an error in the manuscript. And I thought that was rather amusing to me, because it is not like an error in a manuscript. An ordinary error in a manuscript is just to

improve the typographical errors. But when you change the manuscript so that the whole content of the proof is altered — that's a little amusing, to call it a correction. But that's what I remember. You can look it up and see.

Weiner:

Presumably a paper like that would be refereed —

Feynman:

Yes, it was refereed. Probably people in the Institute of Advanced Studies or something. I don't think he had sent it in before I found the correction. But he may have. He may have.

Weiner:

What I'm getting at is that it's possible a referee could point out, "This man published so and so, and this seems to be different than what he'd published before —"

Feynman:

No, that may not — I don't know. Maybe. I don't know. Oh, I see. I thought you meant the referee would find that error in the proof. That's unlikely, because it takes an awful lot of work.

Weiner:

No, no, I don't mean that, but to compare it with the other —

Feynman:

Well, I don't know. Maybe I remember it wrong, but I think I remember it right. It would be interesting to look at it again just to make sure, because sometimes I've looked at things that I thought for many years and it isn't exactly the way I remember it. Usually it's built up a little bit, the story, see? But I don't know.

Weiner:

Let me ask you about publications. Did you feel, in this period, a pressure to publish?

Feynman:

Yes. I felt the pressure to publish because I had invented these methods. I knew, for

example, that I could do Slotnik's problem in such a short time, and therefore I knew I had something that was valuable to other people. I was concerned about the fact that I had not really solved the problems of quantum electrodynamics. The correction which I had proposed, the temporary correction, didn't leave the theory physically possible, because energies would be complex or probabilities would not add up to one. In a limit it might be all right, but I couldn't prove it. So I was trying to find a way of correcting it so that the corrected theory, the modified theory, was also physically consistent. And I couldn't find it. But I thought it was going to be easy, and I was waiting to find it. But I got a considerable pressure, partly from people who wanted to use it. They heard the method was good, see, from various rumors. And Bethe had given some lectures on it. I had taught it to him. I had gone away to Albuquerque for one summer — and I can't get the date for you right away — during this period, when I was improving the methods of calculation. And Bethe wrote me a letter in which he had made a calculation of something using what he thought was my method. He kind of woke up, you see. I'd been trying to tell it to him, and he went away to England to give some lectures, and out of my influence he tried to reconstruct it. He wrote me a letter and said, "I've got everything agreeing alright but I get the wrong answer." I showed him a small mistake, just a small error in his calculation. He had the right idea, he just made a subtraction for an addition or something, and I corrected it and sent it back. He said, "Dear Professor" — he wrote to me, you know — "your student..." And then I sent it back, the correction, you see —" You were a very good student, you've learned it in principle, and this minor error doesn't really matter." Then he wrote back, "I guess I flunk," and so on.

Weiner:

Did you save these letters?

Feynman:

I might be able to find them. That's the way he learned to do it, and he would then teach. He was explaining to people in England, and he wrote me, "Everybody wants to know how to do it. You must write it up and so on. And so for various reasons I wrote it up, without the proof that it was equivalent to anybody else's, but with as much heuristic argument that it's right as I could muster, to make sure that people could believe it. And I argued I made an excuse, that it's better to publish it without the proof because the proof is more complicated than the result, and this is the essence of the thing. Now, during and after this period of time, people were still following the Schwinger method of representing the thing, and I could see papers in which they would start to formulate some problem, and they would formulate it with operators and they'd write a "Schwingerian method," going zing zing zing, and after about 5, 6, 7 pages full of mathematical symbols in the Physical Review, they'd come to a little expression, short and neat, for the problem, which is an expression which, if you had the diagram thing, you'd write down at the beginning of the paper. Then they would compute the answer from the expression. So I knew that sooner or later, something's gotta go. You see, what

they would do is, they would come to this short expression. Then they would say, "This is precisely the expression that one would write down using Feynman's rules." Then they would go on. But nobody had the guts to write down the expression in the beginning. They somehow or other couldn't do it. They had to go through this to believe it. But that's all right. The only person who didn't, the first paper where it was used directly — which I kept looking for, I kept flipping through the Physical Review as it came out — was Ashkin. He'd done some calculation for some experiment, and he said, "We've calculated this using Feynman's rules." Bloop! There it was in writing! Then gradually more and more people did it.

Weiner:

When was that, do you remember?

Feynman:

No.

Weiner:

But it was the Physical Review?

Feynman:

Yeah, it was in the Physical Review. That must have been at least, almost a year after I published my paper, I think. There was a long period in which it was still a miracle — you know? People just didn't have the guts to do it that way, because they felt that they had to verify that that was right first. See, Schwinger had the one advantage that he had demonstrated that his things were equivalent to the standard thing directly. I hadn't.

Weiner:

Let me ask you a question about publication and so forth. Did you or do you have feelings about priority in any of your work?

Feynman:

No.

Weiner:

Feelings of the need to establish your own priority, as opposed to someone else.

Feynman:

No.

Weiner:

Were you conscious of this in other people though, whether other people felt this?

Feynman:

Yes. I think so.

Weiner:

In what way?

Feynman:

I think, like Case was worried about his reputation with this theorem. But I'm not sure. I would say more of them — I don't know about other people but I didn't worry too much about that. No, I wasn't worried about people using a thing, if I invented it. I was pretty sure it would come out. For example, I think that many people think, or thought, that many of the things that Dyson said, he'd invented, whereas (they were) directly mine. So if there was any jealousy involved, it would be of Dyson, that he published this paper which explained all my results before I did, so that there became a lot of lore by high class theorists about Dyson graphs and Dyson's method. And neither the method nor the graphs were Dyson's, and Dyson said so. Dyson's a friend of mine, and I understood the misunderstanding, if you know what I mean. It wasn't that he was trying to steal something from me, he was trying to tell the world that there was something good here, and he had discovered the connection, that the two things were equivalent.

Weiner:

And therefore made it understandable.

Feynman:

It helped to make it understandable to people. And he wrote some kind of paper in some kind of crazy language I couldn't understand, that they could understand. It was like translating it. It's sometimes a mistake to translate it for the author, that's all. It bothered me only slightly. It was nothing that I was really concerned with, ever. I wouldn't like it if today the things were still called Dyson graphs. That would be me — not miserable, but I would complain to you slightly on that score.

Weiner:

How long did it go on? Did that have much currency — this term, Dyson graphs?

Feynman:

Yes. Then it became Dyson-Feynman graphs, with other people calling it Feynman graphs, you see. Probably, through people who knew the story better, it was more and more used — and now, at last, it has come to be, “We write down the diagram for this process.” And that’s the best. That’s the best, because then you’ve become anonymous. “The diagram” makes you feel even better than “Feynman’s diagram.” It’s that “the.” “The” rule for this. That’s much better.

Weiner:

This was already in the ‘50s when they started talking about Dyson-Feynman graphs or diagrams. When did they start talking about Feynman graphs as such? When did you begin feeling this was common

Feynman:

You just asked me — what?

Weiner:

Oh, when the Feynman diagrams came into common use?

Feynman:

Oh, it was first by Ashkin. Then, from then on, it was just a gradual increase. Like even people who would write the Schwinger thing, they would say, for example, “This expression could be obtained directly from Feynman by the following diagram.” They might make a diagram, even. Then they gradually used less and less of the other, and there were more and more papers using the diagram, and it was just — I can’t recount the story. It just gradually came into use.

Weiner:

How about Tomonaga’s work? When did you first hear of it?

Feynman:

I don’t know when I first heard of it. The work itself, I never knew exactly what it was,

and I don't yet know precisely what it was.

Weiner:

You read his paper?

Feynman:

No.

Weiner:

I mean, there's one paper that is often cited —

Feynman:

No. No. I don't think I read the paper. But this must be understood — I don't mean anything disparaging. If Schwinger hadn't been in the front yard at Pocono, or next to me, I wouldn't have known what he did either. I got the same as everybody else. If you can do it yourself, why learn how somebody else does it? So I don't know precisely what the relation of Tomonaga's and Schwinger's work is or the relation of his and mine. I think the relation of Tomonaga's work to my work is very small. I mean, I think he's gone around much closer the direction that Schwinger went.

Weiner:

I think it's the general impression.

Feynman:

But I don't know the precise relationship of their work. But I believe, if I'm not mistaken, although you'll have to ask Schwinger, that everything that Schwinger did he did without knowledge of what Tomonaga did. I hear, but I don't know, that Tomonaga did a very great deal, and did essentially what Schwinger did, except perhaps for working on certain practical problems. I don't know. That's what I hear. But I don't know. I'm sorry, that sounds stupid, but I have never looked into it, and I never read Schwinger's paper in a comprehensible way. I don't know what's in that paper of Schwinger's.

Weiner:

Haven't tried to read it?

Feynman:

Never. Tried in the sense that I looked at it and I flipped the pages, because it's too hard. I read it at a time when I didn't even know what a creation-annihilation operator was. I read it — you probably can prove that by the fact that I refer to it in various places, and get certain formulas out of it — I read it in the same way that I talk to him. When something looks like something, I know that's it, you know? But I didn't follow all the steps. I never followed all the steps.

Weiner:

But you did know, when you talked to him at Pocono, and then —

Feynman:

I know Schwinger — that's what I say, I must have read it in pieces and bits. I know what Schwinger did; I know more or less how he did it.

Weiner:

And you knew while it was being done?

Feynman:

No, not while it was being done, I never knew what it was. Well, something.

