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Richard Feynman - Session I

March 4, 1966

Interviewed by: Charles Weiner
Location: Altadena, California

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Abstract:

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

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Part I: The Early Years

Weiner:

This is a tape-recorded interview with Professor Richard Feynman, in his home in Altadena, California, on March 4, 1966. Charles Weiner will occasionally interrupt with questions. Let's start at the beginning.

Feynman:

You start by interrupting me with a question.

Weiner:

That's right. It saves interruption later. Let's talk about the earliest recollections you have of your family, your neighborhood and your home.

Feynman:

Nothing scientific, just a simple —? I don't know what to say. That's a hard one. I have lots of recollections, little bits and pieces.

Weiner:

Let me ask where you lived in Far Rockaway, was this —?

Feynman:

I was born somewhere in New York, but I don't remember anything except Far Rockaway, and then we went for some time, I remember, to a house in a place called Baldwin, on Long Island, where I think we lived a year or a half a year or a summer. And then, sometime after Far Rockaway, we went to live in Cedarhurst, but then by that time I was nine or eleven. Then we came back to Far Rockaway again. That gives an outline of where things were.

Weiner:

When you have recollections of Far Rockaway, what sort of a neighborhood was it?

Feynman:

There were these two periods, the first and the second. The first time I can't remember very well at all. I remember moving to the house. We walked along. It was a big house. We lived there with my cousins, a cousin family, I mean — my mother's sisters. The two families lived together in a big, rather large house, with a big garden. We had a couple, a man and a woman, who took care of it. I can't remember which period exactly. But the first time — my earliest recollection is of moving to that house from somewhere unknown, where my cousin was in a baby carriage, so I couldn't have been more than five. Therefore it was the first time, and we were moving, so I remember it. The road, the sidewalk — there was nothing. It was mud, it was dirt.

Weiner:

Was this near the beach?

Feynman:

About two miles from the beach. We used to go to the beach very often.

Weiner:

What was your father's occupation?

Feynman:

My father had different jobs and different occupations. The earliest — he apparently wasn't too successful, I suppose. I don't know. I mean in the beginning. The ones I remember are, for a while he sold "Whiz," which was some kind of stuff for automobiles, accessories. We had the garage full of Whiz of different kinds — like Simonize. Then he was in the cleaning business. He had a cleaning chain called Sanis Cleaners, New York, which he had with a man whose name was Brockman. (I'm beginning to remember that.) But the two of them had been partners earlier in a real estate business that didn't work out, until a third man, Paul, I think his name was, was brought in who was clever enough to first oust my father and then his partner. So he got put out of his tree. And then he went into the uniform business.

Weiner:

On the cleaning business, you mean this was dry cleaning stores?

Feynman:

Dry cleaning stores, yes. He told me, but I don't remember that he was the first dollar cleaners. But I don't know.

Weiner:

This would put it, by the way, in the twenties some time.

Feynman:

Probably. I can't swear by the dates. I'm rather confused, after that he worked for a uniform company, as a sales manager of a uniform outfit called Wender and Goldstein, which was a big uniform company. And there I would say he was successful, because he was in the uniform business from then on. I remember most of the time his being in that business. But now that you ask me, I remember all these other businesses.

Weiner:

What was his educational background, do you recall?

Feynman:

I don't know it too well. I understand that he went to high school all right. He lived in Patchogue. He apparently had been taught by his father and also by some special tutors from time to time that his father got for him. He presumably went through regular high school. He went to a kind of medical school, the Homeopathic Institute or something. It's a strange kind of medicine. But as far as I know the story, which I don't know very well, he didn't have much money so he lived in a house where people were very poor, and he got involved trying to help them, and upset. So for one reason or another he stopped his medical school education.

Weiner:

I see. Where was he born, do you recall?

Feynman:

Probably in Patchogue. No — he was born in Minsk. At five he came over to the United States with his father and mother. Then he lived in a town, Patchogue, in Long Island. It's out on Long Island.

Weiner:

I know where that is, it's near Brookhaven.

Feynman:

Yes.

Weiner:

And your mother?

Feynman:

My mother comes from a family which was originally German, but which have been here — her parents came at a young age. Her father was a very successful business in that business. In fact, he was called the “Father of the trimmed hat business.”

Weiner:

What was her maiden name?

Feynman:

Phillips. Her father's name Henry Phillips, and her name was Lucille.

Weiner:

And what was your father's first name?

Feynman:

Melville.

Weiner:

So she was born in this country, and she lived...?

Feynman:

Yes. She had a fine education. She was a relatively successful man's daughter. She went to the Ethical Culture School, which apparently had considerable influence on her.

Weiner:

In New York? Manhattan?

Feynman:

Yes. That's right.

Weiner:

And did she go to college?

Feynman:

No. That's as far as she went. No, she didn't go to college.

Weiner:

So if I interpreted correctly what you said about your father, he didn't?

Feynman:

I don't think he ever successfully went to college. However, he did teach himself a great deal. He read a lot and he studied a lot, because, as I know now, he understood a great deal about science, by teaching himself or by learning from his father or from other people. He was rational. He liked the rational mind, and liked those things which could be understood by thinking.

Weiner:

When you say he was interested in science, or knew about it, in what way was this expressed?

Feynman:

As far as I'm concerned? Well, these two stories are rumors, as far as I'm concerned, but the story is that before I was born he told my mother: "If it's a boy, he'll be a scientist." But you'll have to talk to my mother to verify this. I don't know if I got the story right.

Weiner:

Is your mother alive?

Feynman:

Yes. She lives in Pasadena. No, my father's dead.

Weiner:

When did he die?

Feynman:

1948, probably — 1948, probably — 1948, 1947, 1949, something like that.

Weiner:

After that your mother moved to Pasadena?

Feynman:

Quite a bit after that. She lived in New York a while. She's only been here about seven, eight years. Anyway, he said that to my mother, before I was born. And he played a lot with me. I mean, we had a very good relationship. We fooled around a lot. He'd tell me about things all the time. And the other rumor, which I can't exactly remember, is that when I was very small, he went to a company that made bathroom tiles and got a lot of old extra tiles, you know, and then he would stand them up on my high chair, on end, in a long row — as you do with dominos. Then when we'd get all ready, I would be allowed to push it at the end, and the whole pile would go over. We'd play this game. But you'd have to verify it with my mother, of course, because I don't remember exactly. But he would then take a game. He'd say, "Now we put a white one, now a blue one, now a white one, now a blue one." Sometimes I'd want to put two blue ones together and he'd say, "No, no, must be a white one now." My mother told me that she would say, "Now let the boy put a blue one." He'd say, "No, we have to get him to understand patterns." This was the only thing I could do at this age, to think about patterns and recognize those things as being interesting. After a short time with this game, I could do extremely elaborate patterns. I mean, once I paid attention — two blues, a white, a brown, two whites, to two blues, a white — and so on. So he started in as early as he could with what he said was a kind of mathematics, sort of the shadow of mathematics. He was always playing games and telling me things, which he realized were scientific, bearing relation to science.

Weiner:

This pattern recognition training, you say, was in your high chair, so this was very young?

Feynman:

Very young, so it's not in my memory. It's been told to me. But then there was something else that I do remember, because when I got into kindergarten, which is of course much later, six years old, they had a thing in those days which was weaving, so called. They had a kind of colored paper, square paper, with about quarter inch slots made parallel, a quarter inch wide, and then quarter inch strips of paper. See, one was the

woof, and the other was — whatever it is, warp. You're supposed to weave it and made designs. You see? That was regular and interesting. Apparently that's extremely difficult for a child, and I was especially commented on. The teacher was very excited and surprised. I had no difficulty. I made elaborate patterns correctly by doing this thing, without any difficulty, whereas it was so difficult for most other children that they don't do that in kindergarten any more. So apparently I had learned something already, back then.

Weiner:

Did you do any drawing in this period? Do you remember any?

Feynman:

No, I was no good at drawing, ever. I don't think I was. My father was fairly good at it, and he would try to show me some things with drawing, much later. I don't know the dates, but I remember him explaining to me how to make letters by dark on the one side — the light comes from one side. You know, like sign letters, or they looked like the gold letters that they used to glue up against the butcher stores. If you just imagined that there was a strip of letter there, and just colored it on one side, from the way the light would have caught it, it gives that effect. And so on. He'd show me tricks in drawing, but I wasn't ever very adept at drawing. I didn't pick that up.

Weiner:

Let me ask at this point whether there were any brothers or sisters in the family?

Feynman:

There was a sister. There was a brother that came after approximately three years. Or maybe I was five, four, six, three, I don't remember. But that brother died, after a relatively short time, like a month or so. I can still remember that, so I can't have been too young, because I can remember especially that the brother had a finger bleeding all the time. That's what happened — it was some kind of disease that didn't heal. And also, asking the nurse how they knew whether it was a boy or a girl, and being taught: it's by the shape of the ear — and thinking that that's rather strange. There's so much difference in the world between men and women that they should bother to make any difference, a boy from a girl, with just the shape of the ear! It didn't sound like a sensible thing. Now, I remember that I had another, a sister, when I was nine years old, so it's possible that I'm remembering my question at the age of nine or ten, and not at the age of the other child, because it sounds incredible to me now that I would have had such a deep thought about society at the earlier age. I don't know. I can't tell you what age I was, but I remember that, because it was an interesting answer. I couldn't understand it

really. They make such a fuss — everybody dresses differently; they go to so much trouble, their hair different — just because the ear shape is different? What sort of an answer is that?

Weiner:

There was a nine year difference, then, between you and your sister.

Feynman:

Yes, then my sister came later, when I was nine years old.

Weiner:

I see. Any other brothers or sisters?

Feynman:

No. No. But we lived in this house where there were cousins, and we lived together, so it was very much as if it was brothers and sisters.

Weiner:

How many children in the cousins' family?

Feynman:

There was one three years younger than I, a girl, and a boy three years older than I. We played together.

Weiner:

How long did this go on, living with the cousins?

Feynman:

Well, I don't know. Did I live with the cousins before? Yes. First there was a period when we lived together in this house when I was young, for a few years. Then when I was maybe ten, we went to this town of Cedarhurst, where we lived separately for a while, till I was probably eleven. Maybe we were there two years or possibly three. Then we went back to Far Rockaway again, and again lived with the same cousins, and that was for a rather long period.

Weiner:

Did you remain in that same house during this period?

Feynman:

I can't tell you. I don't think so. I don't know.

Weiner:

Do you know the relationship with the cousins? Were they first cousins?

Feynman:

Yes, first cousins, the children of my mother's sister.

Weiner:

I see. Do you remember what the father of that family did for a living?

Feynman:

He had a man. What did he do for a living at that time, the father? I don't know. He worked in a shirt business for a while. My knowledge now is that the situation in the family was that my mother's sister was a very superior woman, full of intellect, liked plays and reading, and that she married a man whom she thought was of her intellectual height, you know, and so on. But he was a rather dullish, uninteresting, not very able man. So rather than his developing for her a fine home, with the finances and so on, they had troubles, and he had always relatively mundane jobs in shirt factories, or maybe as head of shipping in a shirt business or something of this kind. So there was a certain problem there. I think both of the mother's married — didn't have too good financial luck. The husbands weren't as successful businessmen as the father had been.

Weiner:

That perhaps explains the family set-up.

Feynman:

It does explain why they lived together, definitely. You see, the house was left by the father of the two girls, my mother and my aunt, as part of his estate. Oh, in the first period, he lived there, and so did the mother of the girls. My grandfather and grandmother lived there. My grandfather used to sit me on his lap and give me half a dollar every once in a while. I remember now. And then he died. Then the grandmother

was still there, and she was ill always. She'd lie always on a couch, around, until she died. So that was the first period in that house. Now I remember.

Weiner:

I see. Then you're saying that the grandparents and the two daughters, with their families, at one period lived there together.

Feynman:

That's right.

Weiner:

It must have been a big house.

Feynman:

It was a very big house. Yes. And there were maid's rooms upstairs and so on.

Weiner:

Were there maids?

Feynman:

Yes, there were maids, in the kitchen. And then later on, when — in the later period there was a couple who lived there, who took care of the house, when I was nine or ten. The second period. Let's give these things two names, the first and second period, because there was an interval in between.

Weiner:

There were four children in the house, then.

Feynman:

Yes. Possibly more. Possibly. My memory is not good. There were other cousins, which possibly lived there part of the time. Because I remember them as fairly close for some interval, but I don't know. That would be earlier. The later period was definitely just the two families.

Weiner:

What about books around the house? Your father had these interests. Did he have books at home?

Feynman:

There were books. My father had books. He didn't have scientific books in the conventional sense. He would have books of the kind — not this book, I'm making it up, but something like DEVILS, DRUGS AND DOCTORS, or MEN, MEDICINE and — something. You know this kind of thing. That level of books. In the second period, at the age of about eleven or twelve, something like that, I found, up in the attic, an algebra book that my mother or my aunt had had in school, and I started to work from that. So there were books. It was possible. Why was I interested in algebra?

Weiner:

Yes, why did you use that book?

Feynman:

I don't know, but see, I was interested in the mathematics and the science earlier. I've missed the — the timing has got shot now, because we're talking about books. But my father, you see, interested me in patterns at the very beginning, and then later in things, like we would turn over stones and watch the ants carry the little white babies down deeper into the holes. We would look at worms. All the time playing — when we'd go for walks, we'd look at things all the time, and then he'd tell me about things of every kind. The stars, the bugs, geometry things, and so on. He was always telling me interesting things — the way birds fly, the way ocean waves work, or something, you see, the weather. I don't know why, any more, but there was always talking about the world, from every angle. Not just mathematics or anything like that, but the whole business he was interested in, and he was always telling me things. So he therefore developed somehow, inside me, more or less naturally, an interest in anything rational and scientific. So I knew, by the time I was being put to bed at night (because I'd been pretty good at arithmetic by this time, you see, and I'd heard about algebra), I asked him what was algebra? He said that it had to do with doing problems. This I remember exactly: "It's a way of doing problems that you can't do in arithmetic." I said, "Like what?" He said, "Like a house and a garage rent for \$15.00. How much does the garage rent for?" I said, "But you can't do that at all!" He left me with that answer. He didn't tell me what algebra was. But that's the kind of relation. So I knew that algebra was something interesting, and therefore, I was somewhat frustrated in discovering what it was. I'm sure that it would be perfectly natural, if I found a book marked algebra, to become quite interested in what the heck it is, you see. I don't know what I did with that book. I mean, I can't remember finding the book. I probably got it at too early an age. I can't remember what I did with it.

Weiner:

Did you go on any trips during that second period?

Feynman:

No, we didn't travel much. In the summer we would go to the beach.

Weiner:

That was, of course, nearby.

Feynman:

Yes. That's right. We didn't travel much. I think we weren't too well off. In fact, I know at that period my father was making \$5000 a year, because he let me take the check to the bank, and it was about \$100 a week. So I used to take the check to the bank.

Weiner:

This is the twenties? That second period puts you in the twenties?

Feynman:

The thirties, something. I was twelve or thirteen. 1931, 1932. It's in the thirties.

Weiner:

In the Depression period.

Feynman:

Yes. That's right. He was in trouble. That's why we probably moved back there. There was probably trouble between the two families, a little bit. I guess. I don't know. It's circumstantial. And then they moved back because of still higher financial difficulties and it was the only solution. I can't tell you, but it sounds logical now. I never felt poor. I never felt poor. I never felt anything. In fact, I remember distinctly, when I was going to the bank — you see, it was very good. My father knew how to teach me. He said, "You take the check to the bank." This way I knew how much money was what. And I remember thinking to myself that that's a very nice amount that everything was all right, we lived fine, and that my ambition would be to earn that much money. So I knew I wanted about \$5000. That's all I needed. It's interesting — I remember these things, now

that you ask me to tell about it, I recall all these little dopey things!

Weiner:

They all fit together. Did you ever get into Manhattan to do anything in the city?

Feynman:

Yes. My father would often take me, for instance, to the museum. The Museum of Natural History was a great place. We would go look at the dinosaur bones and all this stuff, you know. It was great. See, I can remember — I remember my father talking, talking, and talking. When you go into the museum, for example, there are great rocks which have long cuts, grooves, in them, from the glacier. I remember, the first time there, when he stopped there and explained to me about the ice moving and grinding, I can hear his voice, practically. That's the kind of thing he would do: he would explain all this stuff, you know — that this ice moves; it looks solid, but it moves, with this tremendous weight and all this height, gradually, year by year, an inch every year, grinding and pushing — and so on. He described vividly what was going on. That's the kind of thing. Not just "This is glacier cracks," you see — quite different. I mean, the ice had to be felt, as you saw the top of it. Then he would tell me, "How do you think we know that there were glaciers all over the world at a certain time?" See, after we would look at it, he would say things like that — "How do you think anybody knows that there were glaciers in the past?" So I said maybe — I don't know, I don't remember exactly. But this is the kind of thing he would say when we were all finished, and then either I would be clever enough to say, "Well, that's the way we..." or I would say, "I don't know." He'd point out, "Look at that. These rocks are found in New York. And so there must have been ice in New York." He understood. A thing that was very important about my father was not the facts but the process, the meaning of everything, how we find out, what is the consequence of finding out such a rock—with a vivid description of the ice, which was probably not exactly right. Perhaps the speed was not an inch a year but ten feet a year. I never knew. He never knew. But he would describe anyway, in a vivid way, and then always with some kind of lesson about it like, "How do you think we ever find these things out?" And there, of course, was standing in front of me, the method. But that's the kind of guy he was. So it's not hard to understand I've been interested in science. It was very good.

Weiner:

Was this a sort of regular thing, or was it spontaneous?

Feynman:

No, it wasn't regular. I don't think it was regular, but it was fairly often.

Weiner:

And you still think here we're talking about the same period, about aged 12 or 13?

Feynman:

Yes.

Weiner:

Before high school?

Feynman:

Oh yes, definitely before high school. Definitely. Earlier, probably. Probably quite early — probably very early. Probably he took me to the museum when I was quite small, and then at other times. I know that my cousin — another cousin — not the one we lived with—lived in New York, and invited me to New York at one point when I was a young kid maybe twelve, I don't know. She's six years older. Something like that. And she invited me to New York. It's a famous family joke, you know — she thought she'd show me New York and the buildings and everything else, and as soon as I got there she said to me, "What would you like to see?" "I want to go to the museum!" was my answer. And so she had a dull day, what she called a dull day, taking this young squirt through the museum. But that was what I thought New York was. The only thing that was good in New York. Because I'd always gone to the museum when I went to New York, and I loved the museum.

Weiner:

"New York is the Museum of Natural History surrounded by water."

Feynman:

Exactly. Yes. Right. Right.

Weiner:

When you went, did you ever go with your sister? She was younger, of course.

Feynman:

She was nine years younger, and she was too young.

Weiner:

That's right. How about the cousins.

Feynman:

She may have come. No. No.

Weiner:

So it was just a father-son thing.

Feynman:

It was our family. The science didn't spread to the other family, but it did go into the sister. The sister is now a Ph.D. in physics.

Weiner:

Your younger sister?

Feynman:

Yes.

Weiner:

Where did she did get her Ph.D.?

Feynman:

She got it at Syracuse University. She got her BS at Oberlin, Ohio. Now she's working for Ames Research in — space, something.

Weiner:

Is she married?

Feynman:

Yes.

Weiner:

What is her name?

Feynman:

Joan Feynman — Joan Hirschberg.

Weiner:

That's very interesting. Evidently your father's influence spread.

Feynman:

She said that it was because she would overhear us talking, and then she would ask me things, and I would explain it to her. That's what she says. It wasn't so direct in her case.

Weiner:

What about school in that period? How'd you do in school? Do you have any recollections of it? This is pre-high school?

Feynman:

I recollect that I was fairly good in school, that I was also a good boy, except for some minor traumatic experiences and difficulties. I was always very upset if something went bad, if I was bad about something. I always tried to be a good boy. That's discipline. Also, I was fairly good in school, definitely above average, but in arithmetic it was very easy. It was too easy. For instance, when I was ten or eleven I was called one day from the class to a previous class that I'd been in, by a previous teacher, to explain to the class how to do subtraction. I had invented (they claim) a better way of doing subtraction than they were using, that she liked. She'd forgotten it in the meantime, and I was called from my class to explain it to them. Everybody says I invented, but I don't believe it. I believe what happened was that I learned that method of subtraction in the Far Rockaway School, and that in the Cedarhurst School they used a different scheme which was less efficient. It corresponded to crossing numbers out and reducing them out by one. I had a way of carrying the little one that you had to subtract, much as you carry in addition; whereas they would reduce the upper number by one, I would increase the lower number by one, when you had to subtract. And probably I learned that in the other school. But at any rate, I was already known as some kind of a whiz at arithmetic, in grade school.

Weiner:

And did your grades reflect this?

Feynman:

I think so. Of course, in arithmetic they probably reflected it very well. I think in arithmetic there was no question. But in the other subjects, I was pretty good but I didn't really like them too well. I probably was average or a little, you know, above average — 80 percent or something. Satisfactory in everything else, but very good in arithmetic. Not extremely good in everything else, no.

Weiner:

Were your teachers in arithmetic women teachers?

Feynman:

Yes, usually. In the later part, when I got back to Far Rockaway and I was [inaudible] arithmetic and so on, I had taught myself enough algebra — I'm just trying to remember, I'll never get the timing right or the order right — but back in the school, probably near the last grades, I had learned how to solve simultaneous equations, linear equations, like two equations with two unknowns, and I made up a problem with four equations and four unknowns. I made up a set of four equations, in A, B, C, and D, to solve. But I made them up — I know I did — I picked numbers for A, B, C, and D, and I made up equations which were right for those numbers, so I knew the answer. But that wasn't it — I took these equations and then I went through a formal procedure for solving them. You eliminate A, or whatever I did, I don't remember, but then you get three equations, three unknowns, and you get two equations, two equations, and you get one equation, one unknown, and there you are. Then I'd get one variable. But, in other words, I made up a problem and then wrote the whole formal solution, but I made up the problem so the answers would be integers, and not just arbitrary, and I gave it in to the teacher of arithmetic in the grammar school. She thought it was so impressive, but she didn't really know much about it, (they don't know much in grammar school) that she took it to the principal, who apparently did still remember a little algebra, who went over it and said it was correct, and signed her name and complimented me for doing that, being able to do that. I remember doing that. That's just to give you some idea.

Weiner:

Why did you do it?

Feynman:

Probably, at that time, to show off. I mean, I don't know, but presumably to show off. I don't know exactly.

Weiner:

Was it a home?

Feynman:

Yes, of course.

Weiner:

Not part of a homework thing?

Feynman:

No, it had nothing to do with the school.

Weiner:

Did you show it to your father?

Feynman:

I don't think so. I don't know whether my father would have been able to follow it. By this time I had taught myself something, about mathematics. He knew the ideas of mathematics, but I don't think he formally could do the things so well. I'm not sure whether he would have been able to do that, or whether I showed it to him. I just remember this. I also remember, near the end of school, when I was in the last grade, being curious about how you take square roots, and asking a teacher who was not a woman teacher, but a man — that's why I remember this — how you take square roots. He was pretty good. He taught science also. He was not really good, but —. He looked it up in a book, how to take square roots. He'd forgotten. He knew, numerically. Then he showed us the rules. And he was always throwing the bull. He was the biggest liar in the world. So he starts out to show us this rule. For instance, he says, you have 116, now you take the 1, take the square root, then you cut off two numbers — and he did it hesitantly enough, because he didn't know it very well. Then he told about doubling what you got, and dividing into that — oh yes, and then you have to add the squares here, and all this kind of crazy rules, if you know the rules. A friend of mine — I had a friend, who asked him, and he showed us, and it came out right. And we were sure that it was a fake that he had just fiddled around to make it work as he went along, because it was so crazy. So we went home and we fiddled to try to find out if it really worked. And sure enough, it really worked. We didn't trust it, when he told us the way of doing it. By that time I had a friend, by the way, who was as interested in science as I was, and we did much together. That was about twelve. Second period. Then we did lots of stuff together. We studied

together; we argued together, we did chemistry experiments.

Weiner:

Do you remember his name?

Feynman:

Bernard Walker.

Weiner:

What happened to him?

Feynman:

He got into business, so to speak — a sort of semi-engineering business, and now is completely business.

Weiner:

Did he follow through on the technical or scientific education?

Feynman:

No. I think it was family influences, the importance of money and business, so that he became more a salesman than an honest scientific man. A mixture. At first he had developed a process for metal-plating plastics, and done some research to develop it. Later, I got a job with him, from 1939, probably.

Weiner:

Metaplast Corporation?

Feynman:

Metaplast Corporation, right. I worked there. Let me tell you about that later, perhaps. But anyhow, that was his intermediate stage, when he was still partly connected. Since then he's gone entirely into business.

Weiner:

Did he stay with you through high school?

Feynman:

No. No, he was not in high school with me. It was grammar school.

Weiner:

Do you have any other recollections of that period? Do you think of anything else that may be significant?

Feynman:

Quite a bit, yes. In the first place, I remember, in the period between when I was in Cedarhurst—this has to do with science, now; there's lots of stuff, it's infinite — but when I was in Cedarhurst, the first time I came there, I had to meet the new boys. You know, you move, and you have to get along with the new boys — you know. I had a thing that they had never seen, which I had discovered by accident. The clothesline of the day, if you light it and then blow it out, smolders red like the end of a cigarette, and it goes on and on. So I used to use it as a clock, to know when to come home, by marking it off in ink. And I carried this thing around. When I got to Cedarhurst I still was doing this particular dopey thing, and the kids saw this thing, and they said, "Oh, that's a great idea." And they all had them, which they used to burn each other, and this kind of shocked me and disturbed me. It was the beginning of the whole thing, you know. But I remember distinctly being upset by the fact that they didn't use the thing in a good way, but they were using it, not the way I meant, see. Incidentally, I discovered also about that time that the device was not as good as I thought, because I put it on my bicycle and drove around, and because of the air blowing on it, it burned too fast. So I discovered a fault in the design of this clock! Anyway, at that period I was very interested in science, during the period when I was at Cedarhurst, which was nine, ten, eleven. I really developed a lot of science. I did lots of experiments. I remember bottles of water with oil would separate — dumb things, you know, just looking. Then later I gradually began to do experiments. I had chemistry sets. Also I acquired a friend whose name was Leonard Mortner at Cedarhurst, whom I've known for rest of life. Accidents were such that we were always together. He lives out here now and I see him once in a while. He's an engineer. He taught me things that I didn't know, and probably the other way around. He taught for the first time, really, about atoms. My fathered probably told me a few times but I didn't remember. He explained to me about water — you can keep breaking it up and breaking it up and breaking it up, and at last, if you break it at the last, to the end — I said, "Why not?" He said, "Well, it turns out there are little particles." I remember him telling me about atoms, so I learned. And then he knew about chemicals enough to know. You see, we had a chemistry set. There'd be a little box full of what might be calcium oxide. Then you'd see in the catalog — you get such a box, costs 10 cents — he'd discover that calcium oxide is the same stuff that they use in mortar for buildings, you know, lime. So we would go in the night and sneak and get a bottle, a

mayonnaise jug, full of that stuff, which was at least worth a few dollars, you see. So I had a chemistry set which had a lot of little boxes, and then big bottles of calcium oxide, borax, and other things which he had found out from his brother were common objects. (I'm just trying to remember things). I also was interested at that time in electrical things. I bought a pair of earphones from some man, to do some experiments. They didn't work, the earphones, and so I took them apart, and saw the reams of wire. You know, it was a kind of child, not really — ruining sometimes, and fooling around sometimes, and sometimes making something. I remember other things, as long as we're talking about it. I remember, at one point, I got in my mouth one of those chemicals from the chemistry set. And I was horrified. Might be poison, you know. Whereas my friend Leonard said to me, "Don't worry. Probably tastes a little sour and salty, doesn't it?" I was amazed at his brilliance, because that's exactly how it did taste. It was sodium hypo-something. Hyposulfite. It was in fact partly acid and partly salt. He knew that all salts taste salty, but I didn't know it. You see, I thought salt that you had on the table tasted some way, and that the other salts didn't necessarily taste the same. But he knew enough. Apparently he knew quite a good deal.

Weiner:

Was he older than you or the same age?

Feynman:

He was not much older, if he was older.

Weiner:

But he wasn't in the same grade in school?

Feynman:

No, there was one year's difference between us.

Weiner:

So you knew him from the neighborhood rather than as a classmate?

Feynman:

Yes. Anyway, in that period I was acquiring a great deal of scientific knowledge of a childish kind. My father bought me a telescope, and I played with that, made tricks with lenses and images, and little fooling's-around. Then there was an experience in which science came in, in a funny way. I was not good in athletics. This always bothered me

when I was a kid. I felt like I was a sissy. I couldn't play baseball, which was to me, at this childish age, a very serious business. I had trouble learning to ride a bicycle. I'd sort of cry because I couldn't do it, you know. Then finally I could do it. I was a rather weakish sort of a child. That's also important, because I was worried when I went to the new neighborhood, whether I would be accepted by the kids. So that I remember — that burning thing was important, that they liked it and they used it, so I was in, for a while. Every once in a while I would get kicked out of the group. We had a hut, for instance—there was a little group — and I'd get kicked out for something. But each time I'd get kicked out for something, I'd invent something to get back in—like a periscope for the hut, or a design for a second story, or something. Anyhow, to get back to the other story, I would go to school, and at some period some bullies began to have fun. When I would come into the play yard, they'd hit me, knock me over. One would stand behind me, say, "What are you doing here today?" — poom, you know. And I didn't know how to handle it. I couldn't handle it. It was really quite a miserable and unhappy experience. But about that time, approximately that time, I overheard some kids talking as we were going into the school, older kids from a higher grade, and one was saying to the other, "Rust? Rust is iron chloride." Well, it was natural for me to realize that what wrong right away, and say, "No, excuse me, rust is iron oxide. Oxygen comes from the air and it mixes with the iron and it makes iron oxide." These kids eyes popped out and they asked me a lot of other questions. They were seniors, you know, in a higher grade. So the next time I came into the courtyard and these guys started to fool around, these other fellows came over and stopped them, and said, "No, we want to talk to him," and so on. They talked to me quite a bit, and they would talk to me quite a bit. You know, I was competent, with their science. Actually I was probably making it easy for them to pass the questions. But they got interested in me, and that stopped the other guys, and everything was all right after that.

Weiner:

Was this a junior high school?

Feynman:

No, this was grammar school. It was sixth, seventh grade, something — no, fifth year. Fifth year.

Weiner:

They were probably in their eighth?

Feynman:

Sixth, seventh, probably, yes. I was in the fifth. I was in the third, fourth and fifth in that school. There are a lot of things. Let me tell more, now I remember. When I was in

arithmetic class, we were learning for the first time presumably — I think for the first time — decimal fractions, and we had problems. One of the problems was $3\frac{1}{8}$. I wrote 3.125. Then it hit some chord in memory and I wrote “equals pi, equals circumference, ratio of circumference is equal to diameter of a circle.” The teacher came by and looked at this and crossed it out, and said, “No, pi is 3.1416.” I remember that. My father taught me about pi before I had learned in the school the decimals for fractions, and explained decimals. See, I was really ahead in arithmetic. I remember him telling me about pi as a great and marvelous mystery. Everything was always dramatic — that all circles have the same ratio of the distance around to the distance across, and that this number, this strange number, is of very great significance, and is a marvelous number. So pi was like in gold letters, you see. So I thought I’d hit pi again at last, in the school, when I saw the decimal, but it wasn’t quite right. I just say these things to give you some idea of the relationship of what I knew from the home to what was going on in the school.

Weiner:

And in the neighborhood, as a matter of fact. You managed to relate it to that.

Feynman:

Yeah. Yeah.

Weiner:

The Cedarhurst period, then, seems to have been a very fruitful period.

Feynman:

Yeah, that is when I really began to change. I had had a chemistry set even earlier, when I was in Far Rockaway, in another house which I have forgotten now. My father gave it to me for a birthday present, or somebody gave it to me for a present — the earliest one. What happened to the earliest one was, it was too early. I played around with a little bit, and a lot of the kids from the neighborhood came, and I showed them the chemistry set. They were big boys, and they took the thing outside, while I stood more or less helplessly by, and they mixed everything with everything else and poured it on the sidewalk and tried to put a match to it. I stood far back, because I was afraid it would explode. It was full of water, of course, it wouldn’t explode. But I still remember that it was brownish and made bubbles, when you mixed everything together. But that was not the way I thought to do things. This, to me, was a kind of destruction. This was not the thing that a chemistry set was good for. You see, I never played that way. I never played chaotically with scientific things. I realized that their real value was in doing something carefully and watching what happened, and it was real pleasure. It was worth it. And this

other stuff was just a terrible, silly way to behave. But anyway, I was too young then to want another set or to do anything with it. In those days I just played. But in Cedarhurst is when consciously the science really developed, until I was doing a lot of things.

Weiner:

You didn't mind playing with someone else if he would take the same approach.

Feynman:

That's right, of course, like Mortner, for example, yes, sure. We played a lot together.

Weiner:

Before you got to high school, let's take a bit more of that period.

Feynman:

All right. Let's put us back in the house in Far Rockaway, the second time. By this time, having had this Cedarhurst experience, it was not long after I was in that house that I got a laboratory in my room. I had had, in the basement of the other house, a sort of lab, here I tasted the chemicals and—oh yes, we also played a trick on my mother there. We put sodium ferrocyanide — sodium ferrocyanide? — or something, in the towels, and another substance, an iron salt, probably alum, in the soap. When they come together, they make blue ink. So we were supposed to fool my mother, you see. She would wash her hands, and then when she dried them, her hands would turn blue. But we didn't think the towel would turn blue. This was all in the Cedarhurst era. Anyway, she was horrified. The screams of "My good linen towels!" But she was always cooperative. She never was afraid of those experiments. The bridge people, the bridge partners, would tell her, "How can you let the child have a laboratory?" This was later, too. And so on — "And blow up the house!" — and all this kind of talk. She just said, "It's worth it." I mean, "It's worth the risk." She understood that it was worthwhile. She didn't like it that we ruined her good towels, at all. But we tried to get the stain out, we boiled them, we did everything. It just turned yellow from iron oxide.

Weiner:

Then she encouraged your father's help to you, too.

Feynman:

Oh, yes. Oh, yes. OK, I'm supposed to get back into the house in Far Rockaway, second time? All right? So now, I'm getting a real laboratory, a better one, in my room. My

father got an old packing case from a radio, a big thing about five feet long and three feet high. I started actually by making things out of erector set motors and so on, and playing. He had said to me, "Electrochemistry is a new field, very important field, so what's electrochemistry? It's the action of electricity on chemicals." I still remember. See, I loved my father. Everything he told me was great. I still remember having a little pile of some chemical; I can't remember what, from a chemistry set, and taking the two wires from the electric plug and putting them into the dry chemical. Nothing whatever happened. But that was my first to discover what electrochemistry was — see; because he'd told me it was a big deal. See, that kind of stuff. Anyhow, we got this laboratory, from these beginnings, which were on the floor — junk was piling up, and then when we got this lab, we put everything in it. I had this laboratory for years, even when I was in high school. I kept it. We moved it and everything. In that house, I was still not in high school, but I was developing electrical interests. Crystal sets — I had a crystal set in the house that I'd used to listen all night to "The Shadow," and ENO Effervescent Salts were being sold on the radio, and all this kind of stuff. Then, by buying in rummage sales, I would gradually get one tube sets and things like that, which I would fix, and connect the battery that I had — this old storage battery.

Weiner:

How'd you know how to fix them?

Feynman:

I don't know how I learned all this. It's probable that what happened is — you see, they probably were not broken, in. an unobvious way. They were probably broken by, when you look inside there's a wire hanging off here, there's a connection. It began that way, you see, and then as you got deeper in it, you got more and more involved, and you learned more about it, you see. That's the way the thing is. At the beginning I would fix, if I could, simple things in a simple way, and so on. I'd be very excited if it would work. Then I would try to get distant radio stations with this thing, and sit up all night, and have speakers and electrical things. Then also, I guess I did — it was electrochemistry — I found out that if you put the 110 volt line through an electric light bulb, so you don't blow a fuse, through water, it would boil the water. Oh, we used to develop pictures in those days, too, in our basement.

Weiner:

Who's "we" in this case?

Feynman:

You know, the cousins, and so on. We developed pictures. I remember using the developing trays, which were waxed, so that they were insulated, putting water in them,

and boiling it — and watching the most beautiful phenomenon at the end, when all the water boils away, and the last bit of water, it's dry, is making sparks, because it's breaking the circuit. And the sparks move around, because it breaks here, but the water flows, you see, and it flows here and connects, and then it makes another spark here, and finally, these lines of salt, and beautiful yellow and blue sparks! It's a very beautiful thing. In fact, now that you remind me, I think I'll have to set one up and see what it looks like, after all these years. I used to boil water all the time with this thing.

Weiner:

Did you enjoy working with your hands?

Feynman:

Yes. I did like this laboratory very much. I worked all the time. I connected the whole house up with wires to my lab, so that I could plug in with my earphones to my radio upstairs, anywhere in the house. Then I used that system in many ways. I would also pipe power down, and you could put a loudspeaker somewhere in the house, and go upstairs and broadcast from it, and all this kind of stuff. I found a way to connect into the radio, and so on. I was really learning — I mean, I'd begun to know something already.

Weiner:

We're now in Far Rockaway. How did this relate to the science courses you took in school?

Feynman:

They weren't related yet. When we did finally get a science course in school, which was in the eighth grade, I was considerably beyond the course. In fact, I had lots of trouble, because I remember, my friend and I — the man drew on the blackboard (I still remember, you know, he's going to explain how a projection system works, you know, the projector that makes pictures on the wall) — so he drew a light bulb, and he draws a lens and so on to explain. Then he draws lines coming out of the light bulb parallel, the rays of light going parallel to each other. So, I don't remember whether it was I or my friend, but one of us said, "But that can't be right. The rays come out from the filament radially, in all directions." I don't know if I used the word "radially," but anyway, we explained. He turned around and said, "I say they go parallel, so they go parallel!" Well, this didn't sit well with us, because I knew, certainly, that no matter what he said, the rays didn't go parallel. But that was the level of the class. We were ahead of it, and we knew the errors in everything that was taught. I never learned anything in the class, except that a meter was 39.37.

Weiner:

You remember that?

Feynman:

Yes. He was extremely good at teaching memory. He had a little side course in which he taught memory. He had a clever little sub-thing; I remember distinctly, because he said, "Now we're going to remember how many centimeters in a meter." You see? He said, "Now, a number is hard to remember, ta-ta-ta" — he would explain something. It was Public School No. 39, so that helped with the first two digits, and I don't remember the rules for the second digits, but I remember 39.37 was the number.

Weiner:

39 was the number of the school, you remember?

Feynman:

That's right. It helped to remember the 39, you see, so you could remember that easily. I don't remember how we remembered the 37, on top of that, after the 39, but he had some gimmick.

Weiner:

Do you recall how you reacted to that course? I think right here you're demonstrating that you have a magnificent memory. This could be through association and through other things.

Feynman:

Of course. The man himself as apparently — as I look now, with older eyes, I can interpret — but the man at that time was a sort of a heavyish, loud, kind of difficult fellow, but he had interesting things about memory. Because for a child, even if it's wrong, it's fascinating. I mean, he can make it interesting. So he was an interesting character. But we had to call him Major Connolly, because he had been a major in the First World War, and so he still had his title, you know. So we called him Major Connolly, without it bothering us any, because we didn't make any significance of it, but it was important to him, apparently. He did try to teach this thing. Nobody — you see, the thing is, you have to realize, it's an actual desert. The school is absolutely empty. Look, that little thing in arithmetic, four equations, four unknowns — the principal had to look at it. Nobody knew anything. And he knew more than anybody, but not much, see. But the thing is, he was a liar. He would tell stories about when he was in the war. He was in an airplane, and he helped the man invent the machine gun which shot

between the propeller blades as the blade went around, because first they used to shoot the propellers off. And he was in the plane when they were making tests, and something went wrong, and they shot the propeller off, and as he was coming down, the wing fell off, and all kinds of stuff, he would say. I don't know whether, at the time, I believed it or didn't believe it. He was difficult. One had to be careful with him. He wouldn't tell you anything right. I knew that.

Weiner:

You knew it at the time.

Feynman:

Yeah, I knew it at the time, because the scientific things, they were cockeyed. And also the stories were often incredible. Incredible. Amusing but incredible stories about his exploits in the war — he was such an important character, he was always in the forefront of everything, a very important development, like the machine gun. And then to save himself, after all the terrible things that happened to the airplane! It was impossible. Shouldn't we have some coffee?

Weiner:

I'd like to — it's very interesting, just the one final point — you had confidence that you knew —

Feynman:

— why don't we talk while we have the coffee?

Weiner:

All right. You had confidence in what you knew, at that stage.

Feynman:

Oh, yeah. Oh yes.

Weiner:

Was this self-conscious confidence? Did you realize that that's what you had?

Feynman:

No. You see, it was confidence because, it was true. I can't explain it to you. It wasn't that I knew that I was smart. It was just that — you see, scientific things are rationally right. It isn't as if I had learned history, and someone would then tell me that the fact I had memorized was wrong, and I would have confidence because I knew I'd looked it up in the book. But the fact that the light had to come from the filament radially, it never even seemed to me as a fact that I had to learn. It seemed to me, from my other knowledge, sort of obvious. How the hell can it come out parallel? There isn't any filament up there, you see; the filament's all here. I don't know if you can understand it.

Weiner:

I do, I do.

Feynman:

The thing was not a matter of confidence in knowledge, but simply an understanding of the world, a world view by which this was evidently false. That's all.

Weiner:

But you can relate it to other things.

Feynman:

It wasn't a matter of confidence. It was just a matter that this was obviously cockeyed. I can't explain it to you. It just seems to me it was wrong, it was clearly wrong. See, I'd already got the idea of what I look at, what I see. That was certainly brought in from my father directly — that things I see, the things that are seen, or the things that even are not seen, but if you look at it very carefully — that that's the way to know what's right, and that many people have said things, without looking, that were wrong, because they didn't look. That aspect was vital, of course, to science. And that I knew. I knew that people, authorities, often stated things that weren't true, because they didn't know, and they would just say it. So this was just one of those things to me — I mean, somebody claiming he knew this, and he had to say it. I knew what he was worried about, I'm sure. I mean, I can't remember for sure my feelings, but I believe I knew, that I had the feeling, and my friend had the feeling, that he was just upset because in front of the whole class we caught him, and he probably would even have to admit that he — If he didn't want to try to defend himself, he would have admitted. It was obviously right. I think we both understood that he was defending himself against criticism, by just simply saying, "It's true." I'm not sure, but I believe that.

Weiner:

That's a mature attitude, but not too mature for the age.

Feynman:

There were two of us, and we would talk about it.

Weiner:

That was the extent of the science, then, in the grade school, just one course?

Feynman:

Yes, it was one big general science course.

Weiner:

Was it a year or a semester?

Feynman:

No, no, it was just one day a week — the science class. The girls didn't go to it. The girls had some other thing. We had shop and science, and they had cooking and maybe dressmaking or something, sewing or something. It was one of these small bits of science. Oh, there's a very important thing I forgot to tell you. I was going on about the last year, approximately the last year in the grammar school. I had gotten pretty good, see. I mean, I'd done these things and I was very interested, and I knew a lot about things. My dentist was to me the scientist in the town — that is, the professional man, who had something to do with science, you understand. So I looked at him as though he were a scientist, because I knew no better. I would talk to my dentist about many things. I would get a rash on my face or something, or some tooth thing. I would ask details, and he would give me details, tell me all about how the teeth worked and how the rash worked, as much as he knew, and he was a source of information in science. Because I asked so many questions, he realized I was quite interested, and so on, and he had, as a patient, a teacher in the high school. He told the teacher in the high school about this kid. So the teacher in high school, whose name was William LeSeur, said, "Let him come around after school one day a week to the chemistry laboratory where I teach and he can play around and so on" — you know, in the laboratory. Help out. Assistant. "Lab assistant, while we clean up," and so on. So I used to go once a week to the high school in which Mr. LeSeur taught. Also another man who was there, Johnson, took a great interest in me — particularly Mr. Johnson — and let me have things to play with and do experiments. You know, he'd teach me stuff, we'd do some experiments, I'd clean up apparatus, and we'd talk about how things worked. I learned a great deal there. This was while I was still in grammar school.

Weiner:

How long did that go on?

Feynman:

Well, during the whole time — probably about nearly a year or maybe it was a half a year.

Weiner:

Your last year in grammar school.

Feynman:

Yeah. I remember that just suddenly, because I remember, in my other science class, with Major Connolly, I gave a demonstration of electrolysis to the kids, for which I borrowed electrolysis apparatus from the high school, and carried it through the streets on my bicycle. I thought it was the most expensive, remarkable piece of apparatus. Did you ever see them? It was the standard equipment, like a big H, you know, with graduated things and two stop-cocks — yeah. And then with platinum electrodes in the bottom. That, to me, was — ooh! — very valuable and marvelous. I carried it as best I could through the streets on my bicycle, and took it to make the demonstration. But I pushed the cork up, that held the electrodes, too hard, and broke the glass. I was terrified and upset by this. This I remember very distinctly—it was very serious. I tried to wax it closed. It was leaking while the demonstration was going on. When I took it back to the guys in the high school, I was rather upset, unhappy — I thought it was so valuable. But they didn't seem to think it was so bad. It was all right. You know, those things happen. And so nothing bad happened to me.

Weiner:

Did you consult with Mr. Johnson and Mr. LeSeur? I mean, did you have an opportunity to discuss specific problems? Did they try to interest you in new things?

Feynman:

Well, we talked about all kinds of things that would come up. They'd be talking to each other and I'd overhear. Or, I'd ask those questions that they may or may not be able to answer. I remember one particular question — it's the only one I remember, because they didn't give me a satisfactory answer. Or maybe they did. Anyway, I remember the question. "If everything is made out of atoms, and they're always jiggling and moving about, how is it that something that you find that's very old — like, you make a screw, now, and you leave it alone a long time, it still has the sharp corners of the screw. Or you

make sharp things. How do the sharp things stay sharp for all this period of time, if the atoms are always jiggling?" In answer they told me about the fusion of metals, that if you put gold and silver together there is a gradual mixing. But I didn't feel that that was an answer. I mean, that's the kind — of level at which the whole thing was. I would ask questions. They would answer something, and so on. I was learning a lot. I can't remember specifically what. I certainly learned a great deal. I saw apparatus, I did experiments —

Weiner:

— apparatus in that environment, a laboratory environment, rather than in your own home-made one.

Feynman:

Yeah. Also, I made a discovery, accidentally, by the seventh grade of school, the last but one. One day I was playing with my various pieces of equipment. I had a microphone, I had loudspeakers. I'd go to more junk and rummage sales and get junk. I had a loudspeaker that had no tube just the unit. I also had earphones. I had this system, connecting all over the house. I would try the earphones around and the speakers and so on. I had them both connected into a double plug, like the one over there — I mean, two-way plug. I was going to plug into the line, to listen, but I had them both connected. So therefore they were connected together. All right? Now, while I had my earphones on, I had my finger accidentally poking in the hole of the loudspeaker and jiggling, touching it, making a noise — and I heard the noise in the speaker. You see, the loudspeaker had a permanent magnet; when I shook the thing, I generated a current. So I had a telephone. I mean, I discovered that I could hear that. I'd discovered a kind of microphone. I hadn't had a microphone before. I made all kinds of experiments. I would talk from upstairs to downstairs and so on. I remember this particularly, because in the history class we were learning about the telephone, and I said I'd give a demonstration of the telephone. So I got a long vacuum cleaner cord and two plugs on each end, and plugged my loudspeaker to one end, to talk into, and the earphones at the other end. I went out of the room, you know, and had the wire running under the door, and listened, and somebody said something, and I came in and told them what they said. It was mine, I'd invented it, see? But I know now, as a matter of fact, that that was the design of the first kind of telephone.

Weiner:

The Bell one.

Feynman:

Yeah. Yeah. That's the way it was in fact. If I had known that (I didn't know that) I

would have been even better in my history class, because I explained that my telephone was not really the right kind. I knew that it wasn't. But I didn't know that it was in fact historically the right kind.

Weiner:

In a couple of cases, you volunteered, then, to do extra things, demonstrations?

Feynman:

Well, that was probably the result of the teachers. You know how the teachers try to involve the children. They would say, "Now, you have projects for this and that" — you know. I don't know how, whether I volunteered or — I can't remember, all I remember is doing it.

Weiner:

It was not at all unusual?

Feynman:

No, I don't think so, there were different —

Weiner:

— other children would bring in something?

Feynman:

I think so. In the same period, before I got to high school, when I was in this Far Rockaway house, my cousin, who was three years older, had gotten to high school and was studying algebra. And he came home — see, he had a great opportunity, he could study algebra. So he came home, and he wasn't very good. Or maybe he wasn't so bad, but anyway his mother always thought the poor boy needed help. She probably overdid it. But anyway, the algebra was a terrible business, and so she tried to help him with the algebra. I remember the first day when he came home and he had this algebra business, and his mother was trying to help him. I sat quietly on the porch where they were talking about it, listening with all ears, trying to understand it. After his mother went in for some reason or other, I said to my cousin, "What is this algebra? What does it mean, that X? What does it mean $2X + 3 = 12$?" No, let me make a good problem, 11 — "What is $2X + 3 = 11$, what does it mean?" He'd say, "X is some number that you don't know, and 2 times the number plus 3 equals 11." I said, "You mean the number's 4?" He said, "Sure it's 4, but you did it by arithmetic. You have to do it by algebra." I've always

pited the poor fellow. I don't know what my reaction was at that time. But whatever way I did it, I wouldn't give a damn — I know I didn't care — to find out what way you had to do it, because it seemed to me, if I did it, I did it. But he was forced in the school to have to try to find a mystic way to do it, which I now know what it is. The mystic way is to write underneath, "Minus 3 equals minus 3" — we add, you know — or 3 equals 3 — we subtract — because equals subtracted from equals are equal, and then — And all this formal business: that way, he was getting thrown off the wagon, because he understood as much as I did on Page 1, which was X with some number and you have to find out what it was. But he was terrible in algebra and flunked most of the time, because there's something the matter with that. I'd learned already something about education, because I tutored later, and to make sure that the idea of the subject was clear, and not to get mixed up with the formalities. I learned that from my cousin's difficulties. But I also learned what algebra was like, that X would represent a number, and that it was not so hard. That was my beginning with algebra, which ultimately led to the four equations, four unknowns business — you had to find four numbers, you see. But I got started with it, and then I began to do the formalities, but I understood it. Anyway, I got interesting in that. But another thing — because of his difficulties, he had a tutor who came, whose name was Maskett. He's now at the University of Virginia — Al Maskett — something like that — Albert? He came to tutor my cousin. I, of course, was the young boy fascinated with mathematics, so I was permitted to sit and listen the first time. Just to listen. Not to say anything. Then after the class, I talked to this fellow Maskett a little bit, and he became very interested in me and helped me a great deal. What I talked to him about was the following. I can remember it now, I can almost see it. I had discovered a formula for doing the following problem. Suppose you want to add 1 plus 2 plus 3 plus 4 and so on — I don't know why, but suppose, you know, up to some number. I had discovered that with the odd numbers, a certain rule worked — like 1 plus 2 plus 3 is 6, 1 plus 2 plus 3 plus 4 plus 5 is 15 — I guess the rule was, you multiply the number, the odd number, by half of one more than the odd number. Something like that. Some rule. Then, at even numbers, I tried to find the rule, and I eventually found the rule for even numbers. So I remember going around — I still remember — "Suppose," I would say to my friends, "suppose a theater has a new idea. Instead of charging a definite amount for a movie, it charged" — because I always wanted to make application of these — "1 penny for the first person who comes, you see, so everybody comes quick, and then the next person who has to pay 2 cents, the next one 3 cents, and so on and so on, until 100 people come. Now, how much money will they collect? In the movie, from this?" So they would sit, you know, and — that was my machine. I could do it, you see. Well, I showed this to Mr. Maskett the first time, and he showed me that the two rules which I had could be made into 1 — you multiply the number by one more than the number and then divide by 2. I'd had to have two rules. I was very excited by this. But he was excited, apparently — I mean, I would judge — from the fact that this little boy had cooked this thing up. So he was always interested in me, and he would tell me little things and help me along — like that, the formulae could be made one, and so on. He kept encouraging me in various ways to discover things. So I had lots of good contacts. Now, you asked me some time ago, before we recorded when I began to feel

original — when I began to feel disconnected from — I don't know what you called it, the umbilical cord, or something. But you see, from the beginning I was disconnected. I was trying to find a formula for adding the integers together because I wanted the formula. I didn't care; it didn't mean anything to me, that this was worked out by the Greeks or even by the Babylonians in 2000 BC. This didn't interest me at all. It was my problem and I had fun out of it, you see. It was always that way. I was always playing my own independent game.

Weiner:

We're still talking about this earlier period.

Feynman:

Yeah. I think I've about exhausted it by now! Yeah.

Weiner:

You were still living —

Feynman:

— yes, in the same house.

Weiner:

Maybe now we should get on to the high school period, if you're ready? I'm not rushing you.

Feynman:

OK. I am ready. It's interesting, just to get the whole thing — you ready? Yeah. And then another tiny thing from that period — earlier, even — is that my father used to sit me on his lap, and the one book that we did use all the time was the ENCYCLOPEDIA BRITANNICA. He used to sit me on his lap when I was a kid and read out of the damned thing. There would be pictures of dinosaurs, and then he would read. He read the long words — “the dinosaur” so and so “attains a length of so and so many feet and the head is so and so many feet,” six feet wide or something. He would always stop and say, “You know what that means, 50 feet high and 6 feet wide, or 20 feet high and 6 feet wide?” “It means, if the dinosaur's standing in our front yard, and your bedroom window, you know, is on the second floor, you'd see out the window his head standing looking at you. That's what 20 feet is, because the height of one story's about 10 feet. Further. If he tries to put his head in the window, he just can't quite make it, has to

break the sides.” I give this illustration because he would translate everything, and I learned to translate everything, so it’s the same disease. When I read something, more or less, I always translate it as best I can into, what it really means, you see. So I learned that then. And the reason I remember this is because I had a friend who had an encyclopedia too — this guy Bernie Walker. He had a thing called THE BOOK OF KNOWLEDGE, which was designed more for children, better for children. I couldn’t stand it. It was very poor. The facts were never sharp, you never were sure that — you know, you had to get the feeling of depth, honesty — I can’t explain it to you. Maybe it’s fixed a little bit to make it easier. You never felt right, you know. Furthermore, the thing that bothered me the most was the index. My father taught me how to use an index, and that doggoned index would have things under W — under W you’d have to look to find “What is a horse?” or “What is the history of the horse?” — under W! This I realized. I mention this because you were talking about the fact that I didn’t respect authority. Not only that but I also realized that there were stupidities in the world, that people made dumb books. I’d already learned a lot by the time I was coming into high school of disrespect for authorities, for certain authors, for opinions of people. I knew the world was a dopey place, and that there were only a limited amount of people who had this rational—you know, like the NEW YORK TIMES was a good newspaper, the encyclopedia was —

Weiner:

— what paper did you read in your house?