Weiner:

— but in '48, because you were —

Feynman:

Yes, because we talked together, we had the physical idea of what starts it, but there's a difference from that and checking all the equations, and I doubt that Schwinger had sat down and looked carefully at my appendix, for example, which explains how to do the integrals. Why does he have to, when he can do the integrals himself? How to get a term by writing down a diagram — that he may never have paid attention to because he could get the term by doing something else. So I don't know whether he really read mine in detail or not. But he knows what's in it, and I know what's in his, but I can't tell you. Perhaps if I look at his paper carefully, I can see that I really did read it, you know? I mean, I'd have to have it and look at it and see if I did read it. That's a good way to look. I doubt that I read it in detail. I doubt that I looked at all of the various complicated sub-things that he had to worry about, like what to do with the longitudinal waves — because I don't think there's any problem with the longitudinal waves. I couldn't pay attention to such a thing, see? So I doubt that I've ever read the paper in any careful way

like a student would try to learn it. I don't believe I've ever done that.

Weiner:

Let me get back to '49. Tell me if this is the beginning of something new. It's the first paper on nuclear physics, "Equations of State of Elements Based on the Generalized Fermi-Thomas Theory," Metropolis and Teller. You did this in '49.

Feynman:

Well, we published it in '49. We did it in '44 or '45, during the war, at Los Alamos. Now, that was part of the war. In the war we needed to know the compressibility of substances under very high pressure, because in the bomb, when stuff went off, it would explode and push into material on the outside. So the pressures were much higher than had ever been made before. So the question is equation of state, which means if you compress to a certain pressure, what density does it get to? How dense does it get? And how hot, and so on. We had no experience to go by. They had very high pressures. You don't have to worry about the details of the electrons in the shells. We used an approximate model called the Fermi-Thomas model of the atom, which is valid—when the outer shells are broken down, when the pressure gets so high that the number of conduction electrons is very high. Not a high fraction, but all the valence electrons are conduction electrons. You don't have to worry about S band, P band and some others that are not conducting and so on. They're all very well conducting, like the outer shell plus a bit of the next shell is all in the conduction band. Then the detailed structures of the orbits and so on of the various levels don't have to be worried about so much. You can use statistical methods. So at the high pressure we had this, and we calculated this stuff — high pressure behaviors of materials. We extrapolated that back, and tried to match it against the low pressure — things like knowledge of the center of the earth, and work by Bridgeman. So we kind of drew lines between them. We'd calculate everything about the atomic bomb, using an interpolated line, but the other end of that line was calculated by theory. I have recently found that that theory is still used, that it turned out to be very useful. War work sometimes proves useful. The properties of materials at high pressures, such as in the center of Jupiter and other situations where people may need it, is worked out by just taking it out of that paper. A lot of other work has been done on top of that paper, but I haven't followed it.

Weiner:

That was essentially not any change. The only reason I brought it up at this point —

Feynman:

— no change —

Weiner:

— was that it comes in this period. (crosstalk)

Feynman:

It's become declassified. I was again not involved. Teller and Metropolis presumably were involved. An amusing story about that paper — I was stuck in Reno and needed money. I'd run out of money. I was taking a trip. It's very hard to cash a check in Reno, Nevada, because everyone thinks you're a crook. Who will cash a bad check, you know? I was looking pretty disreputable, because I like to travel in a car that way. I was coming from the East. I used to take vacations by traveling across the United States. And I had run out of money. I was on my way to some meeting in Seattle. So I went to the Physics Department, the University of Nevada, figuring there'd be somebody there in physics who'd heard of my name — this was after 1949. I'd published these papers, and they would do me the favor of countersigning or telling the bank it was OK. So I went there. I couldn't find anybody. First, it was summertime, and second, I went around, and it turned out that everybody's in mining. They call it physics, but it was geophysics, and all the guys I could find there had never heard of me, because, you know, they were in geophysics. Finally, the head of the department, temporarily, was an astronomy man. There was a man giving a lecture in physics in the Department of Astronomy, whose name unfortunately I don't remember. The poor man is dead now. And he came out. I waited till he came out of the classroom, and I started to approach him. He was a nice man, but a little worried about this funny-looking guy who claimed to be a physicist, and he'd never heard of me either. But he was kind enough to say it would be all right. So I took him to the bank, and they checked it from his account, by my writing a check to him, you see, and then giving me cash. When we got to the bank, I made it for a little bit more than I had told him I was going to make it before. He said that was all right. Then as he was cashing the check I told him I had run out of money because I had met an old girlfriend who was a chorus girl in the States Hotel, and that sounded bad to him. I did it purposely. I was very mean. So I got him nervous. So he said, "Perhaps we had better go back to the office and see if we can establish who you are better." He wanted to go back to the office to check some more, because he became nervous, so I went back to the office with him. I thought one way would be to find a reference to me in the Physical Review — you know a reference to me, in some paper. I was sure I could find it, so I picked up the paper and said, "Look, I'll find one." I picked up the Physical Review, and I kept looking in the footnotes — footnotes all over the place. I couldn't find anybody referring to me in the whole damn thing. I was flipping the pages very nervously, and I threw the thing down in some despair. And it was open at a thing in which I was the author. The paper by Teller, Metropolis and Feynman had just been published, and I didn't even know it was published. Metropolis probably had written it up and sent it in. It was very amusing — here I had a paper in the Physical Review, and I was looking for a reference instead! So I showed him the paper. He was much happier, content, and not so nervous, and let me go.

Weiner:

Anyway, that's a long way from Cornell, to the Seattle meeting.

Feynman:

I think there was something going on in Seattle at that time.

Weiner:

I didn't mean to distract you —

Feynman:

As a matter of fact, Reno was en route from Las Vegas to Seattle. I went straight to Las Vegas.

Weiner:

That's why you had no money.

Feynman:

Yes. And then to Reno, and then to Seattle.

Weiner:

Sort of a roundabout way, isn't it? I didn't mean to distract you from the sequence of papers and work in this very productive period in the '40s —

Feynman:

I think we're done.

Weiner:

That's what I wanted to know, whether you think you've covered everything. If you've told the story on that, or at least told as much of it as you can remember now —

Feynman:

I only remember that it took me a long time to write the papers, and also that at that

time I started some calculations on some problems, like the Compton Effect and so on with Laurie Brown. We wrote something about the Compton Effect, the second order Compton Effect. In other words, I wanted to use the methods. Students who were trying to get PhD's then were given problems for which this method could help. But it also helped a guy named Lomanitz on a problem that he had. In other words, they began to apply the methods to some problems, and some guys would get degrees. Another guy is Robert Frank, who was calculating the fourth order mass correction of the electron, in which I believe he made an error, but we didn't know it at the time. And so on. In other words, there were some graduate students. Also Lord Thompson, who was calculating magnetic moments and meson theory. I showed him with these diagrams how to make the calculation.

Weiner:

Were you using them in your lectures, the diagrams?

Feynman:

No, no. No. The subjects that I was teaching at the time probably weren't advanced enough.

Weiner:

How were you getting along with the teaching? You talked about your first course.

Feynman:

I was all right at the teaching. I didn't have any trouble.

Weiner:

So, in other words, were your lectures — were they lectures, first of all?

Feynman:

Yeah. Well, lectures with problems, and kids would give them, and hand them in, I'd correct them and so on. I worked hard at teaching in the beginning, and they were OK. I think the students were satisfied. Later, you give the same course ever again, and you don't work so hard if you don't reorganize it. I got more and more careless about teaching. And if I teach something I've taught before, it's not any longer a good course, because I borrow so much stuff from before; and I'm so lazy about correcting papers and preparing the courses, I don't think they're any good any more. I think I'm getting less and less careful as a teacher, relatively. I mean, I'm still useful, but I think I used to

be good, really good, relatively. And now I'm lazy. I don't do enough work in preparing the classes. But then I did a lot of work and took it very seriously, thinking exactly what I was going to say and how to explain things.

Weiner:

You got some satisfaction —

Feynman:

Well, the same way, when I prepared these Feynman Lectures on Physics I did a lot of work, and I was perfectly satisfied with that aspect of it. But with graduate courses now, I've given every subject so many times that instead of giving a new and fresh, modern, up to the last minute course in it, I just take some of the old junk and say it again. Like it looked to me all professors were doing when I was a young man.

Weiner:

What about your satisfaction in teaching?

Feynman:

It's therefore less.

Weiner:

Now, at Cornell —

Feynman:

Anyway, then I think I was still giving good courses. I was even when I got here, for a while. But it's gradually deteriorated. It's easy to fix, if I only do the work.

Weiner:

Do you feel, then, it would take you away from other work?

Feynman:

No. I hope to do the work on the next course I'm going to teach.

Weiner:

You mean, in the fall?

Feynman:

Yes, because I'm going to have to re-cook the whole course. See, if I have to make a new course, I'm all right. It's when I'm teaching something I've taught four times before, same subject, that it loses its —

Weiner:

— that's understandable.

Feynman:

Incidentally, shall I talk about teaching?

Weiner:

That's something that must be included.

Feynman:

Well, when? I mean, shall we talk about it now?

Weiner:

If you think it's the logical time, unless you think that at Cornell —

Feynman:

Why don't we turn that thing off and think about it, so we don't waste tape arguing, you know? Then we'll see. First, I noticed another paper called "Operator Calculus with Applications to Quantum Electrodynamics," which was published in 1951. Now, what that thing is, is this. I had invented a new mathematical method of dealing with operators, with ordering the operators according to a parameter which I to this day feel is a great invention, and which nobody uses for anything, and which nobody pays any attention to, but I just take this opportunity to say that I think that that thing is someday going to be — I mean, maybe in history you'll find out that the guy knew it was good or thought it was good and it never was good, or whatever, but I still think it's an important invention, a very important invention. But I haven't found many uses for it.

Nevertheless, in spite of that — and I'm a very practical guy — I still think it's something very important, just as important as I felt when I wrote it. I had used it in order to formulate my quantum electrodynamics. I invented it to do that. It was in fact

the mathematical formulation that I expressed at the Pocono Conference — that was in this crazy language. Dates don't mean anything. It was printed in 1951, but it was invented at least by 1948. I called it operator calculus, yeah. Now, I published it at this time because, after I had given the rules, and proved that they were the same as the other things, or at least let Dyson carry the proof because people don't bother to read my proof. It's too elaborate and funny and the notations are odd, and it's based on the path integrals and they don't know that anyway. But I had to do it for my own purposes. The proof is a paper called —

Weiner:

"Mathematical Formulations of the Quantum Theory of Electromagnetic Interaction."

Feynman:

Yeah, right, right, which is an unnecessary paper; it was only because Dyson had proved that he did it in some way and I had to say how I did it. But the other paper was not completely empty. It has some interest. The other paper, on the operator calculus, I felt — you see, through the years I had invented and accumulated a whole lot of debris. You can't study this thing without noticing things. So I had noticed certain ways of representing spin zero particles by path integrals. I had invented this operator calculus. I had a whole potpourri of junk that I didn't know where to put. Most of it was the operator calculus. So this paper actually contains a few other little things in various appendices, including exactly the right co-efficient to put in the rules of the earlier papers, which I'd never gotten straightened out, like the sines and the factors. So it's kind of a funny paper. It's on one subject, but in the middle of it, tells you what co-efficient to put on some other formula. Also, it contains a whole lot of little bits and pieces of things that I had noticed in playing with these problems. So it was kind of a place to put an accumulation of junk, and I thought it necessary to print. Otherwise I had no place to put it, except a lot of little papers, see. So I had to disgorge myself of a backlog or a bankful of valuable things in there. The central item, the operator calculus is, I still think, a thing that was invented that's very important.

Weiner:

Your description of the paper sounds as if you felt it had completed this project.

Feynman:

Yes. That's what I was trying to do. I was finishing. Right. I wouldn't have the feeling that I had anything else about it that was very necessary or ought to be published, definitely. I put in there everything that I thought ought to be published that I knew, in that paper. So that was the end, so to speak, of my published work in that particular field.

Weiner:

That was '51 —

Feynman:

It's only '51 because it took me so long to write the damn thing. I had terrible trouble in writing papers. I had great difficulty. The only papers I wrote easily were the two papers called "Classical Relativistic Cutoff for Classical Electrodynamics," "Relativistic Cutoff for Quantum Electrodynamics," because I was simply told by Rabi, "Just write it any old way. Get it out. And the improved techniques and so on you can put in a more careful paper later." This suggestion came from an old fellow to a young man, and I did that, and so I found it very easy. But the other papers, in which I wanted everything to be just so, took me a long time to be very careful and to get everything just so, and to find the best way of formulating and the best way of expressing it. So there's lots of delays in writing the papers, and I was spending a lot of time writing papers after I had worked everything out.

Weiner:

So it was not a question of working it out and then just sitting down —

Feynman:

I can't sit down and write it quickly.

Weiner:

Do you actually — then you're actually doing the work in the process of doing the paper?

Feynman:

A great deal of formulation work is done in writing the paper, organizational work, organization. I think of a better way, a better way, a better way of getting there, of proving it. I never do much — I mean, it's just cleaner, cleaner and cleaner. It's like polishing a rough-cut vase. The shape, you know what you want and you know what it is. It's just polishing it. Get it shined, get it clean, and everything else.

Weiner:

What's the longest it's ever taken you to write a paper, once you decide this paper is

ready to be written?

Feynman:

I can't do that. I start to write it, and sometimes I leave it, and what's happening to me now is that I have four or five things on which I've done enough work to easily write a paper, and I just haven't got around to writing them. Four or five — well, that's exactly right. I've found that my trouble is, I haven't been writing things up. I discover things, and then other people discover them later, and then it's not worth writing them up. For instance, I worked out the quantum theory of gravitation to an order infinitely higher — I mean, to a degree, to a detail, infinitely higher — than anybody else that I know. But it isn't complete. There are some slight weaknesses. So I haven't written it up. But it's crazy — that's five years old now. It should be written up.

Weiner:

Do others know about it?

Feynman:

Partly, partly, but it's much further along than they think. It's quite elaborate. I've got all kinds of stuff. It's just a big pile of stuff. In the meantime, people are publishing things which cover various features, so it'll make it easier for me to publish it when the time comes. Still, what's bothering me is that I haven't got through exactly to the end. Probably if I sat down and wrote it, I'd find my way to the end.

Weiner:

That's what I was suggesting; it sounds as if that was your style of work.

Feynman:

Well, possibly. Possibly. But anyhow, it isn't written. I did some work in biology that would have been easy as pie to write at the time, but I never got around to writing the article until somebody else discovered the same thing, a year later. And it would have been useful to them to have had that discovery known before, to help them. And there are other things like that. So I'm in rather a bind now about writing. I can't seem to get to writing things. It's very unfortunate. I've always had trouble writing. I'm lucky — I have no trouble speaking. But I have terrible trouble writing.

Weiner:

Do you feel you do better under pressure?

Feynman:

If the pressure's good enough, yes. Then I can just finish the whole damn thing. I can write under pressure. It's the only way I really can write.

Weiner:

And perhaps you feel less pressure —

Feynman:

I don't feel much pressure. That's the trouble. When I did the work in helium, which we'll come to in due time —

Weiner:

You mean this whole super-conductivity?

Feynman:

Super-fluidity. Helium — at something — I don't know what stage I was in, but I wasn't writing it up; I was just fiddling around. And Barker, who was very clever, said to me that he heard a rumor that Schwinger was working on liquid helium. And so I wrote it up and published it. And, of course, he was only teasing me. That's the way I got it written, though. I mean, that was a good idea. I wish somebody would come along with a similar scheme for getting me to publish a couple of other items. Three things at least I have now that would be publishable, which I haven't written. I can't sit down and write because I have other things I want to do, I want to develop further. Also I'm not very efficient these days. I get lazy. If I sit down here, I fall asleep. You know? I mean, I sit down to write, it gets hard, and I fall asleep. And before I write I should correct manuscripts of speeches. You see, people hound me to give a speech. And I write them. When I give the speech I am not writing it. "Oh, can't we make a tape? Will you edit a tape?" It never works right. Then I give the speech, the tape comes, they've got it all edited, and I can't even get around to fixing it, because I want the sentences not be so lousy. I want to improve it, and if they let me get my hands on it, I stop the whole thing, because I don't do it, and I want to improve it, and it's a bastard. There's a lot of stuff that's being held up by that. And so I've got a big pile of junk I have to climb through somehow. But I want to do some other kind of research. Maybe I should climb through all the junk first. I don't know.

Weiner:

Or scrap it, you know, go on to something else.

Feynman:

I don't know, it's too much stuff, it's good stuff, too good to scrap.

Weiner:

That's interesting.

Feynman:

Yes, difficult, but interesting.

Weiner:

It is. It's a hard situation to understand.

Feynman:

If I found it easy to write, then everything would be fine. I wish I could. Speaking is easy. But writing is not easy.

Weiner:

Did you ever try dictating a paper?

Feynman:

Yeah, in forced condition, I do it all right. If there's some terrible, terrible pressure, then I can get it out, and it's perfectly all right. When there's no pressure, I go back over the sentences. I don't like the way it looks. I fix, I fix, I fix — it's no good, and I never get it done. I get too tired. When there's pressure, I keep doing it, and the hell with it, get it out, and then I look at it right there, it's not so bad.

Weiner:

Do you remember that early statement of your vow when you got to Cornell, that you weren't going to worry about other people's expectations, but just go do it?

Feynman:

Yeah. Of course, I'm in that other bind, too. I'm worried that if I work on problems that are advanced, I always think, I ought to do them. I have a feeling of "ought." I ought to work on high energy physics — right? Why do I? I don't know.

Weiner:

Because people expect you to, I guess.

Feynman:

Somehow I feel I ought to. It's bad to have an "ought." It mixes you up. The net result is — nothing.

Weiner:

A lot of people have thoughts, but they're not on the surface.

Feynman:

Yeah. Well, anyhow.

Weiner:

You mentioned — I don't know if you want to talk about it now — beta decay.

Feynman:

That's later. That comes later. I have helium — helium is in between. Oh, you wanted to know about Cornell, what it was like.

Weiner:

Yes, and then we'll get onto this whole sequence of other work.

Feynman:

I came to Caltech. Oh, and there was something else I wanted to say. Not only do you publish a paper, but you give lectures, you see. You give papers, you give lectures, and you might want to know what kind of lectures on this subject I gave.

Weiner:

Yeah, you mean at Physical Society meetings?

Feynman:

Yes, things like that. I mean, how you go about telling people.

Weiner:

That's a thing that if you don't talk about it now, we'll never have a record of it.

Feynman:

I'm not going to give a complete record. I'm just giving a remembrance.

Weiner:

Yes, fine.

Feynman:

I was invited to the Physical Society to give a — you know, not invited, but I submitted a paper or something to the Physics Society to explain the backwards moving positron, the positrons and electrons going backwards in time. This is after the rumor is around, and I've explained it at Pocono. I explained it — you know, partly. But then I worked out better ways to explain it, and I prepared this paper, and I also made a paper for the Physics Society, a 15 minute thing, to describe it. So I went to the Physics Society and gave this paper, and I wanted Professor Oppenheimer to hear it, and other people like that. I particularly wanted Oppenheimer to hear it because he often said that there wasn't anything to it. He understood Schwinger's and he didn't understand mine. And I thought he would be at the meeting. I'd kind of half thought about him when I prepared it.