Feynman:

I don’t think I read. My father read the TIMES. That’s probably why I knew it was a good paper. Then I went to high school. OK?

Weiner:

Yes. Let me ask at this point, when you were in grade school, did you view going to high school as a new, important stage?

Feynman:

Oh yes, sure. I mean, I was interested in learning, of course, and the high school was opportunity. After all, in high school the kids were learning algebra. I mean, you know, this same business — my cousin would have the opportunity. So, I went to high school. Also, I liked the high school. I knew some of the teachers, you remember, I went to the high school—because after all, they had all this apparatus, they had all these classes. It must have been great to be in high school.

Weiner:

Where was the high school located in relation to your home?

Feynman:

A mile away. We walked every day.

Weiner:

This was Far Rockaway High School? Was that the name of it?

Feynman:

Yes. So I went to high school.

Weiner:

Excuse me for another interruption. This would be in about 1931?

Feynman:

Just so. In the beginning, I discovered the algebra class was impossible for me. It was terribly boring.

Weiner:

In your freshman year?

Feynman:

Well, yeah, because I'd learned the algebra. After all, I was doing four equations, four unknowns, and all kinds of wild things, and they were learning X plus 2 equals 6, and subtracting 2 from each side. I could see the answers before they wrote the equations. It was just terrible. I stood this for an entire semester, half a year nearly, because I was a very timid boy. I didn't just run up and say, "Look, I know all this stuff." But ultimately I was essentially forced into it. I said to the teacher, "It's horrible, I'm not learning anything," and so on — or maybe she even suggested it — I can't tell you now, but anyhow, I was sent — I think partly on my own power, that I had enough nerve to say something — I was sent to the head of mathematics called Dean Ogsberry, who was called the Iron Duke, because he was also the disciplinarian, so it was in some awe and fear that I went to his office. He was a very difficult fellow, in a certain way, — really very kindly, but the boys all thought the other way. But he had to be a disciplinarian. So I went in and I told him my problem. "Oh," he said, "you find the algebra too easy, do you? All right. I'll give you a problem. You sit over there and solve it. When you've

finished solving it, I'll look it over, and I'll decide what we can do." He gave me a problem. He made it up, you know, simplify something complicated and solve for X and some fraction. Well, the darned thing went up to a quadratic equation. I didn't know how to solve quadratic equations, and I kept doing everything under the sun — divide X, put on the other side — all kinds of things. I couldn't figure out how to solve the darned thing, and I was going around in circles, and sweat was pouring out. He said, "Take your time," and all this kind of stuff. I remember it. It was very dramatic, you know. Somebody else was in the office and they said, "Don't listen to us, just go ahead, work, take your time." But taking any time didn't make any difference. I was going around in circles. I couldn't get out the number from the quadratic equation, even by trial and error, because it wasn't one that came out X equals 2. In fact, I realize now he'd simply written some complicated expression with Xs in it, to find X, and it wasn't from a textbook, and it didn't come out easy, and I couldn't do it by trial and error, and I couldn't get it. So he looked at the papers, which were all scrawled all over, you know, up and down, upside down, everything turned inside out. He looked the whole thing over. I was sure I was going to get sent back to Grade 1, but he said, "No, you can go into the Second Grade." He'd probably seen that some of the steps were all right, that I was just stuck in one place, which it turned out is true. The quadratic equations were only in the second year. Anyway, he sent me into another class which was called 2X — not the regular 2 — which was for people who'd flunked math 2 the second year, and had a special teacher known as Battleship Moore, who was especially good with students that weren't very good. So I was put into this class. They had had everything before, but anyway they'd been the lower end of the class. She was very good at teaching this. So I was in that class, and I learned stuff from her, because there were things that came up in that class that I didn't know, like quadratic equations. Some stuff I learned from her, but I quickly learned for myself, you know. I knew where the problems were. Then I would learn by myself. I also remember a thing that happened in that class. See, when I first went into that class I was, along with the others, a dumb one, like the others, because it was ahead of me, so to speak. But one day she writes equations on the board which is something like 2 to the X equals 32, 2 to the X power equals 32. What's X? Nobody can make head or tail of it, it's a new problem, and they haven't the slightest idea. I said, "X is 5." "How do you know?" "Well," I said, "32 is 2 times 2 is" — and so on — "so it has to be." The point is I understood what the problem meant, whereas the others didn't understand what the problem meant. And that's the real horror of teaching. You can teach all the formalities you like, but the unfortunate students never learn what the idea of it is, whereas if you understand the idea it's relatively easy, you don't need the formal things so much. Anyway, the fact that I was able to get X equals 5 out of that made all the other students partly angry, a little jealous, not really, probably — and a lot of them admiring. You know. And I became somebody in the class. They thought that I had learned it somewhere else and that even amazed me more, that they should think that you had to learn something like that in order to do it — you had to study it somewhere. It seemed to me obvious, you see. I began to learn about how the minds of other people work, and it was more surprise to me that they thought I had learned it somewhere, that they couldn't see, after I explained it to them, and then it was obvious. It's the same as

your question, how did I know, did I have confidence, and that the rays came out radially. It was sort of like, the facts of the world. It was almost evident that that's the only thing it could mean, you know.

Weiner:

This implies that you felt there was a certain structure to this knowledge.

Feynman:

I don't know. I can't say. It seems to me if you say 2 to the X equals 32 , I know what it means. X always represents some number, so if the number happens to be up there as an exponent, does it make any difference? I mean, we knew about exponents. Does it make any difference the problem? It's 2 to the something is 32 — the [inaudible] of 32 is 2 to the 5 th. I can't explain to you why. I never understood what the matter with everybody else was. I never really understood what the trouble was. The thing that bothered them was that this was an entirely new matter, and how could this fellow know it? Only by having studied it before. But to me it was not an entirely new matter at all; it was the same old principle. You use X for a number that you don't know, and the answer to, 2 to what power is 32 — It is not something I have to study before. I remember this distinctly, because it gave me a feeling of a difference between myself and the other students.

Weiner:

By this time, your friend wasn't with you in high school?

Feynman:

Yes, he was. Leonard. Not Bernard Walker, but Leonard Mortner had come from Cedarhurst and had moved to Far Rockaway, fortunately, and was with me in high school.

Weiner:

That's really coincidence, isn't it?

Feynman:

Yes, it is. Pure coincidence — ever since. He was at MIT with me. He's out here now. It's very interesting, a series of coincidence.

Weiner:

So you were with him in high school again.

Feynman:

Yes. Now, during the high school years, in the mathematical world, I had another few things. I wanted to learn calculus. I'd heard that calculus was big stuff, after algebra. I can't get the timing right. There's another thing in geometry that comes first. When we were first starting geometry — I don't remember; that must be the second year of high school, because I think you take algebra first — my friend and I, Mortner and I, went into the geometry class the first day or two. And we had heard — my father had told me — that it's impossible to trisect an angle. My father had first told me, it's impossible to trisect a triangle — that is, to find three pieces which are the same area as the original triangle, for an equilateral triangle, and I had done it. For a not equilateral triangle. I had figured it out. That's possible. But they weren't the same shape, and he said, "No, the problem's to make them the same shape, and they must be three triangles." But that seemed to me trivially obvious, that you can't put three triangles together to make a triangle, you see, so I knew that he didn't have it right, and I kind of pushed on him, and he remembered then that it was to trisect the angle. So he had taught me the problem, first incorrectly. So I told Mortner, or he told me, that it's impossible; so we decided we were going to try it after all. So we started to work on trisecting the angle. Well, we knew that bisecting the angle could be done by bisecting the cord that goes straight across, in the isosceles triangle. So, incorrectly, we supposed that the problem was to trisect that line across the face of the angle. We had converted to trisecting a line. OK? You understand?

Weiner:

Yes, I visualize this.

Feynman:

Yeah, but it isn't really true that if you trisect the straight line you really trisect the angle, but we didn't notice that. So we had converted the problem to trisecting a line. Then we started to work on it, and we played around and we played around, and we discovered, empirically, by drawing pictures, that an equilateral triangle, if you drew the three altitudes, that the point where they met, in the dead center of the equilateral triangle, is one-third the way up the altitude. It seemed like. So we made it very carefully, on a big piece of graph paper, a very careful equilateral triangle, measured it as accurately as we could, and sure enough, it's one-third of the way up. We couldn't prove it but we knew it was one-third of the way up. Therefore, we had the solution, provided that we could construct the equilateral triangle in the right place, so that this line would be the altitude, you see. So the problem was — we thought the problem was — to find an equilateral triangle, to put an equilateral triangle, at a given altitude. And that took us a day or two

to figure out. It's possible, of course. We finally got that. While we're getting it, we're thinking. We're riding around on our bicycles — boy, it'll be in the papers! You know, two high school students, hardly starting out geometry, solve the trisection of the angle! I remember riding our bicycles, as we were riding, we'd stop suddenly. You know, we were talking about it — we'd suddenly stop and get off and draw some pictures, and so on. A very exciting week. Then, I had this thing. I went home, and I was drawing it up — you know, making some constructions for angles, to look good. Then I said what about big obtuse angles? So I made a very obtuse angle, and I went through the whole construction on the paper, and looked at the angles, and it was very poor. Then I suddenly realized — horror of horrors! — that we hadn't trisected the angle at all. But of course we had learned a lot of geometry.

Weiner:

These expectations of the newspaper headlines —

Feynman:

Yes, that's always with me. Whenever you do research, and got a partial solution, or think you're getting a solution — or get a partial solution, or think you're getting a solution — or at least, me — I always imagine a kind of a daydream, an exaggerated daydream, of the impression.

Weiner:

Upon what?

Feynman:

Well, colleagues, or something. That you write the paper in the PHYSICAL REVIEW, you see. Or you imagine that the paper — but the paper is very simple, you see. Or you imagine that the paper — but the paper is very simple, you see. For instance, we don't know why E squared over HC is 137, you know. So you daydream. "It is apparent that" — tatata, one paragraph of idea — "so [garbled...] function we obtain" — tata! Just two paragraphs! Send it in to the PHYSICAL REVIEW. Great discovery! A dream. You know? Crazy dreams. Just daydreams. Not serious, just a fun idea, the same as the two kids and the newspaper. The very same idea. "Two children in high school first learning geometry solve the age-old problem of the trisection of the angle." But we didn't realize in fact at that time what a fantastic sensation it would have been!

Weiner:

That was your first day in class, when you walked into class, you mentioned the challenge that you felt for the geometry class.

Feynman:

Geometry. Yes. We knew that was geometry, and I knew that one of the great problems of geometry was trisecting an angle. So the heck with learning all these little bits and pieces, we would go ahead and do the big problem, you see! That's a way to learn, though — I'm telling you.

Weiner:

He was older than you. Were you in the same class?

Feynman:

Oh — no, I guess he was the same age, then.

Weiner:

Because we were trying to establish that earlier. We weren't quite sure.

Feynman:

He must have been the same age.

Weiner:

He was in the same class?

Feynman:

I'm sure he was. We did not have more knowledge of the subject than I did at that time. Definitely not.

Weiner:

It's possible you took the course later, in sequence —

Feynman:

— it must have been the same age. It must have been the same age, because I just know that he didn't know more than I did about it, or vice versa. We did it together. It was the same age.

Weiner:

What other mathematics did you have in high school?

Feynman:

Well, what I had in the classes — ultimately, through the whole high school, I took later solid geometry and trigonometry. In solid geometry was the first time I ever had any mathematical difficulties. I don't know why it was, but in the beginning the solid geometry class was complete and absolute chaos, and I couldn't understand anything. It was my only experience with how it must feel to the ordinary human being. Then I discovered what was wrong. The diagrams that were being drawn on the blackboard were three-dimensional, and I was thinking of them as plane diagrams, and I couldn't understand what the hell was going on. Suddenly I understood what was going on. Then it was child's play again. It was a mistake in orientation.

Weiner:

And in the question of your spatial perception, really.

Feynman:

No, I mean, once I understood it, it was easy. It was just that I didn't think there might be diagrams in three dimensions. Just crazy. In fact, it's dumb, because the subject was called solid geometry, but somehow or other I was trying to understand what he was talking about, when he would draw pictures, and I would see a parallelogram, and he called it a square, because it was tilted out of the plane, you know. And I — "Oh God, this thing doesn't make any sense! What is he talking about?" I caught on after about two weeks, or three weeks, but it was terrifying, for a while. It was a terrifying experience. Butterflies in my stomach kind of feeling — here, this subject, completely un-understandable. And I'd never had any trouble with mathematics. This was terrifying. But it was just a dumb mistake. I suspect that this kind of a dumb mistake is very common, to people learning mathematics — that dumb mistake, the same kind of a mistake my cousin made — he understood the idea but he didn't understand the idea, and then, he doesn't know what he means, "to do it by algebra," and he can't ever understand the subject and always feels that there's a missing piece. But part of the missing understanding is to mistake what it is you're supposed to know.

Weiner:

Doesn't solid geometry imply the development of special perception — that is, to be able to see things in depth.

Feynman:

Well, you can. That wasn't my trouble. That's no trouble. I don't have any trouble —

Weiner:

— no, until you recognized —

Feynman:

I was misreading the diagrams, that's all. He would call a square — you draw a parallelogram on the blackboard and call it a square? OK. But I didn't understand what he means. It's a square out that way. It's not that I can't visualize, it's that I didn't know what he meant. I had no trouble visualizing. In fact, I'm good at it. In fact, I could almost visualize four dimensions. I can tell you properties of four dimensional geometry because I exercised for the purpose, to see if it was possible. I practiced, and I got some vague way of seeing, not very clearly, but usually can tell you things in four dimensions that are right, by a half-visualizing — very poor, very coarse, but not impossible. No, I had no trouble with special visualization. That wasn't the trouble. We also had trigonometry. But when I was in Miss Moore's 2X class, I got interested in calculus. As a matter of fact, I got interested in calculus earlier than that. A new set of books had come into the library, the public library. The public library had nothing. It had the ENCYCLOPEDIA BRITANNICA. It had a few books that were interesting — the ABC OF RELATIVITY. MATHEMATICAL PHILOSOPHY by Russell was the most wonderful book — it was deep — that I studied.

Weiner:

When did you read it?

Feynman:

In the high school era. Exactly when, I don't know. But then they got a series of books called ARITHMETIC FOR THE PRACTICAL MAN, which didn't interest me because it was insurance rates and so on — ALGEBRA FOR THE PRACTICAL MAN, which I read very easily — TRIGONOMETRY FOR THE PRACTICAL MAN, which I read through and then forgot. This was before I got to trigonometry in high school, long before, and I didn't find it useful or interesting. And CALCULUS FOR THE PRACTICAL MAN, which I also got. I remember this time, because I had to lie, and I was a very honest boy. I'd been trained to be very honest. But I went to take the calculus book out, and the teacher — sorry, the librarian — said, "Child, you can't take this book out. Why are you taking this book out?" I said, "It's for my father." And so I took it home, and I tried to learn a little bit. My father looked at the first few paragraphs and couldn't understand it, and this was rather a shock to me — a little bit of a shock, I

remember. It was the first time I realized that I could understand what he couldn't understand. I can't remember exactly now, but I did study calculus one time, and forgot the whole thing, I believe. But at least I studied trigonometry and definitely forgot the whole thing. But some time later, when I was inventing the solution of a certain kind of problem (dopey problem — problems of the kind you're given, that the hypothesis is a certain amount of more than the one side, you see and stuff of this kind — problems like you have a flagpole, how far out will the string be if you hold it taut? You see) — Incidentally, I remember now, everything I would do like this, I would always have a practical problem to exemplify it. I have always thought the thing was no good unless you could use it somehow. I realize now.

Weiner:

Would you try the practical problem?

Feynman:

I would use it to make up these problems, to illustrate the power of my inventions, you see. I'd invent a method of solving such a problem. Then I would illustrate it with practical problems which I would invent, like the flagpole and the string, see.

Weiner:

But you didn't feel the need to erect the flagpole and —

Feynman:

— oh no, no, no — I didn't mean that way. I meant I'd get examples from my mathematics. Anyhow, I was fiddling around with such theorems and developing a number of theorems about triangles of this kind. And I had known the definition of a sine, co-sine and tangent, from the old days. I discovered what corresponds to sine over co-sine is tangent. It's very simple. I also discovered, sine squared plus co-sine squared equals 1. I discovered again, you know. I didn't remember them. But what I did remember, that was trigonometry, was the relationship of the sine, co-sine, tangent, and there were things like cosecant, secant — I remembered that, and there were a lot of relations of some kind. Then there were things with double angles and sums of angles and everything else, all of which I couldn't remember, but I know what the subject was about, you see. So I started with sine, co-sine and tangent, found these relationships, and discovered that I could express each one in terms of the others, in terms of one another. I made a big table — how you express a tangent in terms of sine, or co-sine. See, I worked the tables out. Then I began to work with double angles and proving theories — that the sine of twice an angle is twice the sine times the co-sine. You might be amused at how I ever did that, but I did it, one way or another. I proved all the theories. I found the formula for sine of the sum of two angles and the co-sine, and all these things, and

all the trigonometry formulas, and developed them quite a bit. And each one had its own ingenious proof, which was personal and my own, because I've seen them, and some of them are very clever, much better than in the book. And some of them are also dumb. They go all around Robin Hood's barn to come around here. You can see a shortcut immediately, through here. But that was the character of it. I think I can find that notebook. I'll see if I can find that notebook.

Weiner:

That would be interesting. Were you aware that such tables existed?

Feynman:

Of course, I knew that in trigonometry, that this was the subject. But I was working it out for myself. I didn't care to know what the answer was; I wanted to see if I could do it. It was always the pleasure of doing it. In fact, after I had developed enough formulas for the sine of an angle, sine of a double angle, like five degrees, I could work out the sine and co-sine of 10 degrees and so on. I could make the whole table of sines, every five degrees. So I opened a book and picked up the sine of 5 degrees. I shouldn't have even had to do that, of course, but I did that. I remember the book. It had the sine of 5 degrees, and that was in a square. All the other numbers were worked out by calculations. So I was always interested in the power of the formulas, what they could really do. So I worked out the whole table of the sine. I had a table of sine, co-sine and tangents, in my notebook, worked out, that I worked out from knowing only the sine of 5 degrees. Just games.

Weiner:

This must have taken up quite a bit of time.

Feynman:

Oh, yes. I did a lot of playing around.

Weiner:

This was at home, and in your playing time.

Feynman:

Yes. So when I really got to trigonometry class, it was almost a waste of time. You see, most of the classes that I was in, I would only do the things just, you know, on the side. I didn't care. It was easy.

Weiner:

Well, did you get grades in them?

Feynman:

Oh yes, I got very good grades in them. But I already knew almost everything that was in the sciences and mathematics in my school. I never learned anything in the class — or practically nothing. The teacher of the trigonometry class was Mr. Ogsberry, and by this time I had met another man, whose name was Herbert Harris, who was very clever in mathematics, and we were friends there. Mr. Mortner had gone away, to some other place. Harris and I were in the trigonometry class. I believe Harris was a year older, but because I was good in arithmetic I was higher. Well, Mr. Ogsberry in the trigonometry class said — or maybe it was permutations and commutations — advanced algebra, we also had advanced algebra — in that class he said that he has two problems which from time to time he has given to students, and they have never solved them. This is his great challenge. He thinks that he would like to give them again. He'll give one, and if we solve it, he'll give the other. See? It was a big dramatic business. The first problem he gave was: you're given a parallelepiped, and the angles of the three faces, in the corner, the angles, the face angles, and the lengths of the three sides that make up the parallelepiped. I don't remember whether it was a parallelepiped or a tetrahedral. It was only a factor of three. Problem: what's the volume of the parallelepiped, in terms of these things? So we went home, and we worked for two or three weeks. We developed theorems. We worked together, developing theorems and methods and so on and gradually worked out a formula, the formula for the volume — which in fact is very beautiful. Its square root is beautifully symmetrical. It's the square root of a complicated thing, but it's beautifully symmetric, in the angles, alpha, beta, gamma, A, B, C. But our method was very unsymmetrical. We dropped perpendiculars, and we laid it out from one particular side, and did all kinds of things. Anyway, we worked it out, and we brought it in, and he was very impressed, and he gave us the other problem, which is to find the formula between — maybe he gave us both problems at the same time — to find the formula relating the dihedral angles in regular solids, to the number of the polygon and the number of sides of the polyhedron, or something. Then we worked that out too. As a matter of fact, we used much of the same theorems. And he was very impressed. Nobody had ever solved them before, and we had solved them. So that was a very exciting challenge. They were really good problems. They were good, substantial — I mean, it's not dishonest. For a kid in high school, they were really excellent challenges. They were very good challenges. It's not at all self-evident how to do it, and it was a real excitement to gradually work it out, to get closer and closer to working it out.

Weiner:

When you got the solution, which you now describe as beautiful in its symmetry, did you

think of it in those terms?

Feynman:

Oh, I think so. Oh, yes, because, you see, I think we were delighted with the answer. I don't remember.

Weiner:

Delighted with getting an answer —

Feynman:

— no. The answer. Because they were so kind of simple. I can't remember it now. I can work it out much quicker now. In fact, I did it the other day, when I was taking a shower, because I thought of that old problem. It was — oh, it must have been two or three years ago, when I was taking a shower. But now that I'm an advanced super-expert at mathematics, suppose I had to do that problem. In my head, in fifteen minutes, I worked it out. But I can't do it right now, in two minutes. It was beautiful in the sense of the symmetry, you see. The A, B, C, of the sides and the alpha, beta, gamma of the opposite angles. Actually it's not symmetrical. And we probably checked, because I was wise in those days, that when one of the angles was zero, it gives zero, and if two of the sine's, if all of the angles are right angles, it gives A, B, C, and other things like that. It's a kind of beautiful thing to be able to do all these things, you know? It looks symmetrical. I'm pretty sure that we appreciated the formula. The other one, I never appreciated as much. I remember at the time I didn't appreciate it, because it was given in terms of the number of sine's on the polygon and the number of polygons that meet at a point. And there's only five possible solids, so although it's given in terms of M and N, it's not really a general formula for any M and N, because the only values of M and N, there are only five of them, so you might as well have just worked out five angles, and list them. See, if hasn't got the beauty of the other thing. I remember that definitely, at the time. I didn't care for that problem because that was like asking for five angles, and who cares? Although it was expressed in terms of M and N, it looked quite general, but there's only five cases allowed, so the whole thing was kind of an unbeautiful thing. It wasn't general. What I mean, none of the angles for the cube and tetrahedron, and there's five or three others, and it's not very interesting.

Weiner:

So by this time you had developed some mathematical tests.

Feynman:

Apparently. What I'm telling you now is not what I feel now about the problems. I

remember definitely not liking the second problem, and thinking the first problem was a wonderful problem, and the second problem was not so wonderful. Not that it was easier, but I had a taste, as you say, as to what I liked. Yeah. I remember distinctly the idea, I discussed it with my friend, that there's only five numbers, and what the hell, there's five angles, you might as well work out each one separately and give the five angles in a list and you've done the whole problem. It isn't like an abstract formula, in terms of M and N.

Weiner:

By this time, here you are working out these problems, and it's apparent to you that you have special ability in this area. Had you any thoughts on what you might do with this, whether you wanted to —

Feynman:

I knew I'd be a scientist somehow.

Weiner:

You knew. What was the earliest you —?

Feynman:

I don't know. It was just sort of always that I wanted to do this, for the rest of my life.

Weiner:

Scientist in the general sense of scientist, or did you have any specific area that you —?

Feynman:

No, I had no idea particularly, because, you see, it was like this — like my father would come one day and tell me electrochemistry was the big and important thing, and I would have faith that I would be a great electrochemist. So I'd piddle around with the wires, with the salt, probably to find out what happened, and partly with the daydream that electrochemistry is the coming field. (It wasn't.) But that electrochemistry is the coming field, and that I'm going to be prepared for the coming field-half. And half the interest was this business. So it was always that way. I mean, when he would tell me something, there was always the feeling that this was part of me. Like a little boy wants to be a fireman — his father's a fireman. Although my father wasn't a scientist. No I guess it's not quite the same, but... Anyway, I can't remember distinctly a day when I ever said, "By golly, you know what I think I'll be?" I don't remember anything like that. Nor do I

remember, at that time, any consciousness that I ought to study this or I ought to study that in order to prepare myself for something. Whether there's more money in chemistry than there is in physics, or any such question. It never was a question how I would earn my \$5000 a year. Not that I was confident that I would, but I would somehow or other work at that. It was much more important to do the science, somehow, than it was to choose such a thing. That I'm clear on. I mean, I know, it was not in high school career minded, in the sense of thinking of, where's the best opportunity? Not a whit of it. I was only interested in the stuff. When this man would give me this challenge, it wasn't because I was going to find out if I'm good enough to be a something or other. It was because it was a challenge, for its own sake — completely, always, for its own sake. It was the excitement, the fun, of working this thing out. A certain conceit, in showing off that you can do it, showing to other people as well as to yourself.

Weiner:

And you associated mathematics with science. This was part of science, as far as you're concerned.

Feynman:

Very definitely. Very definitely.

Weiner:

How did that come about, this association?

Feynman:

It came about this way. I had this laboratory, which. I told you about, when I was in grammar school, and by the time I was in high school I still had it, and it was developing further. And I would repair radios in the neighborhood, to make a little money, to get the money to buy junk for my laboratories. I had a photo-electric cell. I made amplifiers — not good ones. Nothing really worked well for me, but I struggled with it. I had lab banks and voltages and batteries and old radios and motors and so on. Electrical stuff. It had really developed from a chemistry laboratory to an electrical laboratory. Now, in this business, I had read in one of my friend's (Bernie Walker's) books, some years back — what year I can't remember exactly — he had one of these, BOOK OF KNOWLEDGE or some book. But there were a number of other books that interested me. It might be interesting. One of the books that interested me very much was a book called THE BOY SCIENTIST, by A. Frederick Commons, and it had a big circle in front, "Knowledge is Power." Then it had arches out which said, you know, "chemistry," "Electricity." I was very impressed with this whole business.

Weiner:

Was it a book from the library?

Feynman:

No, it was my own book somebody gave me. It had little experiments to do and so on. Then there was another book, THE BOY ELECTRICIAN that I got a hold of. It wasn't as good.

Weiner:

Was this during the high school period?

Feynman:

Earlier. Anyway, my friend had a book, and in that book it had a formula for the power that a resistance takes, and also the resistance voltage relation. The ohms are the volts divided by the amperes. The volts are the amperes times the ohms. The watts is the volts times the amperes. The watts are the amperes squared times the ohms, and so on. It has eight formulas or something. And I looked at them. I remember very well, this. I looked at them and I said, "You know, I think that these are many of them, really restatements of the others. They're not independent things." And so I began to piddle around to get the connections, and of course I knew enough mathematics to understand a little bit, but I'd never — you see, you have to learn someday, and it's not taught in school, that you can use X for the amps, you see. It's not taught well — the connection between mathematics and the use of the letters for some quantities in physics. I had to teach myself that, and that these relations were only mathematically interchangeable formulas. You could derive one from the other. So I got interested in the relation of the mathematics to the physics. Then, much later, when I was in high school, I found the formula for the frequency of an oscillating circuit in some book that I read. I was making oscillators and radio receivers and stuff. And it said "2 pi times the square root of LC." I remember this distinctly. And where did the "pi" come from? You see, I had been taught that pi was a golden circle. And here's a pi! Frequencies. See, the watts is the volts times the amps, that's a childishly simple formula. But the frequency — 2 pi times the square of LC — that bothered me. Where does the pi come from? And I worried about it. Then one day I realized that of course, the coils, the inductances, the coil's a circle. That's where the pi comes from. That's where to find it. Then, later, when I was looking at the formula for the inductance of a coil, one day, later, I found a pi in that. They had a list from some handbook — inductances of square coils, inductances of round coils. My mind got to work again. The inductances had nothing to do with the shape of the coils. A square coil could have an inductance too. Whatever the inductance is, the pi still comes. It fascinated me, this pi, and the relation of mathematics to physics, then. And so I got more and more interested in mathematics, but always with some relation to physics.

As far as I can tell, I still don't understand that pi! That beautiful pi — having to do with the relation of the slope of a sine wave and how long it takes to come back.

Weiner:

This ties in with what you said earlier about the — always trying to find an illustration of the mathematical formulation that you worked out; that really is the relationship of mathematics to physics.

Feynman:

Yeah, I'm always interested in the relationship, the practical business — that the thing does not mean anything really unless there's some way to make something go.

Weiner:

It's of a piece, then, the mathematics and the science.

Feynman:

Yes. Yes.

Weiner:

We're resuming now after a brief break for lunch. When we left off, we were in the high school period, Far Rockaway High School, discussing the mathematical learning there and some of the developments in your own thinking on that. Let's get on, now, to some of the science courses, and take it from there.

Feynman:

Well, the first science course I took I think was general science, meaning biology mostly. Or maybe it was called biology, I don't remember. I don't remember much of that, or finding it particularly exciting. I remember only some experiments in which egg white was dissolved in stomach enzymes, or whatever it was. It was interesting, because it always looked to me like egg white would never dissolve in anything, and to see it dissolve was interesting. But really it was not very interesting. The osmosis was an interesting process, the explanation of it in terms of the atoms, but the biology itself was not very interesting. That wasn't because I wasn't interested in living things, because my father had very much interested me in plants and animals and so on, and processes and life. But somehow the course never did anything. I don't remember much of it, so I might as well cancel it out. Then I took chemistry and physics, and in those courses I learned a little bit. But I knew a considerable amount, so I didn't learn much directly in

the course, in the normal fashion. But we had several things. We had, of course, clubs — Chemistry Club and Math Club and Physics Club and so on, where the kids would stay after school who were interested in these things, and work up demonstrations of something, and we'd give a little lecture to the other kids. It was always pretty good. It was good practice, I realize, now that I'm thinking about it. I made, I forgot what, phosphine or phosgene — whichever is the one that isn't a poisonous gas.

Weiner:

Phosphine?

Feynman:

Phosphine, I guess, and it explodes and burns when it comes out into the air, and makes smoke rings and flame — a flame and a smoke ring — and I made that, for a demonstration. I'm just remembering now. A friend of mine, another friend I haven't mentioned, who was a year older than I was, named Elmer Heller, I remember, in the Physics Club gave a demonstration of the properties of sand, which I remember very well. It was a beautiful lecture—that at the angle of repose, if you make the same angle; by making such piles and looking at them, projecting one against the other, you see it's the same angle; that if you had sand in a bulb, a rubber balloon, with water, and squeezed it, the water would go down, not up. You can squeeze the balloon and the water goes down, because you disorganize the pattern of the sand in the water. It's sucked into the holes between the grains. A very nice lecture. This is the kind of thing that we would do in these clubs, and the way you'd learn something, either from the other fellow, about sand in this case, or yourself in preparing a lecture. And they were pretty good, I think, some of them. So that I did. In addition, there was a special class, with a teacher, Mr. Bater, in the physics class, who noticed that I was somehow unusual, and he said to me one day, when we were discussing index of refraction, and after class he called me aside and said, "Listen, you make too much noise in the class. You make a lot of trouble." I was feeling bad, and he said, "But I know why. It's because it's all so boring and uninteresting to you." You see, what would happen in the class is something like this. The fellow would start to explain something. He says, "Because the light is bent toward, away from the normal, as it goes into the glass." So the teacher said, "What?" And the guy would say, "It's bent toward the normal." I mean, it's just a silly game of trying to remember the rules. It was a boring business. Anyway, the teacher said, "I'll give you a book to read. You go up in the back of the room, in the corner, and you read it by yourself."

Weiner:

During class times?

Feynman:

Yes, during class time. "Pay no more attention to the class. When you know everything that's in that book, you can talk again." The class didn't fill the room, so way up in the back was separated. So I sat up there. He'd given me a book called ADVANCED CALCULUS, by Wood or Woods. It's not what they call advanced calculus today, it was more what we call mathematical methods, Fourier transforms and elliptic functions, that kind of stuff — gamma functions and so on. And that was very interesting. I worked very hard on that, and learned a great deal of mathematics in high school, of an advanced kind. I had to learn calculus in the meantime by myself. I forgot to tell you the details. I'd gotten a calculus book and read it and didn't understand it. Then later I got another calculus book. My father and I went to Macy's and he bought me a book, CALCULUS MADE EASY, and I took it home and studied it and wrote a notebook which I still have, and can give you, of this book, that tells me the stuff in it. That was a way to try to get it into my head this time, instead of forgetting it. So I had learned calculus.

Weiner:

When did you learn calculus?

Feynman:

Well, it must have been before the physics.

Weiner:

It was in the high school period?

Feynman:

Yes, but the physics was in the junior year, probably. I don't remember whether I learned this advanced calculus in senior or junior year. If I could find my record and find out — I have it here, high school — then I could find out what year we did all these things. It's easy to figure out. Anyway, I learned advanced calculus, what was called advanced calculus, the determinants, Fourier series and so on. I already knew something about Fourier series from the ENCYCLOPEDIA BRITANNICA. I struggled with every article in the encyclopedia that was scientific. I could understand everything except the article on gyroscopes and the article on group theory. I remember, because I know my challenges that were unsatisfied. The article on electrostatics gave me great difficulty, but by taking a notebook for yourself of what this is all about, and go through very slowly. I went through electrostatics several times, each time getting a little further, until I really understood it. But I don't know if I have that old notebook.

Weiner:

Was that before the physics class?

Feynman:

None of this — I can't remember. The physics class was essentially irrelevant. It was something that was in the — but the knowledge in the physics class itself was nil, for me. However, this teacher, Bater, also taught me something else. One day he told me he was going to tell me something very interesting, and he explained to me the principle of least action, in mechanics — that there was a number, the kinetic energy minus the potential energy, which, when averaged over the path, was least for the true path. This is philosophically a delightful thing. It's a different kind of way of expressing the laws, as you appreciate them. Instead of differential equations, it tells the property of the whole path. And this fascinated me. That was one of the greatest things ever. The rest of my life I've played with action, one way or another, in all my work. I mean, I find that I've loved that junk. I like it. But, I'd found also in the encyclopedia — you see, after that I'd found also in the ENCYCLOPEDIA BRITANNICA a statement that the laws of electricity are such that the potential is distributed so as to make the integral of energy a minimum. And I had proved — it wasn't in the book — I had proved from the formula that in fact you would get the differential equation of electricity from that. It wasn't there. It was stated, but it wasn't there. See, I was really learning. I was getting pretty good. I mean, not to be immodest, but relatively, I was getting more and more mature. I was learning rapidly in the normal order, from a childish fiddling with arithmetic, on and on to really doing real things, at a high level already in high school.

Weiner:

Then the advantage was the extracurricular work that Bater assigned.

Feynman:

He didn't assign. He just gave me a book to look at. You know, the real problem — you have to appreciate, really, all my time in these schools and around there — was the lack of supplies. Perhaps it was good, so that I had plenty of time to worry about elementary things before I was swamped by advanced things. I couldn't get books. The library had no calculus book. When they got it, it was within a week or two, I'm sure, that I took it out—the first guy to take it out. The first calculus book that was in town. I had a thirst, you see. I couldn't satisfy it. I'd heard about vector analysis, for example, and I didn't know what it was. I looked in the encyclopedia, and there wasn't enough there. But I knew it was something useful or important somehow. And Maskett, who by this time was getting his degree in mathematics at Columbia, wrote a thesis on vector analysis. He lent me a copy of his thesis on some subject in vector analysis, and I copied out of this thing another notebook for myself, on vector analysis, and learned, vector analysis from

that thesis. There was one part of the thesis that was some complicated business of transforming coordinate systems, that I didn't think was very important and left out. So when Maskett got his thesis back and asked what I thought of it — "It was very good, and thank you very much, but the stuff about the transforming coordinates I didn't pay much attention to." He said, "That's what the thesis about. The rest was the introduction." But I somehow had always had a wit about what was the central and what was the peripheral matter — what are the complicated details that really are not necessary and not useful, and what were the great ideas on each subject. So when I read Maskett's thesis, I paid no attention to his reciprocal coordinate systems, which was interesting, but more advanced and not very vital, which he had contributed; whereas the description of vector analysis which was in there, I knew that was worthwhile.

Weiner:

You say you've always had this feeling about it. When was the earliest that you can recall?

Feynman:

That was a good example. Later, when I was at MIT, I would read books on fields that I didn't know, like general relativity and so on — or even in the encyclopedia, when I'd pull stuff out of the article — I seemed to have a sense to pull, in electrostatics, a lot of stuff, but when it went to the calculation of the capacitance of an elliptical condenser, which was quite complicated, it didn't bother me that I didn't understand it. I knew that was not so interesting as the general theorems about the laws of the inverse square, and, you know. I had some way of knowing what was important and what was not. The courses themselves, in the physics and chemistry, I don't remember being very intrigued by them. It was a little bit of work, like exercises, to do the things. They were not difficult, so I got very good grades. It just happens that yesterday I was reading about Einstein, who was an individual who didn't like the educational system and got through without it, and I was thinking to myself, well, I went through the educational system. He talks about how it stifles the young because they're so busy doing the work for the examinations and all that.

Weiner:

Are you talking about Martin Klein's article in PHYSICS TODAY?

Feynman:

I don't know, maybe I am. Yes, probably. I was thinking — you know, you always think, what about yourself? You know — I think that the answer in my particular case, that it is really also true, that the thing is stifling, but because of the accidents, starting with my father and so on, I had always been ahead enough that the labor of doing the ritual for the schools, working out, balancing the equation for potassium permanganate, or

whatever you have to do, was relatively easy, so I could do it so easily that it didn't disturb me, you see. I didn't work at it really. I just did it, you see. It was easy enough. And then I had time to play at a much higher level. I was always like that, through the rest of education, because you can see, learning advanced calculus in high school, and calculus, and so later when I got to college it's exactly the same situation. I'm always ahead, so that I don't learn too much in the courses, and I found it relatively easy to satisfy all the necessary requirements as I go along, without it being labor, without it being hard work that would take up a lot of my time. It was relatively easy. Then I had plenty of time to do other things.

Weiner:

You mentioned the physics and chemistry courses. Was it a year of each that you had?

Feynman:

I think it was, yes.

Weiner:

Did you have any contact again with Mr. Johnson and Mr. LeSeur?

Feynman:

Mr. Johnson was my chemistry teacher.

Weiner:

So you did see him again in that capacity. Your prior experience in the laboratory must have been helpful there.

Feynman:

Oh, sure it was, but it's helpful for a relatively trivial job. I mean, the classes, the courses, were nothing. They were just dopey. I mean, they were so simple. And it was always so slow, because everybody was such clunks about it that they would try to remember the laws, and if the guy would say, "What did you say?" then they would change it the other way, because they realized they were on the wrong track, not because they understood a thing. You know. It was a rather pitiful business altogether.

Weiner:

Getting back to Mr. Bater, and the least action principle, he explained it to you? Or did

he give you some other things to follow up on? Did he discuss it subsequently?

Feynman:

No. He just explained it to me once.

Weiner:

Did he give you any references to it, for reading?

Feynman:

No. I still remember — you know, a thing like that — whenever a thing is exciting — it's a funny thing, in the mind — you remember all the subsidiary things that have got nothing to do with it. Like, I know exactly where the blackboard was, in what well — it was in the laboratory — where he was standing, and where I was standing, and everything, while he was telling me this. It was after class, in the laboratory, in the room where the lab was.

Weiner:

It was his idea, to bring this up?

Feynman:

I can't remember how it was brought up, but he told me about this thing, and explained it, by drawing curves on the blackboard of the motion of the particle, and showing the two different curves, and the one that gives the least number is the right one. He just explained. He didn't prove anything, as far as I remember. I don't remember any complicated matter. No, he just explained that there is such a principle.

Weiner:

And your reaction to it was on reflection later?

Feynman:

Oh, I think I reacted right then and there, that this was a rather miraculous and marvelous thing — see, to express the laws in such an unusual fashion.

Weiner:

When was the next time that you dealt with it, when you had an opportunity to reflect

on it again and use it?

Feynman:

That I can't say. I remember that there was a statement of the minimum principle of electrostatics in the encyclopedia, and that I worked it out. Now, there was also, in the book on ADVANCED CALCULUS, a section on how to do minimum problems, so probably I realized that was very important, because it had to do with the least action — now that I'm thinking about it. For all I know, it may have been the other way. I mean I may have been looking at the section on minimum principles and said to Bater, "What the devil is this good for? I mean, you find the size of a circle — the circle's the shortest distance to carry a certain area? Or what?" I'm only imagining. Maybe. I don't know which came first, the chicken or the egg, but I do remember a connection between the theory of what they called the [inaudible] equations, or how to find the minimum curve for certain minimum problems, and the least action. I can't really analyze bits and pieces. It seems to me all the way through this thing, it isn't the question of learning anything precisely, like the minimum principle, but of learning that there's something exciting over there. So the next time, when you look at another book, and you see some partial recognition that this has got something to do with that — this minimum principle, I know it's got the figures in it, this has got to be good, this must be useful in mechanics, that's an interesting bit — I pay attention, see. And that is the key. The key was somehow to know what was important and what was not important, what was exciting, else I can't learn anything. And by having clues — you know, from somewhere — that what's good... I think that the same thing happened with my father. My father never really knew anything in detail, but would tell me what's interesting about the world, and where, if you look, you'll find still more interests, so that later I'd say, "Well, this is going to be good, I know — this has got something to do with this, which is hot stuff" — you know? Like, nobody knows how birds migrate, so this is something about birds' migration, so it's a real mystery, see. Because I know nobody knows how birds migrate, and they're still struggling with it. This kind of feeling of what was important. Or interesting.

Weiner:

Interesting because it's a challenge, and leads to —

Feynman:

Yeah. Yeah.

Weiner:

What else on the high school? If nothing else —

Feynman:

The only thing, the other thing I can remember, now that we're thinking about it, is that there was from time to time something like, "We should have a science assembly," in which we'd try to explain to the other children and make demonstrations of how good the science is. You would think that I would have been the one to be the leader in this, but I remember, although I tried setting up some experiments and fiddling around, I'd always end up not doing anything for the rest of society, so to speak, except in the clubs. But I remember setting up and worrying about something, but not really entering. See, if we had had — (we hadn't) — things like science fairs and so on, in those days, I doubt that I would have been a big thing at a science fair. In fact, there were a few, I remember now, visiting science fairs from somewhere — students doing science things. I didn't find it interesting to make a thing to take to some children's science demonstration or something, although I would visit them. I remember in particular right now visiting a science fair, going around and seeing a boy who had made alum crystals, beautiful octahedral, and had a big demonstration of it. Then I looked carefully at the octahedral, and there was something funny about the lines, the edge lines. They didn't fit with pattern of the direction of the crystal. I looked very closely, and I realized that it was painted wood. So I said to the boy, "Is this yours?" He said, "Yeah." I said, "But that's just painted wood." "Yeah, I know," he said, "but it looks good, makes a good demonstration." I was horrified. I was very much of an idealist and a purist and so on. I thought this was so scientific, anti-scientific — it was an evil thing, you know. It bothered the heck out of me. That's not why I wouldn't enter into those things, but this was an after — I just remember this. I did have a feeling of great respect for science, a love of it, and when somebody would fiddle with it like that, it hurt me. I was hurt by this, because he was trying to fool people.

Weiner:

You thought he should have grown the crystals.

Feynman:

If he didn't, he didn't — if he'd grown small crystals, then these were nice, this is the way it is. You see, the question is "What is it?" not "How wonderful is it!" You don't have to build it up. You don't have to fake it. It's great the way it is, is the sort of feeling. This is some kind of falseness, a terrible falseness. It just bothered me. But I just remember that when I'm thinking of — that they did have fairs, because I did see this there. I didn't enter into that kind of thing. I didn't make demonstrations for other students to watch, except in my own little club. And I didn't have any feeling that I should enter into some fairs and things like that.

Weiner:

What about external influence? By this I mean journals. Did you start reading any of the literature in the field?

Feynman:

No, no. Oh, no.

Weiner:

Were there any available, any such magazines available in school?

Feynman:

I don't think there were, but I don't think — no. I couldn't read that. No.

Weiner:

How about popular accounts?

Feynman:

Oh. There was a year — way back when I was at Cedarhurst, I used to read science fiction, which, because of my love of science, I found interesting. As time went on, I got very upset with it, because often it was, anything could happen, and it got more and more ridiculous and so on. But every once in a while there would be an idea that was scientifically interesting in one of the things. But I gradually stopped that. I used to read them a lot, and then I gradually stopped.

Weiner:

Stopped when?

Feynman:

I don't remember

Weiner:

It was past high school?

Feynman:

In high school, somewhere, I probably stopped.

Weiner:

So, magazines, SCIENTIFIC MONTHLY, things of this kind?

Feynman:

No. I didn't have SCIENTIFIC MONTHLY. I don't know why. I never saw it. Oh, what I did do — when, I don't know — yes, in high school and probably earlier — was to cut out of the newspaper things that had to do with science. And one of the things was a thing called "Explore Your Mind," which was a sort of a silly thing. They would ask questions, and make some — about why, something, and then it would explain. Any articles on science. I still remember an article, "Scientists Meet, Find Atom is a Wave," and there was a picture of a diffraction pattern or something from gold. But I thought the wave was the circles of the diffraction pattern. You know — and didn't understand — but I was interested enough in science that I kept a newspaper scrapbook of science articles.

Weiner:

Whatever happened to that, do you know?

Feynman:

No, I don't know. No.

Weiner:

It would be interesting.

Feynman:

Yes. It possibly exists somewhere.

Weiner:

When you mention here the books, as far as reading goes, there were just a few instances, the Wood's ADVANCED CALCULUS and the encyclopedia.

Feynman:

And the vector analysis from Maskett.

Weiner:

The thesis, though —

Feynman:

That's his thesis. No, just the one book on calculus. Then I had to buy one. It came to the library, but I bought one.

Weiner:

So there's no consistent poring through scientific libraries.

Feynman:

No, no. There was nowhere to find it. That I knew of. I think it's just as well. I mean, I suspect — I know now — this has nothing to do with my history, but I suspect, I kind of try to imagine what would have happened to me if I'd lived in today's era. I'm rather horrified. I think there are too many books, that the mind gets boggled. If I got interested, I would have so many things to look at, I would go crazy. It's too easy.

Maybe. Maybe not. Maybe this is just an old fashioned point of view. You know. There's always these things. But it does bother me a little bit that there's so many things, and it's so easy, and they're watering down. The thing that I loved was, everything that I read was serious, was absolutely — wasn't written for a child. I have never read anything that was written for a child except THE BOY SCIENTIST book, which was pretty good.

But the things written for children, after a certain stage, by let's say high school stage or even the late, late era in grammar school — things written for a child were not good.

Very rarely, things written for a child — there were books on the cave man, you know? Cave men living, and people who lived on trees, and there was a book with a series of families in different circumstances that I found interesting. That was obvious for children. But later on, children's things — I didn't like children's things. Because, for one thing I was very very — (and still am) — sensitive and very worried about was that the thing to be dead honest; that it isn't fixed up so it looks easy. It isn't fixed up and partly faked so that the explanation can be made more simply — for the child, you know?

Details purposely left out, or slightly erroneous explanations, in order to get away with it. This was intolerable. So I ultimately had only to trust the completely mature and odd old things, even though I was only in high school. I think most kids in high school are very mature. Because I know my friends who were in literature — you know, who wrote — and my friends who wrote plays, and they would read the great plays. They wouldn't read the children's plays, you know. It's the same thing. Anybody that's any good in high school already knows that they had to look at the real stuff.

Weiner:

What about other friends in high school? We've dwelt on the ones that shared your scientific interests. What about your wider social circle and your social life, and your other than school life? Let's get that.

Feynman:

Well, first, from the school I had in high school, — earlier, I don't know, there were just neighborhood kids, but in high school, I gradually became a member of a group of about four or five guys. All friends, good friends. We used to walk around together in the street and talk about things, you know — or go for long walks and do things together, and so on. Good friends. And it was a limited group, approximately four or five. It was me, another fellow interested in science whose name was Stapler, who is very good, but (I think) because of his mother's interest and influence, he was driven out and wasn't able to continue. I was lucky. That was the only other science guy. A fellow named Harold Guest was interested in writing plays. He now writes television things for Playhouse 90 or something — Kraft Theater, I think. He's OK. He got somewhere. Then there was a literary fellow, David Leff, who was editor of the school newspaper, who was very interested in writing, but not plays. I guess that's it. There was also this fellow Elmer Heller, but he was a year older than I was, and the connection was not as close as with the other boys, although we would sometimes talk to him, go to his parties and so on. Now, we'll talk about social life outside of the science business.

Weiner:

Girls?

Feynman:

Yes, girls. I didn't have much contact with other fellows. Oh, just the ordinary thing — maybe you'd get mixed in a baseball game, that I couldn't play, kind of trying to dodge it — but you know how it is in high school, you have cliques, and other guys coming in and out of the thing. We didn't have any enemy or anything. It was not very complicated. But then, with the girls — Oh, there was another group of older kids, older people, whom I somehow got in contact with that took some sort of an interest in me, for no good reason that I know, and tried to develop my interest in girls or something. You know, they were older than I was. So there were parties among the young and so forth. There were dances. A dancing school teacher trying to make money wanted to have, on certain Friday evenings in her studio, dances, and she was a friend of my mother's, and so my mother tried to talk the children into going, and so on and so on. Now, let's see. I can tell you about my first date, but it's a sort of a silly business, anybody's first date. There were young kids around and I got the idea that, you know, I kind of liked one of the girls. I was with this older group, and one day I mistakenly, at the beach — oh, we met at the beach. We'd hang around with each other, you know? I said, "Gee, I'd like to take her out. Make a date." Which I never even thought I'd have

the courage to do — you know all this kind of thing. So they said, “Ah!” So they grabbed me by the hair, and they grabbed her, and they pushed us together, and they said, “Dick wants to ask you for a date.” Very difficult situation. Anyway, we made it, and I took her to the movies. My mother taught me, said I must step out of the bus first and help her out, and all this stuff, and I worried about: what am I going to talk about? I still remember what we talked about. It’s so silly, because, you know this first experience. She asked me if I played the piano, and I told her I had tried to learn, and I used to take lessons, for a little while. After I was older — after many long months of this I could only play something called “Dance of the Daisies,” or fairies or something, and this didn’t seem to me a very good thing, and so I didn’t do piano. This and that, we talked about. Later, as we were saying good-bye, she said, “Thank you for a lovely evening.” I was so impressed. I was so happy. Then I found out, on my second date, that the girl said, “Thank you for a lovely evening.” On my third date, when we were saying good-night, just at the door, I said to her, “Thank you for a lovely evening,” and she got paralyzed, unable to say anything, because that was what she was just about to say. So I quickly learned the formal from the truth, you see.

Weiner:

Two things occur to me here. Were you still in the house with the cousins at this time?

Feynman:

No, no.

Weiner:

When did the change take place?

Feynman:

Approximately the second year of high school, we moved to an apartment house that was only a block or so away. I brought the laboratory with me.

Weiner:

Did you have difficulty fitting into your apartment with it?

Feynman:

Well, it was a tight squeeze, in a small room, but it was all right.

Weiner:

But you still saw the cousins and they were close by.

Feynman:

Yes. Yes — about the social more, huh?

Weiner:

Well, you mentioned the music, and that raises another question — what other interests did you get into, other than science, other than school?

Feynman:

Nothing. I was not interested in sports. I was not good at writing, or making any drawing, or clay — nothing.

Weiner:

You didn't have much time for that, did you?