When I went to the meeting, he wasn't there, but I gave the paper, and then Weisskopf got up and said, "This paper is so important and unusual" and so on "that we ought to give the man more time to express his ideas." That sometimes happens. It can be by a vote of the people, to give more time. So they voted to give me more time. But the only trouble was, I had prepared it so perfectly for the time. I mean, I didn't do such a perfect job, but I'd organized the ideas in a certain sequence to get them into the ten minutes, and I'd gone through the whole thing. I couldn't think of what to do with the extra. It's not as if I'd been cut off in the middle. I couldn't think of what to do, what to add to it. Anyway, people knew it was useful. Then I stepped down, and just at that moment, Oppenheimer came in and sat down in the chair just ahead of me. And he turned around and said, "What did you talk about?" I said, "The idea of electrons going backwards," meaning positrons. He said, "Oh, I heard all that. Oh, yes," he said, "I heard that stuff, right? That stuff I heard." I said, "Yeah, you've heard it, but you've never understood it." Now, the response to that was an invitation I found in the mail when I got back to Cornell, to come to Princeton to the Institute and explain all my ideas, in as many lectures as I wished, two a week, as long a time as I wanted, expenses to be paid by the Institute, and so on. He's a very great man, I know. I mean, I understand him. We're good friends. You know. I mean, it's not enemies. I said that because I was trying to get

something across to him, that he didn't understand it. That was honest. He knew that if I were driven to say that that was true — you know what I mean — and it was worth learning. So I said that, and his response was very generous — any length of time I want, any conditions. So I went to the Institute of Advanced Study.

Weiner:

I'm sorry, but when was this? If it was after the Pocono Conference —

Feynman:

It shouldn't have been more than a week or two after I gave this paper at the Physical Society meeting, which must appear in some meeting.

Weiner:

Yeah, darn it, it's after the Pocono Conference?

Feynman:

Oh, yes. Oh, yes. Oh, yes, it's after the Pocono Conference. Not much after, but it's after the Pocono Conference.

Weiner:

Probably in '49. You're still at Cornell.

Feynman:

Yeah. I don't know if the paper was published yet. I'm still at Cornell, definitely, right. Good. That's all right. Well, I went to Princeton, to the Institute of Advanced Study, and there were these smart people there, and they came to the lecture, and I explained it, explained the ideas. And I always had the impression at the Institute — Well, I gave the lectures, and it was very successful. All the questions were very practical. And very sensible. And I was rather terrified of the Institute before that, because it was well known that all these guys at the Institute would talk a good game, you see. Like somebody would say, "Well, isn't that just the same as Smorglepop's theory?" I'll give you another example of it in a minute. At any rate, I gave the lectures, and there were nothing but practical questions like, "If you were trying to do this problem, how would you set it up? Did you mean by this a minus sign there?" "Yes" You know. All perfectly OK, and I gave nice lectures. I gave all the lectures I wanted to and explained everything and went back home to Cornell. I said: "Hey, Hans, the Institute has changed! Something has happened. These guys are very different. They didn't ask anything but

sensible questions. They didn't say, 'Isn't that the same as Porkyschnorp in 1621?' or something like that." "Oh," he said, "I know the reason. I went just a few weeks before you did and gave some lectures on nuclear physics. I started to give the lectures and I hadn't opened my mouth and said two, three, four words, when this stuff started. Somebody jumps up and asks a question. He says, 'Isn't that the same as what Wegischnorp said in 1960, and so on, in a paper in the Weische Physica Acta?' Somebody else, before I can answer, gets up (typical Princeton Institute) and says, 'No, you see, what Bethe is going to say is 'this, that and the other thing,' and what the fellow says in the Weische Physica Acta is 'this, slightly different.' And somebody else says, 'No, it isn't exactly so different, because Bethe —'" He says to me, "So when the third fellow gets up to argue, I slam the table" — you know, when Bethe gets mad he can look formidable — "I slam the table and I said, 'Gentlemen, if you knew what I was going to say, why did you invite me to speak? Now, I want to make an uninterrupted speech, unless you have a specific, detailed, and sensible question.'" Then he gave his lecture. When I followed, they were still smarting under the spanking that they had gotten from Hans, you see. So they were asking only sensible questions. I was afraid that they would just try to tear me limb from limb — you know, saying "This is just Schwinger stuff. You can do it this way. Why don't you do it that way? Why don't you do it this way?" And then quoting some other guys, and making it very esoteric and difficult and fancy. They have a kind of one-upmanship which practical people can see through, but which a poor fellow is fooled by. I wouldn't have been fooled by it, but I would have been terribly annoyed by it, because they wouldn't have been learning from me. They wouldn't have been paying attention if they'd started that game. They close their minds to find out if it isn't the same as something they already know. Therefore, they don't have to learn it. So at any rate, that was my opinion of the Institute, but I had, I must admit, no trouble whatever, and they were a very, very good audience. They listened and asked only sensible questions. Then, sometime after that — I can't tell you when, 1949 or '50, in the summer — I visited Ann Arbor, because I was invited there to give a series of lectures on quantum electrodynamics. It must be a bit of time afterwards. So I gave a whole sequence in Ann Arbor.

Weiner:

This was in summer.

Feynman:

Yeah, in the summer school that they had there.

Weiner:

Have you ever participated in the summer schools?

Feynman:

No.

Weiner:

I guess they were suspended during the war —

Feynman:

That was the only summer school I was at, Ann Arbor.

Weiner:

And you were there just to teach.

Feynman:

Just this quantum electrodynamics. And I was invited, at one point along the line, to Caltech, to give a series of lectures explaining this stuff, my ideas on this stuff, and I gave lectures which I think were too difficult. They weren't clear to everybody. They were too difficult. But this was a chance to see Caltech, and to go around Los Angeles. I had tried many years to get to Los Angeles, by going across the country during the summer, leisurely, by car, and stopping wherever I felt like

Weiner:

What was the attraction in Las Vegas?

Feynman:

Well, I liked — well, just the adventure of meeting various people. I didn't gamble. I can't understand — I can't gamble. I understand the mathematics of the odds. I believe firmly that the games are presumably fair. They're honest. If they're honest, there's no game to it, because it's just a question of how the dice go, and it isn't interesting to me. It's just accident.

Weiner:

So when you go there, you don't gamble?

Feynman:

No, but I'm interested in — if you're interested in my personal life, why, I can go into

that. All right, I'll go back to Cornell — (crosstalk) — you see, I —

Weiner:

Well, I'm sorry, but —

Feynman:

I know, you interrupt, but I was going out to Los Angeles, and giving a lecture at Caltech, and when I was giving those lectures, I was invited to come to Caltech as a permanent position.

Weiner:

Who made that offer?

Feynman:

Dr. Bacher, who in the meantime had moved from Cornell to Caltech.

Weiner:

I see. Had you known him pretty well at Cornell?

Feynman:

Yes. And also during the war. He was at Los Alamos. He was a very good friend of Bethe. Many times we would walk, he and Bethe and I and some others, mountain climbing trips and things like that. Cyril Smith, Bethe, Bacher, myself and some others, young fellows, would go together a lot. Cyril Smith is a good friend of Bacher. So there were three men who liked each other very much. They would often invite other young men along. I met all three of them that way.

Weiner:

And Bacher was here, and he —

Feynman:

He offered me this job, and after much debating back and forth, I took the job. Now, about Cornell. This was something to do with taking the job at Caltech. Somewhere along the way, another thought struck me, which is that the world is in a — did I tell you what I thought about the atomic bomb and everything right after the war? That I

couldn't look at New York City and people building a new building without thinking they're crazy? When I came to New York, I would eat in a restaurant and look down the street, and think of what would happen if a bomb was down at 34th Street, and you're up at 52nd Street, it's less than — you know, is it half a mile? Wherever it is or nearly a mile. But this thing has a radius of that much, or something. All these bricks, all that which was in between — you know, and so on. And I'd realize the terrible thing the atomic bomb represented, and that I had a feeling — possibly because my wife had died, and so I had some feeling of impermanence of things — and also, a general prejudice that human beings were doing exactly the same thing in discussing the world with each other, at the UN and everywhere else, as they had done before. It looked to me — you're an historian, better than I — it looked to me like history was not getting anywhere that they were holding the same kinds of stupid, selfish, national views that they had before. And it seemed to me inevitable that they would be led into a war, and this was just a matter of a little time. And so I couldn't get the idea that there was really a future. I thought it was very imminent. I would even think that people were crazy. They didn't understand it, and they'd go to build a new bridge or a tunnel or a building. I'm glad not everybody believed the way I did, or the whole damn thing would have stopped. It would have been stupid. But I spent a lot of time, at that time, explaining about the bomb to people, in lectures, if I were invited to do so.

Weiner:

At Cornell?

Feynman:

The earliest days at Cornell. I'm just trying to remember odd things there—you know, not work, but things aside from it. Because I felt, as many of us did, that we knew something about the bomb, and that citizens should know more, because the decisions are made (ideally) by people, which the citizens are. So whenever invited, I would give talks on the atomic bomb, and I would accept every invitation of this kind.

Weiner:

Local groups? Cornell?

Feynman:

Well, first I was asked by the women's club at Cornell. The Goodrich Rubber Co. Laboratory personnel. And the Temple back at home, with which I had been once associated. That was fun — my parents had never heard me speak. And I came home. I was going to give this speech. And I teased them a little bit. I was trying to prepare it, and I was worrying, and I told my mother, "I don't know, how does this sound? Many years ago, when I was a small boy here, I was asked to give money for a brick to go into

this Temple. I never dreamed that I would return many years later to give a talk —” Corn, you know! She said, “Well, that’s pretty good, but why don’t you put it more briefly?” You know — she didn’t have any idea. By this time I knew I could give lectures, you know, that I was successful at giving talks. And so when I finally went there and gave the talk, everybody was excited. It was a good talk and everything else, my parents were grinning and beaming from ear to ear, they were proud of me, and everything was quite a lot of fun. But — oh, there are a number of other little things that happened that I begin to remember, which are all in between. But let me try to just remember the key word, “Tolman,” and then I’ll come back. So, I gave those kinds of talks at that time. And then I thought pessimistically. In the meantime, like many other people, you just gradually get to live with it, and you forget the pessimistic view. Possibly the thing is so bad they’ll never use it, but I don’t know.

Weiner:

Were you involved at this time in any of the scientist groups that were taking a role in the politics of the thing, about civilian or military control?

Feynman:

Well, not so much. I limited myself to giving lectures to citizens and other groups whenever I was asked. But there was one thing I was involved in, and that involves Tolman, and I’ll mention what it was. I got a telephone call, when I was on a vacation from Cornell for two days or a weekend or something at my home in New York, in Far Rockaway. Tolman called me and said that they had to write rather quickly a thing for the UN, disclosure of information about atomic energy for a commission of the UN that was worried about it. It was early, right after the war. The United States was going to tell the United Nations what it knew about atomic energy, so that they could make their deliberations as to what kinds of international problems would be involved and so on. OK? It was an important thing.

Weiner:

You had known Tolman at Los Alamos?

Feynman:

Yes. And he couldn’t find anybody else to help him in this emergency. Somebody had to write this. They had somebody else writing about all the plants and the separations, how many mines there were and how much uranium there was in the world, how much fluorine there was in the world, and what there was. Someone wrote a little bit, I think, about the technical problem of mining and smelting, or whatever it is.

Weiner:

Oh, yes, this was a multi-volume report —

Feynman:

It wasn't so very big. It wasn't so very big. It was a report given. And then there was a section — they had about three parts — there was a section which was sort of the physics of the bomb, how the thing worked, the nuclear reactions, the whole business about neutrons, fission, and why you need uranium, why you need thorium and so on, and I was supposed to write that. Of course, as you know, I'm not a good writer. I'm just telling, again, an amusing story, because it was really very exciting. I came in, I guess it was on either Saturday or Sunday, to do this, and I came into the office in the Empire State Building, and he told me what I had to do. There were two girls sitting in two chairs, and he said, "They'll be the secretaries for you to do this." There was tremendous pressure. It had to be done in two days, or one day. OK? So I said, "Why two?" "One will look up things for research for you. We have big files. And the other one will take dictation, while the first one runs around looking up facts." "I don't need the facts." Because I knew, the physics is simple. For a guy that has to find out about mines, he needs somebody to look up how much production is in the Belgian Congo in 1952, but this stuff, I knew inside out, you know, on the level I had to do it. So I only needed one. I said, "You can forget about the other one." Then I said, "I don't need..." You see, this is pressure. This shows you what I can do under pressure. It was very exciting, what I could do under pressure. I said to the other secretary, "Sit out here. I'm going in the office and think." The only way I knew how to do this was to give a speech. You see, I could give a speech. I knew what the problem was. I went in and made up a speech. I prepared it as if I was going to give a speech, see, and I made an outline, and I went back and forth over the various possible sentences, just as if the speech was to be complete with a dramatic — this and that, you know, everything all organized just like a speech.

Weiner:

Then you wrote it out? Or wrote an outline?

Feynman:

I made little notes, as if I were going to give it. I was prepared to walk out and give a speech to 150 people, on this subject, you see. So when I was completely prepared to give the speech, I called her in, and started to dictate. I just kept right on going. All the things, from the beginning to end, all organized, you see. I got a great reputation around there. Tolman kept telling me, afterwards, that the secretary couldn't get over it, that nobody could get over it. I got rid of the researcher, and I gave this whole thing from beginning to end, smoothly and everything. So, anyhow, the thing that impressed me about this was the following. First, after I had written it, it was sent to Bacher, because

he was supposed to have his name on it as co-author. The reason is that Feynman was not an important enough name for such an important document. That's all right. So they sent it to Bacher and he made some corrections and suggestions. But the more important thing was this. The question of secrecy — what could we tell, what not tell — Tolman told me to write it without worrying about that question, and they would worry about the questions afterwards. So I wrote it without worrying about that question. But when I organize something, ideas, it's locked together, you know, like nobody's business. And I had heard that they were talking about the mining of thorium and the mining of uranium. So what the hell do you use thorium for? I explained about what you can make with thorium. Then also, a very important problem, it seemed to me, that's associated with the resources and everything else, is that it's possible to make a breeder that doesn't just use the uranium 235, but keeps up using the uranium 238. That's important you know. So I explained how that works see, because I was told not to worry about it. I worked the whole thing out. Then comes the question, should this or that be included or not? I argued, it better be included, because it's pretty obvious. I mean anybody who knows his physics will be able to figure it out, what thorium is good for. Further, if you take — (and Bacher, in fact, argued) — if you take any of this out, it's so completely organized that there will be big obvious holes in the middle of the thing. You could see the holes, I don't mean in the physics but in the logic. Anyway, the way the decision was made struck me as a very interesting thing for a guy like me to learn. A telephone call to Groves. Groves says, "No. Take it out." There was a little bit of conversation, and that's it. The problem was presented to Groves, and he gave his answer in one — it couldn't have been more than a few minutes. Now, the problem was far more important and far more vital than that, because it involved the posture of the United States toward the other nations. How does it look? You claim to give this information, you're going to give information, and you present this information. You must be very careful about how it looks — how honest you look, and so on. And the decision to leave it out, or not to leave it out, what subjects to talk to other nations about, what other subjects not to talk to other nations about, is made by one man in a few minutes. And ever since then, I've had a much better idea how government works, and what the hell's the matter with it. I mean, that those things that are vital to be decided are decided too easily. I mean, it's great that a man can decide so quickly. So can a die decide quickly? It's just — it's bad. It was a very serious thing. And I found out later that just what I guessed would happen, happened. Morrison, Phil Morrison happened to be there at the time, and he didn't know I wrote this.

Weiner:

Where? At the UN meeting?

Feynman:

Yeah. And then it was presented, and Joliot asked the question, "What's the thorium for?" No, somebody else asked the question, "What's the thorium for?" And Joliot said,

“Oh, they don’t want to tell us. But I’ll tell you, I can figure it out. I can guess” — and then he explained it correctly. Like at the meeting, in two minutes! He explained it correctly. And the whole attitude was: “Hah, hah, hah!” The United States was going to disclose — hah, hah! You know, it was a big careful presentation — we will now give you copies. And it wasn’t there. They read it overnight, and they asked questions in the morning. So Morrison said that’s what happened, and I told him I wrote it, but that’s not my fault. And the breeder came out pretty soon. You know. It’s just obvious possibility. We hadn’t actually made one. Nobody had. So altogether I didn’t care for the way those things were handled, and I still don’t because I presume that decisions are always made, almost always made, in that light fashion. There are an awful lot of light decisions of historical importance.

Weiner:

Heavy decisions made in light fashion —

Feynman:

That’s what I meant. That’s what I meant, yeah. Made not exactly light — he may have sweated when he did it—but it’s not made in a careful way. I don’t know, it just didn’t smell right to me. It smelled like this was a more important matter, and required a little more brains than a quick telephone call. Anyhow. That was disillusioning.

Weiner:

This was very soon after that first year —

Feynman:

Very soon. I’ve got you right back to the beginning now. Right after the war. Also, I had another amusing thing right after the war — just amusing. I got a telephone call from somebody in California, at my home. Some big airplane company. I don’t remember which one it was. I can’t recall. The guy gets on the phone — “Is this Mr. Feynman?” “Yes.” And he starts out. He starts to describe his airplane company. And he tells me that they’re going to make nuclear-powered airplanes — a project, see — and how many people they’re going to have, and how much money there’s going to be. I interrupt him every now and then, “Are you talking to the right guy? Do you know what you’re talking about? What do you want?” He says, “Just let me say what I want to say. Is this Mr. Feynman, Richard Feynman?” I said, “Yeah.” “The man that was at Los Alamos?” “Yeah.” “OK” — so it goes on. And it’s nothing but how many men are going to be involved, how much money is going to be involved all kinds of details. Half an hour. And to me this was a shock, because he was calling from California. I didn’t have much money, and this seemed to me terrible. So finally he shoved it out and he says, “We want to know if you will be director of this division of our company?” So I say, “You’ve got

the wrong guy. There's something the matter here. What made you think that I should be director?" He says, "Because you have the patents on these. Isn't your name on the patents for these things?" I say, "Yes, but that's all right, I won't be the director." My name is on the patents for certain rockets and so on. Shall I tell you how it got there, though?

Weiner:

Yes.