Feynman:

Oh, I may have had time, but I probably spent the time doing something else. I didn't have any hobbies. My hobby was to do this science stuff. I didn't do it all the time. I mean, I would run around the street with these guys and talk all the time, and discuss the world or something, or hang around the drugstore and drink sodas. I don't remember what. But I didn't have any other interests at the time that I was pursuing at all, that I can remember. Nothing. I would repair radios, but that was directly connected to my laboratory and my equipment. Everything was directly connected to it. I did — I forgot to tell you — repair radios, starting in that other house, and later in the apartment house. That must have been the second year of high school. It was in the Depression, so people couldn't afford to have their radios repaired by the regular repair man. It was discovered that it was possible they would hire a silly — even as young a boy as I, to repair their sets. The first person who did was my aunt, who ran a hotel, a resort hotel. She called me up one day and said that her radio was out of commission. Well, she had somebody else call — "This is such and such hotel, do you know something about radios?" "Would you come and repair our radio, please? It doesn't work and stuff. I said, "But I'm only a little boy." They said, "That's all right, we understand from a good authority that you know something. Would you like to try to fix our radio?" I said, "Well, I'll try" — you know. And I came. They still laugh about it, because the "boy" came — a little boy, you know — and had a big screwdriver sticking out of his pocket. I brought the tools in my pocket. I went to the radio. They had a man, a handy man, in the hotel, who was a nice fellow, and when I went to try and figure it out, I found out rather quickly that the

switch wouldn't work right, only because the knob slipped on the shaft. When I went to fix it, I could see it couldn't work because the screw was broken or something. But the man, who was the handy man, after I pointed out what the trouble was, fixed it up. You see, he figured out a way to fix it that I might not have been able to do. He helped me. And so I was successful in the first repair job. Then, it was almost as if — I had good luck with things like this — it was almost as if a course was prepared. The next person who called me up, it was only the plug to plug in the wall had to be fixed. You understand? They got more and more complicated, in the right order. Otherwise I might have been horribly discouraged at the beginning, but by sheer luck, the jobs, — I put up a new antenna. Well, that just means climbing up to the roof. My mother was horrified. One day she's coming home, she sees me climbing around on somebody's roof. I was fixing the antenna. But gradually I got more and more difficult jobs, because people would hear, my mother's friends and so on. And finally I got jobs that were outside the family. I had a job — my friend Bernie Walker and I were working for a printer, and another printer, whom we had to deliver something to, wanted to help me out. He found out I fixed radios, so he got me some jobs, and they were completely people I didn't even know. I remember one of these particularly, just for amusement. The man got me this job. The fellow was to call for me in his car to take me to this place. He was obviously poor. The car was a wreck. We got into the car and he said, "You're only a child, how can you expect to fix —?" All the time going, he's all upset by "only a child," you know, all the time going, talking about this. I felt bad, you know, but I can't do anything about it. He's yakking all the time and he tells me, he's a guy that fixes radios, and so on. So finally we get to the place with the radio. I said, "What's the matter with the set?" He said, "Oh, it makes a noise when you first turn it on, but the noise stops and everything's all right." I thought, "How can this fellow worry about a mere noise when he's got no money?" So I got to the place, and there's this radio. I turn it on — and the noise was so blood-curdling, such a terrifying racket, such a terrible noise, that you could see it was a noise! I mean, the guy wanted to get it fixed. And then it would quiet down and everything would play right. So I turned it on, I listened to this, and I'd turn it off, and then I'd start to walk back and forth, thinking. So he says, "What are you doing? Can you fix it?" I said, "I'm thinking." This only made him make another thing. And I thought, you see. I figured out — how can there be a noise that disappears? Something is changing with time. So something is heating up before something else. So I guessed that the main amplifiers are heating up before the information is coming from the grids, from the earlier circuits, that it's picking up some kind of noise. So I figured, it was probably due to the heating of the tubes, so if I reversed the order of the tubes of the same kind, maybe it would be all right. It would heat up the other way around, you know. So I just thought that. I went to the back of the set and I changed the tubes around and put them back, see. I turned it on. Just as quiet as you please! This fellow — you know, when a man is angry at you and thinks you can't do something, those are just the guys that when you do something, they just love you. I mean, you're a god after — you know? I mean, that's the kind of person that if you do succeed, they're absolutely the opposite. He went crazy. He recommended me to everybody. He said, "This fellow's a genius! I never saw a man who repaired a radio first by thinking!" And I went back and

I changed the tubes around, that's all I did. I was very happy about that, because that was a great success of the mind, you know. I loved that. That was a big success for me, to be able to repair it, after all the things he had said. I love challenges. I always have. In fact, later hobbies at the beginning were not scientific but were always challenges. Picking locks, cracking codes, analyzing hieroglyphics that nobody knows how to translate — you see, they're all the same now.

Weiner:

Talking about these jobs — any other kind of job, more conventional in nature, in your high school period?

Feynman:

Yeah, I had this job with a printer.

Weiner:

What did you do there?

Feynman:

Just odd things. Swept the floor, put what they called the furniture away — that is the sticks of type and so on — delivered the circulars that had been printed.

Weiner:

Weekends?

Feynman:

No, afternoons. I tried to collect bills that were owed, and then I'd take stuff that was too hard for this job printer to the linotype printer, the guy I told you about before, and come back on my bicycle, about four miles, and so on. That kind of stuff. Incidentally, it's amusing how I got the job there, because Bernie Walker, my friend, and I decided we'd got to get some work, you see. So we went around from store asking if they had any circulars to deliver, because those were the days when they would advertise by hand things that you would pass around. We would have some success, but not too good. And this fellow, Bernie Walker, who later went into business, you see — you can see already the disease, I mean the methods — he said, "Hey, I've got an idea. Why do we go from store to store asking if they've got any circulars to deliver? We'll go to the printer, who makes the circulars, and we'll ask him who he wants us to deliver his order of circulars to. We will do it for nothing. Then when we come in we'll say, "We're delivering our

circulars from printer so and so. Do you need a boy to deliver them?" He said, "That's a great idea." I would never have thought of that. He had a businessman's mind, Mr. Walker. So that's what we did, and the printer was so impressed with our ingenuity that he said he needed a boy to take care of the place, and so he hired the two of us, alternately, to do this, you see. He was impressed by the cleverness of this, Bernie's idea.

Weiner:

Was this for spending money? I mean, this wasn't —

Feynman:

Yes, I think so. There was also the attitude that you should do something, work — you know, the idea that to hang around and do nothing was somehow... There was a feeling of some sort of responsibility to earn money. I can't explain it.

Weiner:

Well, it was also the Depression period. This would have emphasized it.

Feynman:

Yes. And also, to do some work like that, that's good. I don't know why, but that was the feeling — we should do it. I'm sure that if I didn't, I would have got the spending money, because it wasn't a lot of money, really. But the idea that you should get a job was somehow right. I don't know (women's tales) yeah...

Weiner:

When you were in high school, were you thinking about college? When did you start thinking about what the next step was going to be in your education?

Feynman:

I don't know when I started to think about that. I must have thought about it. I knew that I was going to go to college. In this particular high school there were two kinds of courses, one called the commercial course, and one called, I don't know, the academic course or something — I don't remember — but anyway, one was supposed to be for college and the other wasn't. There never was any question. I was not in the commercial course. It was my parents who knew I would go to college, and who in fact were saving, in spite of the Depression, as best they could, to make sure that I could go to any college I wanted, when the time came. So they were the ones.

Weiner:

Were you — (crosstalk)

Feynman:

The finances, no — although they had their problems, but the problems were not communicated to me. I was always guaranteed, you know, that — But I did have a feeling of responsibility to make whatever effort I could to help. But there was no fear that it wouldn't go through. So I never really was deeply concerned. I never had a decision, (I can't remember any) when I would go to college or something.

Weiner:

But then at one point you had to make a decision, because you had to apply.

Feynman:

Of course, because I had to apply. I then applied to the colleges. One I applied to was CCNY, I think, because of the financial thing, in case we couldn't make it. I applied to Columbia. I applied to MIT. What else I applied to, I don't remember.

Weiner:

Excuse me a minute. Did you have, at that time, the state Regents' examination?

Feynman:

Yes, we did, on all the courses.

Weiner:

Do you remember how you did?

Feynman:

Yes, I have all the records, if you want.

Weiner:

How did you do? We'll look at the records too.

Feynman:

In mathematics and science it was the absolute top, 99 percent, 100 percent sometimes, or 90, very close. In the other subjects, in the neighborhood of 80 something. I did better in the Regents' than I did in the courses themselves. You know, I was always able, if I had pressure enough, to work very hard and temporarily absorb enough information, say in German, to make a passing grade, and then forget the whole damn thing.

Weiner:

Cramming?

Feynman:

Cramming, yeah.

Weiner:

Then, with decent Regents' scores and these applications —

Feynman:

Fairly decent. Only fair. But good in the math and science.

Weiner:

You applied to these different institutions. At that time, did you know what curriculum you would choose, if you were accepted?

Feynman:

I thought I would go into mathematics, I think. I did, in fact, when I got to MIT, so I don't know exactly what I thought on that, but that's what I did.

Weiner:

Were you accepted at the other schools too?

Feynman:

I don't remember about CCNY. I was not accepted at Columbia. They said "No" They have an examination also, which I took, and lost fifteen bucks. You have to pay, apply, and take the exam. I remember the fifteen bucks. But I flunked — somehow — I didn't get in. At MIT they had a few scholarships, which I applied for and didn't get. I just got in the regular way. I think maybe I did get a small scholarship; I'm not sure, but not the

big scholarship that I had hoped for.

Weiner:

Were there any examinations for this?

Feynman:

No, there weren't examinations.

Weiner:

Based on existing records.

Feynman:

Yes.

Weiner:

How about College Entrance Examination, Boards?

Feynman:

There was no such thing, as far as I remember. There was only this exam at Columbia.

Weiner:

I think these existed perhaps earlier, but they weren't universal.

Feynman:

Maybe. Yeah. One thing, just to put into the record, which I didn't like, at the time, about MIT, was that they required a recommendation from a former student of MIT. I didn't know any former student at MIT. So I went to this man's office. The man talked to me for ten minutes, and then wrote, "This guy's a good man." I think that's all evil, that's all wrong, because it's dishonest. I was a very honest sort of scientific — if I want anything — it's true, you know. And that the school would request such a thing, and I had to go through this falseness — it bothered me. I remember it. I just put that in. That one thing I didn't like about applying to that school.

Weiner:

Why MIT?

Feynman:

It was supposed to be one of the best schools in science and technology.

Weiner:

How did you learn that, that it was supposed to be?

Feynman:

I don't know. My father probably knew that. My father probably thought that — you see, because I think my father and mother discussed it. MIT was more expensive than the local schools, I think. This I'm imagining, because I remember, later, say, how the two of them sat down and they tried to figure out how with the budget they could send me to the best school. They were great — you know? I mean, they were right behind me.

Weiner:

Did you have any idea who was at MIT?

Feynman:

People? Oh, no. Just that it was a good school. I didn't know in terms of people.

Weiner:

So you graduated from high school in —

Feynman:

— 1935. Right.

Weiner:

And what did you do that summer? Was that any different from any other summer?

Feynman:

That's the summer I got the job in — was it? No. What the heck did I do that summer? No. I don't remember.

Weiner:

What about other summers in high school?

Feynman:

I usually tried to work. I worked in my aunt's hotel, a few summers. One summer I was ill. Oh, that was after, when I was at MIT. I was ill from the work at MIT, apparently, over — something, and the doctor insisted that I relax that summer, so I stayed in bed and drank malted milks, and would just go to the beach — I had a great summer. But I was ill. I had gotten ill.

Weiner:

After your freshman year?

Feynman:

Yes, the freshman year. But that takes us ahead. You asked about social matters, and there are a lot of social matters to be discussed yet. Girls. We really didn't get to it.

Weiner:

In high school.

Feynman:

Yes. I don't know if you want to get into it now, or continue in this direction? I thought I'd remind you, before we get into the first year of college, that you left something out.

Weiner:

Yes, let's go back then.

Feynman:

All right. Since I had been a sissy and so on, I was a little bit worried about this relationship with girls. There were some other social things that were very good. There was a Jewish Center in town that got the idea to make a junior something — I don't know what you call it, a "junior league" or something — which was built to make the young kids of the high school happy. It was an organization that was supposed to be run by them. It had its own president and so on, but they would use the buildings that were associated with the Jewish Temple. And it was really, now that I think of it, quite a wonderful effort. They had teachers to come to help, and we were divided into units, like

there was a dramatic unit, an art unit, a writer's unit where the people who liked to write, like my friend who liked to write, would get together, and they would tell the story that they would write, and then the next week some other guy would write a story and tell it, you know. They'd discuss it. And in the art unit — which I joined, for a reason which I will explain in a minute — we started by making plaster casts of heads, of faces. You know, you lie on the floor and put the plaster on and have straws out of the nose and all this kind of thing. And then we'd try to make artistic things, for which I had no talent whatsoever. The reason I joined the art unit was that I had met a very beautiful and wonderful girl who was very, very interested in art, and she was joining the art unit. So, I didn't know her too well — I mean, as a matter of fact, I understood that she was the girl-friend of some other guy, and it was absolutely hopeless, because they were completely... But I joined anyway, you know, so I could be there, because I was dreaming of her. I don't remember the order of events, but some way I met her at a party. I think the first time I met her was at a party at which somebody was teaching us how to neck and showing us how we should kiss a girl — the lips should be at right angles, and this kind of stuff. And then we sat and practiced with some girl.

Weiner:

He demonstrated before the group?

Feynman:

Yes, with some girl. He was a little older than the others. Then we would sit and we'd try to neck. I had a girl I was practicing with. Then, just at this moment, there was a little excitement — "Arlene is coming!" — and everybody gets excited. It was the older group I was with, you see. Everybody jumps up to greet her. And I figure, nobody is that important — you know, this is not the way to behave — so I just kept on...

Weiner:

— practicing?

Feynman:

Practicing. She remembered it later, that she came in, and there was one person in the party she didn't like because he was necking in the corner. Nobody else was doing such a terrible thing. Anyway, we started out on the wrong foot. But at another party, where she was — I had seen her, and then I was really impressed with this Arlene, I understood why everybody had jumped up. I was at another party where she was. For a reason that I don't know, she sat on the arm of my chair. So there was hope. I was excited as the devil. She was with this older group, you know. She was in this junior center, so that's why I joined the art group. There was no science group, there was nothing like that in this junior center, but it was social. There were dances. It was a good thing to join, in

spite of the fact — And so therefore I got to know her, and then I got to know her boyfriend, who, it turned out, she broke up with, or it turned out she wasn't with him, and gradually I got to know her better. I took her to the dance. I told you that there was a lady with a dancing school that my mother knew, so I took her to that dance, and I introduced her to my friends, and the playwright man, by the name of Harold Gast, came up to me to tell me very carefully that "that's my girl" and he's "not interested," etc. I've since learned that that's a dead signal to watch out, when they protest too much. Anyway, he was then my competitor, after that, after his assertion that she was my girl and he would not interfere — a thing which I had not even asked for. But I learned. So he was a kind of competitor. She would go with him, or with me, all the time. When I went to MIT, we wrote letters back and forth. But the day when I really — I don't know what really convinced her, but one day that I realized that I was getting ahead of Mr. Gast was the day we graduated from high school. She was invited to our graduation. We were both graduating together. He had written a play. She sat between our parents — she was a very sensible girl. It happened that I won a lot of medals, for best in physics, best in chemistry, and best in mathematics. He wrote the play, but I was getting called to the platform every few minutes, you know — "Now the prize for physics" — "Now the prize for chemistry" — "for mathematics" — and so on. It was get up and get down, get up and get down, and because of a fluke — a fluke — when it came to English, because of the fluke that they had decided this thing on the Regents' grade and nothing else, which was unfair, because it could be an accident from cramming or an accident of the system — I had been relatively poor in English and these fellows had been very good. And my two friends, the playwright and the writer for the newspaper, neither of them got honors in English because they did badly on the last Regents — and I did! So this was a freakish accident. I know why I did, because for one time in my life I finally broke down in the English. Everybody, I listen to them talk; they're a kind of baloney. They don't talk direct, in English class, and I'm always trying to talk straight. In the examination, in the Regents' examination, they had things like, "Take a book and make a book review," and I would take the simplest freshman book, TREASURE ISLAND, while my friends would try something like Sinclair Lewis' book about the stockyards or something. Mine, though dull, was all right, so the dull teachers said OK — but she had objections to his interpretations of the social relations. You know, she was much more interested. But the real place where I faked it was, they gave us a list of compositions that we could write on different subjects, and for a change they had a few scientific subjects in there, one of which was "The Importance of Science in Aviation." To me this seemed incredible, that the person who had made up the title was an ass, because it was so obvious. I mean, it's such a dumb kind of a subject. But I figured for once I'm going to do what I see my friends do. So I wrote an article, in which I talked about the importance of science in the analysis of vortices, eddies, turbulence, and swirling motions of the air — all the same thing, you see. So I made these big words and I repeated myself, I did all that baloney and I got a very good — I think that's why I got a good grade in the English quiz. I did it for the first time consciously, because I thought it was so silly, nobody knows anything, so it's easy to write about these things. Anyhow, this seemed to have impressed my girl, but the thing that impressed her most was the

following: My parents, my mother — I don't know about whether my father was there, I can't remember — and Mrs. Gast went together down the hallway afterwards to talk to the teachers, after graduation, and they met Mr. Ogsberry in the hall, and he (my mother said) — They said, "I'm Mrs. Gast and this is Mrs. Feynman." He said, "Oh, Mrs. Feynman!" Just like that, you see. And then he makes her think, to try to impress her, what I had — "Your son has something the state should support, and it shouldn't be necessary..." Big deal. He made a great speech. He was very impressed by his students. And Mrs. Gast kept saying, "What about my boy Harold?" "Oh, Harold's all right, never mind Harold — now listen, Mrs. Feynman —" What he was worried about was that the parent might not understand, and might say to the child he must get a job or something. See, he was worried, but he didn't have to worry, because my parents were aware of this. My friend Stapler's parents were not — his mother; his father was dead. She was not that kind. He had to go out to get money, because she needed money, and that was the end of him, really. So I was lucky. But anyway, this girl-friend, Arlene, was standing there, listening to all this, you see. And after that I had a little easier job against Harold Gast. Or at least I imagined that that was the reason. Anyway, I fell gradually in love with this girl, and it took me six years before we got engaged. I met her at thirteen, when I was thirteen. I was thirteen and a half, she was thirteen, something like that, and we got engaged six years later, and we were married another seven years later. I knew her for thirteen years before we got married. It was quite an unusually long situation. We were very much in love by that time. A most marvelous woman. We had a terrific relationship.

Weiner:

You were married in 1940 — something?

Feynman:

1941 or 1942, yeah. Details we'll put in when the time comes, if you want. Then, when I was at MIT — I've cut the end of the high school. Yeah, that was the best girl, the most important girl. There were a few dates here and there with other girls, but this was the most important one.

Weiner:

You won these awards. Were they honors or specific awards?

Feynman:

Well, like medals, you know. I think it was maybe the Exchange Club of Far Rockaway would give a chemistry medal, by giving the money for the medal. The Bausch and Lomb Optical Co. gave the medal for science, I think, and they probably had a policy of giving the medals to the high schools for science, to finance it. So I remember, it was Bausch and Lomb that gave that, because my father kept saying to me, "Listen, you've

got to write them a letter and thank them for doing this. I'll dictate it to you: 'Bausch and Lomb: Gentlemen...'” So we had a kind of joke around the house, “Bausch and Lomb: Gentlemen.” I never wrote the letter, and he'd never dictate any further. But I remember that.

Weiner:

I think this is a fitting conclusion to the high school.

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Richard Feynman - Session II

March 5, 1966

Interviewed by: Charles Weiner
Location: Altadena, California

Transcript version date: December 18, 2024
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Richard Feynman - Session III

June 27, 1966

Interviewed by: Charles Weiner
Location: Altadena, California

Transcript version date: December 18, 2024
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Richard Feynman - Session IV

June 28, 1966

Interviewed by: Charles Weiner
Location: Altadena, California

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Richard Feynman - Session V

February 4, 1973

Interviewed by: Charles Weiner
Location: Altadena, California

Transcript version date: December 18, 2024
DOI: <https://doi.org/10.1063/nbla.orjj.lolq>



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:

Let me test the machine by saying that this is Charles Weiner talking once again after a gap of almost seven years with Richard Feynman in his home as before...I should say that today is the 4th of February 1973... We left off last time — although you haven't had a chance to look at the transcript about it, the very last item we talked about was the reaction to the Nobel Award, but specifically the Nobel ceremony: the whole ritual that was involved, the speech that you gave there, the response to it, how you felt about it. You gave another talk in Geneva under quite different circumstances and how this was an audience that was more responsive and more intelligent about the whole thing. And you said then that it was really too early to put it into perspective, but that is exactly what I wanted — the fresh flavor of it. We had taken your scientific work, in more or less reasonable sequence, up to that time. We had talked about your personal life — your biography, essentially — all the things that you were doing and feeling, the changes in your career, the motivations, why you switched. So now the intention is to really pick it up from then. One way perhaps, since I really know less about this than I did about the other subjects because I had more materials to work with and more perspective of my own is to get an over-view. Maybe we could start off by characterizing the period from '65 to now, in terms of your main thrusts of research, the subjects of the research — this is without too much detail — and then we would establish some kind of structure and go back systematically. Teaching — how this changed. Biographical details — your life since then — if it was just the same or if there were significant differences in it. I'm not even aware of whether you were away at all during the time for extended periods.

Feynman:

I can't remember anything.

Weiner:

OK. Then we'll start with the first year and see what happens. Let me just ask a general question about the main subjects of research. When I saw you since the interview — it was in your summer house in Santa Barbara that you were renting one summer, I don't recall the summer, and it may have been '68 —

Feynman:

It was '68.

Weiner:

That's what I thought. And you had come back from SLAC where you had seen some experiments.

Feynman:

Oh yeah, you came at a marvelous time. If you'd have really gotten that down, you'd have had something.

Weiner:

That was an exciting moment. You were very excited about the results. You we're talking with your graduate student there, explaining it to him with me present, and so I was witnessing something as it was occurring.

Feynman:

Have you got all that?

Weiner:

What do I have from it? I remember my recollection of it.

Feynman:

You didn't write anything down.

Weiner:

No, I want to talk with you about it.

Feynman:

Just cause you got this god-damned tape recorder, it has to be on the tape-recorder or it doesn't count, huh?

Weiner:

I could not possibly recreate the content of that argument but I want to reconstruct the general circumstances. You told me that you had spent considerable time boning up for this high energy course that you had volunteered to teach the following fall, in the fall of '68, so that is something for me to pick, that was the first time you taught that course.

Feynman:

Yeah, I'm very confused, I have taught it three times now and I can't remember.

Weiner:

That was the first time, OK.

Feynman:

If you'll wait a second, I have things by which I can remember. I have the notebook from that course. I have a notebook where I when I do my research or at least I did — I put the date on every page that I was doing things, as a result of you. So if I look at that thing I can remember better what I was doing at different times. There are dates on the pages.

Weiner:

Let's do it. Is it here?

Feynman:

Yes, let's try to see what we can do with it.

Weiner:

Really, that makes a lot of sense. We're starting again and the break was a very rewarding one because we dug up four notebooks. For a minute let me just describe them. These are loose-leaf notebooks with each page containing your work, everything that you asked yourself. It is almost in a first person diary form asking yourself questions and then setting out an agenda for work, indicating you spoke to so-and-so today and you got this idea and then you want to pose yourself a certain agenda and then tackling it. Each page is dated. Some pages have a later date on them because you have gone back to them and said, well, this problem didn't work out or it was solved in terms of the work done on June 5, 1968 or incorporated into that. And so this represents the record of the day-to-day work.

Feynman:

I actually did the work on the paper.

Weiner:

That's right. It wasn't a record of what you had done but it is the work.

Feynman:

It's the doing it — it's the scrap paper.

Weiner:

Well, the work was done in your head but the record of it is still here.

Feynman:

No, it's not a record, not really, it's working. You have to work on paper and this is the paper. OK?

Weiner:

OK. And one other thing, it's not always dated. Sometimes they are chronological on certain problems but in other cases the pages are bound in these ring notebooks on the basis of a specific problem, so you might find '67 and then followed by '65 and something else. What we are trying to do now is pick up from last time so we are starting with 1965.

Feynman:

Well, I got invited to Aspen, Colorado — there's a school up there — for a summer, and I went up there with my wife and Carl and the maid. We got a little house up there and it was very comfortable. I started to do some work in a systematic way. I don't usually work systematically but this time I decided that every morning at 9 o'clock I'd go to work and I'd work — I don't remember whether I worked a full day or a half day but I did come home for lunch. I guess I went home for lunch and came back in the afternoon. Anyway I worked certain — solidly — and my problem was to get into high energy physics, into strong interaction physics. And so I wanted to study everything in the library and everything that was known. I was going to teach myself the subject of strong interactions and this notebook that I see in front of me gives me a clue of what I did. I wanted to study the problem to solve it, to understand the strong interactions.

Weiner:

Was this something that you had been interested in doing for a while and had put off?

Feynman:

Yeah, what happened was this, that in the early days of strong interactions — I had always been interested but I had worked on things as you know like helium and other

things and you might wonder why because the fundamental problem really was in the strong interactions? I always felt, at least in the beginning, that the real situation was complicated, that we didn't have enough clues from experiment, that we were just beginning to learn about it and there weren't enough clues. So that it wasn't anything to really work hard on because you could never solve it, you didn't know enough about it. But as experiment continued, they got more and more clues. When Murray's SU₃ theory worked I was convinced that we really had enough clues and that we ought to be able to start to work on it. I wanted to start to work on it and so I had to learn it again because I hadn't been paying a great deal of attention.

Weiner:

You hadn't been reading the literature or keeping up or even --

Feynman:

I had been. I'd gone to meetings and listened to it and so on. I don't know exactly what my position is but I was easily confused by what people were talking about. I didn't know exactly what was known. I wasn't really up in front of it. So in this time in Aspen I decided that was what I was going to do. Before I went to Aspen I asked one of my students who was good — not my student, but one of the students, Bill Wagner — I asked him what the biggest problem in high energy physics was. You see I didn't even know what the problems were. And he told me the biggest problem was how the high energy cross sections varied with energy. If you want details I can tell you.

Weiner:

Sure.

Feynman:

There's a theorem called the Pomeranchuk's Theorem which said that the cross section for protons and for anti-protons should be equal at infinite energy. And it didn't look at all as if they were coming out equal. This was a serious problem, and the whole theory of high energy collisions was in difficulty. So I started out, with that in mind that that would be a hard problem — the high energy collisions. And then I went to this thing and I studied — I don't know what I did, looked in books and worried about it. Here are all kinds of details which I don't think we need to go into.

Weiner:

This started — the way you are dating this is June 5 — you're using the American style of dating?

Feynman:

Right.

Weiner:

Let me just read that first sentence if I can because I think that this, since it is the first sentence of this problem, this is what you are identifying as the problem in your first sentence.

Feynman:

“The problem is to predict all strong interaction effects, masses of stable particles, resonances or apparent resonances, centers and widths, all reaction cross sections, form factors of currents,” and so on. Then I make some preliminary random notes: what I’m going to assume is true. I’m going to assume that i-spin is true, and so on. I don’t know that it’s worth going into.

Weiner:

No, I just wanted to see if I understood it. OK, so you’re setting up the problem. This becomes almost a classical form for you, from this point on.

Feynman:

Yes — “Assume that electricity and beta decay can be disregarded as not essentially involved, only as presenting information as to the effect of photons. Some decision must be made therefore as to what is meant by photon couplings and also whether there is or is not a correction to perfect electrodynamics. Hence e- e+ scattering and so on is very important to watch” — stuff like that.

Weiner:

I see. Then you set yourself a series of questions which follow.

Feynman:

“Question 1. In analyzing photon reactions and photon form factors must we supply a meaning to the current density at a space-time point to matrix elements of such quantity, such as the matrix element of j/x by j/y for photo reactions and so on? This depends on the unknown limitations in quantum electrodynamics,” and so on. This is a little bit disconcerting because I am not talking so much as reading.

Weiner:

No, I don't want you to read. I just want to use this as a refresher from now on. So at the Aspen meeting then —

Feynman:

Then I made up a program of things to do, so I can tell you what I was trying to do. It says: "Not necessarily in any order or need each be completed, but just helter-skelter problems." This is one of the pages that says: "Look through the literature on cross sections, various energies, disintegration rates, and so on, and make a big summary of all known facts and explain as many as you can. Second, make a judgment on SU_3 an evaluation, and on SU_6 . Decide on the influence of open channel widths on mass relations." That was a thing that I knew about — well, I don't know — that SU_3 rules for the masses were beautiful but when there are different ways in which particles can disintegrate their masses should be shifted by that and why are the rules so good. Then "complete your already begun analysis of non-compact representations." That was something I did very nicely, beautiful mathematics, but never amounted to anything. "Reformulate ordered calculus done in ferromagnetic problem to apply to field theory." I've forgotten that completely. I don't know what that is — I mean I know what it is but I don't know where it — maybe I should do that, that's good.

Weiner:

You know that you didn't though.

Feynman:

No, I know that I didn't. "Recreate no closed loop, formulation and study Yang-Mills for SU_3 ." That would still be a good idea to do. This says "Not important"; to the closed loop formulation and it never was important. The closed loop formulation was finally published in the Wheeler volume

Weiner:

In the Wheeler Festschrift.

Feynman:

Yeah, and I had it worked out already. "Understand Dashen's n-p mass difference calculation." Dashen was a student who got his degree and in his degree he calculated the difference of mass of the neutron and proton by some kind of complicated theory. And I felt uncomfortable because I was on the examining committee and I really didn't

understand it. I had been rather embarrassed from time to time on strong interactions because I didn't understand — I wasn't up. And his bothered the hell out of me. It was a brilliant insult to understand the n-p mass difference and to figure out how he got it, so it was very interesting to me. I wanted to understand that. It has since turned out that it wasn't valid. That's why I didn't understand it. It has turned out that many many things that bothered me that I thought that I didn't understand because I didn't know enough about the subject, turned out that I didn't understand them because they weren't logical, they weren't valid. And so that was a thing that nobody has ever understood — Dashen's calculation — even Dashen.

Weiner:

But you can't make a judgment as to whether your intuition is right about it, if you have doubts about, our own knowledge.

Feynman:

Not when you're not very practiced, no — when you have no self-confidence, which I hadn't. I had no self-confidence. I was learning and so I couldn't say, I don't understand it therefore — I couldn't tell when I couldn't understand it. In fact, I thought at this time because I put it there that it was sound, but it wasn't sound. Now I have "Summary of Data." I start this data summary that I suggested.

Weiner:

That's the first problem you set yourself.

Feynman:

Yes, and I made this data summary, and I found out by plotting the cross sections against one over the square root of the momentum that they were all straight lines and that they went to infinite energy; the proton cross section could very well agree with the anti-proton cross section. This is infinite energy, and if one over the square root of the cross section [???] the energy can come to a constant. So I decided then that the particle and anti-particle cross sections could be equal and that Wagner was wrong. There was no problem there. Everything was all right. Not many other people knew that at the time but I didn't know that not many other people knew that at the time so I didn't publish these plots or anything.

Weiner:

This follows remarkably soon after.

Feynman:

Yeah, a few days

Weiner:

Were you still at Aspen?

Feynman:

See it says "Summary." I don't see much — "High Energy Physics, high energy cross sections" — I don't see much mysterious here. "Acts much like a blackish partly-transparent material," and so on and so on. Then I ask myself some questions.

Weiner:

Well, for people. "Why is nuclear material so opaque?" which follows from this. But was all, this done at Aspen too?

Feynman:

Yes, this is all at Aspen.

Weiner:

Did. You have the time to do all that?

Feynman:

I did it, every day I worked. I just finished telling you. Here are the dates, so it starts on the 5th — "Problem" and by the 8th I have decided there is no problem. All right, I worked three days hard. There wasn't much work. Then there was a conference — well, here's a conference at Stony Brook. Then, while I was studying at Aspen, I found a paper by Amati and Fubini which I have marked here in 1965, "excellent paper, well worth studying." It turns out that was correct — people are still working on Amati and Fubini, but they didn't notice it at that time. They didn't pay much attention in the early days, but I noticed it was OK, and I studied this paper and tried to understand it, and that is what a lot of this is about.

Weiner:

This is still Aspen?

Feynman:

Yeah, it's always Aspen.

Weiner:

What was the total time you were there? Do you recall?

Feynman:

We can tell the dates by months.

Weiner:

I didn't realize that. That is why I was asking. I thought it was a pretty tough thing to condense into a week or two.

Feynman:

Then I discovered — Murray had current algebra formulas, and there were some questions about commentators of currents. There were some questions about things called Schwinger terms and I found a much neater way in my opinion to describe what is equivalent to these commutation equations as properties of the scattering amplitude, symmetries of the scattering amplitude, and that was worked out here in 8/65. That is what this is about — then this checking Murray and showing that seagull terms screw up Murray but they don't screw up —

Weiner:

Why do you call them seagull terms?

Feynman:

There are things called Feynman diagrams. And when you have a diagram of a certain type — people have given names to diagrams of certain types so they can talk to each other — and a diagram that looks like this looks vaguely like a seagull.

Weiner:

I see, OK. It's not Joe Seagull. [Perhaps Jonathan Seagull]

Feynman:

No, it's nothing to do with Joe Seagull. So this is all part of developing that idea. I do a

lot of studying and understanding what I can't understand of other people.

Weiner:

You had everything you needed there in the library?

Feynman:

Yes, in the library, and all I am doing there is leaning. I'm just learning. This is top in time reversal. I'm trying to learn the rules for time reversal. That is January 1966 — that is later. I'm back now. Then I tried to make a relativistic — there are a lot of theories in here that didn't work, so you don't care about all that stuff.

Weiner:

Well, it would be good to make sort of an inventory of the topics over time — not now; it's a waste on the tape — because it shows where you are.

Feynman:

This date is wrong. It must be '66.

Weiner:

Take a look at the next item and see —

Feynman:

I'm sure it's January, '66 but I'm calling it '65. This is nice. Around this time in January I got a demonstration which I have always found is very pretty to explain why relativistic quantum mechanics implies a need for positron, anti-particles, and the relation between spin and statistics. There are other explanations of that — you know, particles that spin a half obey Fermi statistics, anti-symmetric wave functions, even spins obey symmetric, and the wave function is symmetrical. That's true, it's been proven by Pauli and also earlier by myself, but never in an elementary way, and I always felt as Mark Kac says, if you can state a theorem in an elementary way, then the proof should be understandable. If this theorem and its assumptions are all understandable, then the proof should be understandable. So I tried very hard to understand the proof that we need — Bose statistics for spin zero or even spin and Fermi statistics for odd spin — I thought I did pretty well. Now, here's another conference on high energy in Stony Brook. Do you want to go through this notebook this way?

Weiner:

Yes, I think it's OK. I don't think we should do it exclusively on this, but just to fill in what you don't remember without looking at this.

Feynman:

It sometimes says '66 and sometimes '65 — it must be one year or the other. I guess I don't know what year I'm in. Anyway — Rochester '67, but that's later. It's just not put in together. I put the conferences together and I put the date in next to it. I took notes in conferences and put the notes in the notebook.

Weiner:

That's an awful lot of work — of learning —

Feynman:

These notes I don't pay much attention to and never did. It turned out that the notes I took at conferences were never very useful for anything, and I don't take this much notes at conferences any more.

Weiner:

There are two things: one is notes you take and the other is work you do at conferences. But, in either case, here for these years, not much of it ended up, though, in a paper for those years. There may have been a tremendous time lag.

Feynman:

No, here for example, 8/66, "New Theory of Eight Current Components" — big deal. OK, grind, grind, grind, grind, grind, and grind — final result: "Therefore I have too many densities but even with" and so on — no, it seems I still do. End of line.

Weiner:

Bethe on nuclear matter, '66.

Feynman:

Yeah, I heard a talk by Bethe on nuclear matter. I never worked on that. This is coming from the back. Now, in the meantime — I was just looking —

Weiner:

Some of this is '65 again.

Feynman:

Yeah, it starts back here also on the other side. The theoretical work that I did was in the back — calculations, some theory of scattering. In 1965 I decided in the ninth month, that's September, that the theory of high energy total cross sections could be best understood by just diffraction of pie plates hitting each other, and I worked the details out to show how it worked and what problems there were and so on. These are all things that didn't work — well, they did work, that was true. What I really was doing was teaching myself. I wasn't interested in publishing at all. But I did discover a lot of things. You see I thought everybody else knew all these things. In the meantime I was trying, to teach myself. So I learned a lot of things that weren't known, or a few things that weren't well known. And I checked — things that people have noticed later as being simple, sometimes I noticed a little ahead. But the main thing I was doing was teaching myself.

Weiner:

Did you find any particular person in the period that seemed to have the best overall clarity of the subject as far as you could tell? I'm not talking about an individual idea or a paper, but someone —

Feynman:

No, because I'm soon off on my own, worrying about my own way. The best thing was the Amati-Fubini theory which I sort of learned but didn't —

Weiner:

But also the field was changing pretty rapidly even as you were learning.

Feynman:

Yes, most of the things I would write down and then somebody would publish it. You see I didn't know how much was known and how much was not known so it was more a matter of learning. For me to learn, I read a little bit and I work out the rest. I don't really read a lot of what the other guy does. I read what his assumptions are and if they seem reasonable then I work out the conclusions. I don't need to read how he works out the conclusions most of the time. But mostly what I was reading — you see I had a certain attitude the only thing that counts are the experiment. I wasn't interested in theory. I wasn't reading theory, never, except for this Amati-Fubini paper, for example. I would read almost always excremental results, experimental situations, and make up my own obvious theory for it, you see. So that was my attitude.

Weiner:

But wasn't there any theory in the period which seemed to dominate the field, one that you knew?

Feynman:

Oh yeah, there were things like SU_3 and so on.

Weiner:

But you knew that so you didn't have to read any more of that?

Feynman:

That's right, that was a known, but I didn't know how to evaluate that. You see that was a theory which is an approximate theory. There was what we call SU_2 , isotopic spin symmetry. That was perfectly all right and I knew that that was OK and that to read that, that was summarize able in a few words. There was an extension called SU_3 or the eight-fold way which is only approximate. That was much harder to evaluate — when something is approximate and it doesn't fit exactly, then you say, is it really valid or are you just fooling yourself? So I had often set myself in these notes the problem of evaluating SU_3 .

Weiner:

In terms of some specific problem though?

Feynman:

And then there was another idea called the quark model which also needed to be evaluated. I finally decided to make up my mind, about that in 1970 and started to calculate and work on it after I gave a course. I gave a course — this is out to time but it's the hell with you — it must have been 1969 or 1966 or something like that. And at the end of the course I was always feeling confused as to whether I took care of them — the students should learn everything so they can read the papers, right? And I was not feeling confident in the subject. You know I told you I was worried about giving it. So after I gave my course I had about two weeks left. I went all around and I said, listen this is what I talked about, is there anything else? So I went to Zweig and I told him. He said, well, you haven't told them much about the quark model...So I say, Oh, OK. So I sat down and started to evaluate it myself and I found — I think we can find it in here — some regularities among the masses. I did it with this mass squared because I had some good physical reason to believe that it was going to turn out to be a little different than

the way other people did it. And I gave them two mimeographed pages of notes about the mass regularities of the resonances. I was very impressed by the regularities, and they were regularities that were like those of Zweig except for a slightly different way of looking at them in some respects, namely to use the square of the mass instead of the mass. And I was rather impressed — I saw that, I should have taken care of Zweig's stuff, so this was what I gave them. And this particular note of the masses, I sent it to Rosenfeld — I talked to Rosenfeld up at Berkeley some time later and told him about it and he said he'd like to see it, so I sent him the same thing, two copies for him and Tripp, about these regularities. And there was a Hawaiian there by the name of Tuan. The next thing I hear is that Mr. Tuan is publishing a paper on all these things and he thinks they are very impressive and he wants me to have my name on the paper with him. Now, we are allowed to say things that are personal because it is all hidden, right?

Weiner:

Right.

Feynman:

So it was very annoying to me. I didn't think it was important enough to publish it — I think it was interesting, but it was the kind of thing you give your students when you are teaching this class — but he has all the stuff published. What can I do? I can't let him — he can't deny that I did all this — he says I did all this stuff and he's taking it — in fact, it was 99% me plus some errors, and it was misrepresenting what I was trying to say, so I was kind of forced into correcting it and becoming a party to the publication. So we published this note. I am not ashamed of this note. I'm just telling you the circumstances. I would never have published it if it wasn't for Mr. Tuan wanting to publish it. OK?

Weiner:

I see, and you saw it in a prepublication form, or was it the manuscript?

Feynman:

Yeah, a manuscript. He sent it and said, this is what I want to publish and I would like to have your name on it if it would please you, and so on.

Weiner:

But he invited you.

Feynman:

Oh yes, of course, there was no attempt to cover anything. It was perfectly straight. The thing that was embarrassing to me was that I didn't think it was worth publishing yet I couldn't say to him, well, leave me out, you publish it, you think it's so goddamned important, because it wasn't published right. It was screwed up. It was called mine and it was screwed up partly.

Weiner:

So you had to make it right.

Feynman:

First I made it right and then I said, all right, I'll put — it wasn't serious. I'm just telling you that I wouldn't have — that the material — I can find that thing for you probably, the little pieces of paper that contain all that's in this paper that I gave to my class as notes. And it was just because Zweig told me to pay attention to this — then I got interested in evaluating it more in detail, a few years later, because it seemed to me more important. And that's when I wrote the paper with Kislinger and Ravandau(?). We wanted to evaluate the —

Weiner:

You acknowledge in the paper, "One of us (RPF) would like to thank Professor George Zweig for discussions concerning regularities in baryon mass spectra." This is the paper, "Some Comments on Baryonic States." Who is Pakvasa? He is in on it too.

Feynman:

I don't know. Another guy from Hawaii. I didn't remember both names.

Weiner:

That's an interesting background of that, and that is what started from the course. I see.

Feynman:

Yeah, that was just in a course that I had given. Now we find the notes for the course somewhere. That was later, of course. Here in 1966 I am working on a theory of vector mesons and currents which I didn't feel meant anything in the end. I did an awful lot of work and it didn't seem to me that it was significant. By that I mean, I had a little more troubles than I had successes. Of course, by doing this I was learning an awful lot. The way to learn is to have a —

Weiner:

The terms you use are more the characteristic of your style like “is so-and—so real?”

Feynman:

Or “are you fooling yourself?”

Weiner:

Or “What is the real physical meaning of so-and-so,” — and this seems to be very often. It is hard for me to say it accurately from a random look but this is a starting point for many of these papers. In other words, “is this a real thing? Is it true?”

Feynman:

Yeah. Also at that time in 1965 a paper by Adler came out in which he'd gotten some relationships from Murrays current commentators and he was able to obtain the weak axial coupling constant proton in terms of pion scattering cross sections. And that impressed me because that's the relation of two numbers and my principle always was: only those theoretical papers which get a relationship between experimental numbers are worth saying any attention to and this succeeded. That is what got me interested in the current commentators and how I got this other method which I have just finished describing about describing the current commentators in another way on the properties of the scattering amplitude which I felt was more sound.

Weiner:

So that was the theoretical paper which you had —

Feynman:

But I never wrote that up, those amplitudes instead of current commutators.

Weiner:

What standard do you use in judging whether something is worth writing up?

Feynman:

I don't know. I'm a little conservative I think. I think a lot of these things would have been worth publishing, and Mr. Tuan, for example, forced me to publish, which might have been worthwhile. It wouldn't have been any harm not to have published.

Weiner:

The way you outline having read so-and-so papers and now reading over your shoulder in your notebooks, the way you write in the notebook and the way you work it out is very similar to your style in the papers, so it is not a question of taking these little scraps and now putting some words around them, because, in fact, the whole argument and the logic of it is developed right here in the notebooks. So sometimes it seems to me it would not be such a leap to go from the notes —

Feynman:

No, the question is: is it worthwhile, and so on. It's not the question of the difficulty of writing the paper.

Weiner:

That's what I guess I'm getting at — what is your definition of a worthwhile paper?

Feynman:

I don't know. I have no idea.

Weiner:

Do you mean in terms of fundamental insight, of really new knowledge, as compared to a little addendum?

Feynman:

Right, right.

Weiner:

The literature is full of addenda.

Feynman:

I know. Sometimes it is just a matter of writing it over again in a different way and it is like translating it from one language to another in many respects. This particular item that we are talking about — the current commentators in terms of amplitudes might have been significant and I should perhaps not have an excuse for not publishing it except perhaps laziness. It was a point. And more important was the work in 1967 that I found here that would correspond to a lot of the results on duality which are found

much later and the theory of Regge poles as due to a series of sum on resonances which later became the Van Hoff formula. That was earlier. That was important, and I should have published that so that other people would know about it. It would have been useful to them but I don't know if I was selfish or lazy or what, but I didn't publish it. I thought it was kind of simple-minded. I don't know why I don't publish more things.

Weiner:

How much of this got out in the same way —

Feynman:

But all the time I'm doing this I think I'm not getting anywhere.

Weiner:

Well, maybe you set yourself a different —

Feynman:

Yeah, the goals are high. The goals are high.

Weiner:

How much of this got out to the students? You gave one example of something that was —

Feynman:

I usually talk about these things in my lectures. Oh, I gave several talks in seminars on this theory of Regge exchanges, and Murray noticed it and we discussed some things, but it's a funny thing that I don't understand — among the students, there are lots of graduate students working and so on, and I'll give these seminars, and there's never anybody ever picks up any of my ideas to follow up unless the rest of the world does. I mean, at Caltech I'll give a seminar and that was good — it has the duality rules and it had a lot of stuff in it — and they don't pay any attention at the time. It's because I present — my methods are different — and they can't follow it by the standard ways or something. I don't know what it is. I don't understand it.

Weiner:

Do they follow it while it's happening or is it just that they don't realize the significance of it?

Feynman:

I don't know.

Weiner:

How do you know? How do you measure anyway --

Feynman:

— that they don't follow it up, that they don't do anything.

Weiner:

That you can measure because you can see the literature. Maybe they don't understand what you're saying.

Feynman:

Probably not, but they have every chance to ask questions. I try my best to explain it.

Weiner:

You can't ask questions if you don't understand that much.

Feynman:

Well, I doubt that I'm that obscure. I may be obscure because I'm using a different language or something. I have a reputation for being fairly clear. I don't understand it.

Weiner:

It's an interesting problem. Here you're giving them all the goodies, right?

Feynman:

Well, it seems to me that at times I've explained things that I had right and I was on the right track, but they're not complete though. They're not elegant mathematically. The axioms are not sharp.

Weiner:

That would, it seems to me, be even more stimulating to someone because --

Feynman:

Yes, more or less like this, it'll work more or less like that. WINR: And then he picks it up as a problem to make it work out. Well, I guess I'm projecting into a situation. It might be good sometime to talk to a few of these people objectively and ask them what their reaction is.

Feynman:

Well, the best way to find out what I was doing in 1966 and 1967 is to take this notebook. I was teaching myself strong interaction theory. That's what it amounts to, and working on the problems and discovering that I can't [??]. [??] results are very hard to understand, and so on, because, and so forth.

Weiner:

All this time, was it possible that there —

Feynman:

Hyperon decay p-wave, another try that doesn't work. Nobody has solved it. Maybe recently there's a possible lead that they've solved it, but it's still not solved. So there are a lot of things. I was teaching myself problems, teaching myself high energy physics and working on many other problems in high energy physics in those years.

Weiner:

And none of it ended up in those years in the literature but you did talk about it, you communicated it in conferences —

Feynman:

— to students, to Murray, we discussed it.

Weiner:

Did any get involved in conference proceedings during the period, do you know?

Feynman:

No, I don't say anything at conferences, no.

Weiner:

You wouldn't say anything at conferences; you'd listen at conferences. You'd question?

Feynman:

Well, what could I say, I have a theory and I myself don't think it's really very good. Or else I think it's rather incomplete, and so on. But I tell the students or something — I told them when I've given a seminar here at Caltech but those things are never taken up. After all, I didn't take them up myself, did I, so what the hell? What can I complain about? So that's what is happening there in those years.

Weiner:

Now let's on the dates in '65, what was the time period you were in Aspen? We didn't systematically do those dates.

Feynman:

I was just there during the summer.

Weiner:

Was it June, July, and August?

Feynman:

Something like that.

Weiner:

And you were working mostly alone except in the conference sessions listening? Was there anyone --?

Feynman:

Well, I did talk to people there but I always work alone. There were no conference sessions at Aspen.

Weiner:

It was just a summer residence. This is where Uhlenbeck goes, right — the same place?

Feynman:

Yeah, Uhlenbeck wasn't there that year.

Weiner:

I ought to go there some summer just to see what it's like, just to understand it. Are we going to be interrupted soon?

Feynman:

Shall we eat lunch, maybe?

Weiner:

Just for the record we did not have lunch because we were denied lunch. We had missed our cue and we will have it later. We decided that you will take a better look at the period of learning and of deciding what you knew and what the important problems were and of working them out, which didn't end up in the published literature, and that the best way you can do that is to look through your notes.

Feynman:

I've done that. I've seen that book. And I'm saying that we don't have to use these notebooks any more. I don't like this interview where I look at the thing and say that on this page there's such-and-such.

Weiner:

Oh no, I wanted you to talk to me.

Feynman:

So I'll talk to you now and forget the notebooks.

Weiner:

All right, so we talked of the whole sequence of sort of a tuning-up, a transition. By the way, would you regard it as a transition?

Feynman:

Yeah, I would say I was really learning the subject and gaining self-confidence which took two years before I felt I knew what everybody else knew and could do it as well as

they.

Weiner:

And you were doing it partially so you'd have something to tell students?

Feynman:

No, it was the other way around — I would decide to give a course on this subject in order to make sure that I learned everything about it. But even when I gave a course I was not confident that I knew everything that was important. So when I'd give a course, I'd always ask — what I call the experts around here whether I included what they would have included, did I do enough on dispersion theory, what about bootstraps? Ah, bootstraps aren't any good, they would tell me, and they were the experts on bootstraps so that's it. Then one year, I don't remember what year, I gave a course in which I said that the only theories that I would discuss were theories which gave results which agree with experiments, you know, that had consequence in experiments. And that made it so easy to deal with the theory — this is the way I always look at it anyway — you see the thing is, I told them it is not necessary for us to learn all the fashionable ways of solving of working on problems if they don't get anywhere with these fashions. We would just work on the part that works. And there's not much that work. So it turned out to be relatively easy. And then I thought I'd left out a lot of theory and these other guys, the experts, would get angry at me. They want their students to know all this. I always had a crazy lack of self-confidence in this field. And then when I would go to them and I would talk to them about this, they'd say, "That's all right. It doesn't work." And then I felt better, you see. I had picked only the things that did work.

Weiner:

You weren't trying to do a contemporary history of the field — you were trying to sort out the things that would be most useful to the students.

Feynman:

But I thought maybe they would need to know these fashionable methods which I didn't see working, but apparently they weren't very good and they slowly sunk into the quagmire of confusion.

Weiner:

You mentioned the experts around here at Caltech — Zweig, Murray Gell-Mann — who else?

Feynman:

Zacharias and Koichi(?).

Weiner:

What was your contact during this period with experimentalists — not just reading the literature, but were you in close touch with anyone who was working with the big accelerators where the stuff was coming out?

Feynman:

No.

Weiner:

And they didn't look for you either?

Feynman:

No.

Weiner:

How about the other guys who you were in touch with here — the theorists — were they closer? Yes, they had to be.

Feynman:

Yeah, they would run to Berkeley every — Murray would take a trip to Berkeley and come back with the latest data to worry about it, but I wasn't in that condition.

Weiner:

But at the conferences, many of them would be conferences where the experimental results would be —

Feynman:

In conferences everything that was known, experimentally and theoretically, was described and I would always attend these conferences and try to understand as much as possible.

Weiner:

Right. Now how long would you say that that period —

Feynman:

For example, I paid no attention to Regge trajectories because it was one of these theoretical things until the day at one conference a man gave a report which showed a whole series of resonances, called RSTU resonances, along a straight line in the mass square. So I decided that that phenomenon I must explain and that the slope of this line, an intercept of it was related to a power law of a cross section — there were two facts I gotta explain. So I started — I found out that if you had a sum of resonances in sequence and you exchanged each one you would get a power law and I understood it — and that's what I mean by the Van Hoff method of representing. It's sometimes called the Feynman-Van Hoff method of representing Regge trajectory by a sum. And Blankenbleter was very impressed by this. I had explained it to him at a talk I gave at Santa Barbara, I think, or Santa Cruz. And so it was all right. It's just that I didn't publish it. I mean I told about it but I didn't publish it. But it was very early and it was very simple. It came from experiment and it came from the meetings. That's just an example of why a meeting to me is more important. Whereas I could have learned all I wanted about Regge theory from the great inventors of it, by Gell-Mann who was right around the corner, I never paid any attention to it. It seemed to me high-class theoretical complication until an apparent experimental fact which turned out that some of those resonances there — but anyway that apparent experimental fact seemed right and it works pretty well, and ever since then it was my problem to understand that. So I've always taken an attitude that I have only to explain the regularities of nature — I don't have to explain the methods of my friends. I don't have to learn the systems and methods of my friends — only the regularities of nature — and that's a very economizing way of proceeding.

Weiner:

How long would you characterize this period in which you were learning, doing this kind, of work where there were no published results? Would you say through '67 pretty much?

Feynman:

Something like that, yeah. That's right — I see now there were no published results but there was an awful lot of work going on. I had just forgotten what I was doing. Because the transformation — when you remind me how unsure I was of myself in pre-'65, I didn't even know the subject, and then when I taught courses I was always making excuses and was afraid they would laugh at me, you know? But I wanted to give the course and they were all delighted, and this was serious. I really believed that, and I can't

conceive of it anymore, because now I feel like I know the subject.

Weiner:

Well, let's talk about that course.

Feynman:

I don't remember what year it was.

Weiner:

I think I saw you in Santa Barbara in the summer of '68. The idea of the course came up apparently in the spring of '68 or the following fall. Do you recall that, or I'll recall if you don't — what you told me then was that at a faculty meeting, you had wanted to get into it —

Feynman:

Yeah, I remember the whole story now.

Weiner:

— and that you spent the summer really reading, but you told me at the time with a great deal of uncertainty on your part.

Feynman:

I've lost all that — it's marvelous.

Weiner:

But I had known at the time you told me of how much you already had been going. I mean there are a couple of notebooks here filled, indicating that it gave you enough confidence to volunteer.

Feynman:

Well, I couldn't have done it otherwise. I didn't know anything. How could I?

Weiner:

I see. But that fills it in because it was a little bit distorted the way I'd understood it

before.

Feynman:

Well, from 1965 I started to work on high energy physics and I've been working on it intensively ever since.

Weiner:

Now let's talk about this contact with experimentalists. You just said that you had not during this earlier period — '65 to '67, let's say, even into '68 — had this contact. When I saw you, you had just come back from SLAC, where you did have direct contact. Maybe you can reconstruct that — I can help you a little bit because —

Feynman:

I can remember that cold very well.

Weiner:

First of all, how was it that you came to go up there?

Feynman:

Let me put it professionally rather than how I came to visit my sister at SLAC. My sister was up there at Palo Alto so I often visited there.

Weiner:

I thought she was at Syracuse.

Feynman:

Long ago. She moved to Palo Alto and worked for the Haines Research Laboratory. Now she's in Colorado in Boulder. But I visit her often and it was easy for me to go to SLAC, which is right around the corner from her house. It was just a few blocks away. During the summer of '68 I had another summer, just like Aspen, but instead of going to Aspen I went to Santa Barbara, and I worked on something. I went and walked on the beach and every morning I studied. I went into a little room that we had in this house and I worked. And what I wanted to understand was the collision at very high energy. It struck me as possible that at extreme energies things would be simple. There was another reason: everybody else up to that time had concentrated on what they called two-body reactions, and all the theories were based on two-body reactions and so on.