Feynman:

I don't know if you want all these amusing stories, but they're all part of life and so — When I was at Los Alamos, I became more or less friendly with the man in patents, Captain Robert..., can't remember now — the guy who's head of the Patent Office. You see, if anybody thought of anything, he was supposed to give it to the government as a patent, you know, and the government should protect itself against making a mistake and leaving something open. So one day the guy who's the head of the Patent Office sends to everybody a note saying, "If you think of anything, even though you may think that it's already covered, please let us know, because you have no way to know if it's already covered. We would like to make sure the government has the patents on all these things and nobody can make a big profit out of our work." So I saw him at lunch, and on the way back, when we got back, I said, "Listen, that thing that you sent around — if you really meant that, why you'd be deluged with crazy things!" He said, "Come into my office, please, and tell me, what." So I said, "There's an infinite number of obvious applications." He said, "Like what?" "Submarines." "Huh?" "The water comes into the uranium, which is just above critical, and the water makes it critical, it makes heat, boils over, steam goes out the back end — zooooom! See? Rocket." Then I told him how — hydrogen gas, stick it through the uranium, burns out, heat production other end — zooop! Then I said, "Purified uranium" — I say — "you mix it with beryllium — purified uranium, not necessarily a big —" He says, "Piles are covered?" I say, "No, no. Not with ordinary uranium, but partly purified uranium 235, with a higher percentage than normal put in the thing, makes a big reactor, heat." I said, "There are a million of them, things like that," and I walk out of the office. See, I was telling him, there were a thousand things —

Weiner:

— he hadn't thought of that?

Feynman:

No, he was right, you see. Everybody thought all this was obvious. So when I came back, one day he calls me, "Come in, I want to talk to you." I come in the office and he says,

“The submarine is already taken, but the purified uranium reactor and the rocket engine is yours —” I think it was because my name was somehow on the rocket engine that these guys, who were trying to do this, got a look at these hidden patents and figured, “This is our man,” see. So that’s what happened. So I could have been the leader of a great project.

Weiner:

Talking about this idea of being a consultant or, you know, working for industry, other than the early experiences in Metaplast Corporation, summer work and so forth, have you had any other offers or have you done any work?

Feynman:

I’ve had many offers, but I’ve never done any work for industry to speak of. Very little that I can remember. Maybe one or two tiny things. I do go to Hughes Aircraft Company to teach physics, to give lectures once a week to their engineering and scientific staff.

Weiner:

That’s the origin of that wild picture.

Feynman:

Yes. That’s right. That’s all. Oh, I did one little tiny, very minor consulting job for the gas company.

Weiner:

In California?

Feynman:

Yeah. They just wanted me to criticize a report that somebody had made predicting what the gas reserves will be in the year 1985. And, of course, people were beginning to believe the prediction, and they thought that it was not good. It was done by mathematics, by some formulas, and they needed somebody with a mathematical reputation to say that this is no way to do it. It isn’t a good way. It wasn’t. It was absurd. So I wrote that it was absurd. Little things like that.

Weiner:

Yes.

Feynman:

Oh, I did something on a moving picture on time for the Bell Telephone Co. I mean, they were the sponsors for an advertising agency, for Warner Bros. I really worked directly with Warner Bros., who were making the film for an advertising agency for Bell Telephone Co., on time. The name of the film ultimately was, "It's about Time." The reason I take these consulting jobs is because they're different. I don't take a consulting job that involves science more or less directly. The gas company thing was fun because it was cockeyed, and it would be interesting to know what they worry about in the gas business. In the movie thing it was the same. You get inside the studio, see how they make movies, and see how they work — the writers, the director, all that stuff.

Weiner:

What did you have to do for the movie? What did they want to know?

Feynman:

Well, it was a scientific movie. It's about time.

Weiner:

One of the Bell Science Series? Fine programs.

Feynman:

Yes, that's right. So first I had to plan what could be in such a movie. I wrote a report, and the writers there tried to put it in, and I would correct it, and so on. It was a lot of work but it wasn't particularly good. But it was very' much fun. So I do consulting jobs of various kinds for the fun of it.

Weiner:

Now, at Cornell, we got onto some of these talks, and sidelights. We've talked a bit about the teaching at Cornell —

Feynman:

— the school, how I liked it.

Weiner:

Yes, and relationships with the faculty and things in general.

Feynman:

The worst thing is the weather there. I mean, the weather bothered me. Finally, it was a day on which it was cold and raining and slushy. It had suddenly become that way. My car was beginning to skid, and I had to put chains on in the cold. I was trying to tighten those little clamps on the inside of the tire, and my hands were cold and the pressure hurt when I tried to push them. And I said to myself: "This is crazy!" So I decided — that was kind of a deciding moment — to get out of that part of the world. But altogether, other things always add to it. You see, I'm a one-sided fellow. I understand and love the sciences. But there are many fields of intellectual things that I don't really go for, like literature, psychology, philosophy, and so on, unless it's done in a very sort of scientific way. I'm very one-sided. I'm not a wide guy, only very wide in the sciences, but very much in the sciences and limited. So I found that place quite dilute. There were a few people who were interesting, like a man — Griffin I think his name is — in biology, the guy who discovered that bats use radar and was now doing something with seagulls and fish noises. See, he was fun. Of course, there were the people in physics and chemistry that were fun. But we were like islands surrounded by a mass of guys who — I don't know how to say it. To me, they were like mediocre, difficult. They didn't make good sense. They weren't interesting. It isn't exactly that the subjects weren't interesting, because I can find, like, history of the Aztecs and the Incas, and so on, fascinating. But somehow or other, these guys weren't — I don't know how to explain it. In addition to which, the student body is diluted to such a pitch by all kinds of things. Like they have home economics. They have lots of girls studying home economics, and it's supposed to be a university! If I compared the work and the care and the thought that goes into what I ask of my physics students to get a degree, to the nonsense which is all some little silly girl has to do to get a degree in home economics — and I knew precisely what it was, because I had girlfriends and they would ask me questions and I knew what they were doing — I began to get disgusted with the dilution. I couldn't find good students, except in physics. You understand what I mean? I was surrounded — a little isolated. I mean, there was only one interesting biologist, the philosophers were just crazy, and the psychology department was decrepit because they had been dominated by some guy who had measured temperature points on the skin. That was the level at which they were. The psychology students would talk all the time to me in the cafeteria. I was the only guy that would give them any advice. For instance, I would tell them a thing like this. A student would be worrying. She did something with a rat maze which showed one thing, and she wants to change one of the conditions to see if it has this effect; so that's what she's doing. I said, "But first you must do the other experiment with the other condition to make sure that your rats do the same thing. Then when you change the thing, you'll make sure —" There's some discussion as to why, and then a conviction, and then finally she marches off with glee. Of course she has to do this; it's perfectly obvious. She tells her teacher and the teacher says, "No, no, to do that would only be repeating the other

work. And that's not right. You shouldn't waste the time. You must not do that." And so on. Well, I got sick and tired of such a place. You know, all over there was this kind of — You try to do something, try to explain something to somebody, something sensible, and you always get dumbness back. Resistance. Dopiness of all kinds. It was a morass. I couldn't stand it. See, I liked to meet all the people. I'd go with the students, and I'd eat in the cafeteria and so on. But, there was nothing but dopiness. Well, that's all right if you're talking to the secretary, but it's not all right if you're talking to the students or the professors. And so that bothered me. Caltech I find, for my own particular personality, very much better because there are so many science departments and the people are very active. They have the same way of thinking as I do. And so I get a great deal of pleasure. I can talk to them in biology, in astronomy, and all the different fields without trouble. And if I happen to sit next to any student in the cafeteria and ask him about his work or talk to him about anything, I don't get any of this feeling. The odds that I'll get into something where I meet a dope, a dunce — I mean, not a dunce, but somebody who doesn't talk the way I talk and think the way I think — are very long.

Weiner:

So this is your kind of place.

Feynman:

This is my kind of a place.

Weiner:

You had that feeling when you came out here?

Feynman:

Yeah. I was trying to get away from the university, from the fact that they study everything. I didn't want to go to a place that had all this stuff. See, it's all right — they have animal husbandry, they have a hotel school — if they have all these things. It's important to have these schools, but to mix it up is crazy. You see, there are so many other kinds of students. You start to talk to one and it turns out he's interested in the hotel business. It's nothing wrong. I don't mind the guy that's interested in the hotel business, but I have to expect that I'll be talking to a guy that's interested in the hotel business! Let me put it differently. He's interested in the hotel business, but he's studying it as if it's physics, for four years. Huh? You know, he's making a big deal about the hotel business!

Weiner:

So that's what it meant. You knew, when you were leaving Cornell, that this close

relationship you had with Bethe would be —

Feynman:

Yes. That was hard. Yes, that was a negative pressure. Actually, it was very difficult to leave. I was balanced. In fact, I made the joke at the time — you know the one about the donkey that's between two piles of hay, exactly in the middle of two equal piles of hay? Which way should he go, hm? With the one complication that every time the donkey moved toward one pile of hay, the pile on the other side grew bigger. What they would do is, each time I would kind of lean one way or the other and write a letter explaining why I think maybe I ought to go to Caltech, and then at Cornell they'd fix something. Oh, I know why I was talking about these lectures. I'll have to come back to it. I had intended to go to Brazil for a year, for a certain reason which I'll come back to, and my sabbatical was just coming up. And so I finally got the solution to the problem — I mean, of the two. If I go to Caltech, I lose the sabbatical year; I have this year in Brazil, and so on. You see? So I got a letter from Caltech saying, "We will hire you, and then the first year give you, free, a sabbatical year, and a leave of absence with pay." Half pay, I guess, for a half year. On the other hand, I thought, no, because maybe I will stay in Brazil forever, which is something I'm thinking of. They wrote back: "That's OK with us." So I was done. So I went to Caltech.

Weiner:

You mean, they were willing to take a chance?

Feynman:

Yeah, they were willing to take a chance. So that's why. I remember now, I was talking about these lectures and my feeling about the bomb, because I was trying to lead up to the idea that I wanted to go to Brazil. This same business with the bomb and this pessimism kept with me for several years and, by 1950, I still was pessimistic about the world and was pretty sure that I had it right, that nobody was getting anywhere and we were all going around in circles and that we were going to have trouble. Then when we'd have trouble with Russia and so on, we'd bomb each other out, and the Northern Hemisphere would be in a bad way. A lot of the Northern Hemisphere. And the people that are most advanced scientifically will have been killed off. And so, right or wrong, I got the idea that the traditions of science are not — they're fragile. The traditions of scientific thought. I don't believe they're fragile. I think they're very fragile, and easily lost, and that science really has a value. The viewpoint that's involved, the objectivity, the way of doing things is valuable, see? So I thought it was of value, and that it might be destroyed. It could be destroyed because maybe people without this tradition would be the only ones left that would have any power. See? So I figured, I've got to go somewhere where they don't have the tradition, where they're not likely to get involved,

you see, to help develop a sort of strong scientific tradition there, or at least make one step in that direction. And so then I got an opportunity to go to Brazil. I thought, somewhere in South America. I thought first of Ecuador, but I didn't know much about how bad the situation was. I didn't realize that it was worse than I could imagine. And so I ultimately went to Brazil for a year. I was invited to Brazil first for six weeks, and I went just to see.

Weiner:

By whom, by a university?

Feynman:

By a center in Brazil. In Rio they had a Brazilian center for physical research, the Centro Brasileiro de Resquissas Fisicas. Actually, I got invited because I was sitting next to a man named Tiando, who came from Brazil, at a Physics Society meeting. I told him that I was thinking of going to South America. He said, "Come to Brazil," and he arranged this invitation. So I went there for six weeks and was delighted — with Brazil, the music, the life on the beach, the teaching problems, and the university. And there was this little center trying to develop and so on. It was just right.

Weiner:

When did you go for six weeks?

Feynman:

I guess it was 1950.

Weiner:

This pessimism is despite the fact that you knew that you had done first rate scientific work that —

Feynman:

— it was pessimism about the world.

Weiner:

I know, but I'm saying that it wasn't countered by the personal satisfaction.

Feynman:

Oh, of course not. I don't think so, no.

Weiner:

There's no reason why it should have been.

Feynman:

No. No, not that I know of. No. No.

Weiner:

So you went on this six week job.

Feynman:

Yes, and then after that I went to Caltech. Somewhere I got invited to go there for ten months, by the State Department Point Four Programs. So I went down there for ten months and taught physics, and then came back here and went to Caltech.

Weiner:

They paid for that year?

Feynman:

I think so — half pay for that year, yeah.

Weiner:

That was what brings me to —

Feynman:

Oh, I think I didn't take the offer. Oh no, I know what I did. I saved the money, so that if I stayed there I wouldn't have taken it. Some crazy thing.

Weiner:

So you could return it?

Feynman:

Some say — I don't know what I did, some crazy thing.

Weiner:

But at the end of the ten months?

Feynman:

I was here.

Weiner:

Whatever happened to that pessimism?

Feynman:

It's gradually disappeared. More or less. Not disappeared, but been more or less forgotten. Not exactly disappeared. I don't know what the situation is now. It doesn't look as bad. But that may be because of the usual — you know, you've got a threat that hangs. It's like the sword that hung by a string over the guy in the book by Edgar Allan Poe.

Weiner:

In Brazil, you're out of touch with your colleagues, though.

Feynman:

Yeah. That's one of the letters, you see — Fermi.

Weiner:

Because you say in the letter — now, here's where — what you wrote —

Feynman:

Well, from the letter you can get the idea, then, damn it, when I'm in Brazil.

Weiner:

Oh. Yeah.

Feynman:

I returned to Brazil altogether five, six times.

Weiner:

Since then — on vacation?

Feynman:

Vacations. Summers. Go up there, and everything.

Weiner:

Now, you wrote this December 19, 1951, from Rio. You wrote it by hand. Let me just read part of it. I think it's very interesting, this first paragraph: "Dear Fermi:" How come you call him Fermi?

Feynman:

Oh, everybody calls him Fermi!

Weiner:

OK. That's his name. "Being thousands of miles away, I have only heard by amateur radio from friends in the U.S. that you are doing experiments in meson scattering from protons. I don't know what your theoretical friends are saying, so I would like to make some comments, at the risk of only saying what is obvious to everybody in the U.S." Let me just read the beginning of the next paragraph, to give you an idea — I won't read it because it's ten pages. "To begin with, I am of the opinion that Yukawa's meson theory, with pseudo-scalar mesons gradient coupling, is wrong, or at least useless, in its present form, because at least perturbation theory is n.g. and otherwise divergences cloud the issue. But I think mesons are pseudo-scalar and I think the amplitude that a nucleon emits just one may be proportional to —" Then you go on and get into the whole argument. The interesting thing about it is that then you say to him that you're writing to Bethe about this. Yeah, you make that point somewhere in there — and you ask him along in the letter: "Does anyone in the U.S. know about this?" So you're anxious to keep up.

Feynman:

I don't really care if anybody in the U.S. knows about it, frankly. I'm sure that all I care about is that I'm not saying something that he knows already.

Weiner:

I see.

Feynman:

That's really all I was worried about. Here I'm writing as if I'd discovered something important, and I have to act a little modest, you see, because maybe everybody already knows it. I think that's what I was thinking. I've always had an independent view. I don't care if somebody else thinks of something I think of, except when I talk to someone somewhere else. Then I don't want to act as if I'm the only guy that could think of such a thing. It turns out that people in the U.S. were doing a lot better job of thinking than I was.

Weiner:

Well, you had some information. Then you end up, you say, "So I am, with this letter to you and one to Bethe, giving up Yukawa's idea 1934, and am going to the Copacabana Beach to see if I can get one of my own. I get lots of ideas at the beach. Merry Christmas." That's enough. I think what you did say, somewhere in the letter, was that if he cares to, he can make a copy and distribute it. Now, was this a way of making up for lack of meeting together, and groups? Obviously it was.

Feynman:

Well, I don't know.

Weiner:

Do you remember the circumstances of hearing on amateur radio?

Feynman:

Oh, yeah, I was in amateur radio communication with the United States every week.

Weiner:

Through a friend?

Feynman:

Through a friend. Through a blind fellow, by code, at Brazil, with the radio station at Caltech, at which there were friends of mine, a student of mine, who would tell me

things. They asked about the blind fellow, and found, decided that in code it was too slow, you know, too difficult, and he found somebody who had a transmitter with voice. I would talk on that, which is illegal because I haven't got a license, and the guy used to always introduce me by false call letters. He'd say, "Listen, W6J17 is visiting us," or something like that, "and he wants to talk to you." "OK, W6JY17" — we just made up the letters — and then I would talk. I would ask some questions on the situation, you know, data, usually from Caltech, and so on. I worked out a number of problems down there. I also worked out a problem of nuclear physics. I felt that the time was ripe to understand the light nuclei, the level system of the light nuclei.

Weiner:

This was being worked on at Caltech, actually.

Feynman:

Experimentally. And I took down all the data that I had, when I went down there, and I got this theoretical thing, which turned out — it was really quite close to being right. It was one of these other things I should have published. But I wasn't quite satisfied, see. Anyhow, I had this system, which is very similar to what's done today, much later — not today, today they've gone further, but at that time — well, say four years ago. This was many years earlier. Oh, I guess I was five or ten years ahead. I worked out this stuff to predict the levels of nuclei. And Lauritsen was always amused by it, because this guy would come to him, this fellow on the radio business would come to him and say, "Could it be that hydrogen 16 has two levels very close together, at the lowest state, not just a single level?" Or, "Could it be that such and such a level is not correct spin, but is something else?" He was always amused by this crazy guy down there in Brazil. He knew what it was, of course. But it sounded so special — you know, like you picked the nucleus out of thin air and asked about it. It turned out I was right about it, that the magnitude of 16 had three levels, as a matter of fact, very close to the ground level, and it turned out my theory wouldn't always predict it. I would get into trouble. I couldn't fit the data unless there was a coincidence that the lowest state of hydrogen 16 was double, very close together. And that was very amusing, because it turned out later to be right, but at the time they thought it wasn't right. So I had something pretty good. I was able to predict a number of truths. But I wasn't myself satisfied. I had a number of parameters, a pretty big number. I wasn't too convinced that the thing wasn't an accident, and I never published it. But the methods and the ideas used are right. But I never published it, and so nobody knows that I did all this, except Lauritsen.

Weiner:

This was part of the work done in the ten month period of Brazil.

Feynman:

It was that ten month period. The trouble is, see, I went to Brazil several times. But I believe it was that ten month period.

Weiner:

Well, during the one in '51, you were writing to Fermi on this other work. He replied to you —

Feynman:

This must have been the ten months, because it wasn't the summer time. It was in December. It says so, December. So that must have been. Now, the radio — that must also have been the ten month period. I'm trying to remember. Yes, definitely, because I remember the papers in the room and I know which hotel I was in.

Weiner:

It says what hotel, on the paper —

Feynman:

Miramar.

Weiner:

Yes, the Miramar Palace Hotel, Copacabana.

Feynman:

Yes. Boy, I loved that! Yeah, that was fun.

Weiner:

This is where you lived? You lived in the hotel?

Feynman:

Yes. That was great. I also taught physics in the university, electricity and magnetism. University of Brazil. I also taught engineering in the engineering school. I taught mathematical methods of physics.

Weiner:

In English?

Feynman:

No, in Portuguese.

Weiner:

When did you learn Portuguese?