Now in nature, I knew that it just wasn't two-body reactions. When you hit things harder more [???] particles came out until in fact it was much more likely that more particles came out than two, and I felt that they'd gotten a little bit too concentrated on two-body reactions, that equations were open — they would have two bodies, then they would have three bodies, but that's too complicated. There's a certain thing called unitarily, that one of the properties of the two-body reactions is related to the many-body reactions, so you can't just do it by two bodies at a time. And so I knew that we had to do many body collisions, high energy collisions, and I got interested in exactly what happened at very high energy. And I had some idea from cosmic ray data. In fact, I went to visit at Santa Barbara at the University. I went to visit the University a few times and I would go to the library and read about the cosmic ray data at high energy, and there was just a little bit of machine data — the energies were [???]. And what happens is that first the cross section is more or less independent of energy, and second that the transverse momentum is limited. So I thought, well, what else can I say, and then I used as a model, field theory, for reasons that I won't explain. I tried to work out what happened if there was a field which consisted of two parts, one rushing to the right and one rushing to the left, and I took a limit when they were very heavy when the equations of field theory get simpler. So I worked all that out — the theory of the equations at very high relative momentum — and found indeed they did get simpler, that electro-dynamics could be expressed much more simply and all the equations would be simpler but that there was a scaling effect, that the equations looked as if they had the properties — if you did the same problem at a higher momentum, two different momenta, high and double high, OK — that the equation was the same. And that would mean that if you had a certain spray of particles — say a certain number of particles took two-thirds of the momentum of the collision, then that would be the same probability, to take two-thirds of the collision, than double the energy. That seemed obvious from the field theory, so I began to think that that might in fact be the case, and I started to work on things. I worked on different models — the Bremsstrahlung model, and so on. And I had some trouble with the small momentum particle. There seemed to be a lot of them, and one possibility was that there were a lot of them, at the small momentum. And I studied that and concluded at first — you see, all the cosmic ray people said there was a gap at stall momentum so when I saw I was getting a bump at small momentum I was very worried about it. So I studied it because I was worried about it and then I realized that it wouldn't be a bump the way the cosmic ray people plotted it. It would be flat and they said it was a gap — there were no particles in the low energy region. So I went and looked at the data, and it looked like to me that it was statistical, that it was just an accident. They only had a few cases in the data that I looked at, and it wasn't clearly a gap for my money. It was statistics. It was luck. A few of them had a gap but it could have been like the chance that that was one in half or something like that. So I concluded that it was uniform or that it might "bump" in the middle — that it was possible, but I wasn't sure.

Weiner:

The data that you were talking about here —

Feynman:

In the meantime I had invented — yeah, in cosmic rays. I had looked in the library —

Weiner:

OK.

Feynman:

And all this stuff was gradually forming, but I wasn't sure. I had all these models and the variables, the proportionality and scaling and so on, but I wasn't sure of everything, you see, of how the things went. And I had this method of thinking which is called partons — because I started with field theory I needed a name for the particles of the field and the constituents and I called them partons. And you can even find the phrase in there where I call them partons on the right date if you want to.

Weiner:

Great.

Feynman:

And I started to think this way which was very different from the way many people had been thinking. It was very hard to think of two collisions of two things, neither of which was clear in my mind — each proton was complicated and the two had to hit each other. I would walk on the beach and think. It was very hard. And then I went to SLAC to visit and they told me about an experiment in which they scattered electrons off on protons at very high energy, and that's half as complicated because the electron has no — we know the structure but the other we don't know, so it's only half unknown. And they told me about it, and they told me that Bjorken said — Bjorken was out of town at the time, but he said — leave it right there and I can pick that one right up.

Weiner:

We're after lunch now and we resume right after SLAC and the last person you mentioned was Bjorken.

Feynman:

Yes, the fellows that showed me the scattering of electrons from protons said that it was much bigger than they expected and I didn't know whether it was supposed to be bigger

or smaller or what. And then they said that Bjorken had said that there was a certain scaling rule which is now known as Bjorken's Scaling, that something depended only on something else, and that the data fitted very well. He was out of town, but would I explain to them please why he thought there should be such scaling? So I said, why didn't, you let him explain? Well, they said, he is out of town and he explained it very complicatedly — you always explain things nicely. I said, I don't know up from down or what the hell to expect about this. So then that night I went to some topless bar or something like that. I was in a motel, and I didn't sleep very well, and at two o'clock in the morning I began to think about this. I realized that my part-time pictures would work very well and I took a little piece of paper next to a lamp and worked out this thing and saw where the scaling came from that Bjorken had predicted. I realized that that was why he had predicted it. So the next day I came and I said, "Yeah, I know why Bjorken said there is scaling," and I explained it. Drell was there and other guys, and they said, "Oh, I never understood why the high energy wave functions were so important." So I understand it now, yes, very simple, so I explained it. And the next day after that Bjorken came back from his trip somewhere and it turned out that I had explained the phenomenon that he called scaling but not by the way he had found it, by another completely different way — this point of view of parton, which is physically very very simple — it is that the electron scattering can see the constituents of the proton. And so I was quite excited. But one thing they showed me there, they said. "But in your interpretation there would be many partons of low momentum. There would get to be a very large number of partons of low momentum according to this data." I got very excited then. I said, "That's right, though, that's right. The high energy collisions of hadrons indicate that that's the way it goes." But I couldn't explain to them why because I would have had to explain this half understood theory, but that confirmed me that my views were right then. The thing that I had been worrying about whether the number would go up with small momentum, well, here was the direct measure and it did go up so everything was clear. And then I came back and I worried about this theory of high energy scattering, also in terms of current commentators, and all the rest of the things, and also continued to worry, still in Santa Barbara, about the high energy collisions but much more confident that the ideas were right. And then I worked out how the high energy collisions should go — and I don't know if I was invited or somebody asked me if I would come to Berkeley, and I said, yes, and I'd give a talk because I had these ideas how the high energy collisions go. I told my wife before I went that I was going to Berkeley where they have machines, all the apparatus, and they know just how the collisions go. I don't know how the collisions go. I have a theory about how they should go, but I'm going to go up there and give this lecture. As soon as the lecture is over somebody is going to stand up and say, "But that's ridiculous. They don't go like that at all." So I'll know if I'm all wrong, or if they don't object right away to it, I'll know it's right. It's a big moment. So I went and I gave my talk in Berkeley at a seminar. I said how the collisions should go. Of course, they don't understand me exactly right away the first time. So to understand better the various experiments with that, they said, "How do you think it should go in such-and-such a situation?" I said, "Cross sections should be constant." So Paul [?] said, "That's right." Another guy said, "How should it be?" "It

should be constant.” “That’s right and it’s been puzzling us. It’s the only cross section that’s constant that we measured. And then somebody else went afterwards — three people asked the questions and each one I got right. Then the fourth guy asked me a question, and I said, “It should be constant in energy.” He says, “I’m sorry, it varies by a factor of 16 when you change the energy by a factor of 2.” So I said, “That’s an experimental error.” And he’d gotten so convinced from the three that worked that if anything worked at all, if it wasn’t obviously ridiculous it must be right that there was something wrong. So I said it was experimental error. Everybody started to laugh but I kept a completely straight face and they realized that I was serious, that I believed what I was talking about that this can’t be true what he had told me. And then there was a lot of noise and excitement from that, you see, because it was a little nervy to tell the guy when he says it changes by a factor of 16 that it shouldn’t have changed and it doesn’t change at all. And so some other experimenter asked me something and I got that one right. And then Rosenfeld said to the man who had asked me the one that didn’t fit, “I don’t think you explained your experiment to Mr. Feynman correctly. You said it was this way, but actually how could it be that way?” “Oh no, no, it was this way,” he said, “That’s right.” So they rearranged it — it was described to me wrong, it turned out. When he had described it right, yes, it should fell in line, so it was due to a misunderstanding, not an experimental error. But this confirmed me that I had a good idea, you see, and that everything was right, and I came home to my wife and told her the excitement and that everything worked. And after the meeting, after the conference, Mandelstam came up to me and he said, “Hey, how do you do that? How would you have the confidence — it was so exciting — to tell a guy it was experimental error?” He said, “If I had a theory and the guy told me it didn’t fit with the experiment, I’d never have the confidence.” I said, “That’s because you’d never had the right theory.”

Weiner:

And you had the confidence to tell him that.

Feynman:

Yeah, I was just joking.

Weiner:

So when I caught you in Santa Barbara it was in between the Berkeley and the SLAC thing?

Feynman:

In between, right. The Santa Barbara year, therefore, was very profitable. One of the other reasons it was very useful was that there was a — let’s say was it at Berkeley? No, could it be at Santa Cruz? I must have given a talk at Santa Cruz — some short time later

I gave a talk at Santa Cruz — it was the same ideas.

Weiner:

While I'm switching the tape — This is side two of the first cassette. As far as pinning it down, were you invited up to SLAC to give some high school talk or something? This is what you said to me. There was some reason, some other talk that you were going to give.

Feynman:

Oh yes.

Weiner:

And you told me back then that that was the excuse essentially to get you up there so they could show you the stuff. I don't know if it helps fix the Santa Cruz thing or not.

Feynman:

No, no, one was Santa Barbara — that was the electron scattering business.

Weiner:

You went up to SLAC for that?

Feynman:

SLAC, yes, sorry, I was in Santa Barbara, and then I went to Berkeley for another talk.

Weiner:

The same summer?

Feynman:

Yeah, and then I gave a talk, either that one or somewhere where Mat Sands was — I think it must be Santa Cruz.

Weiner:

That's where he is.

Feynman:

Because after I gave that talk — see, I invented some words for kinds of collusions called inclusive and exclusive, and I didn't like the words. And I remember we worried about them. At the party we had at Sands' I asked everybody for better words and we kept — we couldn't think of any better words so we settled for those. So I remember giving a talk there — I must have given several talks. I also went to CERN somewhere along the line, probably when I was in Europe for some reason, and gave a talk on the same subject there.

Weiner:

But the parton idea then originated in the work you were doing in the summer of '68?

Feynman:

Yes, right, that was one idea and the other idea was to look at cross sections a certain way in which it was easy to analyze the consequences — we now call it inclusive cross section with the idea that you measure one particle and you don't care what the others do. At that time people thought that you should measure all the particles. It was much easier to measure one and not pay any attention to the others, and that was the part of the cross section it was easiest to predict, so it was therefore a very great influence because experimenters could see, they could test my theory by measuring the simplest possible thing to measure rather than having to measure the whole cross section. So that was very useful and the words "parton" and "inclusive and exclusive" cross sections now dominate the study of high energy collisions.

Weiner:

The Feynman scaling, so-called, came from that same thing.

Feynman:

That's right, yeah. That's what I was talking about at Berkeley. I was predicting all these scaling and everything else and that certain things shouldn't change and certain things should change and I was guessing it right, luckily. But I had no confidence when I went to Berkeley that I was right. I had told my wife I'm going to find out — this is going to make or break it — I'm going to either feel like a complete jackass or else a genius, one or the other —

Weiner:

Well, you must have been excited.

Feynman:

I was. When the first three things came in and they were all right, I was convinced. So by the fourth question I was so convinced that I had the nerve to tell him it was an error because I didn't know what the cause of the error was it wasn't experimental, it didn't describe what he did to me exactly. He said he had measured deltas on anything, but you can't measure deltas, and he didn't measure deltas on anything, they were just deltas and nothing else. So —

Weiner:

Had anyone else been doing anything pretty close to that, parallel?

Feynman:

Yes, Ken Wilson apparently had worked out the same scaling, or so I understand, several years earlier but nobody had paid any attention to him, and I didn't know anything about that, of course. It was a shame for Ken Wilson. I don't why they didn't pay any attention to him.

Weiner:

Yet you didn't publish yours, as far as I know, until the conference proceedings and the Phys. Rev. letter which were — well, the conference was September '69 and the Phys. Rev. letter was October '69.

Feynman:

That's a full year. So I was giving lectures in various places.

Weiner:

I see, so it was widely known in the community.

Feynman:

Santa Cruz, CERN — that was what I was trying to tell you, I also gave a lecture in CERN — yeah, it was widely known but there was no data. I mean, now there's lots of data that's — I don't know what took me so long to publish it, my usual reluctance to publish.

Weiner:

Well, even here in the conference thing which you wrote up probably after the conference, there's a lot of caution in there. "For this reason I shall present here some preliminary speculations on how these collisions might behave even though I have not yet analyzed them as fully as I would like."

Feynman:

Yeah, well, that's the reason I hadn't published it yet. You see here's what happens to me: I get an idea that I'm going to deduce something. If you have to deduce it then you're sure. I start out with a method of deducing things and understanding things. I was going to explain the Regge theory from the way the Hamiltonian shifted. You can find in the notebooks — I had all kinds of plans of how everything was going to work out. But I don't work in a straight line. So I leap ahead in one direction and I'm expecting to close up with the other and make a finished product, OK? But the leap that went in the forward direction works. I think that's not all there is to it. I think I'll be able to make a bigger do, and I work on it, but it doesn't get any bigger. I get confused or something. But the leap ahead was very important. So this thing at this meeting is illustrative of this speculating — I had leapt ahead. I hadn't really deduced it and I had expected to analyze all this much more deeply and much more clearly but I never did.

Weiner:

Yes, over here, for example — I'll quote again from your introduction. This is at the Stony Brook high energy collisions conference. "I should like to present these guesses for you to see if they are possibly true" — these are obviously cautious words — or if some of them are obviously in disagreement with experiment to learn where I may have already gone off the track in my thinking." That seems to me very similar to what you were trying out in Berkeley — the idea that you listen and you guys stop me if I'm wrong.

Feynman:

That's right.

Weiner:

There's another thing at the very end of it. Again, you say that "I believe the cross section will vary as $1/w$ but this is not on as firm a basis as the others suggestions."

Feynman:

That particular one has never been checked.

Weiner:

It's still not, I see. How much of this was taking old ideas and adding some insights to it, presenting and re-stating it in a new way.

Feynman:

None.

Weiner:

This just came out of that summer's work in '68?

Feynman:

That's right. It came from new ideas but with certain guessing leaps. It goes like this. We have a theory of strong interactions so we can't deduce really what should happen. And we have partial theories which permit you to deduce only in that region — if you say that [???] behave like Regge, then they're going to behave like Regge. OK, that's sort of a prediction, but it's only because this one does like that one, it's not really a leap. I was going to try to describe how high energy collisions behave without basing them on anything. I can't just say this one is going to behave like that because that hadn't been worked out yet. I had to go into a new realm, OK? But we don't have the exact theory so I start with models and see the properties of these models and then try to understand how much of the properties sort of general and sort of have to be are and are more or less independent of them. And then I make other arguments around and about, checking that are self-consistent. For example, if I suppose that the particles which originally come out are not the pis which we observe but rhos which are disintegrated pis will it change my prediction? If it doesn't, if the nature of the prediction is so that it'll leave the answer unchanged, that's very good because it means it's much more likely to be true, because undoubtedly some of them are rhos and some of them are pis, and if the character of the prediction is that the scaling — you have a momentum which will be proportional to something or other, and it doesn't make any difference which momenta I'm talking about, whether of rhos or pis, the theorem is still consistent — then it's more likely to be true. So by all these kinds of arguments I gradually became convinced that something is true. But I am not in a position like of deduction where I have a theory that I establish this stuff, so I get convinced it's gotta get like this, that it's the neatest way to do. And so that's the way it is and I can't — well, that's the way it is. I just never deduce it. I just make it more and more likely to be true.

Weiner:

The critical point in this case came after the Berkeley meeting, right and then the rest of it was —

Feynman:

Yes, but the major push was — the thing that really clinched it was SLAC. I had a lot of these ideas worked out and the fact that the SLAC experiments had this one surprising scaling was so easy and physically obvious to interpret, and with that interpretation I could even get some of the distributions of the parts that I had been talking about. I realized first that the SLAC experiments were very fundamental and secondly that my ideas were in the right — were worth something. And so then the next step was whether the hadron collisions indicated that they really were OK, and so I — yes, I wasn't sure of myself when I went to Berkeley, but I was sure of myself after three questions. The thing of it was like this: it was either completely wrong or it had a good chance of being right, you see. It's hard to explain. It had a good chance of being right although it might have been wrong, and if it were wrong it would be obvious to everybody who had ever looked at one of these collisions. And here were all these guys who had looked at many of these collisions, and they're sitting there and I'm telling them how it's going to behave, and they tell me, "Yes, that's how it behaves and I didn't understand that."

Weiner:

You know what it would be good to do — to go to Rosenfeld and a couple of other guys just to ask them —

Feynman:

— how it looked.

Weiner:

Yeah, from their perspective, quite different from yours, about that moment. I saw him in New York just to say hello a couple of days ago and maybe I'll see him in Berkeley — not to sit down but just to ask him about this.

Feynman:

Sure.

Weiner:

Let's talk about what happened then — the impact on other people and on the field. It may be difficult for you to answer but it seems to me that, as you said, these things are known now by names and are used by lots of people in the field. Do you think that this, in terms of the experimentalists, that this turn was a stimulus to them so that they would take the ideas and go out and try to test your ideas, rather than the other way round?

Feynman:

Yes, very definitely.

Weiner:

Where especially was this done?

Feynman:

In addition to my idea, at the same meeting in high energy physics there were some ideas of Yang and a number of other people — that was described by Yang. And if you put the ideas of mine and him together you get an even more complete picture. Most amusingly, it's interesting that that meeting — that Stony Brook conference is interesting. I have a copy of it. You see the experiments describing their results — in fact, there's a paper by Woodward or something like that (I forgot the name) on the experimental conditions in high energy physics. It's a pleasure to read because he goes through the whole thing and he describes two phenomena: one is the lumping up at low momenta of the pions which are what I was particularly — and another was one other feature, I don't remember, a little bit later, of which he says, "are very puzzling — and it will require some very classy theoretical ideas to explain these." And then I give the paper which explains those. If you wonder why I didn't refer to his paper and use it already to prove it, because I had missed — I had slept through his paper and didn't know that he had pointed out, until I read the proceedings later, in which he was giving a summary of the difficult subject of the very high energy collisions in which he had noticed a few phenomenon, one of which was the bunching up at low momentum — and I was predicting the bunching up at low momentum — but I had slept through his thing and I didn't know that he had told this. Otherwise I would have jumped up in terrible excitement.

Weiner:

Didn't anyone in the audience see this?

Feynman:

I don't know that. I don't know. They asked me questions and I answered them and I don't know whether they were referring to that or not. So that there was already evidence that I was on the right track right there at the meeting but I didn't notice that at that time. So with the ideas of Yang and mine together, they had been very influential. I doubt, I don't know what would have happened without it. It may well be that they would have measured the same things because they are technically the light, the easy

thing to measure with counters. They are harder to measure with bubble chambers. What people usually measured was the probability of getting exactly five particles and that's technically very difficult and theoretically to predict because the chance that you don't get a sixth particle is hard to correct for. The name called them [??]. There was a lot of that stuff and it was impossible to use it to analyze anything. I couldn't use it — that's why I didn't know how the cross sections went in spite of all the published data. This inclusive cross section is the kind of cross section that is very easy to measure. Now it is likely that as the machines developed they would have been measuring just that kind of a cross section anyway — they would have just seen in direct experiment what we were predicting, that it was just obvious. I don't know. But therefore the words become useful because they're just what they need — it just happened to be the right moment, that's what they were going to measure anyway and they needed a word to describe it, so inclusive and exclusive. The parton would never — it was a word — it was an idea, and is a theoretical idea, which isn't necessary from history or experiment — inclusive and exclusive, yes. But partons, that's an invention and that represents something real. In the years since then, I get confused often. See, I found out later that Ken Wilson had talked about this. There was some theory called multi- peripherals or something that had something similar and other people had derived things. So I don't really know sometimes when somebody gives a result whether that's one of my — they're getting it from a paper — I had said that, yes, but maybe they're getting it from somewhere else. I can't tell often where it comes from because now they're using inclusive and exclusive and you can't tell if they got it from me because it's word they all use.

Weiner:

What about, for example, were there very specific elaborations of the ideas that you know of? There have been a great deal since then, right? Gluons, for example.

Feynman:

Well, those are more specific. I can tell you the history of some of those if you want.

Weiner:

Let's keep that for a minute and, let me get back to one thing I remember about the SLAC story from the conversation I overheard and participated in when you came back and that is that you were talking about the language that the people were using and you were reacting to that. You felt that it was kind of an obscure in-language, the language of the existing theory of the various categories. When I listened to you I thought you were talking about the Ptolemaic circles because that's what it sounded like. The impression that I got is that what you were doing in your own work was trying to cut through all that without the necessity of even learning that kind of peripheral terminology.

Feynman:

Well, I certainly wouldn't bother to learn —

Weiner:

OK, well, that's not a separate issue —

Feynman:

One thing I'm reminded of a SLAC story — it's probably immodest of me to tell but it gave me a certain pleasure. A personal friend of mine was talking to some guys from Santa Barbara — when I was in Santa Barbara I used to go up to the university to the library to read about cosmic ray collisions and I'd ask these guys about collisions, and one of these guys told my friend that during that summer I'd come and I'd ask the most naive stupid questions about how collisions behaved and everybody knew it and they'd answer them and then I'd say something dumb and ask more questions. And he decided that I was over the hill, you know, I was asking such elementary stuff he says. And then there's a big explosion — I come out with partons and inclusive and exclusive reactions, and the whole god-damn bit — and he says, "There must be some secret way. He's not over the hill at all. There's some secret way to —." But I was asking the fundamental questions, you know, not the complicated questions, somehow. Anyway, it's a good story and I enjoy it.

Weiner:

Maybe to appear dumb is a symptom of an impending breakthrough.

Feynman:

Maybe — I'll have to start to appear dumb again.

Weiner:

To see if it works? Well, you were talking about some of the background of other concepts, elaborations on things like gluons which is a separate story.

Feynman:

Yeah, well, it was pretty clear in the beginning that there must be some neutral partons too, whatever they are, and they're neutral partons. But very early in the game Paschkos and Bjorken probably — I don't know now who it is — suggested that the partons could be quarks. And one of my ideas about the thing of having still interactions between the partons seemed to me to be contrary to quarks, cause quarks don't come out. If they

don't come out they must, I thought, have strong interactions. Therefore, I thought my parton picture might well be impossible with quarks. So I wasn't saying what the partons were but these guys suggested they were quarks and it took me a long time to come around to realize that that's a very — that it's quite possible that they're quarks and it's a very interesting possibility. But I was slow in that because it didn't seem to me at first to be a consistent view.

Weiner:

It was from them that you —

Feynman:

Well, that I — no, then I learned — sort of, they may have suggested it, but I argued about it. But in the meantime I studied this quark model with Ravndal and Kislinger to believe in quarks. I had to believe in quarks first. So I did that development to convince that there's something right about them, and then when I was convinced that there was something right about them, of course then I was willing to consider them more seriously and to go back to the apparent paradoxes and see that they really weren't paradoxes at all and it was possible. It still isn't perfectly clear that it's possible but it's very interesting to suggest that it's possible. And so I consider that as the most useful hypothesis because it's such a peculiar one and I use it as much as possible.

Weiner:

This is already when you talked about the work with Kislinger and Ravndal. This is June of '71. Well, you sent it in December of '70. Because of so much data that you did with all these matrices — this is the one, yeah — it's like a different approach essentially for you. You ordinarily don't deal with this much data and try to do this kind of thing. It's not characteristic.

Feynman:

No, but all right I'll tell you what we are doing. There was a theory proposed by Zweig that protons are made of three quarks and so on and baryons are made out of three quarks and mesons are made out of a quark-anti-quark. And he got a number of observations from this, that the magnetic moments looked reasonable, and so forth. The question is — then people had made models that the quarks are held together by springs and so on, and there was a lot of talk about these things. But when people who would do that — and the question is: is it right or is it wrong? In other words, are there these regularities? Do the things have regularities which are indicative of the fact that they're made out of quarks or is that just a lot of numerical [??]. Usually when people did it they had a lot of parameters that they could adjust and then they would talk about some other thing — well, in this particular case we'll suppose that this configuration, you see,

because it didn't work and so they'd add another term when it didn't work and so on. And then you couldn't tell after it worked whether it works now because they added the other term or because it's real. So I was not able to make up my mind whether there was hidden in the data a clue that this regularity, that this Zweig viewpoint was really right, or was just an accident. And then Walker had found out that this particular model with springs give remarkably good results for photoelectric matrix elements that he had measured. He had noticed that, and Ravndal was working on this and told me about how it works but that certain things don't work at all. And I saw why they didn't work — certain things involving transitions of half spin — I don't know if I can explain, I don't know one word to explain the difficulty. Anyway, I saw what it was and I proposed an explanation of that and I took my old theories from 1966 about the pieces being held together by springs — the relativistic theory — I had much fewer parameters than anybody else, and I made a rule: the three of us, we wouldn't put any configuration mix; we wouldn't do anything, so that we don't fool ourselves, and we'd just simply calculate and by the sheer weight of the evidence we'd find out one way or another without adjusting things. So it was a matter of taking all the data when and you can't just do it with a piece because you're fooling yourself. You take a piece that fits and the piece that doesn't fit you don't notice. We had to take the whole goddamn shebang. We went through the whole thing systematically to see whether it looked as if it was an accident or it was real. And we concluded that it was very likely real, that there's something underneath all this that looks like that model. And I was really doing that, not for publication, but for my own personal — that converted me into a quarkian, OK. I had to decide whether these quarks were an accident or [???] were likely to have some reality. And I concluded that they were likely to have some reality from the result of that work. So it was an exercise to determine — like all the rest of the studies I made, but I had worked with these — Ravndal had started it, and Kislinger and I got in on it.

Weiner:

What was their status? Who were they?

Feynman:

Students.

Weiner:

They were students. Was this the thing they got their degree with?

Feynman:

No.

Weiner:

Post-docs?

Feynman:

I don't know... yeah, probably post-docs.

Weiner:

I heard the description of this, referred to as sort of a molecular physics of quarks. Would you think that would be an apt way of putting it?

Feynman:

Yeah, very poor. It's physically not very good. The equation is sloppy and everything is kind of dopey, but it was definite. And the question was: we take these rules and we don't change anything — it was merely an exercise to see whether —

Weiner:

What was the result as far as other people were concerned?

Feynman:

I don't know and I really don't care.

Weiner:

There's a nice line in here about — “we have sacrificed theoretical adequacy for simplicity” — right up front you made the point in order to justify what else you were doing. It was massive, as far as the data — this is the thing that impressed me as being so different from the others. There is one point here that I was looking for — well, I'll think of it later. I can't describe it now. OK, now let me ask another question. You used a quark model just in an earlier paper, “Some Comments on Baryonic States.” You had no misgivings about it when you used it?

Feynman:

Yeah, that helped me to get convinced that it was worth — see, I tried it and it gave the parities right. That's one thing. There're two things. The quark model has the right symmetry to give the right states. The question is: did it have the dynamical sense too? It's like Mendeleev makes a chart and says there should be eights and then there should be thesis and things fit in. Then you could say, the reason there are eight and so on is

because there are so-and-such parts in there and then they go around each other and they will produce various effects if they go around each other. The question was to check whether they're dynamically there. — If this is satisfactory too, and that was what this was about...

Weiner:

There was an interesting comment you made in that about that you and who else did you do it with?

Feynman:

Ravndal and Kislinger.

Weiner:

No, I'm talking about the earlier one, on baryonic states.

Feynman:

Tuan and — I didn't do anything with them except fix up the manuscript.

Weiner:

Yeah, that was the one we discussed earlier, right, Tuan and Pakvasa. The specific suggestion you made for experimental people: "We urge that an accurate spin-parity measurement be made."

Feynman:

That was those guys.

Weiner:

Oh, they put that in. I was just curious whether or not — sometimes this is a way of begging off a question to put in that kind of a statement and other times it's a specific insight that you have. You are not taking any responsibility for that in the background of it.

Feynman:

No, that's on the right track though because new measurements have indicated there are particles where there weren't supposed to be. That's just confirming Zweig's ideas,

proving a little bit the way you compare it to experiment.

Weiner:

Well, that brings — we've discussed the parton papers — actually it takes it up even into '71. But there was one other thing that came in, a rather big paper with Thornber. It seems to me this went back to earlier work of your own as well, right? He was following something from your earlier work — is that it?

Feynman:

Yeah, all this — he published it in 1970 but the work was done much earlier. He was a student at Caltech and he wanted a thesis and he started to do this work. He picked up something called the polaron which I had done, made the theory of.

Weiner:

About '62 or something.

Feynman:

Yes and he picked it up, maybe it was '68, or whenever he did it. So we worked together on this thing, I mean I assisted him in how to do it you know, gave him advice, because he did his thesis. He did all the writing and everything else, all the work. I mean I did a lot of work with him so I don't feel like I didn't do anything but he wrote the whole paper, a very good job at the end, but I taught him how to do it.

Weiner:

Is this the kind of thing that would show up in your notebooks, though?

Feynman:

No.

Weiner:

It would show up in his notebooks.

Feynman:

I don't know where the hell I did that. It was in his notebooks. It wouldn't show up in my notebooks at all because these notebooks are high energy physics.

Weiner:

Strictly, I see. This is sort of going back to something much earlier.

Feynman:

That's right.

Weiner:

But I got a kick out of seeing a classic Feynman line in the paper, in the introduction, a simple sentence: "Our approach is physical and direct." This is like a statement of principle —

Feynman:

I don't know whether I wrote that or not.

Weiner:

Well, whether you did or not, it's the influence here that makes it interesting. And then the terminology is almost the same terminology as the letters you write yourself in your notebook. "The simplest question we can ask is this — given an applied field in a crystal, what is the explanation?"

Feynman:

Well, he was very much influenced by me and I can't say who wrote that. I believe he wrote that. I believe that he wrote all that. I'm not ashamed of that because we put so much work together into it. You know what I mean; it's not that I got my name on something that somebody else did.

Weiner:

Right.

Feynman:

He did a lot of it, though. He did a lot of good work and wrote it all up.

Weiner:

Yeah, it's his thesis — I didn't realize, his thesis was Part II, 1966, and this was

submitted November '69. So it's much earlier than I thought. So then he got around to publishing it. I thought it was fresh out of the thesis, but it wasn't. It was probably the first time he had a chance to do something with it. That raises another general question about collaboration in this period that we're talking about, 1965 to now: the collaborators here are students so far or post-docs. Have you ever had any instances in this period, whether it was published or not, where you worked really side-by-side with Gell-Mann or someone like that — I'm using him as an example, or anyone on a very specific problem?

Feynman:

No.

Weiner:

It's been this kind of notebook solitary work?

Feynman:

I always work alone.

Weiner:

Except when someone else draws you into their problems?

Feynman:

Yeah, right.

Weiner:

I guess that has been the pattern, except that when you worked with Wheeler, right, at a certain period?

Feynman:

Well, I was young then. I was his assistant.

Weiner:

Now, getting off the science because I don't know any more to ask you specifically about published work or even about the notebooks, unless the notebooks are going to reflect a lot more since the notebooks show what you were doing the period when there is no

visible published stuff, and then we skipped from the notebooks and we started talking about what was published.

Feynman:

I looked at the notebook to see what was in 1968 and I see all the work on the parton model then, deep inelastic electron scattering, and all the analyses of it are in there, so that's what I told you about, but I told you about it without looking at it page by page.

Weiner:

Right, but what I'm getting at is that does this then represent just about the total body of what your work is in the last several years?

Feynman:

Yeah.

Weiner:

In other words, there's no other sideline, no other dipping in to something else. You're pretty firmly focused — you found your home.

Feynman:

Well, to an extent, yes. I had gotten stuck in a corner, you might say, instead of finding my home.

Weiner:

When you went into it, you were sort of putting down the guys who were satisfying in this field without getting their way out. Now you're in.

Feynman:

Yeah, well, you shine a light in the corner and that's exciting when you run in there but if you get stuck to a wall then there's no use — you have to find another way out, right?

Weiner:

Right, but you still find that you're on the right track, in other words, the things that you are pursuing are still in the general direction?

Feynman:

What are you asking me about now — what am I doing now?

Weiner:

Yes, whether this concern, say from '67 on, which started into the parton thing, has been the thread accounting for just about all of your thinking — the ramifications of this, the working out of it, and the problems of it?

Feynman:

Yeah, but it's very unsuccessful. I haven't been able to ramify very far or work very much out, so I have another of these periods where nothing is happening for the last couple of years.

Weiner:

But the notebooks are reflecting you're struggling with it?

Feynman:

And you'll find missing dates of many months where there's nothing in the notebook.

Weiner:

That's what I wanted to know. I wanted to get the feeling of what you're doing.

Feynman:

'71 and '72 I'm talking about. I mean I exhausted my notebooks. You can't think of the same god-damned thing for five years — three years but not five years. In the same corner, you can't — you have to get out, and I haven't been able to get out.

Weiner:

When I talked to you on the telephone a couple of weeks ago you said you were still excited about the SLAC results and that it's still pending.

Feynman:

Yeah, it's still pending, yeah, right.

Weiner:

Well, what do you mean?

Feynman:

Well, the idea might be right and there are some very nice suggestions that the partons are quarks and so on, and up till now it hasn't really been demonstrated. There are a number of demonstrations that that's in fact correct that can be made experimentally, and they ought to be made so that we get convinced one way or the other. That the partons are quarks are a possibility and a very interesting one — it's sort of semi-paradoxical but not quite. You can't quite disprove it theoretically. It has definite predictions of numerical quantities, the exact numerical measurements it should give, and we should have to measure those things to check it, to see if it's right.

Weiner:

So the burden on the experimental works now in this?

Feynman:

Well, this particular problem has an experimental question that it would be very good to resolve, yes.

Weiner:

I saw a paper at the New York meeting which was the most ridiculous thing because all the man said is that we're making experiments. This is a popular version of it I want to show you — "The continuing search for quarks" — and all he says is that people have been looking and we're still looking.

Feynman:

That's not what I'm talking about.

Weiner:

I know but —

Feynman:

No you don't know.

Weiner:

Well, I thought that he gets into it here. He may get into the Parton thing. Anyway I'm not sure.

Feynman:

No, you don't understand it.

Weiner:

OK, well, let me —

Feynman:

Just let me say something.

Weiner:

Please.

Feynman:

You keep telling me, let me, all right, let me. There's a new idea that the quarks really exist as individual particles, and the other possibility is that protons are made of these things but they can never come apart. It doesn't help any if the proton is made of these particles and they do come apart. That's not necessary to find the quarks. The hypothesis is that maybe they don't come apart. But that's what we have to test experimentally — whether the protons are made out of quarks but they don't come apart. Whether they do or they don't, the experiments that we were just talking a few moments ago in SLAC and so on would determine whether protons are made out of quarks. The question whether the quarks can be found as individual objects is an irrelevant but additional question, OK? It isn't irrelevant of course to physics but it's not relevant to this particular question. So the idea which is the new imagination is not that there are quarks — and I don't care whether these guys find them or they don't find them, at least as far as this is concerned — of course, I care — but the experiment is to find out whether protons are made of quarks. It's not this experiment, OK? I wanted to clear that up.

Weiner:

I see, but this one that Feldman is talking about on the electric production of hadrons, that's closer?

Feynman:

I don't know what Feldman is talking about. All right, just a second, let me read it. No. Yes. No. Wait. It doesn't say in this. You can't tell.

Weiner:

It's such an abbreviated kind of thing.

Feynman:

I'll have to read this to find out — "Released for Thursday: Electric Production of Hadrons."

Weiner:

I don't know what he said in his talk...

Feynman:

The question is whether he's — this looks like it's related to yes, not looking for quarks — yeah, external quarks, but not internal quarks. It doesn't say — it's very poor. The guy wrote it himself but it's very poor. He doesn't say what the experiment is to measure or to find out or what. He says: "The experiment described by the lecture following used a new device called a superconducting tube." This is typical of the journalism — all you can find out about the experiment is that it uses a superconducting tube. It drives you crazy. I read the whole thing and I can't find out what the experiment is.

Weiner:

He probably thought that otherwise it would get too technical but then they left the technical part out and that's the guts of it in terms of the issue. All right, let's —

Feynman:

Don't let me read the newspaper? Will you — that's no way to talk physics.

Weiner:

All right. The reason I'm asking this and the whole point of my confusion on this is that —

Feynman:

OK, this other one is definitely looking for quarks as free independent particles. He says it in black and white. This guy doesn't say "but I'm sure he's not looking for them as free independent particles." Then it's in connection with this other set of examples.

Weiner:

What this led me to think was that there is a possibility that with other development of technique at places like SLAC that it may be possible to get new kinds of results which would make it a very very live issue. FEYNAN: Yes. It is a live issue. It's the issue.

Weiner:

It's the issue but you can't do anything with it until you get more experimental results.

Feynman:

Well you can always guess and make new theory and then find out that the theory predicts that some other experiment for results means something.

Weiner:

Well, you're still working on it.

Feynman:

No I'm not. I'm tired of it. I'm screwed. I'm in a corner.

Weiner:

Is there a group actively at work at SLAC for example trying to solve your problems, by that I mean, going very specifically on thing's which would give —

Feynman:

All over the world people are measuring all kinds of things that have to do with this parton view — hadron collisions, electron scattering and so on — and somewhere somehow they're going to find out whether or not the partons are quarks or what they are, and get more information. But it's not just in one place. It's all over.

Weiner:

All right. That's the current state of it.

Feynman:

Yeah, that's the current state of it, and lots of theorists have added a whole lot of ideas to the original idea so that deeper understanding, for example — a typical example, I said nothing and I have still said nothing because I don't want to say anything about how — you see, I say what will happen at infinite energy, but how should it approach that? At low energy it's not infinite, as you go up in energy, I tell how things get from very high energy but I don't tell how it moves into that direction, like for instance, how much deviation you should expect and should the deviation fall with one over the energy or one over the square root or what? I haven't said anything about that. And I haven't said anything about what happens in wide angle collisions. And there's a lot that I haven't said and a lot of experiments to determine all these things. And a lot of theories are coming out with these various things, and so it's a very active business. But I have gotten myself stuck. I'm stuck, temporarily, in getting any new idea. I said as much as I can say. I've been saying it for two or three years and I'm tired of saying it.

Weiner:

What do you do in a case like that; turn your attention to something new?

Feynman:

I don't know what to do. Do you have a formula for how to become successful?

Weiner:

No, I got my problems too.

Feynman:

I don't know. You got me in a bad mood this week because I am temporarily stuck and I don't know what's happening. I'm thinking about transitions in two dimensions in a solid or something like that for relaxation. But I realize every other week I go down to the beach house, walk on the sand, and think about these high energy collisions and just walk, thinking in circles, thinking in circles like a fly against a window — pzzz bang, pzzz bang, pzzz bang. It's time the fly sat down on the ground and walked around and smelled the flowers and then starts flying and — see, maybe he'll blow in a new direction. That's the condition I'm in.

Weiner:

Maybe change the garden too.

Feynman:

Yeah, that's right; try something else instead of flying against the same window all the time.

Weiner:

Well, there have been other times like that so it's not new to you.

Feynman:

No, no.

Weiner:

Let me ask something which is more background about your teaching during this whole period. We mentioned the courses and how you tried ideas out on the courses. Has the pattern of your teaching changed from '65 to now, that is, the kinds of courses you're giving, the kind of contact you have with students?

Feynman:

Not very much except that I'm getting older so the courses are less good. I don't prepare them as well.

Weiner:

Well, you went through that intensive period of the Feynman lectures.

Feynman:

Yeah, well, I do prepare the — I don't do any of that now — I gave two courses in the last year on high energy physics to keep in the thing and I worked very hard on both of them and looked at everything in great detail, but I don't know whether they were any good or not. I think the more elementary one was pretty good. Now I'm teaching quantum mechanics and I don't think I'm doing such a good job. I'm not preparing it very well. I take old notebooks and use them. It's the old story of the old professor who gives the same lecture so many times. I don't know what's new in the field and probably some younger man should give the lectures.

Weiner:

Well, they can get a certain style from you and a certain direct approach from you and get details somewhere else.

Feynman:

I've never been too happy with teaching. I've told you that before. I never feel like I'm doing anything.

Weiner:

Well, also you made the point that you didn't know what was happening with your students.

Feynman:

That's right. And I still have that feeling.

Weiner:

You'll never know — by your own standards. Maybe other people in the world will know but you won't. Let we ask another point about the change in your life, if any, because when you became a Nobel Prize winner you were a lot more visible and a lot more of a target for people who want you to do things and expect that you have certain responsibilities let's say, because of this position. You told me that your first reaction to the Prize itself — the very new responsibility of dealing with the press on that day of the announcement was something that you had to face and you decided that well, maybe it's easier for you to call them all with the minimum that was expected. Let's talk about that from that time on — was it as bad as you expected that it was going to be in terms of people constantly after you to do things?

Feynman:

No.

Weiner:

Once you had gotten the Prize, I mean.

Feynman:

Oh, from time to time somebody bothers me but I don't do a goddamn thing. I have nothing to do with it. If somebody says, "We need a Nobel Prize winner to sign a letter to Russia on the Jews," I say, "I'm willing to sign a letter to Russia on the Jews but I'm not willing to be a Nobel Prize winner signing a letter to Russia on the Jews." I don't want to have anything to do with the goddamned Nobel Prize.

Weiner:

Some people talk of the GNP and think they mean gross national product, but now I know what they mean by GNP.

Feynman:

No, I try to — sometimes it's embarrassing, it comes up in conversation or something and so on — and I try to avoid it. And I often get letters, you know, complimentary — I don't do anything about it.

Weiner:

No different than in the past then.

Feynman:

No, no different than in the past. Now is no different than in the past. It's just as if it never happened, which is good.

Weiner:

So in other words, when we talked last time about what had happened at the ceremony and so forth —

Feynman:

I realize now it's all a lot of nonsense. I don't know why I worried about what happened on the ceremony. What the hell difference does it make what happens in a ceremony. Ridiculous, the whole thing is ridiculous — although I got a house in Baja, California, on account of that thing, that got paid for because of the Prize.

Weiner:

You mean with the money from that?

Feynman:

Yeah, that part isn't so ridiculous.

Weiner:

So you counteracted it and invested in privacy since you were thrust into the public.

Feynman:

Right, exactly, exactly.

Weiner:

But you got involved in someone else's business — I'm thinking of, if you'd like to talk about it, I find it fascinating — in Gianone's case, the court case, since that was a nationally — I heard about it in New York because it was picked up in the newspaper —

Feynman:

That's another example of the Nobel Prize getting in the goddamned way. This man had a restaurant and he had topless dancers dancing in it and they would arrest him when the girls were doing particularly nothing, just dancing with no clothes on, but they would say that they would do things. And he went around to his customers and asked them if they would help and say what they saw at this time, OK? And I said no, and everybody said no. And he said to me, "It's funny. They all come here to look and they all don't want to —." So I realized that it was really ridiculous that we're not going to — so I said, all right, I would do it. After all, I'm the only free man; I'm a professor, and so forth. So he had some kind of a trial and he said he just wanted me to come up and tell what the restaurant was like and so I went to the trial and I told what the restaurant was like and everything was all right. Then another time he got into trouble again. The first time I said "The thing that bothers me is the publicity that might come from this, that's all, but I'll be glad to do it." And at first he said, "I won't tell anybody, we'll just do it right away in the courtroom and nobody will be there." I said, "OK." So I did that. The second time he asked me again. He had another case, and this time his lawyer knew that I was a Nobel Prizewinner and so on. And his lawyer was a little bit dopey so he doesn't tell me what he's going to ask me, you know, and he gets me on their and he says, "We understand you're a Nobel Prize winner" and all this crap see. And by this time the court reporter had found out and so forth, so it got into all the newspapers and it was just a pain. What I was trying to do was what he'd told me that I was going to do — this is the lawyer — was to describe the restaurant and what went on in the restaurant and that was all I was going to do, as a citizen. But I was unable to do that as an ordinary citizen. It became — it even appeared in a newspaper in Geneva, Switzerland. But that's not fair. It had nothing to do with it.

Weiner:

Well, the part that appeared in the paper that I saw was —

Feynman:

I don't mind — it wasn't embarrassing so much as it's silly.

Weiner:

Yeah, you said — I mean the paper —

Feynman:

It means, you see, the Nobel Prize means I'm not free quite to do what I would ordinarily do, which is to give testimony in a court trial through somebody that I think something's been done that requires some testimony. And I also testified about the — what happened is that the guy gets me up there and then he says to me — he starts to prove that I'm an expert on the — he asked me questions about the general opinion of Californians on this subject. And I say, "Well, I deny being an expert." The judge says, "You can't deny being an expert." Then the lawyer started to prove that I'm an expert by saying, "How many times did you go to this place? How many other places have you gone to?" and so on, so he could prove that I was an expert. "And what kind of people do you talk to?" "I talk to all kinds of people." "Have you ever talked about their opinion on this subject?" "Yes," and so my own lawyer — the lawyer that he got — builds all this stuff up so somewhere in the newspaper it comes out that I go three or four times a night to Gianone's. I used to go for lunch and then go in the evening — three or four times a week, or something like that. And that gets in the paper — all kinds of things. It was crazy. But they tried to make me into an expert and I have to give my opinion as to whether Californian opinion is in favor of this or against this, so I gave my opinion. And so on, so altogether it wasn't particularly good and the Nobel Prize got in the way.

Weiner:

The part they quoted in the paper which is always good for laughs was not so much the Nobel Prize that was incidental, it was "I go there and do my calculations."

Feynman:

I do. I still do.

Weiner:

I know that but to a newspaper. "Imagine a guy going into a topless place just to calculate."

Feynman:

I'll show you. I'll bring up some papers in which there are calculations on the little scalloped-edged place mats. I've got a lot of that stuff — a lot of place mat notes. Here, right here, look, see —

Weiner:

In this notebook.

Feynman:

Look, here's one. OK, that proves it, right.

Weiner:

It's not bad paper.

Feynman:

No, it's good paper. I got too many sheets here. I didn't use all of that. See.

Weiner:

They're folded over loose leafs.

Feynman:

Well, I fold them over because — you see, that's just an example. Yes, I do that and I've done that and I always did that and I still do that.

Weiner:

That should have been admitted to evidence.

Feynman:

I still do that. I've got a lot of those.

Weiner:

Well, I went there with you one time and we talked quite seriously in the bar, maybe it was.

Feynman:

It's easy, a lot of noise, but nobody bothers me down there. I like it down there. Also I've made a drawing, you see, that's on the wall down there. And so [???] likes it very much so he doesn't charge me for my soda water down there. So it doesn't cost me much. I come down and he always is glad to see me, and so forth.

Weiner:

The drawing was another thing we talked about a little bit.

Feynman:

I had started to draw the last time you were here?

Weiner:

Yeah, and you had had some of your stuff in that store. Some friend of one of the models there —

Feynman:

Yeah, I had stuff in Bullock's for two months or something. I didn't sell anything, absolutely nothing.

Weiner:

That's what I wanted to know. At the time they were on display. I didn't see them.

Feynman:

Not a goddamned thing, no. She had taken stuff that really wasn't so very good either.

Weiner:

She selected it?

Feynman:

The good stuff she had already sold. But I didn't sell a damned thing, and then after that — I'd sold other things to other people by other ways — then Henry Dreyfus, who recently died, the designer, he suggested or someone else suggested that they have a little exhibit of my art, they were going to have art exhibits and they were going to have my art exhibit in the basement of the Athenaeum. So I had a show. And the fun of it was that they organized — some secretary had to take care of it, and she called up — I gave

her a list of all the people who had bought drawings, paintings from me. You know they had bought it under the name Ofey.

Weiner:

Yes, Ofey, right.

Feynman:

And they didn't know who I was already. And so that some lady who had bought this painting through my agent which was a —

Weiner:

A waitress, wasn't she?

Feynman:

No, she was originally a model but then she was working in Bullock's for a time as an interior decorator. Anyway, she told them about my drawings and she'd sold a few of them and then they would get this telephone call, "We understand you have an Ofey and we're having an exhibit of Ofeys. We would like to invite you to the exhibit and we would like very much to have your painting." They were all delighted so all the stuff that I had sold all over collected back again and it was on all the walls. And it really looked pretty good to me. It was quite fun, you know.

Weiner:

Your first one-man show.

Feynman:

Yeah. It was really great.

Weiner:

And, of course, the people at Caltech knew that it was you.

Feynman:

Sure. They found out who it was.

Weiner:

Do you still sign it Ofey?

Feynman:

Yeah, I still sign them Ofey but selling them, recently, most of the people I sell them to know who I am not because they make contacts you know, I mean it's not famous that I draw.

Weiner:

So therefore they want them.

Feynman:

I've gotten confidence now that the drawings are not so bad.

Weiner:

Do they do it through you personally or through some gallery?

Feynman:

Usually through me.

Weiner:

You have them here and they come and see them or something?

Feynman:

No, they're not good enough for galleries.

Weiner:

Well, for an exhibition, anyway.

Feynman:

An exhibition is kind of fun.

Weiner:

But are you spending as much time doing that as you were before, I mean, just in your off hours when you feel like it?

Feynman:

I don't do as much as I did — now.

Weiner:

Maybe that's the thing to do now that you're stuck.

Feynman:

Probably. That's probably right.

Weiner:

Well, one other thing that I'd like to get into is the — were there any periods of time that you spent away from here other than the summers? Did you take a leave at all during that period of time? Did you work at another institution?

Feynman:

No.

Weiner:

Did you travel for entire summers or anything like that?

Feynman:

Yeah, I go away in the summer like I would go to England and stay there for a month or something like that.

Weiner:

And you'd work then while you were there?

Feynman:

On and off.

Weiner:

But you haven't gone to CERN, let's say, for two months or anything like that, the way Gell-Mann, for example, did -- took a year of leave?

Feynman:

No.

Weiner:

So it's been no institutional leave. You've strictly been here.

Feynman:

No. Oh, I might be there for three or four days.

Weiner:

Well, that's something different. And what about in the other realm? One time I remember that after we had originally talked you gave a talk before the National Science Teachers group -- I forget what the title was -- and I got a kick out of that because in that talk you talked about your father when you were a kid and he was showing you the tiles and patterns and so forth. I want to tell you a funny story about it. I was sitting at lunch with someone and he said, "Gee, I heard the greatest lecture by Feynman and he told how he got interested in science." I just said, "I betcha his father had something to do with that." And he said, "Yeah." "I betcha a guy like Feynman probably had some pattern recognitions and tiles and probably his father took him to the Museum." "Wow, how did you know that?" It was really funny. And he said, "Yeah, it's just what he said. Gee, you've really got some instinct." That was the end of it. But how about things in that realm. You used to write, for example, in Engineering & Science Review. That was a good outlet and you said they'd publish anything you'd write. You even wrote about Brazilian science, you wrote about science education. How about that in this last period? It doesn't show up in the kind of stuff that I find.

Feynman:

No.

Weiner:

Why not?

Feynman:

I stopped. I think it has something to do with the Nobel Prize.

Weiner:

You mean that you feel that they regard it as too much —

Feynman:

I don't know. You're asking me a psychotically question. One thing is that I have no more stomach for philosophical questions and political questions. One of the reasons is I got — I shy away from them much more positively than I ever did before. I won't even discuss them. I don't know why.

Weiner:

The whole discussion of political questions has become much more sharp and intense, you know, it's not academic any more. You know from other people so —

Feynman:

Well, even philosophical questions.

Weiner:

Yeah, although I would say that there's a great deal of philosophy of science, not in the formal sense, but in how one does a problem and how one —

Feynman:

Yeah, but I mean a formal sense. Formal sense. I don't mean I don't believe in thinking about what I'm doing. Talking about it is another matter. I don't like to talk about it anymore. It's possible it has to do with — I told you about the bet with Weisskopf, didn't I that I would never get involved in a responsible position?

Weiner:

No, I don't know about that.

Feynman:

You know I visited CERN right after I got the Prize and I gave that talk.

Weiner:

Without your coat.

Feynman:

Without my coat. Let's see was it — yes, it was about that time — in fact, I went to CERN and they were having a big meeting, all the big cheeses. So I went in the back of the meeting — the meeting was that Weisskopf was resigning or was finished with his two years.

Weiner:

It was five.

Feynman:

Five years. And that was what the meeting was about. So I went to the meeting and they were all praising him and so forth. You know how he is, he's completely self-effacing and a very nice guy, and so when I came in the back of the meeting he waves to me. Then after the meeting I went down to him and he said, "How about lunch?" I'm only telling this anecdote for its amusement. "How about lunch?" I says, "OK." So we go downstairs — there's all these people, a big milling of people and they'd just been praising him and they were all talking about the same thing, how self-effacing he was and how wonderful and how were the ever going to get anybody like that again. So we go down to lunch. It's a cafeteria down there. So Weisskopf and I go through the cafeteria and he's very nice. He offers to pay for it for me, you see, so I go through carrying my tray. And as we carry our trays we look over to one side of the cafeteria, and the cafeteria has lots of tables, but on one side there are all these tables piled together in a big arrangement and obviously somebody's going to have a banquet. So he looks at me and he says: "Oh, I forgot. They're having this banquet for my farewell." He says: "We have these trays." He says: "Listen, we have to eat over there. Let's just put the trays down on this table here and go over and eat." So we put down the trays he just finished paying for and we went over to eat at this banquet. But that's him. During the banquet I asked him about this. I say: "You have a position of responsibility. You have a position of responsibility." He says: "Yes, and now that you've won the Nobel Prize you too will have a position of responsibility." "Ah ha," I said, "Never." So he said, "All right, I'll make a bet within ten years you'll have a position of responsibility." I said, "All right, I'll take the bet." I don't want the bet was for — ten dollars or something. And Rossi is holding the money. No, not Rossi — who's holding the money? I know the man's name but I can't remember it right away.

Weiner:

Amaldi?

Feynman:

No, no, it's Cocconi. So he says: "But we have to decide what a position of responsibility is." And I said: "A position of responsibility is where you're in charge of what other people are doing and you haven't got the slightest idea what they're doing — you're in charge and controlling it but you don't know what they're doing." He says: "All right, that's a fine definition." So we have this bet. You see he was scaring me by saying now that you've won the Nobel Prize it will be impossible for you to avoid — so I think as a reaction I've been very careful to avoid with 100% fear anything that has any —

Weiner:

Now cassette 2, side 1. Ok, you have five years to go — no, you have two years to go on your responsibility.

Feynman:

That's all. I'm going to win the bet.

Weiner:

We'll see. You never know what happens.

Feynman:

No, it's impossible. I've gotten so hard-assed; as I said, totally irresponsible.

Weiner:

There's one time though, the last time that I saw you which was at the San Francisco American Physical Society meeting, where there was a confrontation. This was one thing that you had not invited but it was a question of —

Feynman:

Yes, that's right. I had invited it all right don't worry.

Weiner:

You mean in the letters. Well, it would be interesting to talk about that because I thought it was an incredible thing. It worked out in a most interesting way I thought. All right,

the issue was that the people concerned with the rights of women, let's say —

Feynman:

Are you telling it for the tape? What do you want?

Weiner:

No, I'm just trying to remind you.

Feynman:

I know what the story is.

Weiner:

Well, let's start from the beginning then.

Feynman:

I got a letter from these people that said that the books that I wrote showed that I thought that women couldn't be good physicists and I looked at the various references and it was kind of ridiculous. In one case I had a woman driver that a cop stops and because it was a woman driver they said that showed prejudice against women. Well, they're sensitive. I didn't think that made any sense. And another example was I told a story about, I think it was Houtermans — after he figured out how the stars got their energy, he was out with his girl and she said: "How beautifully the stars are shining." And he said: "Yes, and I am the only man in the world who knows how they shine, why they shine." She just laughed at him, and I went on and said that in this world when you know something new you're always alone. I was teaching my students about it. This was then claimed that I claimed that a woman couldn't understand nuclear reactions in the stars which, of course, was not the story. I was telling a true story about how this happened the first time. They had no rationality about it, you see. So I wrote back — they raked this over — so I wrote "Don't bug me, man." And then they wrote back they were dissatisfied with the answer and so on and so on and so on. And I was going to this meeting in San Francisco to get a prize, the Oersted Medal. Then I got warned. I had forgotten all about this letter. They wrote me a letter they were going to do about it but I forgot all about it. But just the night before I went somebody called me up on the telephone and said — he was a lawyer or something and had been at a meeting of these people and he said something about this stuff and how do you feel about it, and he gave me the clue that they might do something. So that when I met my sister at the airport I told her this story — I said I had a suspicion they might do something — and she said: "That's absolutely ridiculous. If they do anything I'll tell the story about how you educated me in science and how you're not prejudiced at all and that you've taught me

everything I know and what does a brother to a sister like that, you can't really believe," and the examples given in the book were absolutely ridiculous, she said. And so when I got to the meeting they were giving out circulars which had the letters back and forth between us in which I say, "Don't bug me, man." I answered them.

Weiner:

Yes, I saw those.

Feynman:

And my sister gets a copy of this and she says, "It's just crazy," she says, "I'll say this." And then as I got to the meeting Faye Ajsenberg was waiting. She had been a previous chairman in charge of women or something, I don't know, but she was waiting in the front where I was going to sit. She says: "Hi." She says, "You're my favorite male chauvinist pig," or something like that, and my sister was standing next to me. And so Faye turns to my sister and she says: "Do you realize that this man has a sister that he taught all of physics that she's a successful Ph.D.?" "Yes," she says, "not only do I realize it, but I am that sister." What Faye had thought was that she was going to protect me from these people and as she saw me coming down with a woman she assumed that I'd already been captured by them and was already being harassed, so she wanted to protect me so she gave her this. And then we sat down and Faye said to me, "You know what I'm going to do? If there's any trouble I'm going to get up and I'm going to say that these books made a big sensational effect, how they give everybody a chance to learn physics, men as well as women, and they shouldn't be criticized." So I was sitting there and Serber was giving a talk so I had time to think, which all I need is. If I have time to think I'm all right. It's when a situation comes quick I don't know what to do. So I thought to myself, if Faye can say that I can say that, and if my sister can say that, I can say that. My sister told me there's an awful lot of prejudice, there's really a lot of prejudice. It's hard to get jobs. She had difficulties, and so on. And Faye said there's prejudice, that they really admit that, but that this thing is a ridiculous example. So I said I can say the same thing. I believe there's prejudice too, what the hell? So then I got up and when it was my turn, I simply said that I guess you were there. Do you want me to tell what I said, what I think I said?

Weiner:

Yes.

Feynman:

I think I said that there's a great deal of prejudice — these people, while I was standing there and hearing about, while the guy was announcing me, twelve people came carrying

signs. I couldn't read the signs in the position I was in but I presumed what they were because people began to laugh. Then it was my turn to start. And I said that there's a lot of prejudice against women in business and getting jobs and so on and so on and that's a ridiculous thing and so forth and so on, and that if the — so I was on the same side as these — I didn't say it that way but I just put myself on the same side as these people. Then I said that they'd also claimed that there was something the matter with the books that I wrote and I think it's a ridiculous exaggeration and that they're picking on the wrong problem, and that they shouldn't be wasting their time on me. They should be picketing the guys that are not giving them the jobs and so forth, but if they came here, if this opportunity for them to come here had given the audience again a reminder of the fact that there is this prejudice, then I suppose it's all to the good. They didn't know what to do then.

Weiner:

Do you know what they did? They applauded.

Feynman:

Did they applaud?

Weiner:

The demonstrators did.

Feynman:

I didn't know that.

Weiner:

And then they left.

Feynman:

I said...I remind you again it's all to the good.

Weiner:

You also said that you were going to write them a different reply, that you had been —

Feynman:

Did I say in public that I was going to give them a different reply?

Weiner:

Yes that you didn't take them seriously. It is a serious question but you didn't think that they were serious about it because they were picking on something which wasn't part of the question. You didn't really see their point but their bigger point you see although you disagree with the way they were — why am I telling you what you said?

Feynman:

I don't know what I said.

Weiner:

I was listening and you weren't listening. You were speaking. And anyway, it was a very effective, a very interesting thing, because there was a great deal of affection for you on the part of all of the people and that this —

Feynman:

It was crazy, I think.

Weiner:

And then, of course, the after-discussion I found fascinating where I was on the edge of the crowd. And then you in the best of debating and discussion style talked with people back and forth. They presented their position, you presented yours. Your sister spoke up in the discussion, and it was very good. By the way, I noticed after that — it was a very difficult, a very trying time, and you were anxious to get out of there — but then two students approached you from South America. And then you turned to them with your full attention. This made me think that your interest in South American education in physics has continued. We talked about that at length last time but have you had any further contact with them?

Feynman:

Well, these two, people invited me to come to Mexico and give some lectures if I understand it correctly.

Weiner:

I heard them, yes.

Feynman:

And so I said yes, or something.

Weiner:

Yes, but it was your tone —

Feynman:

Wait a minute, you're asking me what happened. And then I got a letter from somebody in Mexico, asking me to give lectures in Mexico. And I don't know whether those students were the ones that the letter referred to, whether the letter was something else, and another man had asked me to go too to give lectures. So I finally decided yes so at the moment this particular letter came I said yes. And whether I had said yes to whom, I don't know — do you understand? But anyway, I decided to give some lectures, a series of lectures on high energy physics in Mexico. And the guy who called me was Fernandez. After I said yes, he called me on the telephone to make arrangements. He said: "Would you give the lectures in Spanish?" I said: "I don't know Spanish." He said: "I heard some very good lectures of yours in Brazil in 1951." I said: "But that was Portuguese." He said: "Spanish is almost the same." So I said: "No, that's ridiculous I have to do it in English." So I came home and told my wife the story and she said: "Why can't you do it in Spanish?" I said: "I would have to learn Spanish." She said: "It wouldn't do you any harm." So I got a woman to help me and I took lessons and I went to Berlitz and I studied and I worked and I worked and I worked and I went down to Mexico — it was last summer — and I gave six lectures in Spanish, and interviews to newspapers and so on in Spanish.

Weiner:

That's great. Was there a good response to the lectures?

Feynman:

I can't tell. I never know when I'm lecturing or teaching what the response is. Yeah, people kept coming. It was crazy. There was a strike in the university and they couldn't have it at the university because people would find out that the strike was being broken. So they had to have it at a place in the middle of town, a medical institute, and the students surreptitiously snuck out of the university to go all the way to the medical institute about six or eight miles away to hear the lectures. So I suppose it was a great turn-out, considering the difficulty.

Weiner:

Have you kept up with the people in Brazil? There have been a lot of changes since we talked about it.

Feynman:

No, not since the big changes. But my friends were in Mexico. Lopez was in Mexico also giving lectures in the same school as I was, in fact, in the same series of lectures so we had many goings — over of old times. My wife and children were with me in Mexico and we had a very good time.

Weiner:

Look, we don't have too much time because of your schedule.

Feynman:

We have lots of time. We have until three.

Weiner:

Well, it's five to three. That's what I'm getting at.

Feynman:

Oh, is it? I see. Don't worry about it.

Weiner:

Let me harp back then to something. This is not bad for a very short period and there's probably a lot more that we can get into but something before I forget I want to get back to — something that McMillan said that I just ask you, because I was talking with him many months ago about Los Alamos and experiences there. And he said: "I suppose you know about Feynman's quadrilogues" I said: "Quadrilogues?" And he started to tell me about them. Just for personal pleasure if for nothing ease I would really like to hear about them.

Feynman:

Well, I don't remember much but one day we were driving home from somewhere. I used to be rather sensitive to drink and I guess I was a little drunk. And so I — I was in the back of the car and I just started to talk as if there were two voices and then three and then four, so I had a complicated conversation. That's something — the quadrilogues, you know. Anybody can make a monologue, but who can make a — and I

don't know what the story was that I made up. They all were laughing and I don't remember it very well.

Weiner:

They remember it vividly, at least he did, but I had the impression this is not — was this the first time then?

Feynman:

I just think I did it once but maybe I did it more than once.

Weiner:

You haven't preserved that. I thought that was the kind of a thing that you do as a kind of a continuing activity — it was just that moment of creativity then.

Feynman:

That's right.

Weiner:

Well, I really don't know, frankly, without taking a look and studying some more things and without looking at what we've done to identify specific gaps, what more we should get into for this period. Your family has grown in size since we talked?

Feynman:

Well, one thing I would like to say: I just gave a final course recently in high energy physics and the guys took notes and we quickly published the notes. And at a meeting — I was invited to go to Hungary too and I went to a meeting in Hungry. So when I was invited to the meeting I didn't think that I had anything new to say but fortunately I thought of something new just between the time I accepted the invitation and the time I went. I usually don't like to talk at a meeting unless I have something new. But I worked on something new which was that we would be able to measure the charges on the quarks by looking at the products. And I wasn't careful enough. I made a mistake. And it was found out by somebody else. So when I went to Chicago someone said — I was talking about this theorem and some guy in the discussion said that you measure the charge by measuring the charges of the products, and I didn't know whether he got it from me, having heard the talk in Hungary or if he had invented it himself. That was the kind of embarrassment I'm always in. I don't know whether other people have done the same thing. And then a Glenis Farrar, in the audience said: "No, that isn't true. The

thing doesn't work except if you assume such-and-such." I say: "No, no you're wrong." She says: "No, you're wrong. So the chairman said: "I think you two should discuss that afterwards." So we discussed it afterwards and I found out I was wrong and she was right, so that that thing is incorrect. And so I have nothing of that high energy charges by looking at the products.

Weiner:

It's wrong on the basis of a mistake in the theory?

Feynman:

I didn't derive it right. I made a mistake.

Weiner:

I see. Was this published?

Feynman:

Yes.

Weiner:

How could it be? Oh, published on the basis of the students —

Feynman:

Yes, first notes, and then as a course in a book. And also I gave the lecture in Hungary and it was published there. That's the first time I published anything that's wrong so therefore I am now decaying gradually into nothingness — my first flaw.

Weiner:

You're out of blip — you know, one point doesn't make a curve.

Feynman:

But I'm not proud of that. It didn't work out. I have no excuse because I knew that my derivation wasn't complete. I should have gone over it many times and thought about it again. I never — only the week before I had given a talk at Hughes and the guy had said to me: "Will you explain how you get that result better?" And I couldn't explain it, and that should have been a clue that it wasn't simple.

Weiner:

I want to ask about Hughes in a minute.

Feynman:

I give lectures every week.

Weiner:

You're still continuing — that hasn't changed?

Feynman:

Yeah.

Weiner:

That's been a long time now. Do you still find it satisfying?

Feynman:

I don't know. I do it all the time so it must be satisfying, yeah. It's not for the money really.

Weiner:

The guys are good — they respond?

Feynman:

Yeah, it's kind of fun giving these lectures. It's a little diversion. This year they've decided what I should do is give a different lecture on a different subject every week. See what used to be would be a course in mathematics or a course in physics or some branch. They asked me to give a course in biology which I did and a course in astronomy which I did.

Weiner:

At what level?

Feynman:

Advanced, complete. Oh, it's easy to give a course. You want to give a course in astronomy and you come from Caltech, it's easy. You read stuff, OK? You decide you don't understand enough about this quasar bit so you go over to Schmitt and you say: "Hey, Schmitt, what's the latest stuff on quasars?" Then you go over — and you're talking about the development of the stars — and you go over and say "Have you got anybody around here that knows it?" "Oh yes, Clayton is writing a book." Clayton says to you he is writing a book — would you do him a favor and read the manuscript. "Sure." So you read the manuscript and you know more about the development and the history of stars than anybody else could because the latest book — you're criticizing it and discussing it with him. So it's very easy for me to keep up with any subject. I go to see Goldreich and find out what the story is on the orbits and the planets and so forth, the pulsars and so forth. It's very very easy. I don't use the library. I just use my friends.

Weiner:

What about the biology? Who do you use?

Feynman:

The same thing. Well, with the biology I had done some work myself so I knew a great deal, and this particular course in biology I did somewhat differently. I could do it by going to the biology department but instead of I took the Scientific American — it seems like their articles are rather complete. I have a big bunch of old ones and I just went through and made an index of all the biological articles and just put them all together and made a good course out of it. And it was complete because that was the latest work too. If I had had any troubles — oh yes, I did. From time to time I had something new; then I'd go down to the biochemistry department and ask them for the latest stuff so it was easy.

Weiner:

Did you do any work in biology in this last period? We talked about the little side trip into biology.

Feynman:

No.

Weiner:

When was it that you had done that work anyway — the phage work? How do you pronounce it — phāge or phäge?

Feynman:

Phāge. I used to call it phāge. I don't know.

Weiner:

You haven't done any of it since then?

Feynman:

No.

Weiner:

And, the Hughes lectures — are they recorded, by the way? Has anyone kept them?

Feynman:

No. I gave a series on lectures on statistical mechanics about nearly ten years ago and stalling around — they made very good notes of them — and it turned out a very good course, I think, now that I look at them. And I thought the notes weren't very good and I kept stalling around: "I've got to review them before they're published," and I never did it and so on. And finally the damned things are published ten years later — and the publisher didn't say that these notes were ten years old.

Weiner:

Well, the subject's not old.

Feynman:

Yeah, but the subject advances rapidly. Anyway, this has just been published so if you look in the — if you find it, you'll see it just came out this year.

Weiner:

What about tape recordings? It would be interesting to have a record of those things.

Feynman:

Only for a historical purpose.

Weiner:

Well, that's what I'm only asking for from a historical point of view. Think of it sometime if you can.

Feynman:

Yeah, the biology course was a good one and the astronomy course was a good one. Right now I'm doing a different subject every week. Each lecture is a different subject.

Weiner:

You mean from physics to biology to —

Feynman:

No, a different subject, like in physics or in mathematics, like the four—color problem or the properties of liquid helium or what we know about [???] — stuff like that — black clouds and so on, black holes.

Weiner:

How do you select them — on what basis — just to give them an overview?

Feynman:

Well, they gave me a long series of topics and I just pick one or another.

Weiner:

But these are things that they think are of special interest because of their work?

Feynman:

No, they're just, interested in culture.

Weiner:

I remember that great photograph of when you got the prize of pulling up in front of the place and thy rolled out the carpet. That's a good photograph. There was one other question about Hungary I wanted to ask: what kind of a meeting was this, and was it a kind of ceremonial international thing or was it really a working meeting?

Feynman:

No, it was a working meeting. It was not an international meeting exactly. I mean it wasn't part of some international organization that I know of. It was not like a Rochester meeting. It was some kind of — three countries — Hungary, Austria and somewhere else — think that they're starting a series of meetings. It was nothing very —

Weiner:

But they brought people like yourself. Telegai was involved.

Feynman:

Yeah, lots of guys from outside came.

Weiner:

Was it the kind of meeting at which you could learn something useful?

Feynman:

Yeah, it was a good meeting. It was a whole week and there was plenty to do and it was on a very limited subject.

Weiner:

The high energy meeting at which I understand you broke your knee or something. What did you do?

Feynman:

I broke my kneecap.

Weiner:

How are they going from what you've seen over the years? Is this the same kind of continuing tradition of the same kinds of people coming together? Because in the early period — we talked about the meeting where you got up and said: "Mr. Block has an idea that I would like to tell you about," and you talked at the following meeting on that, and these were very exciting things. Has it continued in that same tradition?

Feynman:

No, they've gotten too big. For example, they have parallel sessions which they never had before. Parallel sessions means that more than one thing is going on at a time, in

fact, usually three, sometimes four. And when I went to the meeting in Chicago, I was only there two days before I broke my kneecap, but I had a great deal of trouble making up my mind which of the parallel sessions I was going to miss. Sometimes I'd miss them both, but sometimes there were two things I would be interested in at the same time. These guys who organize this imagine that each guy is a specialist and only interested in one lousy corner of the field. It's impossible really to go — so it's just as if you went to half the meeting. Therefore half is not much better than nothing. You might as well stay home and read the reports.

Weiner:

In your case you would have been better off if you had stayed home.

Feynman:

In that particular case, yeah. So, no, that meeting is too big. A meeting that I enjoyed much better was a very small meeting at MAL whose real purpose was to educate them at MAL to a large extent. I suppose they had a meeting on partons: what good are they? Something like that — a limited field for two days with plenty of time to go over all the features. And there were not parallel sessions. They drive me crazy. If more than one thing is happening at the same time and I can't be at both of them, I just can't stand it.

Weiner:

Yeah, it's awfully frustrating.

Feynman:

I'd rather not — I don't say I have to go to both of them but I always worry and they often come that the same thing is happening. At the Physical Society this used to happen worst when I was working with liquid helium. There was helium and there was nuclear physics. And, of course, everybody assumed that helium and nuclear physics were at right angles. And I wanted to go to both of them. That was when I really got screwed.

Weiner:

But you don't go to Physical Society meetings very much?

Feynman:

I don't go to the meetings much anymore. I don't read The Physical Review anymore.

Weiner:

That's one though, isn't it, open there? No, I don't know what it is.

Feynman:

No, that's not. That's a book on polarons.

Weiner:

I see.

Feynman:

Did I ever tell of having worked on some problem with polarons?

Weiner:

In the early sixties?

Feynman:

Yeah.

Weiner:

I can tell you in a minute. Yeah, I think so.

Feynman:

Anyway, this Thornbill work is a continuation.

Weiner:

Yes, that I know, but that's obvious in the work itself. I do think -- I'm not quite sure. Let's see — "Slow electrons in a polar crystal" — that's '55. "The Pathenthalical Technique." Then "Work on the mobility of a Polaron."

Feynman:

Yeah, that's it.

Weiner:

Let me tell you the truth and then we can talk about a lot of interesting things, things

that I would find interesting, but since the time is getting near and I have nothing very specific in terms of the scientific work and since we said we'd sort of focus on this last period, I'm really through.

Feynman:

All right, so were finished.

Weiner:

But there may be something on your mind that we haven't covered.

Feynman:

I don't think so.

Weiner:

Ok. That's surprising, isn't it?

Feynman:

I told you nothing had happened in the last five years, seven years.

Weiner:

Do you still think so after looking at those notebooks?

Feynman:

No, that's my business — it didn't work. A lot of stuff didn't work. In the last year or so, I mean the stuff that I did in '68 had a big influence and the possibility that there are quarks is very exciting and I'm waiting for experimental results. But as I tried to express I'm a little bit frustrated. I'm tired of thinking of the same things. I need to think of something else. Because I got stuck — see, if it would keep going it would be all right, but it's hard to get any new results.

Weiner:

There's one question I wanted to ask — it's because the field is in that state, it's not only you with your particular approach to it, but there are a lot of people clustered around with their own approaches, and they are all in this state —

Feynman:

Well, unfortunately, you see I have always been unfashionable. How this parton thing has been so successful that I have become fashionable. I have to find an unfashionable thing to do.

Weiner:

This is what I was implying before that you're stuck in mess that the rest of physicists are. But if that's the case and generally you think of the competitive thing as of someone saying, "Well, someone's going to come with the solution to this; maybe nature, but maybe one of us." If you take a look at the field, where do you see it coming from? Is it a question that one of six guys could do it? Let's say that we're talking about some development of the theory.

Feynman:

I don't know. There will always be young people.

Weiner:

The ones you don't even know yet.

Feynman:

That's usually what happens.

Weiner:

But you don't see a particular center or a particular cluster of people at a place that you think might be more likely to really see through these things, other than yourself?

Feynman:

Well, maybe Murray — his way of thinking.

Weiner:

You know I've never talked with him. I've never met him.

Feynman:

Many of the other people I don't find — except for young people, people you never heard of, all of a sudden coming up. I don't know anywhere where they're studying it in

a way that I think is sensible except Murray.

Weiner:

What we should do is that I should get together with you again after a reasonable period of time and see what's happened then.

Feynman:

All right.

Weiner:

OK, great, a good point to stop.

Weiner:

Just for a brief epilogue to quote from an entry in the notebook which sort of summarizes some of the things you were telling about the state of the parton ideas.

Feynman:

7/7/71 it says, "Problem 2. Your parton views have been only partly useful in a merely qualitative way. Others have not been able to get anywhere further with them. The reason is they are not part of a precise mathematical structure. Therefore parton views must be expressed in a precise mathematical framework. This was your original intention and it was never done. The most important practical problem is to relate experimental — experimentally? — distribution of partons, for example from deep inelastic d-p scattering to hadron experimental results, that is some frame in which all the very high energy hadron, hadron experiments can be related to the parton distributions using the scattering current couplings. Continued." Do you want me to read more?

Weiner:

Yeah, I think it's fascinating.

Feynman:

"To deal with the second part in relation of hadron collisions to parton distributions, a number of problems are here suggested. (Note: at one time you thought that the problem: given a set of partons, what distribution of many particles does this yield was important. I now think it is very complicated and should be avoided because there are indirect decays and interactions going on slowly forever. Is it rhos which go into pis or

pis that are directly produced, and so on? So, although there may be a point in trying to relate products from different reactions, an attempt at an absolute determination of reaction products of highly multiple collisions should be deferred perhaps and only two-body reactions amplitudes or inclusive total probabilities should be studied. Thus the Mala way of summing is useful, though it is a series of special problems.” I don’t know if you want me to read special problems.

Weiner:

Let’s see how far down this goes. Well, you can’t go through the whole thing. I just wanted that as an idea. Is that the last or close to it?

Feynman:

No. 7/71, no. It’s got a few ‘72s in it. Here’s some stuff from ’72. “Colored quarks and their interactions.” How by having colored quarks interact to octet coupling, octet in color, you could have the whole thing — what we find is that inside the protons the particles appear as a bose particle but inside — but the quarks — but they can’t be bose particles. So one way is to have them in three colors and I’m analyzing that here. [???] with spin. Lots of preparation for my course.

Weiner:

Yeah, what course are you giving at the moment — this semester?

Feynman:

Nothing very interesting — advanced quantum mechanics.

Weiner:

Oh yeah, you said you were teaching it. OK, I think this will be terrifically important. Maybe on the transcript of this, especially for some of the earlier stuff that you were talking about from ‘66, ‘67, where there’s no other record of it, you know, it might be good to check back on a few of these entries to see if I have it right.

Feynman:

Here’s a speech for a colloquium 1973. That looks good. These notebooks are going to help me with historians... They make me feel very important.

Weiner:

Why not? Yeah, they're great. What about relating correspondence to this?

Feynman:

I don't answer many letters.

Weiner:

The only one letter we really talked about in great detail was the letter to Fermi from Copacabana, you know, the letters back and forth?

Feynman:

There's also a letter I want to get — you ought to have — it's just very amusing. It's to Bethe, Hans Bethe, in which I conclude that particles, first that the V-meson might have a unique energy of disintegration in spite of the experimenters, in which I went down and I looked at all the tracks in the cloud chamber and decided they weren't measuring it right. And then further on I mention what now is called associate productions, I say these particles must be made in pairs, which was before associate productions.

Weiner:

When was this?

Feynman:

1953.

Weiner:

I'll tell you about a letter to Bethe that I do have — not with me —

Feynman:

Have you got that thing still running?

Weiner:

Yeah, for this purpose. I have the date of that letter — here — Feynman to Bethe, March 13, 1953, 5 pages. For my purposes I put "technical".

Feynman:

That might be it.

Weiner:

Let me check it out.

Feynman:

That's probably what you got. You got it.

Weiner:

I think you gave it to me.

Feynman:

I probably gave it to you.

Weiner:

There's only one other really deep letter like that which is written to Weisskopf — January 4 - February 11, 1961, a bunch of clippings. These are mimeographed — a total of 15 pages.

Feynman:

That's right, that's on gravity.

Weiner:

What I'm going to do is to key on the transcript, to make a note that those letters exist so that one could check through on those. And what I'm going to do with Judy here is to

—

Feynman:

— get these notebooks from me somehow.

Weiner:

Yeah, let her know about them. OK, then I'll shut it off for sure now. Right? This is an epilogue on February 5th — after you've had a chance to look at the transcripts of our 1966 sessions to see what we missed.

Feynman:

Yeah, no, '66 was it? Yeah, that's the sessions. This has to do with the old sessions. I noticed two pieces of work that I didn't notice there, or I remembered two pieces of work. One of them was after the paper on slow electrons in a polar crystal, which appeared around page 705, according to this index. I invented this method to solve complicated interaction problems which was applied in the case of statistical mechanics to the slow electrons in a polar crystal, but the method would apply also to field theoretic problems possibly. And so I tried with Koici Mano as a student, tried to develop this technique and show how it applied in meson theory, and that was a — the trouble was the meson theory itself was an inconsistent Hamiltonian that we started with so we didn't make much sense of it. But it gave all kinds of effects that were physically right for the rather silly problem that we set it up for, and it did show promise to be applied to general meson theory. The reason this is important is that everyone always says that one of our troubles in the meson theory is that we have to use perturbation theory and I was trying to show that we don't need to use perturbation theory, but unfortunately for the real meson problems, the real meson problem that we did work out was a physically unsound problem and so it gave physically unsound answers. This paper was published as a thesis by Koici-Mano and also appeared in a Japanese theoretical physics journal some time. It's [??].

Weiner:

How long a period of time did it take?

Feynman:

For him to get his degree, he must have worked a year or so. It was hard work. And also I applied it to quantum electrodynamics and got some very interesting mathematical things in it, but I never published them. I didn't think it was sufficiently significant.

Weiner:

Are they in the notebook — those things that are unpublished? FEYNAN: No, they won't be in the notebook. See, the notebooks are written since 1965 and we're talking about something nearer 1955 or '56. It'll be in some old notes somewhere.

Weiner:

That you still have somewhere?

Feynman:

Uh huh.

Weiner:

Let me ask just a question — we were talking, about students the other day — take him, for example: do you know anything about his subsequent work? What happened to him?

Feynman:

I think he's in America now.

Weiner:

But he went back to Japan for a while?

Feynman:

Yes. OK, now there's another problem I worked on somewhere along this period and I can't get you the exact date without looking up a lot of things. Professor Kac — I often say Kac and you write "Cotts." It isn't Cotts.

Weiner:

It's Kac. I corrected it on the other copy.

Feynman:

And Mark Kac came here just after he had developed the solution to the Onsager problem using determinants — Kac and Ward, that's a reference. [??] at Caltech. I realized that the argument, I thought, could be very much simplified and the result could be expressed in a very simple matter in terms of paths, integrations around a path on the lattice that the Onsager problem is built on. We discussed it at considerable length and I showed it to several students, Michael Cohen and so on, but I never published that either. It did get into the literature — a form of it. I needed one step that I had a little trouble in demonstrating or I thought I had trouble in demonstrating or, well, I didn't have trouble, I thought it was all right, but people said it wasn't demonstrated and they criticized Mark Kac and Ward too, and it became Feynman's conjecture. It was finally proved by a man named Sherman, but I didn't recognize the conjecture in the form that it was. It was some complicated way of saying what I was going to say. But there's a very simple way of doing path integral and I have since seen a man that it's all that it's been — I don't have a reference here. In 1964 or '70 or something like that —

Weiner:

'64 or '70?

Feynman:

1964 or 1970, 1970 or '71 — I'm not sure what year — it was worked out by some Russians exactly the same way, and it appears in the back of — oh hell. Anyway, that some work that I did — you do it by paths —

Weiner:

The idea of Feynman's conjecture was nothing from what you published but from other people referred to that you had developed?

Feynman:

Yes, they must have heard from somebody that I had done this.

Weiner:

They incorporated it into some published thing. So maybe ask Kac for the subsequent references since he's right there.

Feynman:

Yes, fine, that'll do it. And that's the only things I can think of.

Weiner:

OK, that's not bad — it's just those two fill-ins.

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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:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

You hadn't really had any exchanges.

Feynman:

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

Weiner:

Was this a group?

Feynman:

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

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

Weiner:

Are you going to leave Fermi now?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I visited Chicago later, much later.

Weiner:

Yes, I have letters about that —

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

What did you do, hike in mountains there?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

Jim — you mean Aage Bohr?

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

Did you see him after Los Alamos?

Feynman:

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

Weiner:

Oh, sure, in '62.

Feynman:

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

Conference.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

How about Feller?

Feynman:

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

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

Weiner:

This was a conscious part of your plan?

Feynman:

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

Weiner:

Black and white, that's scotch?

Feynman:

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

Weiner:

Was he amused by this?

Feynman:

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

Weiner:

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

Feynman:

Yeah. It was amusing.

Weiner:

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

Feynman:

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

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

Weiner:

Was there a big enough market for them?

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

I understand that part, but I mean, yourself.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

They have a conflict.

Feynman:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

Was this a friendly type of exchange?

Feynman:

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

Weiner:

This is Marcel Schein?

Feynman:

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

Weiner:

You mean you saw Berkeley?

Feynman:

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

Weiner:

What was your impression of him as a person?

Feynman:

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

Weiner:

What would have been your objection to the machines?

Feynman:

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

Weiner:

When did all this change?

Feynman:

Gradually, with the recent guys with the newest machines.

Weiner:

In the 50s — since '55, do you think?

Feynman:

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

Weiner:

Who did you meet when you were there?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Berkeley, then, was no attraction for you.

Feynman:

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

Weiner:

On that trip, you did see Caltech.

Feynman:

Only in passing.

Weiner:

Just to look at the campus.

Feynman:

That's right, yeah.

Weiner:

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

Feynman:

Yeah.

Weiner:

I see, for the six weeks —

Feynman:

That's right, yeah.

Weiner:

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

Feynman:

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

Weiner:

Was this one of a series of international symposiums?

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Pauli you had met at Princeton, right?

Feynman:

Yes, I had met him originally at Princeton.

Weiner:

As a student.

Feynman:

Right.

Weiner:

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

Feynman:

Yes.

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

This was in —

Feynman:

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

Weiner:

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

Feynman:

I think that takes care of everything.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

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

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

Weiner:

It’s interesting to me personally.

Feynman:

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

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

Weiner:

Smug.

Feynman:

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

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

Weiner:

You hear voices.

Feynman:

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

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

Weiner:

He didn't look at you?

Feynman:

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

Weiner:

You thought you needed to set the record straight.

Feynman:

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

Weiner:

Throw that in their faces.

Feynman:

Yeah.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Well, let me tell you what you said.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Yes, I think so.

Feynman:

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

Weiner:

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

Feynman:

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

work, living here, working here.

Weiner:

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

Feynman:

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

Weiner:

These were graduate courses?

Feynman:

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

Weiner:

Whom did you get acquainted with on the faculty?

Feynman:

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

Weiner:

Where did you live?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Yes, that helps fix some dates.

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

That was some Cornell work.

Weiner:

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

Feynman:

Yes.

Weiner:

What got you interested in that?

Feynman:

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

Weiner:

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

Feynman:

Others thought of it.

Weiner:

And published it?

Feynman:

Yeah.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

The cosmic ray group here, Leighton, Anderson —

Feynman:

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

Weiner:

Were you considered sort of theorist-in-residence?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Why didn't you —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

To express your ideas, you mean.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

You involve yourself.

Feynman:

Yeah, yeah, sure.

Weiner:

Were there any weekly colloquia that you would attend?

Feynman:

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

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

Weiner:

Let me just —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Had you been concerned with the helium problem before?

Feynman:

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

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

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

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

Weiner:

This Brazilian interlude here was another trip to Brazil?

Feynman:

Yeah.

Weiner:

Was that for a summer?

Feynman:

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

Weiner:

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

Feynman:

That's right, a series of papers.

Weiner:

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

Feynman:

No, three.

Weiner:

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

Feynman:

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

Weiner:

Yeah, that's what I meant.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

I see, because that was your current work.

Feynman:

Right.

Weiner:

Did you meet Tomonaga on that trip?

Feynman:

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

Weiner:

Oh, when was that?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

When, several years later?

Feynman:

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

Weiner:

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

Feynman:

Which time?

Weiner:

Later — I mean, after the 1953 conference?

Feynman:

Yes.

Weiner:

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

Feynman:

OK, '55.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

In Tokyo?

Feynman:

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

Weiner:

You found Yukawa easy to talk to.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

What I'm asking you is —

Feynman:

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

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

Weiner:

That was in March, 1954.

Feynman:

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

Weiner:

Why not?

Feynman:

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

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

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

Weiner:

Detlev Bronk?

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

But this is quite a revealing story.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Yeah.

Weiner:

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

Feynman:

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

Weiner:

Things were beginning to happen, that's why.

Feynman:

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

Weiner:

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

year the fall of parity.

Feynman:

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

Weiner:

Yes.

Feynman:

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

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

Weiner:

What have you done with the press since?

Feynman:

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

Weiner:

Francis Bellow.

Feynman:

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

Weiner:

Yes. He has a little book, I think —

Feynman:

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

Weiner:

Earl Ubell?

Feynman:

Yeah, right. He's good.

Weiner:

There's no longer any Herald Tribune, though.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

When was that?

Feynman:

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

Weiner:

Yeah, I think so.

Feynman:

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

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

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

Weiner:

How did this help in explaining experimental...?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

That's right, yes, exactly.

Weiner:

Published in '55.

Feynman:

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

Weiner:

Volume I, Chapter 2. I see.

Feynman:

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

Weiner:

Who is Michael Cohen?

Feynman:

A statement.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

No, the energy spectrum.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

That was in 1954, right?

Feynman:

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

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

Weiner:

So this was earlier work.

Feynman:

Earlier work that they continued doing until they finished it.

Weiner:

Then there's an article with Speisman...

Feynman:

Speisman, yeah.

Weiner:

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

Feynman:

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

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

Weiner:

Did this have much effect on theoretical thinking?

Feynman:

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

Weiner:

In your paper?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

No. There’s a business about the poloron.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

It's working.

Feynman:

"Slow electrons in a polar crystal?"

Weiner:

Yes, there were two papers.

Feynman:

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

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

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

Weiner:

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

Feynman:

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

Weiner:

But the paper that you did publish —

Feynman:

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

Weiner:

Rather unique —

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

But you did publish an article on superconductivity and solidity.

Feynman:

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

Weiner:

It was Reviews of Modern Physics.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

No. No. No. [Slowly]

Weiner:

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

Feynman:

No.

Weiner:

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

Feynman:

No.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

What was your attitude on that?

Feynman:

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

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

Weiner:

Ok. That's interesting —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Right, it is.

Weiner:

With de Hoffman and Serber — this was a holdover?

Feynman:

Right.

Weiner:

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

Feynman:

All right.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I think so. Right.

Weiner:

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

folklore now, and so —

Feynman:

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

Weiner:

Yes, Morton Kaplan did get it from Block.

Feynman:

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

Weiner:

I read the proceedings of the conference.

Feynman:

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

Weiner:

Yes, at the end of the discussion.

Feynman:

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

Weiner:

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

Feynman:

Yes. That was amusing.

Weiner:

All right, then —

Feynman:

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

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

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

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

Weiner:

How long did you work on it?

Feynman:

Overnight.

Weiner:

All night long?

Feynman:

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

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

Weiner:

Right away?

Feynman:

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

Weiner:

All right. Do you want to turn this off?

Feynman:

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

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

Weiner:

I see.

Feynman:

So we put both names on it.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

Well, you visualize a sheet, and —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

What, weak interactions?

Weiner:

That's right, this business.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I met my present wife in Geneva at that time.

Weiner:

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

Feynman:

Well, I can't tell you.

Weiner:

That's an important event.

Feynman:

I met her at the beach in Geneva.

Weiner:

When were you married?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

Oh, no. No.

Weiner:

You remember the paper?

Feynman:

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

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

Weiner:

Was this one summer?

Feynman:

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

Weiner:

Where did you do this sabbatical year?

Feynman:

Here.

Weiner:

Oh, I see, instead of a vacation.

Feynman:

Yes.

Weiner:

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

Feynman:

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

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

Weiner:

You felt at home, then.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Well, it was an interesting interlude.

Feynman:

Yes, it was fun.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

It was sent to Physical Review Letters.

Feynman:

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

Weiner:

Let me ask you some —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Some historical thing, perhaps?

Feynman:

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

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

Weiner:

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

Feynman:

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

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

How long did you do this, three years?

Feynman:

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

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

Weiner:

This represents two full years that you put in.

Feynman:

That's right.

Weiner:

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

Feynman:

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

Weiner:

It was published in '65.

Feynman:

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

Weiner:

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

before.” I’d say this is volume 2.

Feynman:

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

Weiner:

Well, it says freshman and sophomore.

Feynman:

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

Weiner:

Oh, here it is, June, ‘63.

Feynman:

Ok — that’s when the preface was written.

Weiner:

Last year and the year before.

Feynman:

Ok. Good.

Weiner:

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

Feynman:

I started in ‘61.

Weiner:

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

Feynman:

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

Weiner:

I see. That fits very nicely into this —

Feynman:

Hole.

Weiner:

Way of accounting for time.

Feynman:

Except for '63.

Weiner:

Sounds like I'm your bookkeeper, you know.

Feynman:

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

Weiner:

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

Feynman:

Somewhat.

Weiner:

Because of their use beyond Caltech.

Feynman:

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

Weiner:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

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

Weiner:

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

Feynman:

Yes.

Weiner:

They were science majors?

Feynman:

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

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

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

Weiner:

The discussion followed?

Feynman:

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

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

Weiner:

This is even classical physics without electrodynamics, then.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

But would they go into teaching?

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

You wrote an article recently about this —

Feynman:

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

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

Weiner:

Do you have any foreign students in your classes here?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

Yes. What year was that?

Feynman:

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

Weiner:

Who else was there from this country?

Feynman:

I don't remember.

Weiner:

A large group?

Feynman:

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

Weiner:

That was a conference on quantum electrodynamics?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

At high energy.

Feynman:

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

Weiner:

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

Feynman:

Guessing; just guessing.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

How about the ideas, though?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Yes. I think so.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

The time is ripe for it now.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

—

Feynman:

We'll see something missing, probably.

Weiner:

We'll see that things are missing.

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

“Give us a quote we can use.”

Feynman:

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

Weiner:

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

Feynman:

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

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

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

Weiner:

Like an old movie.

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

No. You tried it out on me.

Feynman:

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

Weiner:

No, you had the idea.

Feynman:

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

Weiner:

You must have done it right away.

Feynman:

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

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

And Weisskopf invited you go give the address.

Feynman:

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

Weiner:

After?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Spontaneous reaction.

Feynman:

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

Weiner:

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

Feynman:

Yeah, all right.

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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:

Want to turn off your tape now?

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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:

I'd like to get onto this business of being notified by MIT, preparing to go there, and so forth.

Feynman:

I don't remember the exact notice about MIT, but there were a few preliminary things that happened. They had fraternities at MIT, and the fraternities were looking for students, so they had smokers, as they called them. Big deal, you know — kids — where you go to meet the fraternity boys, because they're looking for good pledges. At MIT there are only two Jewish fraternities, and those were the only ones that were interested. I went to the two smokers. Or at least I remember one of the smokers. And there, at that smoker, they were very kind to the freshman; they wanted them to be impressed with the fraternities. It was the old upper classmen who lived around New York who would look for these New York boys going up, and the particular smoker was for a fraternity called Phi Beta Delta, which I later joined. They found I was interested in science, and two boys, one who was in the mathematics course and one was in the science course, Art Cohn and Eli Grossman, who were going back to be seniors, started to talk to me. Mr. Grossman asked me questions about mathematics, and told me, after asking, that I knew enough mathematics if I knew calculus, and it was silly of me to take the regular calculus course for the first year; and that it was perfectly legitimate at MIT to take examinations in a course — you don't have to pass. So you could take the examinations in the math, and he would advise I take these examinations for the first year of math, and then I could start in the second year right away. So that was the important part of that.

Weiner:

Was this in the fall?

Feynman:

This was in the summer, before I went there.

Weiner:

In New York City?

Feynman:

Yes.

Weiner:

Oh, so they had their smoker in the city --

Feynman:

Looking for the city boys going to MIT. They had smokers in other parts of the county for other members of the fraternity, you understand me? They were what you'd call now looking for pledges — what they'd call pledging, looking for good pledges.

Weiner:

Rushing is the word, isn't it?

Feynman:

Rushing. Exactly the word. So they were very pleasant to you, very nice. He made this suggestion, and I did do that. I took the two exams for the first and second half year of calculus. I had to study it a little bit ahead, at home. I remember this particular thing, because every problem I would do, I would get wrong. My father said, "How are you doing?" I said, "I get everything wrong." "You're not doing too —" I said, "No, it's OK, it's just little trivial mistakes, sine's, just a question of getting in practice a little bit." But when I got to it, I passed them both satisfactorily, so I could start in the second year math. I made a discovery, when I got to MIT and the second year math, that if I'd had enough wisdom, I could have also taken the examination in the second year math.

Weiner:

Second year was what, advanced calculus?

Feynman:

Well, it was what they called differential equations and something else, I can't remember — oh, integrating in three dimensions, volume integrals, and differential equations. But I was also quite capable at that, and didn't realize it, you know, so I hadn't thought to take that exam. So I was wasting time in the first year of mathematics at MIT, which was the second year math.

Weiner:

Did you live in a dorm there?

Feynman:

Then, when I got up, the other fraternity, SAM, Sigma Alpha Mu — those boys also came around. They were going to drive up, and they said, "Why don't you drive with us? We'll take you to MIT." So I got in the car with those fellows. My mother still remembers the day her little boy left home — you know, cars and strangers who were going to drive him all the way to Boston — it was a big day! But to me it was only a great, happy excitement. We had quite an adventure in the car, because it would snow, and the car skidded, and it was a long trip and we talked about things. I was treated like more of a man, you know. It was a big deal. You were grown up. So we got up there, and I was staying at the SAM house, because they had driven me up and they asked me to stay at the house temporarily. And in the morning, when I woke up, they were very upset. They said, "Two men are here looking for you." It was those two boys from the Phi B D house, and they took me to the other house. I guess I was a little bit dumb. I mean, I didn't realize the trouble those fellows were having rushing me. But I was wanted by both houses, which was a pleasant feeling, because you remember I had had this sissy-ish feeling and so forth, but it disappeared at MIT because of all this attention. I went finally, anyway — after a lot of detail which I can't remember — I joined the Phi D B house, where these two guys were, ultimately, and became a pledge there. The day I said I would be a pledge, everything changed, of course. Then I had to carry matches for the other guys, and the friendliness disappeared. Anyhow, I mentioned the fraternities, because I would like to say, about social matters, that the fraternity was for me extremely good. Phi B D had had a great difficulty, just previously, and had almost disintegrated, and they had built up a compromise. They had two groups of fellows in the house, the academic, studious type who knew something, and the wild social guys with cars who would zip around, and knew all about girls and so on. And they almost broke apart, because of the interests, differences in interests of these two. But they had made a compromise, which was that the wild Indian social fellows had to maintain certain grades, and if they didn't they would get lessons from the other guys, and they would have to work a certain number of hours and not go out unless they got such grades. Whereas the other fellows — and this was an important thing — would have to take a date to every dance, would have to go out to the dances and do these things. It was important, you see, to them. And the other fellows if necessary would get them dates, if they couldn't get them themselves. But they had to, you know. And this was an interesting environment for me. The same went, of course, for the pledges. The pledges had to make, earn, certain grades, which was easy for me, but also had to get dates for the dances. We had formal dances. We got dressed up in tuxedos. We had to run around and get dates.

Weiner:

Where did the dates come from?

Feynman:

Oh, the schools in the neighborhood, and friends, girlfriends of guys' girls, guys who already had girls — you know what I mean? Their sisters or somebody. I was frightened of girls when I went there. I remember when I had to deliver the mail. I'm just trying to tell you the differences. It's interesting, how social attitudes develop. When I had to deliver the mail; to take the mail out from upstairs. It happened to be a time when some of the juniors had a few girlfriends, two girlfriends. There were just sitting on the steps talking, and I just didn't know how the heck I was going to be able to carry those letters past them. Girls scared me. This whole business scared me. So that was the condition of it. I did get the letters through, and on the way they said, "Oh look, isn't he cute!" and all this kind of stuff, which scared me and worried me, you know. Anyhow, this same fraternity had that the pledges had to learn how to dance, and so they would get girls to come, their girlfriends and friends of their girlfriends, to come on certain days of the week, and they'd turn on the records, and they taught their own pledges how to dance. This was a really impressive, important thing to me. I realize now, you know, I got a much better social confidence and everything else. And I know that that fraternity was a very important thing in my life. I know it — I mean, as far as social things — because, although it was hard to do, it forced me to do it. It's easy to not do it, it's scary, and it's easy to not to do it, but they made sure I did it. They taught me to dance. And so the confidence came relatively rapidly after a while. My first date was a blind date. But they were careful not to make the guy embarrassed by having some harlot. They were really serious about it, so they got a pretty good girl for the first date, whose name I remember and so on. I can remember, just to tell you a joke, the first girl that they cooked up for me — I couldn't get my own date, and they got me a girl called Pearl something. So I was from New York, so I said, "Oh, Poil. Poil." They said, "No, you must say Pearl, because she'll be horrified if you say Poil," and so on. So I made believe I couldn't do it, and I would say, "All right, stop me in the middle, I'll go through the thing," you know. "Pearl, Pearl, Pearl, Poil, Poil," and they would hit me when it was right, and I couldn't get it right. Then when I met the girl I said, "Listen, I understand your name is Pearl." She said, "Yes." I said, "You see, I say it very nicely — Pearl." She said, "Yes." Then I told her that they'd been worrying about how I was going to pronounce it, "So if you don't mind, when I introduce you, I'll do it" — peculiarly, you see. So every guy, I would say to him, "This is my goil, Poil. This is Poil so and so." Of course she knew I was kidding, but they were horrified. Anyhow, it relaxed me a lot, all these experiences. I think it did a lot for me socially. In the meantime, my girl Arlene was back at home, and we were writing letters, probably every week.

Weiner:

Was she still in high school?

Feynman:

No, she was out of high school, I think. Maybe she was in high school one more year. But we wrote letters all during the MIT period, and later on at MIT I had her up for dances from time to time, to visit the school and have her for a dance and so on. I went out with other girls when she wasn't around. But we were engaged somewhere along the way, and in spite of that I kept going out with other girls. It was understood between us that that was all right, because it was just impossible that any other girl could mean anything to me. I mean, it was just clear. Maybe naive, but it was true, and it worked out that way. She wasn't upset by it. When school started, after I was there a while, I took courses, all of which I don't remember in very much detail.

Weiner:

Were you majoring in mathematics?

Feynman:

Oh, yes. That's interesting. At first I was in the mathematics course. It doesn't make any difference what course you are in, really, very much. For the first year or so you take more or less the same thing. You take physics and chemistry and electrical engineering, mathematics, and so on. English. Somewhere around the first year I began to get upset. This wasn't right. The mathematics, I looked at it, the mathematical things, were too abstract. They weren't connected to anything, mathematics. And I went to the head of the mathematics department. This was in 1936, now, so you know it's still in the Depression. I said to him, "Sir, what is the use of higher mathematics besides teaching more higher mathematics?" So he said, "Well, you could become an actuary," calculating the insurance rates for an insurance company. This didn't sit well with me, see. He also said that a man who asked that kind of a question is perhaps not right for mathematics. And I thought the thing I ought to do — I mean, I liked to get my hands dirty. I'd had a laboratory. The physical world was real, and the mathematics, I had become enthralled with, but not for itself, really — you know what I mean? It was fascinating, but my real heart was somewhere else. So I decided, I have to get my hands dirty, I can't stand these abstract things. So I changed to electrical engineering, because there was something that was real. But then some few months later, I realized I'd gone too far, and that somewhere in between — that physics was the right place. So I moved around a little bit at the beginning, and ended up with the physics course.

Weiner:

Were you penalized in terms of courses?

Feynman:

No. No way at all.

Weiner:

All the courses —

Feynman:

Yeah, they were so close — yeah, they were all the same. No, there was nothing. It was just a question on the future. I had to worry about what I was going to the next. When I looked at the mathematics program, we would be talking about integrals or something and I didn't see what the hell good it was. The only thing I could think of was, this stuff was good to teach somebody else, but it wasn't good itself. I still have that feel about mathematics.

Weiner:

The physics course that you took, the first one — was it new to you, or was it stuff that you'd covered?

Feynman:

No, it wasn't very new me. It was mechanics and it was a little bit boring about inclined planes and statics and dynamics. It was always the same. I always had learned the stuff before. There were a few things — because I can vaguely remember — that were interesting. One was the laboratory. I had not really used my laboratory to make physical measurements, you know, in the usual way. I used it to play around and make a radio, but I'd never used my laboratory in the sense of an experimental, numerical research. So the first experiment, which was dropping weights to see the acceleration of gravity, was only vaguely interesting. But the beautiful experiment, which I still remember, in the lab, was this. There was a ring. You know, other experiments — dropping with the apparatus, with spark caps, with wheels, with all kinds of things. There was a hook on the wall; I mean a nail driven into the wall, and a ring of metal, a metallic ring, an amelus, whatever you call it, like a big washer, a big thing. It said: "Hang on the wall, measure the period, calculate the period from the shape, and see if they agree." I loved that. I thought that was the best doggoned thing. I didn't care as much — I'm just trying to remember now — I liked the other experiments, but they involved the sparks and all the other hocus pocus, which was too easy. With all that equipment, you could measure the acceleration of gravity. But to think that physics is so good, not that you can figure out something carefully prepared, but something as natural as a lousy old ring hanging off a hook — it impressed me, that I had now the power to tell what something as dumb as that was going to do. It didn't impress me so much that I had the power to figure out what would happen if I had lots of oscillators, I had the experiment carefully prepared, with all this equipment to measure just so — but that in a lousy ring hanging from a hook — that impressed me. So I liked the physics lab, a little bit, because I hadn't quite

done that kind of thing. I also remember later, a few years later, in electricity, that we had a rotating disk. We had to measure the ratio of units, of electrical and magnetic units. We had a rotating disk, Faraday's rotating disk, with a magnetic field, making a current, and you measure the current, but charging a condenser to a certain voltage, so and so many times. Anyway, you measure the ratio. I remember I was working on it with a friend named Ted Welpin, who I'll come back to, because I'm now in the sophomore year, by mistake. I have to go back. But anyhow, at that time, the teacher came and said, "You know what you're doing?" I said, "Yeah, we're measuring the speed of light."

Weiner:

Because, the ratio of units —

Feynman:

Yeah, yeah. And we knew it, and he was very happy, the teacher, John Wolfe, you know, that somebody knew what the heck it was. Because, you know, the whole system is really quite poor. I mean, you have a lot of wires to connect to buttons. You have a piece of paper that tells you what to measure next. And unless you know so much, you can't get any pleasure out of it. I had to know, in order to get any pleasure out of that ring, what physics could and couldn't do. I had to be very advanced to appreciate that ring. I had to be advanced enough to understand that it was marvelous, that we were measuring the speed of light by rotating a wheel at hand speed — that some miracle — where these high numbers come from, you know — it's quite exciting, to do it in a room that big. And so on. So I feel that somehow education isn't right — the appreciation of it — there's no motivation. Motivations are not carefully handled. And the motivation is the whole — makes love, makes good out of it, I was motivated because I'd had other experience.

Weiner:

What about the other guys in the lab?

Feynman:

I don't know, they just did all these things.

Weiner:

Well, a lot of them of course weren't necessarily physics majors. Or were you in a class of physics majors at that time?

Feynman:

I don't remember now. The sophomore year probably included a lot of other people.

Weiner:

Other people from the engineering curriculum and so forth.

Feynman:

Right. All right — now we get back to the freshman year, or the beginning of MIT. Was it freshman or sophomore? Somewhere along the line, either the freshman or sophomore year, probably the sophomore year, after I had discovered that I knew so much math — because, you see, I had gotten ahead in the math. I had extra time. So then I would take another course ahead of time. So at the beginning of sophomore year — (if there's anything in freshman year to come back to, I'll try to remember — I can't keep these things separate) — somewhere, I believe, in the sophomore year — because, you see I was ahead. I had time. Do you understand? So I had another space to put another course. Oh yes, I remember now. In my freshman year, my friends were in the senior year, my two friends at the fraternity, and I roomed with them. They were studying a course called mathematical physics, taught by Slater, an advanced course. There's a book on it. It uses a text by Slater. Theoretical physics, I think it was called. Big deal. They're studying this thing, and I'm sitting there, a dumb freshman. So one day — it was only a few weeks, a month or two — I hear them talking. They're worrying about a problem they can't do. So I know something, I say, "Hey, why don't you try Bernoulli's equation?" They don't know what I'm talking about. See, I had read everything in the encyclopedia. I'd never talked to anybody. I had mispronounced everything. Everything was mispronounced, my notations were cockeyed. Finally we communicated: "He means Bernoulli's equation — ha ha ha!" So they tried that equation and it solved it very nicely. So they talked with me, and they would discuss the problems after that. We talked about these problems. I couldn't really do them all, naturally, I wasn't that good, but I did do little bits and parts, and because they discussed it with me, I learned a tremendous amount. So I figured, in the sophomore year, I could take that course. After all, I had discussed with them the problem. I knew what the level was. I wasn't making a mistake. I was getting confidence, you see. Whereas the first time I only took one year, now I'd decided to take the senior course in my sophomore year. MIT was free enough that, if you had the guts, you could do it. Well, I remember this — the first day —

Weiner:

Did you have to get permission of the instructor?

Feynman:

I don't remember. Some permission, probably. I remember the first day, going into the

class, because I had to wear my ROTC uniform, which only first and second year people had to take, you see.