Feynman:

Well, when I thought to go to South America somewhere, I thought I'd better learn the language first. I didn't know where I was going. And there were two languages, Spanish and Portuguese. But they had a good language teacher in Cornell, with a new method, a language laboratory they called it. So I picked Spanish, because more countries spoke Spanish than Portuguese. When the first day of classes began, I was walking into the class. The cutest young, beautiful, blonde kid — just cute, a student — was walking right in front of me, and she was headed for Portuguese class. I said, "The hell with it, why should I do Spanish? I'll learn Portuguese." See? And I started — but I said, "Listen, that's no way to make such decisions," and I went back to the Spanish class. Then my first chance to go to South America was in Brazil, which is Portuguese. So I made a mistake. I should have followed the blonde. Anyhow, I learned Spanish in that class, and then when I found I was going to Brazil, I found the man, whose name unfortunately I don't remember, in the psychology department, who was from Portugal. In a few weeks he taught me Portuguese, based on the Spanish. I simply converted my newly learned Spanish by mispronouncing to Portuguese. I learned Portuguese that way. It was very poor Portuguese, but it was something. So I came down to Brazil speaking Portuguese, after a very crude fashion. It turns out that technical Portuguese is much easier than you think. You have to learn a couple of tricks, like "tion" becomes "cao," so that "radiation" becomes "habiacao" — you know, you just mispronounce the vowels in a certain specific way, and so on. So the longest words are easy, and it's a simple one, like "blue sky" or something, that's impossible. So I found I could do it, at the beginning, and I spoke Portuguese from the beginning in my lectures.

Weiner:

Did you feel isolated there? Was there anybody on your level, in physics, that you could really talk with?

Feynman:

No. But they were pretty good. There was Leili Lopes who was pretty good, and we did a lot of research together. I would explain to him what we were doing, and he'd explain back and argue and so on. It was not impossible, and we got along pretty well. We did some work on the meson theory and deuterons, and showed that the meson theory didn't give the right answer for the forces for the deuterons. That was discovered — that work we published a little bit of. But that was discovered later by Levy, who was supposed to have solved the problem with forces from meson theory. But he didn't really; so finally he caught up with us or at least with some of what we did.

Weiner:

You wrote to Fermi in this case — I just happened to stumble across it —

Feynman:

Because I was interested in the problem, and I heard he'd done the experiments, so I wrote to him.

Weiner:

But evidently you didn't have too much data.

Feynman:

I got data from Lauritsen, from radio.

Weiner:

And you were in contact with Bethe.

Feynman:

I wrote a letter to him. I don't know that I was in much contact. No, I don't remember — no, I don't think I wrote lots of letters all over the United States — no, I just wrote to Fermi.

Weiner:

Did you have a good time there?

Feynman:

Oh, yeah. It was great. I had a great time there. That was a pretty good period of time.

Weiner:

When did you make up your mind that you were going to definitely go back, to take up the new post at Caltech?

Feynman:

I don't know. I guess when I got to realizing what the real situation was in Brazil, or something, and gradually became more — I don't know what happened to my ideas. See, these plans were made earlier. I had a year to learn the language and so on. So I think that I was gradually changing, maybe for the reason you say, that I was becoming successful, or for whatever reason. Anyway, I took the problem less seriously, that part of the problem. I've always done what I could for Brazilian science, but I don't think... It's gone down backward now, because of political difficulties. My experiences in teaching in Brazil were very interesting, and I learned a lot about science in Brazil, and gave lectures on it, and so forth and so on, but it's getting beyond the range of what you want.

Weiner:

Have you ever published anything about your experiences there?

Feynman:

Only one thing; I did write an article on teaching science in Latin America.

Weiner:

Where was that published? I'd like to look it up some time.

Feynman:

I can give you a copy. My best published is Engineering and Science Magazine. They publish all these little articles. It was a speech given. I was invited to come to a conference on teaching science in Latin America, and to be the keynote speaker at this conference, that people from all over Latin America came to. And so I talked about teaching science in Latin America and described what I thought was the trouble with it, as the keynote speaker. But I gave a much better speech at the end of the ten months, when I was in Brazil, as to what I found. That was really sensational.

Weiner:

What?

Feynman:

Well, it's a long thing. Shall I tell you about it?

Weiner:

Well, it's up to you. It's late, so whether you want to get started on that, or not —

Feynman:

Well, we can go on forever. I'm a very complicated man. I mean, I've got all kinds of side things, infinite amounts of them. So it's up to you to make up your mind what pieces of me you want to cut off, because if you don't cut off some, you're going to get more stuff than you can swallow.

Weiner:

Yeah. Well, I think maybe on that I would settle for the article, take a look at the article, you know.

Feynman:

Well, I'll tell you, if you want, the experiences from which I derive the information as to what science was like in Brazil. One thing, though, that would be worth mentioning — at the time I also taught quantum mechanics at the center, or somewhere; once, when I was in Brazil, somewhere I taught quantum mechanics. And I had a few students from different places. It's the only time that I taught that I really felt satisfied by the teaching. When I teach here and other places — I get a kind of frustration, which is the following. If the student does well, you don't know that it's you that taught him; he may be a good student, so to speak. Other people have taught him, and so on. And if he does well later in his research, you don't know whether you did anything, or whether he did it himself and so on, or other teachers afterwards. If he doesn't do very well you feel, well, the students aren't doing well, and you get frustrated, right? So I never get a feeling, in teaching in the graduate school. Even when I have them as research students, I can never see what I did, if anything.

Weiner:

It's hard to measure.

Feynman:

Right. I don't know what I did. But when I went down there, these guys came from a vacuum. They came from Argentina, and they had never learned anything, but they studied on their own. Then I teach them quantum mechanics, and now they know quantum mechanics, and they never knew anything about it before. It's small time teaching — a smaller group. You know the student, and you can see what you do to the student. And two of the students that I had in that class, at least two — two of them from Argentina, though there are others too, but I'm not sure — have turned out to be very effective young physicists. They do good work, and I see their papers published every once in a while. One is Sirene and the other one is Amati. That's a pleasure, because I know that I had a lot to do with those students.

Weiner:

Don't you get this feeling about any of your other students, at Cornell or Caltech?

Feynman:

No. Just between us, I don't know what it means at this level. I have never had a good student that hasn't disappointed me in some way. I've put a lot of energy into the students, but I think I wreck them, somehow.

Weiner:

Then at the same time you said you couldn't tell, you don't know.

Feynman:

What I mean is, I've never had a student that I've felt that I did something good for. In fact, I don't think I've had very good... I don't think I do very well. For instance, like Oppenheimer is a teacher. He had a group of students, he had some way of running them, and he had 30 graduate students, and there are many famous physicists who are known as Oppenheimer's students, because they are. They learned their damned stuff from him, in the graduate school — from him indirectly. He worked in some indirect way; I don't know how the hell he did it. There are people who are Sommerfeld's students, no? Famous guys that are Sommerfeld's students and they know that they learned that crap from Sommerfeld, somehow. I don't feel that way about anybody that I had as a student.

Weiner:

These are all earlier periods that you're referring to. Oppenheimer's teaching days were a lot earlier.

Feynman:

Yes, earlier, but what difference does it make? I can list the students, an every one of them mediocre guys, somehow failures, some way or other. Now, there were a few students who were good, but they didn't get in too close a contact with me. There's one good one — yes, it's not true. I have one good student, one more example, somebody that I know I did something for, and his name is — there's another one of these idiot blocks. Berman. Sam Berman. And his story is also similar to Amati and the other guy, you see? If they come to me clean, I'm all right. He was a saxophone player; I think it is, in a band in Florida. And he married the singer of the band and so on, you know, this kind of a guy. Then he decided one day that this field isn't very good, because he didn't get a job; but the boss of the night club's nephew got it — something like that. He decided this is a hell of a business and decided to go into something else. And he took up physics. I don't know why he picked physics. And so he started to study physics, and he went very rapidly. He got to the graduate school fast. But he wasn't very educated, you know what I mean? He didn't know a great deal. But he was a very smart man. Then, when he was a graduate student, I was his teacher, for his research and so on, and then I taught him. So I know that I made a difference. I mean, he is one of the characters and he does good work. So that's OK. I guess the secret is they have to come to me without somebody else rooking them first or something.

Weiner:

And you have to have the feeling that he's your student.

Feynman:

I guess so. Otherwise I don't believe in it. Yeah. Otherwise I don't know who the hell did it. Or something.

Weiner:

It's five till twelve. I really think, whether we know it or not, we've been going a long time, and maybe this is the logical time to break.

Feynman:

Yeah. Just let me finish with students. I'll think of some more students. Most of them are some kind of failures. The guys from Cornell — but that's the same. They're Bethe's students, too; and they're no good either.

Weiner:

Bethe has a reputation for being a good lecturer.

Feynman:

He's a good lecturer, but he has the same trouble I have. I'll bet if we sat down together and looked at our students, we wouldn't be very happy. I think he probably doesn't realize. If he sat down and thought about it — you might ask him sometime what he thinks, just for the hell of it.

Weiner:

By the way, he has agreed to sit down and talk. Just a specific small thing on —

Feynman:

The sun.

Weiner:

I want to talk with him on the 1938 business, you know —

Feynman:

The sun.

Weiner:

The three papers. What sun?

Feynman:

The energy of the stars.

Weiner:

Oh. I thought you meant, son. Yeah. Yes, the energy of the stars. But the thing that he did, the three part paper in R. M. P.; you were probably too young at the time to be aware of it.

Feynman:

No, I told you the story about the paper starting, and they wanted a course. They were going to have a course at MIT based on those papers.

Weiner:

Oh, yeah, of course.

Feynman:

Sure. No, I was not too young.

Weiner:

Anyway, this is the three paper series.

Feynman:

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