Weiner:

Compulsory?

Feynman:

Yes. And I had my uniform on. It was a dead giveaway. Besides, I looked very young and so on. These were all seniors, and I was a sophomore. I was quite — partly worrisome, but partly I was a little bit proud, you know? I mean, inside. Everybody had these green and brown cards to fill out, from senior and graduate students. There were many graduate students in this class; graduate students from other schools would take this to get, you know, practiced up. See? Good course. Top of the heap of advanced physics. And I had a pink card! It was all very — I was obvious. And I was feeling pretty good. I'd pull out my pink card. And there sitting next to me is another guy in a uniform with a pink card. His name was Ted Welpin. So there was somebody else in the school who thought himself good enough to take this course. So we both started to talk to each other, happily, to discover the existence of another peculiar nut. And right away he said to me — I think the conversation started by my noticing that he had a book on tensor analysis. I said, "Oh, you've got a book by so and so on tensor analysis. I was trying to learn it. I wanted to get it at the library and couldn't find it." He said, "Yeah, I'm bringing it back. I'm trying to get another book that somebody's got out called such-and-such on something" — and sure enough, it was a book that I had out. So we really were made for each other. And that was terribly exciting, because we then would walk together, you know, the first few days we met each other. And he had learned relatively theory quite — which way was it? No, I guess it was the other way. I can't remember which way now. I'm probably remembering another time; I don't remember which way it was. He had learned relativity theory and I had learned quantum mechanics, or vice versa. I think I had learned the relativity and he had learned the quantum mechanics.

Weiner:

Where had you learned that?

Feynman:

At MIT, in the freshman year, reading the books in the library. I spent time in the library reading. I read Eddington's book on relativity, and I learned a lot. I think that's right, now. And he had learned quantum mechanics, which I hadn't learned very well. And so we taught each other — very quickly, you know, in just weeks, back and forth, and very soon we were on the same level in all these fields, relativity, and quantum mechanics. Oh, we had lots of conversations. It was a terrific educational experience, to have

somebody of your town type to argue back and forth, to learn. We were matched. It was great. I learned tremendous amounts from him, and vice versa. Together we would work things out — you know? But then we learned this course together also, and the course began with Lagrange's equations and so on. Being always the practical fellow, all I was interested in was the problems. A ball rolls on an incline, something, and I would work it out directly. I didn't have to learn the Lagrange. Next problem. I could do it directly, I didn't have to learn the — I do it in five minutes; he takes thirty minutes, grinding out through the Lagrange, you see. My friend would always do it the other way. As the problem went on, they got harder and harder. About a ball, spinning around in a paraboloidal, you know? Then it would take me an hour, him half an hour. But I did avoid — I really learned the Lagrange very well, because I wanted to make sure first that it was necessary to solve something that was really worthwhile. I always had this feeling of judging the thing against its actual application use. That's a very amusing thing. I realize it, now that you're asking me all this, but I see the same relation with him. I was challenging the Lagrange to show me that it was necessary — you know? He would do it his way, I would do it my way, and we always compared times. But of course, my way would take ingenuity, whereas the trick of the Lagrange is that you can do it blind. It's like analytic geometry compared to ordinary geometry. It's slower, but it's blind, it's sure fire. Well, this was the same situation. I still think you can do it better. But anyway, there were one or two problems where I was behind, near the end. But it was worth it. I was a good experience. Then we learned other things together in this course. I did learn things from this course.

Weiner:

Slater was teaching this.

Feynman:

Yes. I did learn things. In spite of my having gone over it for the second time, because I had done it with those students. I hadn't done it very thoroughly, just bits and pieces. But I did learn something from that course.

Weiner:

Was this a lecture course?

Feynman:

Lectures and problems. Lots of problems that we would have to do. This was, you see, for seniors and graduate students, and that was about the level on which I could learn something, when I was a sophomore.

Weiner:

Was he a good lecturer?

Feynman:

I think so. I don't remember. I learned something from the course.

Weiner:

Did you talk with him after the lecture?

Feynman:

I don't remember.

Weiner:

You don't remember if he had a relationship with you —

Feynman:

No, I don't remember. Not at that time. I didn't. I had a relationship with him later.

Weiner:

After you left MIT?

Feynman:

No. No, at MIT, but not in my sophomore year.

Weiner:

I see. That was a one semester course?

Feynman:

Yeah. Then, another thing happened, with both Welpin and I. A Professor Philip Morse discovered that we were good students. And in those days, quantum mechanics was not — there was not a good course in quantum mechanics at MIT. 1935, 1936, 1937, maybe, by this time and there still wasn't a good one. So there was no place for us to learn. But we realized we really wanted to learn it right. I mean, we had taught each other bits and

pieces, but — the same thing, you must go over it thoroughly and do it again. We asked him where we could learn, what books would he recommend? He said, “I’ll teach you.” So once a week we would go into his room, into his own office, and he would sit down in his black — we would sit down in two chairs, and he would spend the time to teach us quantum mechanics, for an hour — maybe it was more — an hour at least, a week. Poor Welpin had some sort of a disease where he fell asleep. He was unable to stay awake. It’s not boredom, it’s some peculiarity — it’s so serious. No matter how it was, if he sat still, he’d fall asleep. At the time he didn’t know how to handle it. He has since found that if he stands up and walks around, it helps. And furthermore, if he lets himself fall asleep, it only lasts five minutes, and he wakes up. But still, it was for him terribly embarrassing, because Morse would make this effort, and there he would be, asleep in the chair. But anyhow —

Weiner:

— Morse was young at this time too, wasn’t he?

Feynman:

I guess so. It didn’t seem to us so young, but he was young. Professor Morse then taught us quantum mechanics. But not only that, he gave us a research project in quantum mechanics to work out, calculate the energy levels of the light atoms, by a variational method he had invented that was new. It was different. And he knew how to teach. He was great for us, because also, when we had results that were useful for the astronomers at Harvard, we made a visit to Harvard to discuss the atomic energy levels with the astronomers, and what information they needed, and what maybe we could calculate for them, like the intensities of lines and so on. It was very, very good.

Weiner:

Who were the astronomers, do you remember?

Feynman:

I don’t remember the name of the astronomer. I do remember, though — amusingly enough — as soon as I started to talk, everybody laughed at me. I mean, they laughed at me because — I mean, Morse — it was “hydro genic atoms” — like hydrogen — that was a word I needed, an atom which had one electron, like lithium, total ionized, you know? So I said, “Well — the kind of wave functions we were using were like those of hydrogen so I said, “Well, we start with hygienic wave functions,” and so on. I was always careless with the words and pronunciations, and they kind of teased me a little bit about that. It wasn’t very serious, but I remember that.

Weiner:

This was at the Harvard Observatory?

Feynman:

Yes. I said, “The way we’re doing this, we start with hygienic wave functions, with perimeters in which we varied to get a minimum energy.” We had done these calculations. We did also work on an adding machine. You see, we learned a lot, Welpin and I. A computing machine — an old-fashioned adding — not that —

Weiner:

— I see, I know what you mean.

Feynman:

We learned a lot. Morse really brought us —

Weiner:

— he wasn’t teaching you — it wasn’t really a course?

Feynman:

No.

Weiner:

It was just extracurricular —

Feynman:

— Right. Well, he made a lot of effort for us, see?

Weiner:

How did he come across you?

Feynman:

— I don’t know —

Weiner:

— you didn't have him as a teacher?

Feynman:

No, I guess I didn't. He came across us somehow. I guess we went in to him, because we knew he was one of the men who knew quantum mechanics. It was a little backward. He knew quantum mechanics well, and so on. We probably asked him where we could get a reference or something. I'm only guessing. I can't remember. But he volunteered to teach us quantum mechanics. We had a course in atomic physics. There was a regular atomic physics course, which consisted in telling us that the square of the angular momentum was L^2 , and this and that — you know, the R levels and so on — which I don't think we learned much, but it was mostly that stuff. But this quantum mechanics that he taught us, we did learn a great deal.

Weiner:

This was about the time of your junior year?

Feynman:

Probably, yes, that he started us. Now, a thing happened there, between the two of us, in which Welpin taught me something of first rate importance. It was very important, what Welpin taught me. I had, fooling around with relativity and quantum mechanics, cooked up an equation which I claimed but the relativistic quantum mechanics. See, the Schrodinger equation was not relativistic. And by making relativistic varying forms and so on, I made the great equation. It is the equation which in fact Schrodinger had originally written, but I didn't find it, I didn't look it up, and it'd called the Klein-Gordon equation.

Weiner:

And you had come across this on your own?

Feynman:

But I cooked it up. I mean, it's easy — you generalize to relativity — and I had worked this out and shown it to him, and it stood in front of him; I played around with it and looked at it, and saw it for a long time, and I said, "This is it, this is relativistic quantum mechanics." So he said, "All right. Let us calculate the energy levels of hydrogen, and see if they agree with the right energy levels." This was a terrible shock to me — that in fact, we could actually do a real problem. You see, in spite of my practical attitude, although I had an equation that was so esoteric and marvelous — relativity, quantum mechanics,

grand invariant and all these wonderful things — the wonder of the formalities were impressing me, but not the question, what would actually come out? And to think that we really could figure out hadn't even struck me, so I said, "How? We don't know how to do that, we're too young" — you know what I mean? So he said, "Well, let's see — equation so and so, the potential of the hydrogen atom ought to be ZE^2 squared over R —"

Weiner:

This is Morse, you're saying?

Feynman:

No, no — my friend. And so we sat down, the two of us, and he showed me how to do a real problem with an esoteric equation. I cooked up the equation, and he tested it against a hydrogen atom, and it gave the wrong fine structure. We looked it up, and it's the wrong fine structure. So that was the end of it. And that was a terrible, a very important lesson, which was not to just rely on the beauty of the thing, and the marvel formality, it's relativistic, it's quantum mechanics, but to bring it down against the real thing — and that you can. It's hard to explain. These things have to be learned — that you really can bring these things to examples, and should. I had to learn it from him. I was so impressed with the principles. So, anyway, we did that. We discovered the Klein-Gordon equation, and, with his help, discovered it was also wrong. But that's the kind of thing we were doing. We weren't doing too badly, for young fellows.

Weiner:

What about other courses? Chemistry —

Feynman:

Well, we went through. I studied chemistry, analytic chemistry. I did pretty well with it, because it was science, and I liked it. And I had to learn things, in chemistry. I mean, I had to remember all kinds of stuff, that the test of aluminum was some organic compound that turned red, and so on. I learned a lot of chemistry. I had to. It's not something you can do by logic alone. But I also used my extra time, later — near the senior year, I had nothing to do — you know? I'd taken the senior courses, some graduate courses. I took stuff that interested me. I wanted to study metallurgy. I studied metallography, for instance, because it was a field I didn't know anything about. I was always interested to learn something about which I didn't know anything — to see what would happen, you know, in metallurgy, and metallography. I remember that course in particular. That's when I discovered for the first time the very great use of your knowledge of physics, the universality. I thought metallurgy would be a different subject, you know, about metals. And in the class guys were always getting up and saying things

like — you know, these were the metallurgy boys, who were now studying their senior course in metallography or whatever it is — we learned how to grind the samples, look at them under the microscope, all this stuff. So these guys would say, “We were working in the foundry, we had a sample of such and such a steel and when we pounded it something happened.” I had not such experience, so I knew I was at a disadvantage. But I didn’t really realize what a terrible advantage I was at because I knew about atoms. I knew it was nothing but piles of atoms. There were many things I would understand were possible or impossible without the experience. So I had to compensate to some extent. But I didn’t have confidence during the course that I’d really know anything. It was only after the final examination because the final exam came, and I tried to answer every question that I could. See, I didn’t have much experience, I couldn’t remember all this stuff so I tried to answer all these questions on what seemed logical, sort of semi-reasonable — I mean, what might happen with such things — as best I could. After the exam, I came out, and there’s these metallurgy experts talking — you know, the usual aftermath, after the exam. And they say, “How’d you do on the question of why chrome nickel something doesn’t corrode?” or something — you know. “Well,” I’d say, “You know, it’s the face —” I don’t remember now what. I’d say, “I think it’s because of the face it allows, and there’s a different packing on the surface, and so the oxygen —” “No, no, no, no! You see, it’s on account the —” something else. And so we settled questions like this, and I really felt, I was demolished, because these guys who (I thought) knew something told me everything I’d said was cockeyed. And in the end it turned out I had an excellent grade, and I had been right about all these things, you see, when the grades came. So I learned that really physics is a very useful background for what looks like different fields; that the world is the same, the physical laws are not so un-useful — you know what I mean? They work. Yeah, they work, and you can use the ideas in different fields, and you are ahead of the other guys, because there are a large number of things that are self-evident to you that they have to learn. But of course, you have to learn experience too. I’m not trying to say just — both together are much better than anybody. But it is true that studying physics is good all over the place.

Weiner:

Your physics course, you seem to have covered...

Feynman:

Well, there were courses where I learned something, yes. Let me tell you what I felt were good courses. This was a course given by; I believe by, Harrison, who was an optics man — that’s right, isn’t it? George Harrison — which was called Experimental Physics, which I found fascinating. It was a lecture course. In addition, there was a laboratory, but the lecture was interesting. It started with, “How would you make a plane?” — you know, and the business about grinding three surfaces against each other; how to make a device that sits in a definite position, but making a dot for one leg of the tripod, a line for the other leg, and let the other just sit on the surface of the plane, because if you

make two dots to put a strain into the legs — all kinds of interesting things about how to replace a thing. You take it out, you put it back, it should be in the same place—not by having all three dots laid, because it strains the legs, and so on and so on. Lots of interesting ideas, logical and interesting, about experiments. And these challenges — the storage battery: how much energy should it be possible to store per cubic centimeter of material? How much energy can we actually store in a storage battery? Very much less. Why is it impossible to put the energy in and be able to get it out at a much higher concentration than we can with storage batteries? Nobody knows! A great challenge! He knew how to teach the subject, you know. These were the challenges of experiments physics. The storage batteries were not so good. They're getting better. Your little cadmium batteries are much better than they were. But I knew that someday — that there's a problem that needs to be solved. And I learned about many problems that need to be solved from him. And also the beauty of — It was a great course. I learned a lot from that. That was a course I learned a lot from. In addition, the laboratory itself, in that course, was good. It was free laboratory. On the wall was list of 50 problems. You'd pick one out and do it.

Weiner:

For the entire semester?

Feynman:

Whatever you could do it in. I did two of them. One was diffraction of light from sound waves generated a liquid, benzene, by a high frequency oscillator. You know, you make sound waves, and the index varies; light going through makes diffracted image. Welpin and I did that together. The second challenge I took was to build a machine to measure the ratio of velocities of two rotating shafts.

Weiner:

To build it from scratch?

Feynman:

Yeah, yeah — make it — actually work it out, yeah. So I had to design how to do it, and build the darned thing, and I made something, but my bearings were all loose and my machine shop work was very poor, and so it didn't work.

Weiner:

Well, it required precision.

Feynman:

That's right, and I wasn't good enough at it. He, in fact, gave me a great compliment for design and everything, and said that it was only because of the machine shop work — that this would have really been a practical solution to the original problem, that they had been presented, but it was several years earlier, and they didn't need it; that I had quite a clever device for it. He was always presenting this kind of problem, and it was quite exciting. The course had other problems in it, that other fellows took, such as to make a bolometer which can detect a candle at six miles. I mean, it was good! It was great. And the guy would figure it out because it's within range. You see, these things were all real. They're all possible — they're just not impossible — and a fellow would get a lot of power out of it. It was an excellent course.

Weiner:

this was a one semester course, again?

Feynman:

I don't remember. One year, probably. Yeah. Then, of course, as I got into advanced course at MIT, it was true, I was learning things. I'd take a course in optics, with an experimental laboratory, and the optics course was so detailed that I would learn — the techniques of Wallenstein prisms, and techniques for measuring the index of refraction, and so on, stuff that I hadn't learned by myself. So in the more advanced courses, the more specialized, you see — like metalology itself — there more things that I didn't know. So I would take these specialized courses, near the end, and learn things in them. Another thing that I learned something about was this. We had at that time a thing called the NYA. That was the National Youth Administration, which helped to finance. Kids would have jobs helping professors in some way. I got — with Welpin, as a matter of fact — a job helping Professor Warren in the X-ray department. We were making some tables of the universal lattices of some crystals or something. We had to do it on computing machines. We developed faster and faster methods, in which he would do something and I would take the number off his machine and do something. We had a lot of fun, getting it faster and faster. The problem he'd given us, however — we calculated, we did it as fast as we could possibly do it, we were as clever as we could be, and then we calculated how long it would take. Seven years. And we came to him with the estimate and explained it to him, and said to him, "This is as fast as we can do. Look at the clever ways we're doing, you know. He said, "Boy, that's great. Now let's calculate how long it would take." So we talked him out of a job. I mean, not the job — he gave us other things to do—but this was a mistake. It was too much calculating.

Weiner:

Was this during a semester? This was semester thing?

Feynman:

This was some of off hour's job — maybe after school or something. No, it wasn't a summer job. No. Later I got the same NYA job in the laboratory of Stockbarger, a man who made single crystals, large single crystals of lithium, fluoride, and other alkali slides. In his laboratory, I just did things like put up shelves, fix lights, and so on. He tried to give me some experimental thing to do, like to make a certain substance (I don't remember what it was) out of lithium oxide and so on, with a furnace. I built the furnace and tried to make it. It didn't work. And to do some other experiments that he wanted to do. He gave me good things to try, but I guess I wasn't capable. When I was doing some other experiments with glass tubes, the glass crystallized in the furnace. I never got anything working good, except making the shelves and the lights, for the work with him. He had given me some good jobs, but they were always a failure. He also told me to design a device to measure the position of a shaft that's oscillating in the lathe. You know, you can buy them, but he thought I should make one. It was money troubles always. And I designed one, in which the motion was communicated through a rotating needle, by the method of the fire drill for making fires, where you have bow, you know, which you wrap around a stick and move it back and forth, and you get a big magnification. He thought it was a great design, you know. He said, "Ok, you design it and build it so on." I designed it and built it, but because of my lousy machining, imprecise machine work and so on, it was too wobbly and didn't work.

Weiner:

Were you impatient in your machining?

Feynman:

No, I tried. I always had a respect for machine work, and I always wanted to be able to do it. I always liked the machinists. I always thought they were great men, you know, who make these things. I don't understand why I was so lousy at it. I tried very hard. I had some fun, when I was working on something for Stockbarger, making screw drivers or something — I don't remember what — for him. I had to go down to the machine shop often, and they had a student shop. There was a guy in the main machine shop called Andy, who was supposed to give the students tools. So I always asked for tools. He was always teasing me, that I was so lousy in machining, you know. One day, he has a great brass ring in his lathe, and I come for some tools. And he's going crazy. He's got the ring, it's put in the lathe, he's trying to get it straight, so he has one of these needle things, like I had been trying to build, and he notices it's wobbling — you know? Then he has a lead hammer and he hits it somewhere, and he starts again and it still wobbles, and he's going like this. "My God," he says, "I've been doing this for thirty minutes and now you guys come and you want some tools and interrupt me!" and so on. I say, "Listen, Andy — I have a little work to do in there. It'll take me fifteen or twenty

minutes. When I come back in fifteen minutes, you'll still be doing this, and I'll show you how to do it." I mean, the only thing I could do was make a joke. You know. He laughed, and I went out to do my job. The only way to counter a guy is to make a joke, you know, because he tells me I'm no good and so on. But while I'm working for fifteen minutes out there, I'm thinking like I never thought before — how to do it? How to do it? How to do it? You see? And by the end of the fifteen minutes, I went back in there, and sure enough, the poor guy is still fiddling around. But I had a way figured out. He had chalk that he had been trying to hold up against the thing, to make a mark, you know, but the hand wobbles and everything. So I says to him, "All right, start it up, I'll show you how to do it." By this time he's so desperate, you see — he's absolutely desperate, he's been working for forty minutes, you know, he's just crazy — so he says, "All right." I say, "Give me the chalk — you see." And I watched the needle. And it goes zig zig zig zig zig, as the thing goes around. I had a pretty good sense of rhythm. I liked to play drums and so on, you see. And I saw the rhythm. Oh, first I said, "Andy, which way does that needle go when it's up?" You know? He says, "This way." So I watched. Zig zig zig zig zig, and I think to myself "rhythm" and I hold the chalk. And I move my hand in the same rhythm as the needle, you see, near the disk, near the wheel, and at just the one moment, one bang, against the disk — you understand? So what I'm really doing, I'm pushing the chalk against the wheel at the moment the needle the highest, but getting in time with the God damned thing, see. You can't do it in one sweep, it's incredible, but you can get in time. Bing. So I said, "Tap it here, with the mallet." He hits it. "I think it's too hard." I had to say that, because it might not work. He turned it on. Very smooth, with only a slight motion — jig jig jig jig jig — you know? I said, "Just a minute, we'll get it a little better." Just again. Same trick. Because no matter how small the nation, the time timing's the same. Then I say, "Here, tap it very lightly here." He taps it very lightly here, turns it on — mm mm mm, perfect! I felt great! I was no good at machine shop work, but that was my great triumph, when I told Andy how to straighten that out! I always wanted to be machinist, for some reason or other. I mean, this was a great moment. I can't do it, but I won. And another time I got a great kick out of it — see, what I loved was the practical man. A great respect for the practical man, probably idealistic and incorrect, but I thought a man who really could do these things had real knowledge of some deep kind, so I loved these guys. So another time, I was in another shop. I had sent down to bend some sheets of metal for reflectors, for the lamps, the lights; I was putting in for Stockbarger's room. They didn't have any big metal bender down in physics. I had to go to the chemistry shop. So I'm down there bending this thing, and I see, there's two guys down there, and I can hear them talking. They don't know me from Adam. They don't know I don't know anything about machine shop. They have a circular disk, a copper ring, with a screening over it, for some kind of a generator that's across it, and they want to drill a hole in the center of the screening — you see? To make a nice hole, a half-inch hole or an inch hole in the center of the screening, for some purpose. They're making some kind of a generator, a gas generator or something. So they don't know how to find the center of a circle, and they're going around with a piece of paper, and thinking and talking to each other and asking some other guy, "How do you find the center of a circle? I can't remember how to find the

center of a circle?" On the paper they've drawn the circle. They're trying to construct it. Ok. Again I have time to think. You see, the whole secret is to have time to think, while I'm sitting there bending — sooner or later, they're going to come to me. How do you do it? How to do it? How to do it? I know how to find the center of a circle on a compass. That's easy. But how to do it better? So I glance over and I see a lathe wheel, a pulley wheel, standing on a shelf, and I get a great idea. See, so I act like the old machinist from way back. Finally, they come to me, and they say, "How do you find the center of a circle?" I say, "Where's your circle?" So he shows me this piece of paper. "Is that your circle? What do you want the center of that circle for? That's just an old piece of paper." I say, "That's what you want to find the center of." You know? Practical man, you know. So I say, "How accurate do you need it?" You know, wouldn't move — I say, "How accurate do you need it? Is a sixteenth of an inch off all right?" "Oh, yeah, that would be all right." I say, "Ok." So I put the thing down on the table. I go over and take the wheel, which is nearly the same size. I place it over the disk that they want to find the center of, and you can see with the eye, it's about half an inch around, and you can make the half an inch the same all the way around, within a sixteenth, very easy. I drop a pencil through the center of the shaft, you know, that fits, and it pushes and opens the screening at one point. I say, "That's the center of your circle." They kind of crawled away — you know, the old machinist, the great machinist, told them how to do it, instead of this theoretical baloney with a compass! I loved this kind of nonsense, and I enjoyed the game. Play, yeah.

Weiner:

Yeah, play is how you can describe it.

Feynman:

That play. That was my play, to make believe I was a great machinist.

Weiner:

Let me just for a minute diverge. You mentioned the rhythm of the chalk, and that you liked to play drums. Will you pardon me for asking, when did you start doing that?

Feynman:

I can't figure it out. I've often thought about, when did it start, but I don't know. I didn't really play drums. I played bang, bang against the wall. I played toy drum. I made rhythms, like hitting something against something else. I didn't really play. I never played trap drums or any real drums with music. I was not that kind of drummer.

Weiner:

Were you interested in music?

Feynman:

No.

Weiner:

Did you listen for recreation or relaxation?

Feynman:

No. I never did and I still don't.

Weiner:

Was there music in your house in Far Rockaway?

Feynman:

No. No. Music is one thing that didn't —

Weiner:

And still doesn't?

Feynman:

That's right.

Weiner:

The sense of rhythm for its own sake.

Feynman:

I liked rhythm, yes. I do listen to drums. I mean, I could listen to good drum music from Africa, or something like that, but not music, the usual music.

Weiner:

I'd like to ask another question on the physics courses. From what I gathered, the courses could be termed theoretical or mathematical physics. There was one you

mentioned, the course with Slater, and the other was the informal course with Philip Morse. Were there any others?

Feynman:

We took courses, yeah, but I don't remember them. That's right. Later on, there was a course offered for graduate students in nuclear physics, theoretical nuclear physics, or something like that, because Bethe and Bacher had written, in the REVIEW OF MODERN PHYSICS, a good summary of the situation in nuclear physics, and so it was thought that there ought to be a course at MIT in that. So for a special graduate course, this course was to be given by Frank and Morse ultimately. There were going to study this work and describe it.

Weiner:

Which Frank?

Feynman:

N. H. Frank. It was a high graduate course. It was considered the most esoteric in the whole — you know, way up there, nuclear physics, big deal. Welpin and I thought we would like to take it, but we were afraid we might not be allowed to because it was so hard. We must have been juniors, or possibly seniors, I don't know which — probably seniors, I guess. So I remember that I went to the course the first day, when the registration was beginning, and there the whole room was full of graduate students, because there was a great desire for this. The whole room was full of graduate students, and Morse and Frank were up there in the front. And I walk in, and he looks at me — Morse does — he says, "Are you going to take this course?" And I felt terrible, you know. I said — "I hope —" "You going to register for this?" "Well... I hoped to..." "And how about your friend Welpin?" I said, "Yeah." He said, "Good. Now we have three guys registered, we can give the course." One graduate student had had the nerve to register for it, and then we had two. They were afraid they would flunk it. It was supposed to be — you know, it was hard to learn — but they all wanted to listen.

Weiner:

They were all auditing it?

Feynman:

They were all auditing the darned thing. And he was happy that we were going to register. I was afraid, when he said that, that he was going to say, "Look — this is way out of bounds," you know. So anyway, Welpin and I, the two undergraduates, saved the graduate students, so that Morse and Frank could give the course in nuclear physics. I

don't remember the course. I don't think I learned much from it. I think it was too high for me, because I don't remember it.

Weiner:

When you introduced this idea of the course, you mentioned the REVIEWS OF MODERN PHYSICS, the article. What was the link? That sort of set the stage for teaching this? How did the article itself relate to the teaching of the course?

Feynman:

Well, that was the — they used this article, you see, to teach the course from. You see, nuclear physics knowledge had not been really organized anywhere. There weren't any good textbooks in nuclear physics. They were rather poor. They were textbooks that would tell methods of arranging a Geiger counter, but there wasn't a textbook which told what people were thinking, how to analyze it, and so forth. The articles that they had written were not really review articles. The great men, Mr. Bethe, Mr. Bacher, had worked out a lot of new things in those articles, you see, which organized the knowledge of the deuteron and the nuclear forces. It was a very new organization of the knowledge of nuclear physics, at the time. So it was a very important contribution that these two men had made, and it served. It would serve us as a very good—good to learn, because it was a good place to begin your understanding of nuclear physics. It wasn't really a rehash of the old stuff that was known. It was a new reorganization of the knowledge, plus a lot of additional calculations and so on.

Weiner:

Had you read this article?

Feynman:

No.

Weiner:

In the course of the course, you read it.

Feynman:

Yes. I can't remember the course. I presume we didn't get much out of it. It probably was too difficult for me. I don't remember it very well. I don't remember anything very well.

Weiner:

Did the auditors stay with it?

Feynman:

I can't remember any more what happened. I just remember that much of it.

Weiner:

Any others of this type?

Feynman:

Well... no, I don't remember. We took courses in X-rays and other things. I don't remember whether it was senior or partially graduate. We were taking more or less advanced things. It was a good opportunity. MIT was a good opportunity. Oh, there were also courses that were supposed to be humanities, to develop your — you know — with which I had very little patience. Those courses in English, which we had to learn, in case we ever had to write a patent was the excuse given. There were other students in the fraternity house, for example, good friends of mine, who liked it — who liked to study French literature, who liked the English course, who were interested in writing patents. But I didn't. It was to me a pain in the neck, it was a bother, and it was a thing I didn't like to do. In fact, I used to cheat a little bit in the English class. We had exams every day to see if we had read the book of the day before, you know. There was always a quiz at the beginning of the day for ten minutes, so you'd look over at the guy next to you, kind of, because you didn't feel like reading it. I lost my moral sense for a while in the English course. It was all forced, and the testing of whether you'd read it was forced, and the whole thing was kind of, to me, a bit illegitimate. I didn't like it. And I always fought; always felt I had to fight the humanities, in order to keep working on the things I really wanted to work on. They were just a pain in the neck. That was the attitude. I don't know if I have the attitude now, but I'm just telling this, so you'll see the picture of the guy.

Weiner:

You felt they were intruding?

Feynman:

Yes. I didn't see why I had to worry about that now. After all, how do you spell something? Suppose I make a mistake? (This was my attitude — it was the attitude at the time.) You make a mistake in spelling. What does it mean? It means that the damn language is irrational. It's just a stupid method of spelling. Some guy ought to make some

progress. If those English professors would sit around and figure out how to straighten out the spelling, instead of teaching this idiocy all the time — they had no feeling of progress, no feeling of development, like the sciences or anything. They don't try to do anything about anything, they just sit there and write commentary about what the other guys are — You know, this kind of attitude. And my mistakes in English spelling, if they're taken to be a serious lack of intellectual achievement by somebody, it's his mistake, because English spelling is a ridiculous and unnecessary thing to learn. You see what I mean? That was the attitude. And then the philosophy — I had looked at books in philosophy. For a while in high school, I tried to be a philosopher, sort of. I got the idea that I had sort of grown, that I got from biology which I was first interested in, into chemistry, to physics, to mathematics, and then to philosophy. A hierarchy of intellectual climbing. So I did a little philosophy, trying to prove that God didn't exist by logic, or some such thing. You know?

Weiner:

Did you read anything?

Feynman:

No. I used to look a little bit at Reade. But when I read, I realize how stupid things are. Like my girlfriend Arlene, for example, was trying to read Descartes, and I started, I looked at Descartes' first few paragraphs, in which, starting from only the fact that he is, he proves that God is perfect. Now, I know enough about logic to know that there are some things that don't come out of some things. That is, you can usually guess from the axioms what kind of thing was going to come out, you know? And it isn't possible to get that out. Therefore there's something wrong with the reasoning. So I look at it, and it's obvious where the errors in the reasoning are, and I pointed them out to the girl. But I rapidly learned that philosophy, as far as I was concerned, the philosophers who were respected were really quite poor and rather stupid people — at least, from the modern point of view. It seems to me that there were trivial errors in logic which were obvious. Very poor, it seemed to me. Therefore, when we had in the English course to learn about the philosophical development of ideas in the modern society — you know, the philosophers, what they said — it was called, something about the mind.

Weiner:

THE MAKING OF THE MODERN MIND?

Feynman:

Yes, something like that.

Weiner:

By Randall?

Feynman:

Right. We used it as a textbook. And there was so much stuff in there, so much nonsense, while the modern mind was being made! Much better I should use my modern mind. You understand what I mean? Yeah, it was a pain in the neck. Because I had to remember that so and so said this dumb thing, and that this guy said that dumb thing — it seemed to me. That's the way it seemed to me. Once in a while, somebody would say something smart. Big deal, about Bacon — you know? — big deal, about how he really understood how we should do experiments. So I look up old Gilbert, who did experiments in those days, and what he said about Bacon. Then I felt the same way. He said this guy was a good philosopher, a good scientist for a — something, for a prime minister. "He writes science like a prime minister." Then I realized that historically, while he's telling what people ought to do, Mr. Gilbert was doing something so real — while this guy's telling what we ought to do. And we get the idea in the history that the science was developing because they followed Bacon's principles of how to do it, you know, and this kind of stuff. So the more I learned, the less I believed anything. Anyhow, I just didn't read, I didn't like it; altogether, I tried to object. Now, you had to take humanities, and later you had options in humanities. There were things like French literature, and so on, but there was also philosophy, astronomy, believe it or not, as one of the humanities, and something like psychology.

Weiner:

Astronomy was descriptive astronomy?

Feynman:

Yeah. So I took the astronomy, naturally, because it was the minimum escape. I also took the philosophy.

Weiner:

Who taught the astronomy?

Feynman:

I don't remember. Stewart — a man named Stewart, I think, although you'd have to check if such a man exists. But the astronomy course was taken from I think a book by Baker — a simple course. It was a simple course in astronomy. It was nice, it was easy — that was good. I learned stuff. It was fine, because it was science. In the philosophy

course — I could tell a little story about the philosophy course?

Weiner:

Sure.

Feynman:

The philosophy professor, whose name I believe was Robinson, who died not long after, was an old man with a beard, and he spoke in a mumbled fashion which I found incomprehensible. I swear, absolutely incomprehensible! I do not exaggerate. Only once in a very great while would a few words ever hang together. And I listened. Every day we'd come and we listened to this guy, and once I heard him go, "bmbmobmbooo, the stream of consciousness, rmbmoob..." That's all. That's about the level. I understood nothing else. It turned out that the other students were gradually understanding him. They were learning how to hear him. But I, of course, having this block against philosophy in the first place, never got to understand him. And I just felt it was a waste of time. We used to sit there through the whole hour, then go out. One day, at the end of the class, he says something. "Brmbmbmrmb," — and then there's a wave of excitement through the students! Something important he said, at last! I mean, nobody had ever batted an eyeball before, but now all of a sudden they seem to comment on the great interest of this thing. So I ask a student, "What did he say at the end of the class that everybody seemed to be so excited about?" "That we have to write a theme, he said, for the final grade." I said, "On what?" "On what he's been talking about all year." This was absolutely true. I'm not exaggerating. I remember what it was. So, I had heard the words, "stream of consciousness." That's a problem that always interested me. What happens when you go to sleep? My father had taught me to think about the world from the point of view of a Martian coming down and asking you questions. Suppose a Martian didn't sleep. He'd be very interested in the question of sleeping. He'd say, "How does it feel?" You go along, your mind is working, your mind is working, all of a sudden, what does it do? What happens, when you go to sleep, to the ideas? Do they go slower and slower and gradually stop? Do they suddenly turn off? What happens? It was interesting. Ok? So I decided, "Well, that has something to do with the stream of consciousness, so maybe I can get away with this, I can't do any better." So I would work on my philosophy by going up to my room in the afternoon, pulling down the shades, and getting into bed, and watching, introspecting, what would happen when I went to sleep. I would do the same thing at night when I'd fall asleep. So I'd fall asleep in the afternoon for a little nap, and at night, and I'd work for quite a — we had a long time to work on this theme — so I worked quite a long time, and I kept introspecting, and noticed a lot of things, because if you practice you can think deeper and deeper into the sleep moment. Anyway, I had a lot of observations, which I included in a theme, ultimately, and at the end of the theme, I remarked about the difficulty, that of course I'm only knowing what I'm thinking, what I'm trying to think — what am I thinking? You know the difficulty of introspection. And I said, "This is exemplified by the well-

known poem, ‘I wonder why, I wonder why, I wonder why I wonder? I wonder why I wonder why I wonder why I wonder.’” — which I made up. So I sent in the theme, handed in the theme. Then, sometime later, he was reading. He brought some of the themes. The professor brought some of the themes to class, and would read one of the themes of somebody. Then he read another one. And near the end of this one, he says, “Mbmbmbmbmbmbm” — so I realized from the rhythm that this was my theme. I swear, I had not recognized it until near the end, that he had been reading my theme. That shows you the level at which I understood this guy. So it turns out I got an A on the theme. Pretty good. But it shows you what you needed to do to pass philosophy. I always kept fighting everything. I remember, we were supposed to read — Oh, in one darned course in humanities we had Goethe’s FAUST. I don’t know how the heck I ever got into that darned thing. And the end, after reading Goethe’s FAUST, we had to write a theme about it. Well, I had read it; I didn’t make head or tail of it. I mean, I’m dumb, I admit about these things — I couldn’t make head or tail. So we had to write this theme. I said to my fraternity brothers, “Listen — I can’t do it! I can’t do it! There’s a limit, I just can’t do it, I don’t know what I’m going to do.” So I said, “I’m simply going to hand in a paper saying I refuse to do this.” So the fellows said to me, “We have a better idea. Why don’t you write a theme on something else, a long one, the same number of words, that’s completely irrelevant, and then put it a note that says you felt you could not make enough sense out of the FAUST, cause you’re not capable, but it’s not the work you want to avoid, because you’ve done the work on something else.” That’s what their suggestion was. I said, “All right, I’m willing to do that” — because they were worried that the guy would think I didn’t want to do anything. So I sat down and I wrote a theme on the limitations of reason, that there are certain problems that cannot be solved by thinking about them alone — you know, aesthetic meanings and value judgments and various things. I don’t remember about the theme now, but a lot of discussion of the limitations of the method of science and so on. I wrote this theme. The boys looked at it and they said, “Listen, we’ve been thinking it over. I ain’t gonna look too good. It isn’t gonna work. Why don’t you — as long as you got all this written — say a few words about how it’s got something to do with FAUST?” This is true. So then I sat down, after I wrote the whole theme, and I wrote a page and a half or two pages more. I said, “This problem of the limitations of reason is well illustrated in FAUST. Faust represents Reason” — or Mephistopheles does, or something, and Faust love of life, I don’t remember what, but I had enough vague knowledge of the thing to be able to make something up. But the original theme was written completely independent of this idea. Then I put this crap at the end. The trouble was that with this teacher, you had to go into his office to discuss your theme. So he went into the office, after he had read it, and then I looked at it, and he’d written at the top, “The introductory material is excellent, but the connection with FAUST is not too satisfactory. I would advise a better proportion between the FAUST material and the introduction.” And I got a good grade, like B plus or something. Yeah, he saw that it was put together. Anyhow, I got away with it. But I always would struggle like this. We had to write on — to make some commentary on any one of a series of themes, and one of them was Huxley’s “On a Piece of Chalk.” You had to write a book review, an analytic analysis of the work. I

couldn't do that. Instead of that I wrote what might be called a kind of parody — not exactly, but an imitation — on a piece of dust, in which I told all the things that dust did. You see, he told about all these chalk cliffs and so on. It was the nearest scientific thing I would always take. See, there were a lot of books that were not that — I'd take the nearest thing. Then I'd talk about all the things that dust did, the center of condensation for rain, the effects of colors of sunsets, and so on, the burying of cities, and all these things.

Weiner:

Did you save that one?

Feynman:

I don't think I have any of these. But I was always escaping. I'm trying to emphasize that my humanities effort was always somehow to figure out how I could, by using science, escape the humanities. I fought it to the bitter end.

Weiner:

How about extracurricular activities at MIT? Anything, any clubs or student activities, other than the fraternity social functions?

Feynman:

Practically none. I did try to play a little squash for a while, with my two friends from the fraternity.

Weiner:

Not on the team?

Feynman:

No, I never got to a team. No.

Weiner:

School papers, or —?

Feynman:

No.

Weiner:

Clubs or anything?

Feynman:

No. No.

Weiner:

Were there any science clubs?

Feynman:

I don't know. I don't think so, no. No, I don't remember anything like that. The extracurricular activities there were girlfriends and going for walks in the city and stuff like that, learning to drive a car and so on. There were rather intensive discussions of physics all the time, in your spare time, with friends, Welpin and so on.

Weiner:

You mentioned in previous conversation a paper that you published during MIT. I think there were two. There was one in 1939 —

Feynman:

Yes.

Weiner:

There was one I think you said that was earlier, that hasn't shown up on your bibliography.

Feynman:

Yes. Mr. Vallarta was there. He was interested in cosmic rays.

Weiner:

How do you spell his name?

Feynman:

Vallarta. And we had become friends. I had been in a class of his. And he said to me — he told me about a problem that he had that had to do with the question. It was not correct, he did not analyze it, but of course, cosmic rays came from inside, or outside, of our galaxy, and he imagined that if they came from outside of our galaxy, they would be scattered by the material, the stars of the galaxy, by their magnetic field, so in galaxies like the Milky Way, there would be, say, more or less cosmic rays, than in the direction perpendicular, where there's not so much scattering. So he asked me if I could figure out which way it would go, and what the effect was. I was rather amazed and happy. I don't know where the thing began, but I got interested in the scattering of light by clouds, and things like that. I don't know which one came first, whether I had found the problem and thought to use it on cosmic rays, whether I told him about it, or which way it was. Maybe it was the other way. Anyway, I was working on this scattering of light by clouds and fog and so on. I discovered that, surprisingly, it was a very difficult problem. One of the things one learns in school which is incorrect is that problems are relatively easy, if they're formed, you can set 'em up, you can solve them — which isn't at all true. And there are a vast number of simple problems to state that are very difficult to analyze, and one of them is the way that light coming in and scattered by a certain angular function from each droplet finally works its way through the cloud layer. Incidentally, I did discover by fiddling around that there was a delta function, which is the original direction of the light, which comes at the other end of the cloud — there's still some delta function left. And this I didn't believe, because I was sure that fog smeared the image. But I remember going out, when it was a foggy day in Boston and looking at a building far away, which was a very light contrast but a very sharp edge. And I realized that my mathematics was right, that the sharpness of the image is still there, although it's lost against the contrast. It isn't that fog smears, it's only that it decreases contrast. You know, it doesn't smear the image — which is obvious, but I didn't notice. I noticed these equations and thought, "That's wrong." Then I looked at the phenomenon and found that that was right. But anyhow, I was working on this scattering, and I discovered, proved, that the net effect is zero. It's a famous theorem which other people had proven. I didn't know that. If it's uniform for the outside, just as much is scattered away from a certain direction as is scattered in from some other direction. And I proved it. So in the end I didn't need the formulas from the scattering of the cloud, but could demonstrate that this was the case. Vallarta thought that this was very interesting, and that that was the solution to his problem. There was no effect of the scattering, of the stars in the galaxy. He thought we ought to write this up, and we would write it together, because he would explain the application of this to the thing, and he would make some remarks about it, that we assume the stars don't absorb the cosmic rays. We wrote it up, essentially, and the proof of this thing is in there. The theorem has been proven by others in other applications, but we didn't know that, so we published it, and no referee found out that it was published before.

Weiner:

This was published as a letter to the PHYSICAL REVIEW.

Feynman:

That's right, a letter.

Weiner:

And I think the name of it was "The Scattering of Cosmic Rays from the Stars of the Galaxy."

Feynman:

You've got it.

Weiner:

March 1st, 1939.

Feynman:

And then Vallarta — when we were writing up, Vallarta says, "Listen, I'm going to put my name first." He was joking. He's a very pleasant man. I don't mean anything. "The way we arrange it, see is I put my name first, because I am the senior scientist." He was humorous partly, you know? It was no problem. So he put his name first and then my name came next — "Vallarta and Feynman" — you see? Then later, just after the war, Heisenberg wrote a book on cosmic rays, and this paper — he wrote on every paper, a kind of summary — and this paper had no place anywhere. Nobody referred to it. He didn't know where to put it. So it was mentioned at the end of the book, and it says, "The effect of the stars of the galaxy in scattering cosmic rays has been analyzed by Vallarta and Feynman." Therefore I was the last word in the cosmic ray book by Heisenberg. And when I saw Vallarta sometime after, I said, "Did you see the book by Heisenberg?" He says, "Yes. You're the last word in cosmic rays." He had noticed it too. He says, "That's what I get for being the senior scientist." He even remembered this little joke he had pulled.

Weiner:

So your citation was the last word.

Feynman:

Yeah.

Weiner:

We're now on the first side of the second reel of tape, and we've just finished talking about Richard Feynman's published paper, a letter to the PHYSICAL REVIEW, March 1, 1939, dealing with scattering of cosmic rays. There was another paper in that period. Would you like to discuss that now?

Feynman:

Yeah. We had to make a senior thesis at MIT. You had to write some kind of a research thesis. Incidentally, that reminds me of something else. I'll come back to the paper. During that time, there was discovered by two men at MIT (that I knew in the optics lab) a method of coating glass with film so that it was not reflecting. They discovered it. Independently somebody in General Electric, Katharine Blodgett or something — And one of the kids, for a thesis was given the job of developing the formulas for the reflection from such layers. He asked me for advice, and so I worked out the whole theory of the reflecting from layers — incidentally — that's not published, but that was also worked out at that time.

Weiner:

Was it published as part of his work?

Feynman:

No. For my thesis problem — Oh and somebody else had another problem. I would help the guys. Anyway, my problem — I went to Slater, and he gave me a problem, which was — (he was in solid state, he was interested) — why does quartz have such a small coefficient of expansion? He thought that maybe the possibility was that the quartz crystal has moveable — see it's silicone dioxide, SiO_2 so I think there are oxygen's clinging to silicones, and in the motion the oxygen can swing back and forth, and it's a bent angle, turning back and forth, like the bores on the governor of an old steam engine, and when it turns — when this is oscillating, it's the same idea — it pulls the heads of the steam engine together, the ends, because the bore goes out by centrifugal force. And so the bent bottom will be shortened — I mean, it will be pulled together by the motion — and this will compensate the ordinary effects which tend to make something expand, so that the expansion will be much less than usual. Can I work out any details or estimates or something to show that in fact that's the reason that quartz doesn't expand? All right, that was the problem. I was very interested in it. The first thing I did was, I looked up the forms, crystobalite A, crystobalite B, crystal forms, and so on, to get the idea of the bonds and the angles and so on. I got in the crystal business. Then I realized I'd have to figure out how a change in forces will change the dimensions of the crystal. So then I got involved — I'm just telling you how I did the problem, because it's quite a long business — then I got involved with the connection between the

forces between the atoms, and the forces — all together. For example, if a crystal is compressed, what is the compressent strength? Supposing I assume certain spring constants between all the atoms and I want to know what the elastic constants of the whole crystal are. I realized that what I had to do there was an infinite bridge truss problem, like the guys in applied engineering with bridges with a lot of members. I had an infinite number of members. But, because of the periodicity, I had an advantage that I could work out. Then I gradually developed the theory of the connection between the elastic bonds and the elastic — a theory which is developed by Blackman and other people — but those things never bothered me. I never looked at anything. I always tried to do it myself, because I'd learn something, maybe get a different idea. I never looked up. So I worked that out, and then discovered by fooling around that I could get it for a principle of energy minimum, that there was a much better way of formulating. I was learning a lot. And also the idea of stress tensors in crystal and so on. I had developed the idea of stress tensors and so on. I had to learn about stress tensors. And then as I kept working with this — see, I was getting away from the original problem, I was moving away from the problem, not purposely but this is how my mind led me — I began to think about the stress tensor, about a plane that goes through the middle of the crystal somewhere. See, how do you define the stress tensor? It's all right when you put springs on and a certain number of forces across a plane, but they're not springs, they're electrons, according to quantum mechanics. Question: what do I calculate to carry the stress across the face? Then I got very interested in the whole problem of stresses and molecules, just as well as in crystals. For example, in hydrogen di-H₂O, water, the two hydrogen atoms are a little bit more than 90 degrees, and the question is, is that because the hydrogen's push apart, and how much is the sheer strain and stress in the bonds? You work it out. In other words, can you cut a plane through there and calculate the shear across the plane through the hydrogen and oxygen and so on. So I tried to develop a stress tensor for the field that would be right for any point in space in quantum mechanics, and I developed the formula for the stress tensor. But in order to stress the formula for stress tensor, I first had to find the formula for the force. You see, the divergence of the stress was equal to the force. And I noticed, I found that this force was nothing but the electric field exerted by the other particles; since that electric field, the forces from that, could be expressed as Maxwell's tensor, I would combine Maxwell's tensor for the electric field with the stress tensor of kinetic energy of the Schrödinger equation, and I did find the stress, a formula for the stress. It turns out Pauli had previously found a formula for the stress, but as a matter of fact he only found part of it. He did not have a stress tensor which worked at any point in space. I noticed that later. But anyway, in the meantime I'd found this theorem about the forces, but the force on the nucleus is nothing but the electrostatic attraction of all the electrons, the distribution of the electrons being determined by the Schrodinger equation. Slater found this interesting and unusual, and hadn't known that. He challenged me, and said, "How do you explain van der Waals' forces that way?" So I went back and I proved that the van der Waals' force could also be understood that way. Then he said it was worthwhile, I ought to publish it. So I wrote it up. And he said, "Let Conyers Herring look at it." He was good at these things. Herring looked at it, and there was a long session. He said,

“Take this out.” I had the proof that the integral of F times F times HG, where H is the Hamiltonian, was the same as HF star times G, integrated. And I proved it with the Schrödinger equation.

Weiner:

Where was Herring at the time?

Feynman:

At MIT. Herring says to me, “Take that out, and write instead such and such equals such and such because H is Hermitian self-adjoining.” I said, “What does that mean?” He said, “That’s what it means. It means that equation is true.” I’m just saying this to show the level of what I knew. See, I proved everything by hand — I didn’t go into the general. So he took that out, and so on, and then I proved this theorem, and it was in this paper.

Weiner:

This paper was in —

Feynman:

Which Slater thought ought to be published, and Herring also thought ought to be published.

Weiner:

You submitted that directly to PHYSICAL REVIEW?

Feynman:

Right.

Weiner:

This was the one called “Forces and Molecules,” published August 15, 1939.

Feynman:

Yes. But my thesis at MIT contained also stresses and molecules, which I’ve always felt was a real contribution also. More important, even. I mean, it was an editorial contribution, which isn’t published anywhere. But they thought only the force part was

worthwhile — which was only a part that I needed for the other thesis.

Weiner:

Now, if I remember what you said, Slater proposed this problem as your senior thesis.

Feynman:

This problem about the temperature effect, yeah.

Weiner:

And out of that came this —

Feynman:

Right — which was acceptable as a senior thesis. Of course, you could wander around and get another problem, but I got my senior thesis on stresses in the space in molecules. For that thesis I needed a theorem, and this is the theorem which is a lemma, so to speak, in the other thing, see?

Weiner:

I see. In the senior thesis at MIT, in that period, was it a question of choosing a professor, or were you assigned one?

Feynman:

No, I think you kind of went around and asked guys what kind of problems they could suggest. I don't know how they did it, because I don't see how we could do it with our seniors. It must have been a tremendous effort — to find problems, at that level, that a senior student could have any chance of doing — it seems right remarkable. I would be terrified if I had that problem, as a professor today. At any rate, I would like to make some comments about the paper, which is really after the publication. Within a short time after the publication, I knew more, and I realized that that's a very trivial thing, that theorem — that that's nothing but the first order perturbation theory. It's an obvious statement from the first order perturbation theory of Schrodinger, and it's not really very much. It's an obvious conclusion, and an interesting one. I realized that nobody referred to it. It was rather a useless theorem that nobody referred to. And I realized that it was not only useless but it was obvious. I could have written it in half a line. It was nothing. Therefore I was quite surprised when, in 1948 or 1949, I went to Michigan — (I mean, a change in the timing, just to tell you, for my own amusement, because I always felt a little bit ashamed of it, on the grounds that it was too trivial and useless, not very

interesting.) At Michigan I was giving some lectures on electrodynamics and Ted Berlin said to me, "What do you think about the controversy about your paper?" I said, "What paper?" He was talking about "Forces and Molecules," and he told me that there was a big debate as to whether the theorem is true — it's called the Feynman-Hellman theorem or something like that, because somebody else has published a similar thing — and there's big excitement about it, they're arguing whether it's true, and somebody in Germany said it's not true because I only included the electrostatic forces, there are also the kinetic forces, and so on — I said, "No, that's not right." He said, "I know. We've been arguing back and forth. They claim you made two mistakes that cancel, and you were just lucky, and we claim that it's not that you were lucky but you knew what you were doing." I said, "Well, I knew what I was doing. It's correct." You know? Apparently it had caused interest, and it is interesting to physical chemists.

Weiner:

And you were unaware of that?

Feynman:

I was completely unaware of all this discussion.

Weiner:

You hadn't seen any reference in the literature?

Feynman:

None.

Weiner:

You didn't know that your paper was being discussed.

Feynman:

Right — up until 1949. I still have never seen any references, only — I've seen one or two references. Somebody's written me a letter saying something about the paper, or something.

Weiner:

Berlin told you about this.

Feynman:

Yeah.

Weiner:

Was he at Michigan in attendance before he went to Hopkins?

Feynman:

I think so. I think it was Ted Berlin. Anyway, somebody, in 1949, but that was the first time I found out — which amused me a lot, of course. I just tell you that because I had always felt a bit ashamed, and it was unnecessary, and it was funny to find people arguing about something like that because it was so obviously true and so trivial.

Weiner:

Now, this business of assigning a senior thesis was routine. But you were assigned other problems, extracurricular ones.

Feynman:

I wasn't assigned.

Weiner:

Well, I mean, you just sort of got together with the professors on these things. Was it a small enough group for them to know you?

Feynman:

Oh yeah, they all knew me, yeah.

Weiner:

They knew you because of your work and your interests.

Feynman:

Yes. Yes. Then, there's another thing that I can't — I was just thinking, I'm not absolutely sure whether this was done at the time, when I was a student at MIT, or later. When I was at Princeton for my Ph.D., to study for my exam, I went to MIT for a few months, to read and study, to be alone and nobody would interrupt me. Therefore, if I remember something happening at MIT, it is not absolutely necessarily. So, I remember

this happening at MIT. It's rather interesting, because I developed; I got interested again in mathematics, a mathematical theme. I had noticed or had read somewhere that for linear differential equations you write D for a symbol to represent derivative, and then D squared to represent the second derivative, D cubed the third derivative — but what is D to the two and a half? I would think to myself. Or 6.71? You know? I got interested in such things. Or, just take, what is the square root of 1 plus D squared? What is E to the AD? Well, I found out what E to the AD was, on $f(x)$ it's $f(x)$ plus A, but some suggestions and hints from the ENCYCLOPEDIA BRITANNICA article on calculus of finite differences.

Weiner:

This is the same 13th edition you'd used in high school?

Feynman:

It's the same — no, I knew enough from high school — no, I had it at home, you see, when I'd go home for vacations I'd read the darned thing. I would be — more — I mean it has a lot of stuff in it. Couldn't understand anything when I was in high school. So I began to develop the question of a function of D operating on another function of X, and I developed quite a lot of theorems, and lots of amusing and interesting things, and did solve the problem of the off-derivative, where R is not an integer, what it is. I believe I did this when I was at MIT, because I remember finally going to Vallarta and telling him about it, and him saying that this was just the operator calculus of [inaudible] something. It isn't exactly, but it's extremely close to it. But I didn't know about the operator calculus of — I didn't bother. Those things never bothered me, because after all, that was only invented in 1907, so I was going along good, you know? But I did invent that by myself. I did lots of things. I worked a lot, but it was pleasure, you know. I liked to play these games.

Weiner:

What happened with this? It was just a personal thing; there was no application of it in particular?

Feynman:

No. No. I applied it at other times later, my knowledge of that.

Weiner:

How did you preserve it, in a notebook?

Feynman:

Yeah. I've got that notebook somewhere.

Weiner:

And then you referred back to it?

Feynman:

No, from memory. I mean, it's easy to understand what you're — If you understand something, you can remember it, when you work it out yourself.

Weiner:

Some time in your senior year, early in your senior year, you had to think of graduate school, and when was it that you knew you'd be going to graduate school? Then what were the steps leading up to the choice of the school and final application?

Feynman:

Ok. There's one little complication in that. There was a mathematics exam for college, not an exam but a contest — it was like an exam, whoever got the best thing won something. I don't know whether this was Pi Mu Epsilon? No, some other exam. Some big mathematics competition. And the math department didn't have enough good men. They had to have three men on a team, or five men on a team, or something. And so they called me and said, "You used to be in mathematics, would you enter the contest?" I said, "Look, I'm not in mathematics. Mathematics students are learning a lot of stuff that I don't know and they're in the other colleges, and I don't think I can do it." They said, "Well, look, we need the guy, so why don't you do it, just to —?" I said, "Ok." Really, it wasn't false modesty. It was a big surprise to me, what happened. Because then they gave you some old exams. They were happy that somebody, that they had enough guys, you know, so they gave me some old exams, and I went through them to see the kind of problems that they had. And they were quite difficult in certain ways, in other ways not, and I had to review some subjects, like analytic geometry, which I had forgotten, partly, to do certain problems. I worked a little bit at it, but I didn't take it very seriously. I was sort of sitting in, you know? Then the exam thing came around, and one of the prizes was that one of the winners, one of the five, would be chosen for a scholarship to Harvard for the graduate school — I believe, if I remember right. The method, as I understood it, was they didn't want to take the winner, because they didn't want to be stuck with a nut of some sort, by accident, right? So they made it that out of the first five they could choose, Ok? Anyway, I was among the first five. I have since found out from somebody from Canada, where it was scored, who was in the scoring division—he came to me much later and he told me that it was astonishing. He said that

at this examination, “Not only were you one of the five, but the gap between you and the other four was sensational.” He told me that. I didn’t know that. That may not be correct, but that’s what I heard.

Weiner:

First five nationally, or among certain states or schools?

Feynman:

The whole nation. Yeah. It was quite a thing. I was surprised.

Weiner:

There is a national mathematics competition that still exists.

Feynman:

I think it still exists.

Weiner:

At Cal Tech, we did it for Case, he’s here in some kind of —

Feynman:

Well, I don’t know, anyway — I think you have to look up to see if that was true, that there was this gap, but that’s what somebody told me later. Anyway, I was one of the five. This has to do with the question of the graduate school. It must have been in the fall of the senior year, I presume, because I did get an informal feeler from Harvard, would I be interested in going? To which I said “No” because I’d already decided to go to Princeton by that time. That’s funny. It sounds incredible. Why should I be so sure that I didn’t want to go to Harvard? I don’t understand that now. It would be interesting to look into it. It sounds incredible. Why would I make a decision to pay to go to —? Oh, I know. Because at Princeton I didn’t have a scholarship but I had a job as research assistant to Professor Wigner.

Weiner:

You had a job!

Feynman:

You know what I mean? He gave me — they needed research — I would go to the graduate school, and I would get paid a little bit, for being research assistant for Professor Wigner. That's great stuff. I mean, that's a great opportunity. And I'm guessing now that that was the good thing that helped me decide not to take a free scholarship to Harvard, but to be able to be an assistant to Professor Wigner was something. I'm guessing. I think that's the reason.

Weiner:

When did the assistantship start?

Feynman:

I don't know, but this is what I remember. I do remember one other thing. I remember three opportunities. One was Harvard, which I threw away, directly, when the feeler came. It had something to do with this exam. Maybe it wasn't Harvard. I think it was Harvard. The other was this — the other thing — I mean, I can't remember the order of these things — but originally, when the time came to worry about graduate school, I went to Professor Slater who said, you know, "What are you going to do about graduate school?" I said, "I'm going to MIT." He said, "No, I think it's better for a student to go to some other school, to find out how the rest of the world looks, instead of only to one school." "Yeah," I said, "but MIT is the greatest school in science and in engineering in the whole country, and I want to go to this great school." He says, "You think this is the greatest school in engineering and science in the country?" I said, "Yeah." He said, "Then you must go to some other school." He didn't mean that it wasn't, he meant that it wasn't that far out of proportion, which is true. I mean, it's good to get a different point of view. So he forced me not to come to MIT. I would have gone to MIT if I'd been allowed to, but he said he wouldn't allow it, he wasn't going to let me go to MIT for my graduate work. I must go somewhere else. And I got this opportunity from Princeton, and I went to Princeton.

Weiner:

Do you remember if you sought the opportunity? Made application?

Feynman:

I can't remember. I don't remember how I picked the schools or anything.

Weiner:

Do you know who was there at the time?

Feynman:

I knew that Wigner was there. I ended up, when I got there; I was the research assistant to Professor Wheeler, instead.

Weiner:

How'd that happen?

Feynman:

I don't know, that was up in the administrative system of Princeton.

Weiner:

Had you thought of a specific field in physics?

Feynman:

Well, I was going to be in theoretical physics. By this time it was clear. Something in theoretical physics. But I never thought of theoretical physics as being a field that's splitable, in those days — just anything in theory. Because, you see, my mathematical talents had finally overcome my experimental talents. I used to play around, but I played around less and less and did more and more analysis, mathematical, like these theorems and these papers, so I was becoming theoretical instead of experimental.

Weiner:

And these papers had been published, so they had a chance to judge you.

Feynman:

Yes. They must have gotten some letters from MIT. I don't know how they did the choosing. I don't know how I picked the school, either. Probably Slater said, "A very good school is Princeton." I don't know. Then I went and said Ok, or something. I just remember not considering the Harvard feeler. It was some informal suggestion, because they were trying to — you know — they didn't want to make a big deal by saying "It's you" and then you'd refuse and they'd have to pick somebody else. They don't like to do that. It was a nice — I think — anyway — I guess. That's all I remember about the choosing of the next school.

Weiner:

Did you start immediately or take some time off in between?

Feynman:

I had the summer off.

Weiner:

You graduated in 1939, June, and then started Princeton that fall.

Feynman:

Yeah.

Weiner:

What happened with graduation, anything special? You'd completed everything; you got your degree, Bachelor of Science in Physics —

Feynman:

Right. The graduation. My parents came to the graduation. My girlfriend, Arlene, came to the graduation. We were probably engaged. I think we were engaged by that time.

Weiner:

Yes, you'd indicated earlier.

Feynman:

Yeah. I drove. It was the first time. I had just learned to drive, and I'd never driven so far. I got sick on the way, from tensions. So when I arrived for the graduation, I was ill to my stomach, and went to the hospital. At MIT they put me up in the solarium and fed me good food. It only took a day or so to calm down. Then I was Ok. It was the strain of driving such a long distance. I never did do it before. I was Ok by the time the graduation came around. That's all I remember of it. I remember my sweet girl. So I graduated. And I had to wear an academic outfit in order to graduate. And also I remember that they teased me, that Princeton didn't know what they were going to get — that Princeton was an elegant place, and I was just, you know, a rough guy, and so on. Not really worry about it, but I did take it seriously, that there was a matter of — you know, Princeton has a certain elegance. And I was not an elegant person. I mean.

Weiner:

Elegant, you mean in certain aristocratic sort of position?

Feynman:

Yes, exactly. I had no such feelings. I was not good in social things. By this time, I'd taken out a lot of girls, danced a lot and so on, and was easy about it. I could go to someone's home and kind of invite myself to dinner, and stay around, and I was informally Ok, socially. But in any formal social situation, I was really quite a clunk. I just didn't know, how to — just talking to people, I was Ok, but I was kind of a rough, kind of a simple character, as far as society goes. But I wasn't worried about it. I was just sort of half-proud of it, you know, that kind funny feeling, but knowing that I had to be a little bit careful so I wouldn't hurt people over there — you know, do something real dumb when I got to Princeton.

Weiner:

Did you win any awards, medals, citations on graduation at MIT?

Feynman:

I don't know. I don't remember.

Weiner:

It was different, then, from high school.

Feynman:

Yeah. I don't remember.

Weiner:

— different standards, I mean different types of awards. They don't have every little —

Feynman:

— I don't remember any particular awards.

Weiner:

What did you do that summer?

Feynman:

That was, I think, the summer that I worked with my friend, Bernie Walker, in the Metaphase Corporation, if I'm not mistaken. That summer — it was interesting, because of the world. Every summer I tried to get myself a job in the Bell Telephone Laboratory. I would go down. They hired a few students for summer. I would go down, and Bill Shockley was there. He had been a teacher at MIT, and he would escort me around the place. And he always tried to help me along, and showed me the laboratories. It was always pleasant to visit. Then we'd go to a restaurant and eat with him and other guys. But he never succeeded. He didn't succeed, until some year, 1940 or something, when I was by that time in Princeton, when I finally did get a job, but we'll come to that. That was one thing. I tried to get a job with Bell Telephone and never got it. Then, during this particular summer between schools, or perhaps the summer before, I can't be sure; I wanted to get a job during the summer. And now I was pretty good in physics already. So I got letters, from Morse and from somebody, saying that I was good in physics and so on. But they had no contacts. They knew nobody to send me to. Somebody in electrical testing laboratory, testing electrical meters, and somebody possibly in Bausch and Lomb or some other place, optical company, for designing lenses — that was the level of physics in those days. It's really interesting. But I went to these two places and they didn't have an opportunity. I didn't know where else to go. There was nowhere to go. There was no place to use that talent, anywhere. While I was worrying about this, I met my friend Walter on the beach. He had come back from France. He told me he could metal-plate anything. He picked a peach pit out of the sand, and said, "I could metal-plate this." I said, "Yeah, but how do you get the contact?" "Ah," he said, "that's the secret," and so on. Then after a while I got this job with him. He offered me this job.

Weiner:

By this time he had this business?

Feynman:

He had this business, and I said I was looking for a job, so I got this job with him, metal-plating plastics.

Weiner:

Just checking that, that summer — I've got a note on it, I think.

Feynman:

I see — how did you find out about the Metaphase Corporation?

Weiner:

Oh, there's an article in CURRENT BIOGRAPHY. Bits and pieces.

Feynman:

You'd better turn off this thing. You're wasting time, telling about that.

Weiner:

Ok. I think if you're satisfied, that takes us to Princeton — which is quite a different period, although, of course, this is continuous development. If you want we can continue on now, or take a break.

Feynman:

We can continue on now. This business about being a sissy disappeared at MIT. When I got there — I'll just mention it, because this was an important thing in my life, even though it's not an important thing in any absolute sense — it bothered me, when I was in high school. When I got to MIT, I was afraid that my reputation would come with me. I realized that it was partly reputation, you understand what I mean? When I wasn't there very long, there was a hazing. There was a fight always between sophomores and freshman, some kind of a mud fight, you know, standard thing on a certain day, and this year the sophomores decided to kidnap the freshmen and put them up in the woods somewhere in a big building, so that they were all tired out when the day came — or some kind of a dumb scheme. So they were quite successful. They would go to one fraternity after another, with a great group of sophomores, much bigger than the number of freshmen in any one place, and gather them and put them in trucks and carry them away. All right? When they came to kidnap the freshmen in our fraternity — they were guys that really knew how to fight and everything — they knew they were beat, so they just gave up. But I, not wanting to have anything happen, you know, I fought like a demon. I went crazy — I moved as fast as I — they couldn't tie me up, they had a terrible time, guys sitting on my arms, sitting on my legs — because really what I was fighting was the possibility that I wasn't a fighter, you know. And I went nuts. So I got the opposite reputation; never worried about it anymore after that. See, they had no way of knowing, because they had no connection with the high school, and when they took us up to the place and chained us to the floor, or whatever it was, I was always picking the lock of the chain, trying to get out, and always fighting. Every time a sophomore would accidentally get near me I'd grab his leg and try to knock him over, and I'd get hit in the head, but it didn't make any difference. The whole point was, I was fearful that anybody might think — so I overdid it to the point that there was no more problem. So that disappeared. It also probably disappeared because of the confidence I got because this girl who was so beautiful and wonderful and everything else found no fault of this kind with me, see. That was also what you worried about — that girls would think you were a sissy. Dumb, but that's life. So anyhow, for various reasons, including my self-confidence because I was in an institution where I was doing so well and my girl was

falling in love with me and I was winning over all competitors with somebody I loved as much as I did, and the general success in life, I became very much happier. Whereas in the past I had had these little worries, I had no more little worries any more. I haven't had any since. I don't have any problems of this kind like I used to have worry me — how do I look, what am I like? So I got out of that when I was at MIT. Probably the usual growing up, although I have reasons, I claim to understand it a little bit in terms of this fight and so on. But anyway I did change at MIT, my personality, my fear of girls; my young and timid frightened and somewhat insecure character disappeared.

Weiner:

What about your father's reaction to this, the bearing fruit of his early —?

Feynman:

I think he was quite happy with all this.

Weiner:

Anything that you could call manifestations? Did you write back and forth to your family while you were there?

Feynman:

Not very much. I wrote mostly to my girl. I guess I did write to my mother all the time. But I would visit very often, for vacations, and I remember once my father saying to me — “Well,” he’d say, “young man, I helped you get started in science and sent you to MIT to learn something, so you should come back to your Old Man and teach him something.” He said, “There’s something I never understood that I want you to explain to me.” I say, “What?” He says, “They talk about an atom in an excited state emits a photon, which is like a particle.” I say, “Yeah.” He says, “Now, the particle is not in the atom ahead of time — huh? And is not in the atom afterwards — one less photon — huh? It just comes out, this particle? Explain that to me, please.” I say, “Father, I cannot!” He said, “I’ve been frustrated. All these years I worked —!” I tried my best. I said, “It’s like sound coming out of a box. The sound is not in there ahead of time, but it comes out —” He said, “Well, it’s the energy of the vibration?” “Yeah, well, it’s the energy that comes out in the form of the photon.” He says, “Yeah, but a photon is a particle, is it not?” I say, “In certain ways, yes, in certain ways —” He says, “Come on, now!” I never could get it straight for him. So he was not satisfied.

Weiner:

This was quite a different level from his earlier general —

Feynman:

Well, he'd read these things.

Weiner:

He was keeping up, on his own?

Feynman:

He'd read these things. He knew the idea, that light was like a particle, that photons existed, levels...

Weiner:

This is more than a normal, healthy interest in science.

Feynman:

No, his was pretty good. He read a lot. He read in the papers. He tried to understand all these things. And the things he couldn't understand, he'd remember, and maybe he hoped that his son, after all this — I remember him definitely putting the question first that way. He says, "All right." He says, "I pay for you to go to MIT, and now I want the answer." He says, "That's what I did it for, to get the answer to this question." I say, "Well, you're not going to get it," you know.

Weiner:

You told me there was a small scholarship.

Feynman:

A small scholarship that got bigger. But it was small. They had to pay a lot of money, to keep me there.

Weiner:

About the Metaphase position — this was between MIT and Princeton — anything particularly interesting to say about it?

Feynman:

Well, no, except that I was happy because I was successful. The man had just got the

process. He'd just found it. My father always suspected that he had stolen it from somebody in France. We have no way of knowing. Anyway, he had this process. He had not developed it, worked it out. It worked on one kind of plastic only. He worked on Bakelite, sandblasting certain things, and so on, and he would precipitate silver in a silver solution, and then hold, and then plate. The problem was, it should hold. If you tried it on something like cellulose acetate, it didn't hold. Anyway, what I contributed was, first, I discovered ways of doing it with many more plastics, by not just sandblasting but by chemical disintegration of the surface, in the case of cellulose acetate by sodium hydroxide, and so on. So I got it to work on more plastics. Because nobody had worked on it, you see. It was easy. Then, one of the problems was that the silver precipitation, a large amount of silver precipitated in solution, not only on the object. He was going to license this process out. That was the plan. And then it would be a bit complicated about recovery. They couldn't let them recover, because they would learn too much about the process. They'd have to send the bottles back to us and so on. So I found that by using formaldehyde instead of sugar, that I could precipitate a hundred percent of the silver much more efficiently on the surface — in fact, so perfectly that there was no silver left in the solution worth recovering. So that simplified everything. So I made a lot — I got a kick out of it, because I was able to solve a number of problems for the company. Ultimately the company failed. Just at the time when I — at the end of the summer — they had at last got one job, licensed it to somebody to metal-plate plastic pens. As a matter of fact, it was very successful. They sold a large number of these pens. They were all over the place. But in a month or two, the layer began to blister on a lot of the pens and come off. The problem of solving that — what caused that and how to get rid of it — was not solved by my friend Bernie. I wasn't there at the time. But he didn't solve it in time. It was probably solvable. My guess is that it was some special impurity in the filler of the plastic that was not very important, that if they would play around with the plastic — anyway, never mind. But I did pretty good when I was there. Meanwhile he was out running around trying to sell this thing, see. It was an amusing job. But there were only three of us in the company — he, his father or somebody who was in charge of selling or something only, and me, the bottle washer —

Weiner:

— you were on salary? —

Feynman:

— yeah — a guy to help me, Bernie's brother, who helped me wash and clean up and take care of the laboratory. Just four or five altogether in the company.

Weiner:

You were paid, just for the summer.

Feynman:

Yes. I mention this because of an amusing story which I will tell you, if you want. Many years later at Los Alamos a new man came whose name was Frederick de Hoffman, now president of General Atomics, down here in San Diego. He came, and he came from England, where he had been for a while during the war, and he told me a story — that he had been working with a group trying to find a way of metal-plating plastics. I just sat there with a straight face. He was just telling me — what he'd been doing in England — it happened to come out. I said, "How did you do?" He said, "Oh, we had quite a big group," and he mentioned about 10 or 20 people or something who were working and doing different experiments. I said, "Well, what happened?" "Well, we were behind. We finally decided to give up." "Behind what?" He said, "There was a great big company in the United States, the Metaphase Corporation, that was doing it." I said, "Oh." (This is immoral, this thing.) Straight as anything, I said, "Oh. What made you think they were such a big company?" "Oh, they had great big advertisements. They had full page advertisements. They had articles in the plastics magazines." They did, they had worked out articles with their products and big advertisements about what they could do. They weren't selling anything — just five guys, you see. So I said to him, "Tell me, just tell me something. You people in England look at America. I wonder what your view of it really is. Would you tell me what you would think the chief research scientist of that company would look like, how he would operate, and so on?" He said, "Sure. He would have an office, glass walls, metal this and that, there would be several laboratories, he might have 20 or 30 men. When they had special problems they would come in to him with the problem, and they would ask, and he would suggest something to do," and so on and so on and so on. I said, "You know, you're talking to the chief research chemist of the Metaphase Corporation." That was great. I mean, it was one of those great moments. I told him that there were five guys, that you can't believe advertising. It sounds great but there's nothing there.

Weiner:

Well, that took you through the summer, a pleasant summer, and to Princeton. And that's a long story.

Feynman:

Yeah. I think I'd better rest.

Weiner:

We're resuming now after a break for dinner. I think we have reached Princeton, in the fall of 1939. So let's take it from there.

Feynman:

All right. I'll try to remember a few things of the first few days. One of the things in the first few days had to do with the social life. My father took me down there, I think. Anyway, I wasn't there long before some man came to me and said, "Professor, the Dean is having a tea. We'd like you to come," and so on, "and ask your room-mate." My room-mate was a man I didn't know, named Lou Siret who's a chemist of rather reasonable renown. So I asked him, and he thought it was a great idea. I was scared because of this same thing I was talking about — I'm not so good at this. "The Dean's tea" — it sounded so silly, you know, and high class. He took it in stride, because he was that kind of guy. And we went to the Dean's tea the first day I was there — it was a Sunday, I guess — and the Dean, Dean Eisenhart, was in the line going in. I told him my name, and he said, "Oh yes, I know, you're from MIT," and so on. I was kind of pleased by that. Then, when I went in, I was looking around, where to sit and everything, I was concerned with all these matters, and there were some girls around. I felt rather stiff. Then I heard a voice behind me say, "Would you like cream or lemon in your tea, Sir?" It was Mrs. E., and I said, "Both, please, "— absentmindedly — and then there was a nervous laugh that I could hear and she said, "Surely you're joking, Mr. Feynman?" Then I had to turn around and figure, what was I joking? What was the question? It was really quite — so I started out on the wrong foot with the social things. At any rate, another thing was, I went down to the town to buy supplies and I was carrying a wastepaper basket and some other things, and Eisenbud, who was a theoretical physicist — we met — he passed me on the street. "Ah," he said, "you look like you're going to be a good theoretical physicist. You've brought the right tools — there's an eraser and a wastebasket." I remember all these things, because the first days of going to a place are very impressive. Now, another thing, the reason I went to Princeton was that I had seen that in the papers in the PHYSICAL REVIEW there were lots of articles from Princeton — a lot of work, good work, from Princeton. I knew that.

Weiner:

You were reading the journals, at MIT?

Feynman:

Oh, yes. By that time I was looking at the journals — which reminds me of something I don't know if I should go back to.

Weiner:

Oh, sure.

Feynman:

Very interesting, now that I remember, at MIT. It's interesting for the question — let me go back — about something that Welpin and I did. We would talk together all this time, you know; then we'd develop these things. And we finally decided one day that the way to make great discoveries is to work on problems in the forefront. Right? And the way to know where the forefront is to take out the most recent things of importance, and we would work on it, and we would, so to speak, do something. So we went around and we found two things that were important, as we thought — one was some kind of a wave tensor calculus, or something like that, published by some Japanese guy, and the other was the application of tensors to electrical engineering, by a man named Krohn or something like that. We thought this was forefront stuff. So I took the electrical engineering tensor business. He took the wave vector calculus or whatever it was. He studied those papers as best he could, to try to work them out, and I read the stuff about electrical engineering — because we figured all we'd have to do, you know, is make one more step on top of what the other guys did. And we learned a lesson, of course — that first, we were chasing the wrong... The wave tensor thing was nothing. It was some crazy nonsense that never meant anything. The application to electrical machinery — I don't know how important it is in electrical engineering. It seems to me that those papers are known, because when I mention them people know them, so it may have been of some importance, but it was really just reformulation of certain electrical engineering things, in a rather formal way, in tensors instead of equations. It didn't seem to me at the end to be anything. So we failed, but we learned an important lesson about how to do research. Because there are many people who feel that the way to do is to find out what problem the big guys are working on, and work right there. It isn't such a good scheme. You can be chasing the wrong thing, if you haven't thought about it yourself.

Weiner:

Getting back to Princeton — you said you'd read —

Feynman:

— I had read the journals, yes, and I knew things that were going on because I'd seen the articles, and I knew that from the Princeton cyclotron research lots of work came, good work. I also knew that at MIT they had a marvelous cyclotron. They were very proud of it. MIT was self-confident and proud, and everybody at MIT thinks it's great, and I thought that it was great. It was essentially gold-plated, if you know what I mean — I don't mean literally — and the control board was in another room, with special glass panels and knobs and everything. It was very nice. I'd seen the cyclotron. But I knew from the journals that not much was coming from the MIT cyclotron, relatively, and therefore the Princeton cyclotron must really be something — you know? Of course, the MIT one was big, in two rooms, and so on. So I got to Princeton, and the first thing, when I was there and I went to the physics building, I asked immediately, "I want to see the cyclotron" — because I was very excited. And they said to go down in the basement and the room down at the end of the basement — which seemed to me

incredible, stuffed away...Anyway, I went down in the basement, and I walked into the room where the cyclotron was, at the end of the basement. And it wasn't 15 seconds before I understood why the Princeton cyclotron had lots of results, why Slater had told me to go to another school — I understood the whole thing. The whole mirage, the whole idealism of MIT collapsed. Because I recognized something in that room, which was the same as in my laboratory at home. The cyclotron was in the middle of the room. There were wires all over the place, hanging in the air, just strung up by somebody. There were water things — there had to be automatic water coolers, and little switches, so if the water stopped it would automatically go on, and there were some kind of pipes and you could see, you know, water dripping. There was wax all over the place, hanging, where they were fixing leaks. The room was full of cans of film at crazy angles on tables. You see, completely different than at MIT. A place where somebody was working! Where the guy who was working was close to the machine, could fix it with his own hands. It was not in an insulated box with knobs. I understood it immediately, because I'd had this experience in laboratory. It looked like my kid laboratory, where I had everything all over the place and the tools were put down where I last had 'em. And I realized that this was really research, and that I had been fooled — that good engineering design is what they had at MIT, in a kind of abstract way, but not the real work with the machine, that they were separated from it. I understood it very quickly, as soon as I saw the machine. I loved it. I knew I was in the right place. They were guys of the old — the way I had felt when I was a kid. Fiddling is the answer. Experimenting is fiddling around. It's not an organized program, elegance — it's impossible. I noticed it. I mean, I realized right away that Slater was right. I had thought that was the best school in the world, and here was a thing I'd imagined must be three times as great, ten times as large, and four times as elegant, in order to get that much more research. But as a matter of fact, it was smaller and completely inelegant, and that was the secret. So I loved Princeton right away.

Weiner:

Reminds me of your English friend's description of Metaphase, his image from the ads.

Feynman:

Yes. Exactly. The same idea. The fact of the matter is that the Princeton cyclotron had a fire, because of all those wires hanging in space, and they had a great deal of trouble on account of that fire, so in a certain sense I was wrong.

Weiner:

Who was running the Princeton cyclotron then?

Feynman:

I don't know. I think there was a man, Delsassa, who was important. But I don't know who all — I don't know the men's names. I don't remember the men's names. But I was of course friends right away, met people and so on — all the graduate students — great place.

Weiner:

Where did the rest of them come from? You didn't know any of them before?

Feynman:

No. Nobody. In Princeton they all lived in the same place, the Graduate College, so called — in a building, all in one place, where all the graduates in every field, except theology, lived together, and ate supper in the same great dining room, with academic gowns which we had to wear — which was one of those things that bothered me a little bit. When I saw the other guys, though, with their torn-up academic gowns, I realized something that anybody who's mature knows, that the elegance, the social elegance of people, when they're in socially elegant positions is not — there's always a few of them who inside are smiling at the whole thing too, you know. It isn't true that it's very fancy and everybody believes in it, and that you're some kind of a funny guy because you don't quite get in the swing. There are a lot of people in it that are smiling, or that even are elegant and superior, that understand what it is — that it's a show. But you don't know that at first. Anyhow, we ate together, living in the same houses and buildings, which was a very pleasant thing. There were so many people to talk to, to discuss so many things, in science and archeology and religion or whatever it is. We'd argue with the Catholic — what was he, a Catholic priest — and so on, a guy. Yeah, lots of stuff. So it was very good. Lots of conversations in graduate school. OK. Now, when I got started, I had this job with Professor Wheeler, instead of Wigner. I don't know how that happened.

Weiner:

You were just told when you came there that this was it.

Feynman:

Yeah. Then I had to meet Professor Wheeler. The first time I met him, I was amazed at how young he looked. He didn't look very old to me.

Weiner:

Was he an assistant professor then or associate?

Feynman:

I don't know. We met, and then we had to discuss some things, that first time. He's an interesting man. The first time, he said, "Well, we have so and so much time, we'll meet at such and such a time, we have so much time to discuss things." came into his office and sat down, and he took out his pocket watch and placed it on the table, so he could see how much time — and then he would explain what we should worry about. A very nice fellow, with whom I'd learn, and so on. So I figured — the next time, I'd bought myself a dollar watch, see? So he came in, put his watch down on the table. I took my watch, put it on the table. I was imitating him, a little bit, you see. And he laughed very hard. We laughed at each other. Then we got a silly streak. (I tell it only because of the way you people are.) Like two teenage kids, you know — they can't stop laughing, you know? One makes the other one laugh by making funny noises. It's a ridiculous thing. We both collapsed and we couldn't work for quite a while. "Look, we have to get serious here and get going —" "Yes, Sir" — you know — hah hahhah! Stupid! But that's the way we started our relationship, and we were very good friends, naturally, as a result of this idiocy at the beginning. And we've always been very good friends. Now, with regard to the work with Professor Wheeler, I can't remember everything, all the things we worried about. We worried about a number of problems that he would give me to play with, associated with scattering theory, and some problems that I know specifically what it was — if you want the problem?

Weiner:

Yes.

Feynman:

Dumond and somebody else, Jessie Dumond —

Weiner:

— did some work with Kirkpatrick —

Feynman:

Possibly Kirkpatrick—had worked out a way that by x-rays you could figure out the electron momentum distribution in an atom. It's not Kirkpatrick. The electron distribution in an atom. And he had made the argument by a sort of semi-classical view, that if you had x-ray static moving electrons the frequency is shifted, so that the shape of the Compton line tells you the momentum distribution of the electrons. And he thought it was very interesting because it was a direct measure of momentum distribution, that the wave functions, Fourier, transform a square. And it sort of demonstrates the dynamic atom, in a very pleasant, in a nice way, a direct way. Wheeler said, "We have to demonstrate, we should demonstrate to what extent this is exactly true," because he used

the WK — he used the Fermi-Thomas model of the atom, in order to make his argument, and that isn't really quite legitimate, because you had to think of this scattering from each little element, and one element will scatter, like a statistical way. It's not clear. It's all right for the energy atom, but is it all right for the scattering? And so he set me the problem, to find out to what approximation its right, and with a great deal of complicated effort, I showed everything was right if they used what they called a WKB approximating of the wave functions. I said, I showed. He probably suggested the way of doing it. Anyway, I was doing work of this kind for him. And this led us into the problems of scattering, theory of a scattering of electrons and so on, from atoms. We kept talking about scattering, and how quantum mechanics could really be described by a succession of scatterings, that the Schrodinger equation meant that a particle keeps on scattering in succession, a view which I still hold as a useful one for looking at it. He wanted — he got rather ambitious — he gets these periods, I realize, of ambition, in which he's going to rewrite quantum mechanics entirely in terms of scattering, and we start to set up the ideas to do it. We didn't do it, but I remember discussing it. I would often go to his home. He was a very nice fellow, inviting me for dinner, and I'd meet his children and his wife, and then we'd sit afterwards and work on something or other and talk about something, and so on. Now, of course, the graduate school itself, where I was — how it ran — as far as I can remember, I can't remember anything being assigned. No classes that you had to take.

Weiner:

No courses?

Feynman:

No, not that were required, as far as I know. I mean, this is my memory. I don't remember anything required. I remember that there was some examination at a certain point which I had to take, preliminary exam or something.

Weiner:

Qualifier.

Feynman:

I suppose — for the Ph.D.

Weiner:

What about a Master's?

Feynman:

Maybe there was a Master's or something. I don't remember very well.

Weiner:

You didn't take a Master's. You went straight for the Ph.D. —

Feynman:

No. Right. So there was some examination, but there were no required courses, which was good for me. I liked that. I did take a course by Professor Wigner on solid state, and Professor Wheeler gave a course on nuclear physics. These sorts of things I really needed at this stage. As his assistant, I took notes, to write up the notes of the nuclear physics. Near the end of the course — I just remember this thing about the notes — he was explaining a relatively complicated formula that Professor Wigner had worked out for symmetry groups in the nucleus. There was a certain quantity, Chi, which was the characteristic of the group representation of the something or other — all of which doesn't mean anything to me. So I figured that the number had to do with the amount of interactions between particles in the same state and different states, so I made a little diagram — which, it turns out, was equivalent to the Young diagram, group theory — for this problem, which counted the number of neutrons and protons and squared, counted how many pairs there were in anti-symmetric states, and how many there were in symmetric, and subtracted, and got the same number for chi — it was clear, which told me the difference between the number of interactions between electrical particles in the same state, and those in which the function was anti-symmetric. Some of them gave, by an empirical way, you see, not deeply understanding what this was, but by half-guessing and by trial and error, I cooked up a rule which gave me the same answer as Professor Wigner but seemed to me much simpler to operate. It was much simpler to operate. I've used the same scheme again and again, and it's a way of developing new knowledge for me.

Weiner:

What was Wheeler's reaction to this?

Feynman:

Wheeler was very pleased, because his attempt to explain the subject, in his course, had ended up by simply saying, "It's a result of group theory, that this is the formula that Professor Wigner's demonstrated," and was unable to explain in a direct way. When he saw this thing, he thought it was fine. So he sent me in to Professor Wigner, and I went in and said, "Why don't you do it this simple way?" Whereupon I got a very long answer. He was good. I got a long answer from him which I did not understand, in which he

claimed that although I — he studied it. He took a few days off about it. This bothered me. He came back and told me that although I got the right answer, the method was not legitimate, for some reason. This I don't understand, and didn't understand. I don't understand what the matter with it was. As a matter of fact, as far as I know, I believe that other people later showed that this was legitimate, although they weren't just checking mine, but for some other application somebody else found the same thing. And it is all right. It was just a — this was very happy and exciting for me, because now I was discovering things, you see, that were very close to the present time. All this time I'd been discovering things, always closer, and it was very close to the present time. It wasn't an important thing, but it was fun to notice that I could do what Professor Wigner could do, easier — although he claimed I was doing it illegitimately, I still thought I was right. It was half logical, suggesting there ought to be a difference like this, and calculating the difference, then discovering how to double something before I took the difference or something like that. Then it would work — you know? Some little something.

Weiner:

What kind of a lecturer or teacher was he?

Feynman:

I liked his course in solid state. I thought it was a good course. I remember many things from it, ideas, why is a solid a solid, and all these deep ideas at the beginning. It was a good course. I don't remember any other courses.

Weiner:

Just those two?

Feynman:

Those are the only ones I remember taking. This sounds incredible but I don't think I — you'd have to look at some records to find out if I were in any other courses.

Weiner:

Was it in your first year when you took them, or second?

Feynman:

I think I took those when I first came, somewhere in the beginning. I don't know. I don't remember my education in the graduate school. I don't think I had to do anything. What I think was the education was to worry about problems, to talk to friends — see, I

did a lot of things. I would hear that Eisenbud was working on a deuteron, and that he had a terrible calamity, Wigner had made some suggestion as to why something or other in the deuteron, why he had a quadruple or something, and it turns out it was a mistake, after a year and a half, and that you can't get a quadruple moment from this. And all the graduate students would be standing around and commiserating with all the bad luck in the world, you know, that this could happen to a student, but that's the way life is, the professor makes a suggestion, and it turns out not to be a good one, that's your bad luck. I don't know what this thing was, but we would talk, you see. I don't know what this thing was, but we would talk, you see. I'd try to understand what the problem was and what the logic was, about everything, you see, that was going on. I remember a distinct thing at Princeton, that somebody came to me — they were very interested in the fact that there was capture from the L shell but not from the K shell, in some element. And that's very strange. How could that be? How would you capture it? There are all these formulas for K shell capture rates, and how would you get an L shell capture rate? It was a very interesting puzzle, because I don't know how to calculate anything, at the time — you know, not much. It was the beginning of real stuff, real research. Of course, the reason it was from L and not from K was that the energy wasn't enough to pick up a K electron and leave a hole there. It was a small energy. That was easy. But then — that was pretty clear — but the question was, what was the rate from an L shell? Everybody had always given the formulas for the K shell. It doesn't sound like much to me now, it's so easy to do, I can do it in my head, but in those days, to really do a real problem — take a standard formula from somebody and make it independently, a new one, just as good as the guy who discovered the other one —! I was very impressed with my new abilities, you see, that I was learning the real objects, that really connected with research with the cyclotron. I was getting near the front, you see. I was very happy with these little things. Then there were some fellows doing research with magnetism, some friends, and they had ellipsoids, and they would explain it, and I learned about demagnetization factors and so on. I believe, although I can't guarantee it, that most of my education as a graduate student was through my own studying, through worrying about problems, through talking to friends, and very little courses. And that's the way it was in those days.

Weiner:

How many graduate students were there in the department?

Feynman:

Not very many. There weren't a real lot. There must have been — I'll bet you it's in the neighborhood of 20 to 30.

Weiner:

So you knew just about everyone?

Feynman:

Oh, yeah, you knew everybody.

Weiner:

How about professors? You mentioned Wigner, Wheeler —

Feynman:

Yeah, you got to know most of the professors.

Weiner:

Who were the other ones there?

Feynman:

I don't remember. There was Ladenburg, who was from the old school. I talked to him about things. There was Robertson, who was there for some length of time. And then there were guys down at the cyclotron who were sort of — they must have been assistant professors — somewhere between graduate students and professors, but you never cared which was which down there.

Weiner:

How about Smythe, was he there?

Feynman:

Yes, he was there.

Weiner:

Compton? Karl Compton?

Feynman:

No, Compton wasn't there. But I'm guessing. You might find on the records that I took a lot of courses. I'd be interested, because I cannot remember. I do not remember studying at Princeton, but I do know that I got a lot of information; I learned an awful lot at Princeton, through my assistantship with Wheeler, doing these problems. I was working all the time, about one thing or another. And then there were the other graduate

students in other fields, such as in the psychology department or the astronomy department, who'd ask me a problem. See, I was doing something all the time. They'd say, "Listen, there's this theory of statistics about when something is bigger than something, you throw out something — you know, you're supposed to take a mean," and so on. "Now, we know, in astronomy, when we measure the intensity of starlight" or something like that "that every once in a while, something impossible happens, like the plate doesn't develop, or there's a cloud in the way of the telescope, or something crazy. Now, what are we legally allowed to do? It seems incredible we should take that and average it with the others to find the average. So we want to throw it out. But sometimes we don't know the cause. The whole plate is dim. But we know it's cockeyed. So can you make a rational method of analyzing statistically how and when we can throw out data very far off?" You see? Now, that problem's been solved by other people. That's not the point. I was doing things like this all the time. And I worked it out, I know now, in a completely right way. Right way of dealing with this problem and the theory of statistics. And so on. So there were discussions, and talking among the graduate students, and perpetual mental challenges from all directions, one thing after another, that I was working out. And learning by the process — I think — and I don't believe I took many courses.

Weiner:

How about experimental work? Did you have anything to do with the cyclotron at all?

Feynman:

No. No.

Weiner:

You weren't in the lab all this time at Princeton?

Feynman:

No. I wasn't. I was in theoretical. My assistantship was with Wheeler, so I was doing theoretical work. I think I essentially learned from Wheeler. It's possible that I learned somewhere else. There were little things. I got a reputation after a while — I was good at doing integrals, calculating integrals, if it was possible, and all that. I remember one particular thing. I saw on the board an integral. I didn't pay much attention to it. I had been on various boards — in other words, all the mathematicians were trying it. And then Wheeler said, "Oh, by the way, I've got an integral. I haven't showed it to you. I've been showing it to everybody else. Can you do it?" I looked at it, this integral, that all these guys had been working on — I looked at it. He said, "Can you do it?" I said, "Well, would it be all right if it comes" — it's a double integral, see — "would it be all right if it comes out a Bessel function?" He said, "I'm not responsible for the answer." I

said, "All right." Then I came back in an hour —"It's an integral of a Bessel function." It was easy to express. I had worked it when nobody else around could do it. This was the result of Mr. Bater's giving me that book.

Weiner:

The ADVANCED CALCULUS?

Feynman:

Right, because the kind of advanced mathematics that was involved in doing an integral of this kind of stuff was in that book. I had had, you see, unlike most of the others — certainly not in the mathematics department, they never do much work, you know — I had had five, six, by this time six years that I knew that stuff, you see. And that's a rather old guy — I'd been doing these kinds of things, you see — whereas other students had just about learned it a year or two before. Because of Bater, I had learned this stuff earlier. I was really good at it by this time. This was technical stuff. It wasn't anything brilliant, it's just that I was facile at mathematics, mathematical manipulating—like a guy that's good at doing arithmetic fast in his head. That kind of stuff. It's very useful to be able to do.

Weiner:

In setting up problems as well?

Feynman:

Yeah, you can check yourself out. Yes. You see, this has always been useful to me. When I was in high school, we had an algebra team, and I was on the algebra team. It was a crazy thing, where we'd meet together with another school, and they'd open from an envelope problems that somebody invented somewhere, and they'd announce, "This problem is 45 seconds," or maybe "two and a half minutes," and they would write it on the board. You had 15 seconds to think and then you'd work like a demon, and you'd put a circle around your answer. It didn't make any difference how you got the answer. This is what I loved. I used to practice, to do this, and I would get very very fast. The ability to do algebra fast, which later became the ability to do calculus fast, always stood me in good stead. I told you that at MIT, it didn't bother me to do these terrible, these organized where I knew the subject, because I could do it easily. It was because I could do it fast. They'd have a set of problems to do, you see, that would involve a lot of manipulating, and the ordinary guy would have to do a lot of tedious finagling. But for me, I could do it very quickly. So it was easy to get rid of the homework. I always kept up this ability to work very quickly with the mathematics, so as to get rid of the homework. I still had it when I was at Princeton.

Weiner:

At Princeton, working with Wheeler, you got there in 1939. In 1941, I don't know what part of the year it was, whether it was the early part of the year or not, you made a reference in one of your published papers to a presentation of a paper with Wheeler at the Cambridge meeting of the American —

Feynman:

— all right, now you want to know how that developed.

Weiner:

Yes, unless it's skipping stages.

Feynman:

No, I'm just thinking. I don't think it is skipping anything, except the personal life business.

Weiner:

Well, we can get back to that.

Feynman:

Right. There was a problem —

Weiner:

— it was February, 1941 —

Feynman:

— never mind, I know what you're talking about. When I was at MIT, I had read a lot. I didn't explain that. I forgot to. I was in the library a lot. I read advanced books. That's the way I taught myself, I read lots of stuff — I was very avid for reading and studying and learning. I read about general relativity, I learned it from a book, and I read a lot of quantum mechanics along with Welpin, and all this stuff, by reading. And so I read the advanced books on the problems of the day, which were quantum electrodynamics, the problems of the interaction of the two electrons according to relativity, and retardation in an atom was not solved, really, as far as they — it got infinities, the theory had infinities, had self-energy difficulties. They had these great formulations in Dirac's book

and in Heidler's. I couldn't quite read the books. They were too hard for me. But I did know what they said about the problem, that they couldn't solve it, and I also got vague ideas about what was the matter with the problem, that it had something to do with the infinite self-energy of the electron, and another infinity that came from the infinite number of degrees of freedom in the field. I almost parroted the words in these books. I knew about the self-energy, because I could understand that. That was the famous classical thing, that the energy of a point charge is infinite. It was still in quantum mechanics. And the infinite number of degrees of freedom problem I didn't understand exactly. I thought it was the half H omega that every oscillator state has, added to infinity, would be an infinite amount of energy. You know what I'm talking about?

Weiner:

I'm following the outline of it. Go ahead.

Feynman:

Each mode in a cavity was supposed to turn into a harmonic oscillator, and the energy at the lowest state of harmonic oscillator is half the frequency times h , and there's an infinite number of modes. You add together, you get infinity for the energy. But this is irrelevant — a shift to zero of energy and get rid of it — but I didn't know that. I thought that was something to do with the infinite number of the degrees of freedom. Well, I got an idea at MIT which seemed to me quite evident — first, that an electron doesn't act on itself. That's crazy. It's a silly idea, an electron acts on itself. It can only act on another charge. Why make it act on itself? The other thing was that where part of the trouble was — these fools who made the energy act on itself — and second, they said that the field had an infinite number of degrees of freedom. But any field I ever saw was generated by a source, by a mechanical object. There's no other field. And it's not free in any way. It's completely determined by the object. I was thinking classically. So there isn't any degrees of freedom more than the degrees of freedom of the charges which had generated the field, and that's a finite, not infinite, like the infinite number of modes in a box, you see. So I figured, that's all going to be simple. I could straighten it out. I won't have any field. I'll have a direct interaction, where the only so-called field allowed is exactly the field generated by the charge, so that there's not really a field, it's just a mathematical thing. When you shake one charge, it drives another. Delayed. It's not that something necessarily goes between the two, it's just — that made a complicated thing, all that field with its freedom is a lot of baloney; as you shake this charge, it pulls on the other. And this pulls back. Not this pulls itself. It seemed to me that was clearly the physics. I explained it. I talked to Welpin about it. Someday, I would straighten these guys out. This attitude about these guys, though, always kept with me. I always felt, if they said that they couldn't solve the problem, that they didn't know anything about it, you see. It wasn't right, necessarily, to read all the details of what they had done. It was no more necessary to read what they did, because they didn't know the answers, so therefore I could fool around with it myself.

Weiner:

You didn't even examine their starting points?

Feynman:

No. I know quantum mechanics and I knew relativity. That's all they had. I mean, that's the starting point. That's all they really had, and the Maxwell equations of electrodynamics, and they put them together, with their physical ideas, and they ground out all this stuff, and it didn't work. All right. I'd start. I know what the thing — there's nothing else to it, but the quantum mechanics and electromagnetism. It's not necessary to read all that stuff. The problem is clear. It's hard to — really, you are in the same position as the other guy. What else did they have? Nothing. I knew they had nothing else but quantum mechanics, relativity, and the Maxwell equations, and I could permit myself to change the Maxwell equations in a region in which experiment hadn't yet — could not notice a difference if I wanted, say, short distances—or something. I was free, where if I'd followed them — I don't know. Maybe relativity falls for some region that we haven't measured yet. You know. It's always possible. So I knew what the problem was. Anyway, that was my general idea for a solution, which I had at MIT. Well, by the time I got to graduate school, I had learned that there was a serious trouble with the fact that an electron does not act on itself — that the action of an electron on itself was used to explain the force of radiation resistance — that is, when you accelerate a charge, you have to do more work on it than you do if it's neutral, because after all you have to generate the radiated energy. And this work is done against what force? I mean, what are you pushing against? The electron acts on itself. So if I have electrons acting only on other electrons, I'm stuck with the radiation resistance problem. I knew about it. I had learned that that was where I was in trouble. So Wheeler had given me some problem that was too difficult, that I fiddled around with some idea to explain — to describe electrons and positrons in paired production, and you don't have to have spin a half because you have two particles, and maybe the mathematics is simpler. The Dirac equation always seemed a little obscure. This was a hope to make it look easier. It was too complicated. It didn't make a simple representation. But I was working on that, and kind of giving up. And so I fiddled on my own stuff, and one day when I came to his office to report, I said, "Well, I'll tell you frankly, I got pie-eyed, I couldn't get anywhere. But I want to tell you some idea that I was working on," and I described the physical idea. What I had worked on was this. Suppose there are two charges in the world. You shake one, it shakes the other, but that shaking would generate a field back on this and shake that. And so when I shook this, and the other would make an action back on this charge, that might be force of radiation resistance. So Wheeler said immediately to me that the force would depend upon the mass and the charge and the distance of the other charge — in fact, inversely as the square of the distance of the other charge. I thought surely he had done the problem, but he just saw all that right away. Then, if there were charges in all directions, the answer would come out, infinity, if you integrate. I had

noticed that too — the inverse square over all space gives — and so on. Then he said, “And another trouble is — you see, you can’t have radiation resistance depend upon the charges, it depends only on the other charges, it depends only on the charges to accelerate it, how it’s accelerated.” And he said, “And the other trouble is that the action back is delayed. It comes back not at the time when you shake the charge, but there’s a delay here, and there’s a delay back.” Oh, I hit my head. Oh, what a dope, you see. Because it was clear then, I’d only described reflected light, which goes out, hits something, comes back. But Wheeler went right on from there and said, “But, there’s also the advance wave solutions of Maxwell’s equation, and maybe the reaction back is by advance wave. They would act at the right time.” Then he went through a little idea, that perhaps, “Suppose we had a medium around here infinite in all directions, and that the action in the medium is by retarded waves — it would slow it up by the index of refraction — but the action back is by advance waves, not slowed up by the index.” I said, “Why?” “I don’t know, let’s try.” And then we could calculate the effect back, from all the charges in the medium, and it didn’t integrate to infinity now because of the index. The phase was slowly changing. You see, the retarded wave was changing its phase as it went deeper in, but the advance waves coming back didn’t have this phase retardation, so it was getting out of phase. So by taking just the contribution in the first wave zone, so to speak, Huygen’s principle, we estimated how much it would be, and it was the right order. It looked like the right thing. It was independent of the masses and charges and density and so on in the medium, and it looked like the right action back. All this was done sort of like a continuous lecture, when I’d come in like this.

Weiner:

This was the same day? This was one event?

Feynman:

Just one event. I’d go into his office and I’d tell him this idea, and that it doesn’t work, and he says to me, “Yes, it doesn’t work, it depends on the mass and the charge, and it could be advanced waves, let’s try this,” and we worked it out and it’s about right. Maybe an hour, a half hour. It was very exciting, you see. He said, “Now, you go home and you figure out what proportion of advanced and retarded waves it is, and also try to figure out what happens to the advanced effects if we put a test charge next to this one, because the test charge should get an advanced effect when we shake this charge, if they are advanced effects. What happens to it? Is it there? And if there’s any paradox.” Well, that was the beginning. After I did lots of work on this.

Weiner:

Do you know when that was? Can you date that?

Feynman:

Mm. Not exactly.

Weiner:

Was it in your first year at Princeton?

Feynman:

I think not. It was probably in the second year.

Weiner:

It had to be before February, 1941.

Feynman:

Yes, so it was probably in the fall of 1940, or a little earlier — yeah, fall of 1940, probably. I was sent home, so to speak, back to my room, to check these various things, to really make sure that we had done it right on the blackboard, to find out what, to get the numbers exactly right, to know exactly how much half advanced wave, to find out why there was only retarded waves going forward and pure advanced coming back, to check that a particle nearby would feel no advanced effect — which I did differently, I couldn't do it in the arbitrary positions sphere, but only an infinitesimal distance away. And all this work was quite tedious and difficult, because the methods were not good. As I kept working on these things, with Wheeler's suggestion of what to try next and so on, I got more and more power — understood the generality. We knew that the answer must be independent of the shape of the box, and we'd try to prove that. You get more and more powerful. You understand it better and better. We did many examples of understanding, of the way of refraction would have worked if the system had advanced waves. He made lots of suggestions of what to study, and I would go and look at these things. I'd take it back, and he would suggest that we ought to think about how a material would, index would work if it was generating advanced waves, and so on, gradually putting the pieces together and making sure we understood how advanced and retarded waves worked. And things about paradoxes, sort of — you know, you get a clue from the advanced wave that something is going to happen, and then you don't do it. You know? I mean, if a particle has an advanced wave — take two charges that interact. You shake one down, the other should shake earlier, should have made an effect on this one earlier. So you sit there. You see, there's no — there's an effect on this one earlier, so you have — that comes from the other — that means you're going to hit it now? No, you just don't hit it. Well, then, what happened? Where would that effect come from? You see. Or you say, if there's no effect there, then you say, OK, I'll hit it — then there should have been an effect there. So there are evident paradoxes. Now, in the closed

universe system, where everything was closed, there was absorber all around, there were no paradoxes. We eventually proved its equivalent to the retarded theory. But we felt that we can't have a theory which would have been paradoxical in some other environment. We therefore had these paradoxes, which I studied and argued, and would make explanations of the paradox, how it straightened out. And then I would challenge my friends to try to explain them—my friends in the graduate school. Many discussions. Many arguments.

Weiner:

Did they follow you on this?

Feynman:

Oh yes. It's a problem. You could explain the difficulties, and they would challenge you. I would say, "I think the difficulty is straightened out by supposing that for every — supposing that there's no device which has a response, which is a discontinuous function of the stimulus, but only continuous functions, so there's always a solution in between, when you get a kind of half bump ahead of time, and then you're not sure — is it there or is it not there? — when the time comes to hit, you're not exactly sure to hit strong or weak, and you kind of miss a little bit, and that is self-consistent. It's so weak you're not sure, you don't know which way — so if it's a continuous function —" Then they would argue, "Well, that's not necessary, we could have mousetrap devices and so on" — you know. It was like the old days with Bohr. I felt like that. You see, we knew the history of science, and that guys like Bohr would argue with — the uncertainty principle, or Heisenberg — they would argue about what would happen, how you would really not be able to measure something, in spite of the fact that someone would think you were, because if you built the apparatus... We had the same kind of reasoning. Then I'd analyze what would happen with each of their devices which they would use to break my paradox. It was quite a pleasure. It was just like the old days. It would hold me for two days, to find out why I couldn't — with this device that they had to beat me, what was the matter with it, you know. And so on.

Weiner:

Who were they?

Feynman:

The graduate school.

Weiner:

Can you remember the names of them?

Feynman:

The most I remember is Bill Woodward. We had many arguments about this paradox with Bill Woodward. John Tuckey in mathematics, I discussed a little bit, not so much the paradox but some theorems and some topology, which are fixed point theory and so on, which just meant that I had an intersection of two curves, and other little things. We talked with the graduate students. They discussed these ideas a lot. I can't remember all of them. But I was also developing quite carefully the general theory of this thing. An ever more improved form. I don't think I need to bother you with all the forms. We wrote the darned thing in 65 different ways. I mean, three or four different ways, all equivalent.

Weiner:

Over how long a period of time?

Feynman:

I can't remember. Then there would be a question, "What about energy?" See, we had a principle of least action, finally. We represented along by least action instead of Maxwell's equation, because in Maxwell's equation you have one field, and a one field would have to act back on its own generator, which is action on itself. But it was on many fields, one for each particle, a thing which — Wheeler was very close to the literature. He knew that an idea like that had been suggested by Frankel. He also found a paper by Fokker, who had a least action principle, which he showed me a very short time, after, which had advanced waves. Fokker had noticed, if you try to write interaction with the least action, you got both advanced and retarded, and that's just what we got, fifty-fifty, so therefore we could have a least action. I found a way to write Fokker's rather complicated looking action very simply, using direct delta functions and so on. Gradually we got cleaner and cleaner, to understand this thing, in so many ways — we solved all kind of — we really studied it. We knew it inside out. He knew how to tell me what to do, so we studied every aspect. We knew what energy meant, what happened to the theorems of energy, what happened to everything, you know, what happened to paradox, the way it worked, how to prove it in the neatest possible way, and so on. And he told me to write it up, and that it shouldn't be any more than 20 pages, a beautiful and simple thing, or something like that. So I wrote it up, and it took me 29 pages. He didn't like the write-up of the thing. He didn't like it. He said — by that time he'd changed his mind, this was such important and wonderful stuff that we ought to write it as a sort of grand thing, and so he started a grand program of five papers or something, which I didn't really understand, and this was going to be part of it. In other words, he started by wanting a short paper; he ended by making a long paper. And my 27 page thing — I don't know where it is, I have a copy of it —

Weiner:

— the draft manuscript, you mean?

Feynman:

Yes. Yeah. I also have much of the paper on which I did many of these derivations, the business about energy and things, somewhere. I have the manuscript, the pieces and bits and so on. I was using a certain kind of paper at the time that my girlfriend had given me, that her father had gotten from some defunct company, and so you can tell what was being done when by the character of the paper being used. I have this stuff, which I'll try to find. Then, along with doing this, somewhere, we felt that we really understood it far enough that Wheeler suggested that I give the Research Conference — you know what that is? It was something like a seminar we had every week.

Weiner:

In the Physics Department?

Feynman:

In the Physics Department. You usually invite people from the outside, to talk about something.

Weiner:

A sort of colloquium?

Feynman:

Exactly, yes. We also started a Journal Club, but that was a small thing. I attended that. But there was the big weekly colloquium. So Wheeler decided we should give this stuff at the colloquium, it was worthwhile, and that I should do it, because it was a good lesson for me to learn how to talk, or something. I said, "Gee, you know, it scares me." He said, "I promise to answer all the questions" — so that I wasn't too petrified. It was my first experience at a real technical talk, see. Professor Wigner was in charge of the colloquium, so after I said I would do it, he told me that he had heard from Wheeler about the work and he knew something about it. I think we had discussed it a little bit with him. And he thought it was important enough that he had taken the liberty to invite especially Professor Henry Norris Russell from the astronomy department, the great astronomer, you know, John von Neumann from the mathematics department, the world's great mathematician, and Professor Pauli, who was visiting from Zurich, would be there. And Professor Einstein had been especially invited — and although he never comes to the

colloquia, he thinks he will come! So I went through fire on my first. I must have turned a yellowish-green or something, because I remember Professor Wigner assuring me not to worry, that they're very nice men, that on Professor Russell he'd just given me a hint — if Professor Russell falls asleep during your lecture it doesn't mean it's no good, it's just because Professor Russell always falls asleep, but he's listening; and that if Professor Pauli is nodding "yes" during the entire lecture, don't be too impressed, because the man has palsy and nods "yes" all the time. He was trying to make me feel better, you see.

Then I came to the thing and gave the lecture. First of all, not having too much experience, I put equations all over the blackboard, all the equations on the blackboard, all over, filled. So I was busy for an hour and had a lecture filling the blackboard, and didn't go to the tea previous to the lecture. Professor Einstein came by while I was filling the blackboard and said "hello" and asked me where the tea was, you see, and he went on to the tea — and I kept writing these equations.

Weiner:

Was this your first meeting with him?

Feynman:

That I now realize that it probably wasn't. I'll come back to that as soon as I finish the story. I met him another time either before or after — I'm pretty sure before, now, because he knew me; he shook hands and went upstairs to tea. Then, everybody came in and so on; I got up to give the lecture. I can still remember [looking] in front of me to see the envelope, and pulling out my notes, and the hand shaking — I can see it, the shaking hand because it was quite a thing. And I started to talk about the subject. And then a thing happened that has happened ever since, and is just great: as soon as my mind got on the physics and trying to explain it, and organize the ideas, how to present it, there was no more worrying about the audience, as personalities! It was all in terms of physics. I was calm, everything was good, I developed the ideas, I explained everything to the best of my ability. Not very good because I wasn't used to giving good lectures, or giving lectures, or something, but at any rate there was no more nervousness — until I sat down, you know. Then came the questions.

Weiner:

How long did you talk, do you remember?

Feynman:

It must have been the standard time, more or less — an hour — maybe I went over, I don't know. Anyway, Professor Pauli got up immediately after the lecture. He was sitting next to Einstein. And he says, "I do not think this theory can be right because of this, that and the other thing —" it's too bad that I cannot remember what, because the

theory is not right, and the gentleman may well have hit the nail on the bazeeto, but I don't know, unfortunately, what he said. I guess I was too nervous to listen, and didn't understand the objections. "Don't you agree, Professor Einstein?" Pauli said at the end of his criticism. "I don't believe this is right — don't you agree, Professor Einstein?" Einstein said, "No," in a soft German voice that sounded very pleasant to me, and said that he felt that the one idea, the one thing that seemed to him, was that the principles of action and distance which were involved here were inconsistent with the field views, the theory of gravitation, of general relativity. But after all general relativity is not so well established as electrodynamics, and with this prospect I would not use that as an argument against you, because maybe we can develop a different way of doing gravitational interaction too. Very nice. Very interesting. I remember that. Then there were other questions, and Wheeler answered Pauli's objections and so on, but it was so like fireworks, I can't remember now the objections or the answers. But Wheeler did answer, as he'd promised, everything. Oh, now comes another amusing feature which I should mention. While I was preparing all this, working out energies, doing these things, the obvious problem was to make the quantum theory of this. We had only the classical theory. The problem was to make quantum electrodynamics out of it, and Wheeler started to do that, and said he'd got it. I didn't know how he has it, and he wouldn't tell me how he did it, because I was supposed to prepare my lecture and do this and that. And he scheduled himself for the next week. I was scheduled to give the classical theory of action and distance or something like that, was my seminar subject — so, the quantum theory of action and distance was the next seminar. So Professor Pauli said to me, on the way up into the laboratory afterwards, "Tell me, what is Professor Wheeler going to say? How does it work, the quantum theory?" I said, "I don't know." "Oh," he said, "the professor doesn't tell his assistant how he has it worked out? Maybe the professor hasn't got it worked out!" It turned out that Professor Wheeler had been overambitious and thought he had something when he didn't — and he canceled that lecture. Professor Pauli was a very astute fellow.

Weiner:

Let me ask — this was based on the idea that if you can solve the problem for the classical case, that it's a question of the next step to apply the quantum theory?

Feynman:

Well, my feeling was, even from the very beginning, that the first thing we had to solve, before we solved the quantum work with electrodynamics, was to straighten out the difficulties with the classical electrodynamics.

Weiner:

Because intrinsically there were difficulties?

Feynman:

There were. And they were similar sounding. At least, the one about the self-energy. It wasn't cured by quantum mechanics and the infinite self-energy of classical physics. So I figured, first we must cure the classical physics. That was my MIT idea. Then we must turn and see of those ideas would work in the quantum physics. And I didn't ever think that it was going to be direct and easy to go from the classical to the quantum, but that it was necessary to go in this particular order, or at least it was a good route. Whereas Professor Wheeler felt at this stage somewhere that the transition to the quantum mechanics was very obvious, very simple — whereas I didn't know how to do it, and I didn't see; it seemed to me quite difficult to make the transition, and I didn't see what he was going to do. He mentioned a few things to me which I didn't understand very well, and it didn't seem quite clear. But then he never talked, he didn't say anything else. Apparently he had some idea which didn't work. I never did know exactly what it was.

Weiner:

Was that lecture of his presented at the colloquium?

Feynman:

No.

Weiner:

Canceled, it wasn't postponed?

Feynman:

That's right. In this matter there was also another thing. At the same time, he had sent in to the Physical Society that he was going to give a talk at the meeting on the quantum theory of action and distance, and he never told me what that was either. I had to go to the meeting. This was some months later, or something, because you see you have to send in ahead of time, so maybe a month and a half later, something like the quantum theory of action and distance. He went to give the paper and he still didn't tell me what it was going to be. I had to go to listen.

Weiner:

Where was this?

Feynman:

Washington, possibly. I had to go to listen, because he didn't tell me what it was. Then he gave a lecture in the beginning of which he talked about our little ideas, about action and distance and so on, classical theory and quantum theory and so on. Then he changed and said, "Relative to this problem," and then started to describe another thing, which is the question of van der Waals' forces between atoms when they're so far apart, further apart than the wave length of the virtual interactions, the virtual photons that go between them; when they're more than a wave length apart, where the wave length has to do with the natural frequencies of the atom. Then the van der Waals' force law changes from inverse R to the 6th to R to the 4th or something — something he had worked out, an interesting problem that he had worked out earlier that I knew about. He had talked to me about it before. But it was standard electrodynamics. It had no relation at all to these ideas. And he had dragged my name into the thing and not told me what it was, you see, and then began to describe these things that had nothing to do with it. They were perfectly legitimate things. But I was upset somewhat because he had done this. He had said it had something to do with the other theory, and it doesn't. So I got up in the meeting — it wasn't very nice of me, maybe, but this is what I felt like doing, so I got up in the meeting and said, "I criticize it on this basis." I criticized his talk on the basis that his introduction had nothing to do with the second. I felt I must protect myself from the implication that the work we did continued into quantum theory had something to do with this. So, because my name was in it, I had to protect myself. I had to explain that what I did and so on, what we did together on this thing, I don't believe has anything to do with that, and I don't see the connection and so on. He admitted, no connection.

Weiner:

On the spot?

Feynman:

On the spot — and on the way out, talking to me — we were good friends, you see — he said to me, "I shouldn't have given the talk at all. You're right. I thought I had the quantum theory, and I don't, and I thought I could use the other thing under that title." I said, "You could have, if you didn't make out that it had something to do with the other theory." You know, something like this. Anyhow, we were friends about the thing and everything. I think that the man had bad luck. He thought incorrectly that he had a solution. And you can imagine the importance of it and the excitement of it would drive him to do things a little unusual under the circumstances. I was a little bit unhappy that he couldn't explain it, but I think the reason he didn't explain it to me was not that he wouldn't have if he'd had it. But he hadn't quite got it at any point, you see — and the few little things, attempts that he had done to explain it to me, I'd shot full of holes right away, and saw the troubles. And so he didn't — he never had it to tell me, is really the reason he didn't tell me. I think that the poor man believed that it was going to be easy, to such a point that he would get it tomorrow morning. And so he never told me what it was because he didn't have it yet, until the day of the talk, and then he was stuck for

something. That's what happened. So I never felt, you know, that he had been trying to do anything dirty to me, or anything like that. I just felt he mistakenly believed the answer was just around the corner.

Weiner:

Was the abstract of the talk published?

Feynman:

Yes, I think so.

Weiner:

But nothing else.

Feynman:

I believe the abstract is close to the subject that he talked about.

Weiner:

Yes — but no paper followed?

Feynman:

No. No. Then what happened is, he got engrossed in writing up our work, in a grand and wonderful fashion. He didn't take my 27 page thing. Then he was worried about something, clearly — I was working on the quantum theory, I had nothing to do — so he kept giving me little problems, like testing the energy principle, and this and that, this and that, which apparently must have driven him mad because I kept solving them so fast. But I was starting to work on it. I did start to work on the quantum theory of this thing, because I had nothing else to do, while he was writing this grand work. But the quantum theory was not so easy to arrive at, and his worry about how fast we would get the answer was, unnecessary.

Weiner:

Let's try to get some dates in here if we can.

Feynman:

Yes. I'll just go back to one thing, about whether I met Einstein before. Oh — dates on

these things? I can't get you dates good on these things. I don't know when I gave the talk.

Weiner:

Well, was it before that Cambridge meeting of 1941?

Feynman:

It was before, but not much before. It was not much before. It was probably more or less a similar time, because probably what happened was, to give the talk at Princeton and then we should have a paper at the meeting. So probably right after I gave the talk at Princeton, I suppose, was the talk in Cambridge.

Weiner:

When was his talk?

Feynman:

Afterwards, because the quantum theory came afterwards.

Weiner:

It could have been the spring meeting.

Feynman:

Probably. Probably. Yes, very likely.

Weiner:

So the February meeting in Cambridge and the spring meeting is in Washington —

Feynman:

Probably. It's a good supposition. You can check that. Now, I do remember that meeting in Cambridge, which I otherwise would have completely forgotten.

Weiner:

You started to tell me, excuse me, about having met Einstein before?

Feynman:

Well, I'll finish, I'll come to the Cambridge thing, because it's a small matter. It was just the same thing — we should present this paper. I had no experience, and meetings scared me, and I worked the whole think out very carefully for the time, and I practiced, to do it in ten minutes, and all this other stuff. And I wrote the entire speech out, and I practiced it in my room with a friend to time me, and all this kind of stuff. It took me longer than ten minutes, and I heard the bell ring, and I got nervous — and I read the whole speech. So it was dull, impossible for people to understand, and uninteresting. There were some questions, two questions, I think, afterwards, which weren't very sharply to the point. I felt a little bit disappointed that it wasn't apparently understood. It must have sounded sort of crackpot, like guys were bored listening to this guy read it, and so on. I've since gotten much better at speaking, but that was very different. That was the way I gave the first one.

Weiner:

That was really your first public —

Feynman:

— first public thing, outside of Princeton, yes. But I really had been driven through the wringer on the Princeton colloquium, I think. So I don't remember much this Cambridge thing. There wasn't much to it. I gave the paper, there were one or two questions, I think Wheeler answered one of them, and that was all there was to it, you see.

Weiner:

When did Einstein tell you about how Tetrode had done some of this same work in Europe?

Feynman:

Tetrode and Einstein had had a discussion about something—I think a paper that Wheeler discovered.

Weiner:

In the paper in 1945, you referred to the Cambridge meeting of February, 1941, where some of these ideas were presented. Then you said, "I don't know whether it was at this meeting that Einstein mentioned this, or after the meeting."

Feynman:

I didn't say that. That paper of 1945 is really written by Wheeler. What's the name of the author?

Weiner:

The 1945 paper is "Wheeler and Feynman."

Feynman:

Ok, but it was mostly written by Wheeler, and references, things like references, to Tetrode and all this other stuff, that's Wheeler. Wheeler liked references. I didn't like references. So if he signs out that Tetrode in 1837 did something, 1897 did something, that was Wheeler's discovery I believe, although I'd be glad to look at it more closely. Let me tell you about the one other meeting with Einstein. Wheeler felt our work was interesting enough and important enough that we ought to discuss it with Professor Einstein, and so he made an appointment, and we went to Einstein's house. We were introduced in his study, and we both sat at the desk — he was behind the desk — He wore this sweater, without a shirt under it, no socks — just like everybody says — and was such a soft, nice man in the discussions, at all points. He was such an interesting man to talk to. We explained the ideas, and he made some comments about them, and then later came to that lecture. Yeah. So I did meet him, and it was quite a thing. Wheeler thought the stuff that we did was very important, and I believe he's right; although it never turned out to be useful, I think it was a very interesting possibility. It could have been right. I mean, you can't tell ahead. And it was an interesting new way of looking at electrodynamics that was different from what had ever come before, so it was worthwhile. He was right that we should talk to Einstein and give these colloquia, because the potential possibility that this was a solution to quantum electrodynamics was — unless Pauli could see through it, but he didn't make clear to me what he saw, it seemed to me and to lots of people that it was quite interesting. I wish I could remember what Pauli's objections were. It would be most interesting to know how deep he could think at that time. I'm sorry. I have no way of recalling. Wheeler may remember.

Weiner:

That might be the only way really to find out. Now, you mentioned he wanted to do a grand paper. He was doing that while you were working on some problems.

Feynman:

Yes, and making the quantum theory.

Weiner:

The culmination of this grand paper was what?

Feynman:

The culmination was, his grand paper has never come out. This grand paper, as a matter of fact, gradually grew until it was to be a series of five papers, of which this thing is Part 4. Isn't that so? Isn't there a nutty footnote —?

Weiner:

Yes, here it is, right here.

Feynman:

Well, I was never with him on that. I could never understand.

Weiner:

This article, paper, written in 1945, presented in 1945, says that "This has been in preparation since before the war by the writer and his former student, R. P. Feynman."

Feynman:

Right.

Weiner:

"The accompanying joint article, representing the third part of the survey, is the only section now finished."

Feynman:

Third part, all right.

Weiner:

"The war has postponed completion of the other parts."

Feynman:

Right. Now, I was not involved in the other parts. See.

Weiner:

I see. Is this essentially the paper that you originally had prepared?

Feynman:

It's modified, but the subject that I worked out with him, that we worked out in 19__ — before the war — in which he asked me to write the paper — I wrote this thing up in 27 pages, which we could have sent in to a journal, but he began to think, "No, it's too great a business, we'll write it good." And that of course made delays, and got interrupted with the war, and he got it so big that it was five parts — the whole reorientation of physics from a different point of view. I never went along with him on that. I mean, you know, with the idea that it's so marvelous, it's a reorientation of physics, you have to write five papers, and all of physics is turned upside down. But I felt that 27 pages were what it deserved. This was written mostly by him. See, it was a rewrite of the 27 pages, so to speak. I wouldn't say a rewrite because he didn't use the 27 pages as a basis, but the same ideas, are developed, which I tried to write much more briefly, and which he tried to write in an historical context, about the arguments of Tetrode and Einstein — you see, it's a relatively long thing, and I didn't really write it, you understand. I worked with him. But it was not in the spirit in which I thought it should be written.

Weiner:

Were you present when this was delivered?

Feynman:

Submitted. Instead of being submitted in its most simple way. Now, what I had done to simplify it — I didn't like this, I felt this wasn't a good way to present it, it makes it too complicated, there's a more beautiful way. Then when I needed it later, when I started to talk about quantum theory and so on, I printed my own paper.

Weiner:

In 1948.

Feynman:

Yes, in which I reviewed this, so to speak. But really what I was trying to do is write my own paper, but I couldn't say everything again, so I had some things I had to curtail to make it shorter. But in 1948 or something I wrote on the classical theory of action at a distance, or some such something. I can't remember the name of it.

Weiner:

“A Relativistic Cut-off for Classical Electrodynamics?”

Feynman:

Yes, and in that paper in the beginning is an introduction to the ideas that we had worked out. Now, let's see. Yeah. But it's also modified a little bit. Yes, you see the mathematical formulation of action at a distance, and how it's equivalent to the standard theory. I've only got one page to say everything, you know, because I can't — I never was in a position to publish it the way I wanted to. I had to refer to this thing. But I did it the way I wanted to. I had to refer to this thing. But I did write this presentation, so I improved it, but it's still very much too brief. It's a little unfortunate. It was published rather briefly in connection with a modification of it, in this paper. But I had to make an excuse even to publish what I did. See, I said, “A brief summary of the point of view is given here,” and I referred to this paper of Wheeler's.

Weiner:

In another paper in 1948, you refer back to the 1945 paper — you did refer to it in your paper on “Space-Time Approach to Non-Relativistic Quantum Mechanics,” and in that paper you said that the 1945 paper was an attempt “to quantize this theory presented in 1945 led to the present paper” — that is, to the new paper on quantum mechanics. Then later —

Feynman:

Yes — well, that's the quantum theory, but here in the classical theory I refer to this paper too.

Weiner:

Yes. But at one point, though, you did say, I don't know the exact paper, that this, the idea presented in 1945 will not work in quantum mechanics.

Feynman:

That's right. I don't think so. Right.

Weiner:

When was it that you were convinced of this?

Feynman:

About that time.

Weiner:

About 1948, when you wrote this paper.

Feynman:

Yes, when I finally did do quantum electrodynamics.

Weiner:

I think before we plunge into that —

Feynman:

— I'm still not absolutely sure of that. But some of the ideas certainly, almost certainly, don't work. I think the idea that an electron cannot act on itself is probably false. Because an electron and a positron are the same particle, reversed in time, in some situations, when the pair have been newly produced. Electron and positron do interact with each other. And my other view is that that's the same particle, backwards in time. And so I don't believe my own ideas. See?

Weiner:

I see. Before we plunge into this thing, which does represent really a new departure, I'd like to go back to some of the setting around Princeton in the forties.

Feynman:

Well, before we do that, I would like to instead tell some of the subsidiary things that we thought of, associated with this theory. There were so many side things. One was to try to represent gravity by a somewhat similar method. Another was a — rather hard to explain in a simple way. We tried, we began to thin that space — it was interesting, space is nothing but the result of experiences or interactions between objects. So it's not nice that we write our theory in terms of coordinates and distances, because after all it's only the interaction of the objects that counts. And so we tried to re-express the electrodynamics without saying anything about geometry or dimensions or space, by inventing an idea which is, if this particle can act on that particle — you see, we would order along a world line the moments by some parameter, the time, proper time, but not necessarily proper time. The scale is not defined. We don't want any lengths. But there's an order, by this variable. Now, there's a connection between points on two lines. That

is, this one can affect that one, the retarded connection, and there's another connection from one value of the variable, the parameter, along one path, to two values on another path. That connection is really, if you had the geometry, that this time on this path is on a light cone from the two times on the other path. So therefore we had a one to one correspondence, rather than a two to one correspondence, of points on one line to points on another. And that's all there is in nature. You can describe all these things, and the orders of them, you see, to send signals back and forth, back and forth, and watch the way these timings change. We would hope to describe all of the electrodynamics through just the ordering and the mapping of these lines, these points, from one line to another. We didn't succeed in that. But just telling you some of the things we worked on.

Weiner:

This is all at Princeton, though.

Feynman:

Oh, at Princeton, yeah. Some of the things we tried. We tried some rather elegant things. We also developed a number of modifications of electrodynamics which were within the range of experiment, which could be distinguished experimentally, but which were possible ones, in which the interaction was not exactly on a light cone but slightly off. Then, there was another thing — I don't remember. We tried all these different ideas. We had this new expression of electrodynamics, and so we tried to drive it as far as we could in every direction that we could.

Weiner:

When did the idea of the thesis —

Feynman:

— oh yes, I remember the other idea.

Weiner:

Good. Whenever I try to lead you away —

Feynman:

— we got this space-time view, this view of action integrals and action in distance, and pads in space and time instead of fields, and so on, that we were thinking about. Then Wheeler called me up one day and he said — I answered the phone in my dormitory — “Feynman, I know why all the electrons have the same charge and mass, all different

electrons.” I said, “Why?” He said, “They’re all the same electron.” So I asked him what he meant by that brilliant idea. He said, “You know, we always make the world lines go one way, but suppose the world line of an electron is one enormous knot, going back and forth in space-time, just one line, going back and forth. Then when we cut it in the place of present time, we’d have a large number of intersections, which would represent electrons.” I said, “Oh. Yeah. Very nice.” He said, “It turns out that the back section, where it’s going the other way, the proper time’s running the wrong way, corresponds to a negative charge. You see why?” Then he explained why. I could see it from our action principle: you change the sign of BS and change the sign of the charge. So that was — I said, “Yeah, but where are all the positrons?” “Well, maybe they’re hidden in the protons somewhere or something, the back sections.” I said, “Ok. It’s a nice idea.” But what I liked about the idea was that the positrons were electrons going backwards in time, and that world lines could be inverted. This idea I kept in my mind, although I didn’t go so much for the fact that all electrons are the same electron. He always liked to prove it to the most dramatic point. I just took the backwards-moving electrons as very likely candidates for the positrons. Because here we had a theory that we could represent both electrons and positrons in classical physics in a very simple way, by reversing, by letting the world lines go backwards and forwards in time. So it was pretty good. He had a lot of good ideas, Wheeler.

Weiner:

Yeah. He seemed to be able to deal with the ones that you brought in too.

Feynman:

Yes. Well, what he did, you see, things like — I’d like to remark that the moment he mentioned advanced waves — that is, against causality and all this other stuff is against cause, the causes would precede the effect — no, the causes would follow the effects instead of preceding them, and so on — I didn’t ever say, “But that’s impossible!” or anything like that. I was not ever upset by any of the obvious troubles, as against some principle of causality or something. This was from the training we had in physics from Einstein and Bohr and so on. See, the history of physics was that a crazy idea like relativity, which is so evidently nutty — like when one man thinks two things are simultaneous, some other guy riding by doesn’t say so — or, that you can’t measure simultaneously position and momentum, or something — It had been discovering that you must always think carefully about the real experimental situation before you cavalierly say such a thing is impossible, you don’t like it. So I never objected to any of these crazy ideas, on those grounds. I never said, for instance, “How can it go backwards? How would it know when it’s going to meet an electron?” I knew that that was something we would have to study — that that wasn’t obviously cockeyed. The fact that there were protons and not positrons were an obvious trouble, but I let him get away with it, so someday we’ll discover how the protons go, wind up in this knot, too. But never mind. His brilliance, the wildness of his ideas, apparently impossible ideas, did

fall on fertile soil, because I never objected to what other people would immediately have objected to, you know. All the books would say we can't use advanced waves because this would mean effects would precede causes. But things like that never bothered me. I don't give a darn. I never thought in terms of cause and effect necessarily, anything.

Weiner:

Well, the whole concept of time involved here — just even thinking of the philosophical thing —

Feynman:

Those didn't bother me, until we would sit down and analyze and find out, this is necessarily against experiment. That was clear, that we always had to do that, because you see, it would be too easy to object — it was a lesson that you can't object to Einstein's ideas on Page 1 in spite of the fact that they look like they're wrong. How can something shrink when it moves? Sit down and analyze if it's not impossible. But it isn't impossible, see? That we had learned. I'm telling you this because it shows something about the history of physics, the connection — that the lessons from these other men were just precisely that. Don't take it too quick that it's obviously wrong, just because it says something nutty, because you have to first make sure that the nuttiness is really nutty. In other words, take a real experiment; think very carefully that you will get an advanced effect that is directly opposed to what actually happened. When we tried to do that we didn't get anywhere, see? We didn't find such a thing. We got around all the paradox. So everything's OK. So I know. These things never bothered me. And as soon as I tell people these ideas, they often come to me with all this, "Wait a minute, how's it going to —" But I never had that trouble, in the beginning. The history...

Weiner:

— was it a self-conscious reference to history, or an absorbed tradition?

Feynman:

Probably an absorbed tradition. Just an absorbed tradition — that you know that nature can look very, very strange, in the fundamentals, and yet produce in the end the natural phenomena in a way, very different looking than you would think at first. It's all right. You've got to think it out, you can't just jump that it's wrong.

Weiner:

Just to return — how did this relate to the choice of a thesis topic?

Feynman:

Well, I didn't get the thesis yet. The next stage was to make the quantum theory. Then I started to work on that, very seriously, for my degree. The problem was to make a quantum theory of this classical theory. And the classical theory, in the form I preferred to express it, was the principle of least action, involving particles only, no field, and in which there were two times, different times — the interaction occurred at two different times. It wasn't just velocities involved, but coordinates of different times in the action principle. The usual action principle, about coordinates at a certain time and velocity — but these would involve, interactions were instantaneous, anything interacting was at the same coil at the same time. This involved delay, and it meant that there was no Hamiltonian for the system, and none of the standard things that you have to have to convert by the standard method to quantum mechanics — find the Hamiltonian, find the momentum operators, and so on. There was no momentum operator, because the action was of a new form. So the problem was to find a quantum theory of this thing. It was not so easy. And I started to work on that for my degree.

Weiner:

When did you start on that?

Feynman:

Well, presumably about the time when Mr. Wheeler started to write this paper, which must have been in 1941-42. 1941 or 1942.

Weiner:

1941 — you got the degree in 1942, so —

Feynman:

It was probably not long after I had given the paper in the meeting in Cambridge and so on, because I probably wrote the thing up about that time, between that and the spring meeting. We're guessing it was the spring meeting. Yes, probably it was written up by me about that time, and then Wheeler said, no, he's going to write it, and I said, all right you write it — and then I went and worked on this other problem. I can tell you about that problem, but that's another story. Which way do you want to turn now?

Weiner:

Well, I want to get all these events in this 1941 and 1942 period. The reason I kept mentioning the thesis is that I have probably assumed that the other things were directly

tied in.

Feynman:

They are. It's a direct attempt to get the quantum electrodynamics. And now I'm stuck with the problem, as I saw it: I have a classical theory with an action, but not with the Hamiltonian, and how do I go from the classical theory to a reasonable quantum analogue? The standard method of going to quantum mechanics from classical mechanics assumed there was a Hamiltonian. Well, there wasn't — in this form. If I expressed it in the terms of fields or something, there might have been. I was very reluctant to do that, maybe incorrectly. There probably was another way around. But I insisted always to represent only the particles. In fact, it was because my idea was to get rid of the degrees of freedom in the field, and so I always [crosstalk]... — the field back again, sure. And I loved it. I had a minimum principle of action which involved only the particles, with delayed interaction, just like I wanted — the program of MIT was completely satisfying, to make a classical theory with direct action and distance, with delays as well as advanced. Only the coordinates of the particles were mentioned in the fundamental law, which was the principle of least action, and everything was satisfactory. MIT, first step, to fix the classical physics, was done. And OK — on to the quantum theory. Well, that — I'll tell you what happened there, OK? I started then to work on the quantum theory of this thing more or less freely, while Wheeler was fiddling around trying to write the paper, once in a while being asked by Wheeler to check some energy thing or something, but it wasn't very often. He told me at some stage, "Don't bother to work on it, I've already solved it, the quantum thing" — but this was all through this year when he was having trouble. But I worked on it anyway. "You've solved it? But you didn't tell me, I've still got to find out," and so — The first thing I tried — I tried a number of ways. What I tried to do was find the quantum theory in a more general situation, any kind of action, not necessarily this particular action, by making simple models of actions which had the same consequence, such as a harmonic oscillator interacting with another harmonic oscillator, with a delay. Put the essential in but keep everything else simple. I solved that problem, the quantum theory of that, by figuring out what the oscillators would do in their interaction, and it turned out that's two new modes, the usual way; even if there's a delay you find two new combinations, each of which oscillates like a simple oscillator. I quantized those, and I was trying to get back to the original coordinate somehow. Although I could do the harmonic oscillator case, it was too simple and didn't give me a clue as to the general theory. I was struggling with this problem, when I went, for relaxation, to the Nassau Tavern, to a beer party. That's at Princeton. And there was there a new fellow who had come from Europe, whose name was Herbert Jehle and he came and sat next to me and said, "What are you doing?" You know? And I told him what I'm trying to do, the quantum mechanics. I said, "Listen, do you know any way to go from action principle to quantum mechanics, from classical action to quantum mechanics?" He said, "No, but Dirac has a way, has said something about going from Lagrange to quantum mechanics." Well, that was closer — the action is the integral of Lagrangian. It's a little closer to the Hamiltonian.

So I was quite interested to see that, and he said he would show that to me the next day. The next day we went to the Princeton library and he got this paper by Dirac, from the shelf, and we went into a little room there. They have little rooms there for discussions, with blackboards, and we looked at the paper. And Mr. Dirac said — it's a little technical now, but — there's a kernel, a function, that takes you from the wave function at one time to the wave function infinitesimal time, epsilon, later. I could write an equation, but

Weiner:

— can't now —

Feynman:

No. And he, Dirac, said that this kernel was analogous — Well, to make a technical word that somebody else did — the transformation operates between X and X prime, when X is the coordinate of time T , and X prime is the coordinate of time T plus epsilon.

Weiner:

Resuming the discussion now, we are going to review the discussion that was previously garbled on the tape (section just preceding, not transcribed) At the end of the last reel, we described the situation with Jehle and the Dirac paper, and you were about to describe what Dirac said and what you did with it.

Feynman:

Yes. Will you please check what's going on with that tape, that everything's going onto that tape correctly, please.

Weiner:

All right.

Feynman:

To explain what was in the paper, I want to refer to some equations. I had them written in space-time approach to non-relativistic quantum mechanics, written in April, 1948, for the REVIEW OF MODERN PHYSICS. There was a quality in quantum mechanics, defined by Dirac, which was an integral kernel to carry the wave function from one time to the next instant of time. It's defined in the equation just after equation 33, the function of X prime. And Dirac pointed out that this function in quantum mechanics was analogous to the exponential of i times epsilon times the Lagrangian, where the

velocity for the Lagrangian you put X minus X' over epsilon, and for the position just X . It said in the paper that the two things were analogous. Jehle showed me this and I read it and I said, "What does it mean that it's analogous? What is the significance of saying that something is analogous to something else? "It just means that it's similar, it's analogous in some way." I said, "I don't know. What's the use of that? It can't mean anything, it has no use." Jehle said, "You Americans, always looking for a use for a thing?" So I said, "Well, Dirac must mean that they're equal. It doesn't mean anything otherwise." He said, "No, Dirac doesn't mean they're equal." I said, "Well, let's try and see if they could be equal," so I substituted one expression for the other, and calculated what the wave function would be at the next instant, and found that if I didn't make them equal but rather proportional, by multiplying by a constant, that as a matter of fact it was equivalent to a statement of the Schrodinger equations. So I worked out the Schrodinger equation from that right on the blackboard, and turned around to Jehle and said, "See, Dirac meant they were proportional." But Professor Jehle said, "No, no, Dirac didn't know that, you have just made an important discovery," and he was very excited and copied everything into his notebook. I didn't realize — I was only trying to interpret Dirac — but he realized that I had discovered something that wasn't known. He said, "You Americans, always trying to find a use something! That is a way to discover new things." He was quite convinced of it after that. I never was sure, really sure, that Dirac didn't think they were proportional until way later, in 1947, when I saw Dirac at Princeton. He was lying on the grass, and I said to him, "By the way, Professor, you know that paper in which you say those quantities are analogous," and so on. He said, "Yeah." I said, "Did you know they're proportional?" He said, "Are they?" I said, "Yes." "Oh. That's interesting." That's all. He didn't seem to be very impressed or anything. Anyway, apparently Jehle was right; he didn't know that they were proportional. At any rate, then I had connection between the quantum mechanics and the Lagrangian. I could explain the Schrodinger equation direction in terms of the Lagrangian, and gave the wave function at one time in terms of the wave function an instant later. It must have been a day or two afterwards, I was lying in bed thinking about this thing, and thought, "What would happen if I wanted to get the wave function, and at finite interval later suppose that the interval was divided into a large number of small steps? That to carry the wave function one step I'd have to multiply by either the i epsilon times the Lagrangian and integrate, and then to go to the next step I'd have to multiply that by the i epsilon again and integrate again and so on, for the end steps." And so I saw this multiple integral, this end fold integral standing there, with all the variables, and the product of all these exponentials of i epsilon times the Lagrangian. But the product of exponentials is the exponential of a sum, so that it was clear that it was the exponential of the sum of epsilon times the Lagrangian for each successive position, so to speak, on a path. I could represent the coordinates that I was integrating over as a succession of positions through which the particle was supposed to go, and then this quantity, this sum, would be like an integral, the integral of L , which is in fact the action. So I suddenly realized that to get the wave function at one time and the wave function at earlier time, I had to multiply by E to the i times the action for a path running from the original position to the final position, and sum it over all possible coordinates of the

path. I say it more clearly now, probably, than I visualized it in terms of paths and so on. I probably had it more or less in terms of coordinates, instead of thinking of paths, but I had the idea. I saw the action expression.

Weiner:

You were thinking of this at the time.

Feynman:

Yes, I saw the action expression, suddenly, so to speak.

Weiner:

Did you see mathematical symbols?

Feynman:

In the air, in the head. Yeah. You see the action coming on. And I said, "My God, that's the action! Wow!" I was very excited. So I had filed a new formulation of quantum mechanics in terms of action, directly. I got up and wrote everything out, and checked back and forth, and made sure it was all right, and so on. My aim was immediately to try to substitute the other action, the action with the delay in it, for the more simple cases that I had done before. There were two problems that developed. The first was that the action I'd had for electrodynamics involved only, either that there were no interactions, was relativistic, involved particles according to relativity, and I couldn't see how to do the relativistic case by this path integral way. It didn't work. There were certain square roots in the formula for the action, and when you integrate over the paths the square roots became imaginary and there were all kinds of trouble. On and off ever since I have tried and tried to extract the Dirac equation from this kind of wave, without putting in special operator or other mathematically different things, and I've never succeeded in getting a satisfactory way of doing it. There are several ways but I didn't like them.

Weiner:

Has anyone else made an attempt to do that?

Feynman:

People have, yeah. I've seen them do the same thing I was trying, getting about the same, usually less, and being more or less satisfied. But it's no good. No, you can't do it — only by putting operators in it, different algebra in and so on. I was trying to understand the different algebra and the Dirac equation by some more simple view. At any rate I

still, however, had the possibility of describing the interaction of non-relativistically moving electrons, like interaction through light. That is, I'd take the light correctly with all its delay, but the matter would be described by a non-relativistic approximation, and for that I did know what the action was, and that was perfectly clear, what I ought to substitute in the action to make the quantum mechanics. There was one difference. My action would have, in its interaction, a delay. See, let's say there are two particles interacting. Then the coordinates that have to be described — two coordinates, say X and Y for the particles, one X, the other Y — and the paths over which I've integrated give Xs and Ys at different times, all different times. It was easy in my form to represent an interaction — this is between X and Y — is instantaneous; the coulomb interaction is instantaneous. Then I would have to take the X and Y and the same time. But it was very easy in the action form to simply suppose that the interaction was not at the same time, but the time would differ by the distance over C — that is, there would be a delay or advanced effect. So it was just as easy to write instantaneous interaction as it was to write delayed interaction. Then I realized that I had a representation of quantum mechanics which was in fact quite powerful. I would like to interrupt to say, I had to develop some views and ideas about the viewpoint and the conventional viewpoint of physics at that time. All of the quantum mechanics had been previously represented, as well as ordinary mechanics, by differential equations, and the equation of Schrödinger, $d(\psi)/dt = H(\psi)$ had with it the philosophy that we know all the conditions at a certain time, and the equations tell us how to find the conditions at the next instant of time. So we need to know enough — the present, in other words, determines the next instant. Well, if we had a direct interaction from one particle to another, not from fields but a direct interaction, then what's going to happen during the next instant does not depend on where the other particle was. So if the action is instantaneous, it's easy to write a differential equation because in knowing only where the particles are now, at X and Y, we can find out what they're going to do next. But if there's a delayed interaction, to know where the particles are now is not sufficient. You have to have known where it was. And in fact, the field variables, from this point of view, were merely some way of keeping track of what the particle had done in the past, so you could predict the future. So you had to have an infinitely large number of new variables, to keep track of what's going to happen to particle X because Y had been moving in such and such a way in the past. Because all you have is the present position of Y. You see, the difference between instantaneous interaction and delayed interaction, which is physically not great, was mathematically infinitely great, because there was a complete change in the formulation. You have to have an infinite number of field variables, where you could have an instantaneous action before. For example, the coulomb interaction was represented in an ordinary Schrodinger equation, but if you wanted represent a delay in that interaction, you had to invent a whole new system of electrodynamic field. But with regard to this action method of describing things, in which you had the particles out in space and time, an instantaneous interaction and a delayed interaction appeared to be equally representable. One was just that the two times were equal; the other was that the two times were different. So I realized or thought that I had an inside track on these otherwise complicated situations. I could represent complicated interactions, what used

to be complicated, due to electromagnetic field, oscillators and everything else, by as easy a way of representation as an instantaneous interaction. There was no real difference in it. Also, as soon as the interaction was delayed, the concept of wave function then had to disappear. I didn't believe any more in wave function, because the wave function gives the amplitude, to find the particles in various places all at a given time. And I was going to get the same thing later. But if there's an interaction with a delay, that's not a convenient way of expressing things, because to know the amplitude, to find the two particles at the same time, is too special a thing. And it's not enough to know what happens next. But in the action form, in this E to the IS business, with the delay, I still could analyze things, by a more complicated idea, a different idea. Which is — it's a little hard to express — but the idea of the wave function just would disappear. I wouldn't have the wave function, but I would have an amplitude that objects would get from one condition to another. But the condition couldn't be so precisely specified if they're still interacting. For example, if they came from far apart it was easy. If they came from far apart, the interaction could be neglected, and then as they come together, the E to the IS with all its delays starts operating, then they go far apart again and the interaction's no longer important, and I can get the amplitude for that. But I don't have any wave function in the intermediate region or even precisely at the beginning and end. So I had to develop some different physical ideas. Many of these are discussed in my thesis, on how the physics should look from an action point of view when there are delays — later on, the thesis that I wrote on this subject later.

Weiner:

Why don't you talk a little bit about the things that were in your thesis? This is the logical point to approach it.

Feynman:

Well, it isn't, but all right. Let me — no —. Then when I put that action is, I thought I had the right electrodynamics, and having been trained by Wheeler to check everything — you know, what happened to the energy theorems, what happens — while I'm fooling around — I got into slight difficulties, and got kind of jammed up here and there. I can't remember exactly how. And stopped. I felt I couldn't get it. Incidentally, at that time I got involved with some war work, which I'll describe how later, and decided to put the thing away, that I couldn't do it. Professor Wheeler said to me, though, that I ought to just take the E to the IS part, for the ordinary quantum mechanics, that that's enough of a thesis — I think, as a matter of fact, that he probably felt that the work I did on the classical theory was really enough for a thesis, but he had always tried to get more out of me. Anyway, the E to the IS stuff would be enough or ordinary action would be enough for a thesis, I didn't have to do the electrodynamics too. But it was funny, I was a strange fellow, because I thought, "No, that's not important at all, because that's exactly equivalent to the regular representation — it's equivalent — so it's just another way to write it, it doesn't add anything. What I really have to get is some contribution,

make something different, which is this electrodynamics." So I wasn't satisfied to do that. So I stopped working on it, and worked on the war work for a while and then realized that I had a problem, since I had simply put it in the drawer at the time when I started the war work at Princeton. I realized, already I had enough experience with research to know that you can't read your own papers later unless you really write down something, for a fool, what it is you're really doing, you see — what everybody is — not just at the equations. So I realized that I'd better write this down or I would forget so much, I wouldn't make head or tail of the partly analyzed research. So I asked for leave from the war work — I don't know, a month, three week, six weeks or something — and it was granted. So I went back to do it and it was psychologically — and it was guarded. And I felt very guilty because the first day I just relaxed. I lay out on the grass and looked up at the sky all day and I felt kind of guilty about it. I did this for two, three days, but then my mind started to think. I was supposed to be writing it up, but instead I was sort of thinking about it. Then I got ideas which straightened it out, or at least I thought they straightened it out. I finally thought I had everything straightened out on this retarded interaction theory and all. Instead of just writing it up so I could understand it later, I wrote it up as a thesis and handed it in, showed it to Wheeler and Wigner, during all this time, of course, and got my degree successfully at that time. I didn't finish the thesis. It turned out later, as I found out later, there were still difficulties, and the thing was not satisfactory, but I didn't realize it at the time, in my excitement. I thought I had solved what difficulties I had previously seen. But it was only a temporary error, by which I thought everything was all right, and published the thesis. The parts of course that are all right are still a representation of quantum mechanics without a delay a delay and so on. But the generalizations that are contained in the thesis are probably erroneous, as written.

Weiner:

But you subsequently referred to it — or did you ever make reference to it in your published works? Or did you just absorb it?

Feynman:

I mostly absorbed it, but there were things in the thesis which I didn't publish in other work. Apparently, you see, since I worked independently, there were a lot of things that I would notice that were noticed by people before, often written up satisfactorily but very elaborately, perhaps, or not very clearly, whereas I saw it very clearly and had it nice and neat. But there's no use publishing something like that. As a matter of fact, there's too much of that kind of publication — the same thing somebody else does, you do it better — a little better, not violently better, so that it means anything. Then you republish and republish. There are things that students may have difficulty — It's interesting; I just get off on a tangent here. There may be some idea that's difficult to understand the first time you study it. For example, Einstein's theory or something like this. And a man trying to learn it can't understand it. Later he finally understands it — say, when he goes to teach

it, he finally understands it. He thinks that his particular way of understanding it is a very much clearer than the way it was presented to him before. Therefore, big deal, he puts out a paper — new way of looking at it! Actually, it's not a new way. I mean, maybe it is a little bit new, but it's very personal, and it's not sufficiently different. It's not really that much clearer than the other way of doing it. It just happens that that's the way he happened to come finally through the wicket, so to speak, you know, in that direction. And there's an awful lot of that kind of papers, in which somebody supposedly understands somebody else, and is going to improve on the demonstration of it. But it doesn't really improve. It's just a different demonstration. So, there was much in my thesis which was of this kind, things that other people had done, and I never even checked the references, but presumably other people had done. But there was a discussion of principles of least action in classical mechanics, and the problem of the definition of energy and momentum under these circumstances. They were defined in a general way. I think that this is quite early for this definition, but I don't know if it wasn't published earlier — certainly it's been published since. There were a number of analyses of ideas, of the difference between action principles and the ordinary differential equation things, when there is delay in the action. There was one problem of interest which I've never solved, which I tried to solve a little bit after the thesis. I might as well mention it. I wanted all the consequences of classical mechanics for which you needed Hamiltonian's, Lagrangian's ordinarily, to be generalizable to the case where the actions had delays. I had here a method of doing quantum mechanics which at least I thought was satisfactory. Its ordinary mechanics is easy enough. You take the variation and get it. But there's one branch of mechanics which still came from Hamiltonian's and the classical world, and that was statistical mechanics, in which you could demonstrate that the probability of different states was E to the minus the Hamiltonian over KT , and so on. Question: if you had a mechanical system working on principle of least action, in which there were delays, not solely the simple action, you're given the action function but there are delays and so on, what is the statistical mechanics corresponding to it? I believe that a system with delayed interactions of this kind can get into equilibrium at some temperature, and therefore that certain motions have certain probabilities. But to find a general description of what is the probability of a given motion at a finite temperature, directly in terms of the action of the mechanical system, when that action is not simple, it's just a function of X , I've never quite worked out. It was always something I wanted to do. That one I didn't work out. But I did do many other things, asking many questions about how the ideas were to change, of ordinary mechanics, when instead of having the action be a function of velocity and position, it would be functions more complicated, involving at different times and so on. A sort of generalization of most of the things in mechanics, with the exception of statistical mechanics. And some of those things are discussed in that thesis.

Weiner:

How many pages did it run?

Feynman:

Not very long, I don't think. You can find out.

Weiner:

Yes, I've got a copy on order.

Feynman:

Forty, I guess.

Weiner:

Then this was published by Princeton?

Feynman:

Well, I didn't know whether it was published or not. They took fifty bucks from me and I went —

Weiner:

— well, it was made available, I think, on University Microfilms; some were then published in DISSERTATION ABSTRACTS, and someone can call for it if they want.

Feynman:

Well, I didn't see it for years and years, and I kept asking Princeton for a copy and I couldn't get one. Only about two years ago, a year or two ago, that I finally got to look at it. I have a copy in my desk, at last. I seem to have lost all the copies and not had any.

Weiner:

It would be interesting if we could find out from University Microfilms, (that's how a person would order) how many requests they've had over the years, to see whether anyone reads these things or not.

Feynman:

I don't think these are a good source — really.

Weiner:

People don't go to them?

Feynman:

Today people don't go to theses. Things that are any good in theses I believe should be, and are, always published more widely in a regular journal. It was only because of the war, as a matter of fact, that this was not more widely published. It was because it was interrupted the work. Ultimately, after the war, I began to publish bits of it, in this article on "Space-Time Approach to Non-Relativistic Quantum Mechanics," and so on, but it's not organized the same way as the original thesis. It's a little different. But it's the same subject.

Weiner:

Just going back to the oral exams that you had to take as qualifiers, I guess you'd call it

—

Feynman:

— yes, I forgot to talk about that. That is earlier, of course.

Weiner:

Yes, this would probably be some time in 1941.

Feynman:

And Princeton, as far as I can remember, had no restrictions on the courses. On the opposite hand, they had a qualifying exam, and a very stiff examination, where I think you had to do the written exam for either a whole day or two days, and an oral exam where you had to go from one group to another. There were at least three groups of professors to get examined by. And so it was quite difficult, and there was a lot of atmosphere, graduate students always worrying about it, and various graduate students having flunked it once or possibly even twice. You only get three times. Worrying about it and studying again for it and so on — it was quite a thing. To study for it, I went to MIT, where nobody knew me anymore, it would be quiet, and worked in the library, stayed in a room that the fraternity fellows gave me, and studied there, without interruption. The way I did it was I just simply reorganized all my knowledge of physics. I wrote a book, I mean a notebook, which is called "Things I Don't Know About." And I summarized all the subjects as best I could. Actually I was kind of proud of that notebook. You see, I brought each subject down to some kind of kernel, like statistical mechanics, that the only thing, I realized by working back and forth, that the central item was the probability of occupation of a level is E to the minus energy over KT , and that

all the various other forms and so on come from this in various ways, and I indicate how. So I organized every subject in physics so that I knew where the essential was, and what the derivative was. There was a way of putting it together so that the subject was no longer so complicated to me. When you learn something, there's so many parts and pieces, it looks complicated, and it's hard to see the pattern or order of it sometimes. And when I went through all these subjects, I tried to grind it to a minimum memory proposition, you see. And that meant a logical understanding of the interrelations, and I worked quite hard on developing this thing. I worked on it with a sort of consistency — instead of only on the subjects I thought they would ask, I did all the rest of the physics that I knew exists, such as quantum electrodynamics, quantum relativity, and other things are in there too. They were all in that notebook. I knew that they wouldn't bother me with that, but I wanted to kind of finish my summary of physics. And I used that summary from time to time when preparing a course in some branch. But nowadays, I know. I mean, I've got the views in my head so well that I don't even — I don't know where the book is, I'll try to find it. That's the way I studied. I don't know how long, maybe a month, two months, something like that. Two months, probably. Then I went back to Princeton and took the examination, wrote things and went through the thing, passed it all right.

Weiner:

How about some of the questions on the exam that you remember? The oral, I guess.

Feynman:

Well, there was something that I had trouble with about a rainbow, about which color was at the top of a rainbow. You know, students are more afraid of the examinations, and they think they fool the professor, (I'll give you an example) but they don't. They asked me which color was on top of a rainbow. I said I didn't know, but I could figure it out. They said, all right, figure it out, and I drew a drop of water and said, let's see, now, the red rays are bent more than the blue — And the professor said, "Would you draw a curve of index refraction against wavelength?" And I thought: aha, I got it wrong. So when I drew the index versus wavelength I turned it around so that the index was higher for the blue end than for the red end — the opposite of what I just said. When I looked at my curve I just drew, I said, "Oh yes, excuse me, it's the other way around." But I drew the curve because I knew from the question it was backward, and I thought, I'm fooling him. Of course, he'd just given me a hint that I was wrong on the other. It was just a game. Robertson had asked me a very interesting question about relativity and aberration, which I answered incorrectly. When you look through a telescope at a star, because of aberration the star looks like it goes in a little circle. And how would it look the other way? So I argued it was all right, the earth looked like it goes in a little circle too, from the star. We had quite a tussle there, and I talked him into thinking everything was all right. As a matter of fact, that's quite wrong. The circles are different size and they're not related. The relativity fails because we're talking about an accelerating system.

You go in a circle and it changes velocity. But anyway I convinced him that I had answered it correctly, when I had not. Then there was one good question I remember, because it was rather fun. Wheeler asked me, "Here, I'm reading from a textbook on optics by Jacobs and Why," which was a very good standard work on optics, and there it said, he deduced, that a hundred atoms would have fifty times the intensity of light if they were randomly phased as one atom. I said, "Oh, that can't be, it must be a hundred times as much intensity." Then we looked, what's wrong with the derivation? And it was some subtle error in logic. But it's easy to make subtle errors in logic. I was rather shocked, though, to discover that these men could — first, that it was in a text; second, not the error was so — anybody can make a mistake like that — but that you don't think about the consequences of the result and realize that it's absurd. Because the same logic on two atoms, precisely the same — it didn't make any difference how many — would say that two atoms would be just as intense as one. And that's a little fishy, and they should have worried about it a little bit. Anyway, that struck me, to find such a gross error in a book.

Weiner:

He proposed that as a question.

Feynman:

Yes, to find out, he asked me what did I think and how would I criticize it, yes. Actually I think he put it in the form, "Would you deduce that result, please? How did they deduce that result?" And I must have said, "I don't know how they deduced that result because I think it should be a hundred." A kind of trick —

Weiner:

Without indicating that the result was wrong —

Feynman:

— I believe so, yes, without intimating that it's cockeyed, yes.

Weiner:

He quoted it as if it was official.

Feynman:

Right out of the book, and asked me for the derivation of that result. I didn't fall for that one. It was a pretty good exam, altogether.

Weiner:

After the exam, as you indicated, you got involved in some war work.

Feynman:

I remember a friend of mine, Fox, said to me, right after the exam, asked me what kind of questions and I said, "The one I had the worst trouble with and got wrong, came close to getting wrong, was which color is at the top of the rainbow?" So this guy Fox says to me, a graduate student, "Oh, that's easy, I'll tell you how to do that. You know, there are two bows, when the sun is good there are two bows. Right? You know that?" I said, "Yeah." He said, "Well, just remember — reds repel — but that's wrong." So I'll never forget. The reds are next to each other. So he told me to remember, "Reds repel, but that's wrong." It's so silly that you can't forget it after that.

Weiner:

Then you were involved, as we discussed, in some war work, which we'll get to. Then you did the thesis. Let's get the next steps here. The commencement took place in June, 1942?

Feynman:

Yeah. Yeah. My parents came. It was a regular commencement, with academic gowns and so on.

Weiner:

Anything special?

Feynman:

No. My parents were proud and all this.

Part III: The War Years

Weiner:

Then you mentioned about the war work that you got involved in at Princeton.

Feynman:

How'd I get involved with the war? What relation was there with the war? Well, first, I

had sort of patriotic feelings, you know, because during that time, in 1941, even before the war began, there was lots of pro — helping the country business, you know patriotic feelings. There was a lot of talk, a lot of businessmen going someplace, Plattsburg, in upstate New York, and making their contribution, and all this kind of stuff. So I thought I'd better. I mean, my ability to do physics and so on might be of some use in the war. I tried to find a place where this technical ability was useful, rather than just — I mean, I didn't want to — I didn't think it was wise just to go in the usual way. I wanted to make some use out of what I could do. So I had a friend, previous fraternity brother, who'd been in the Signal Corps, the ROTC connected with the Signal Corps, so he referred me to some important character in the Signal Corps, a major or a colonel or somebody, in New York. I went to his office and talked to him, and told him I was technically able, and the Signal Corps could use such technical things and so on. He told me I should join in the regular way and maybe go to Plattsburg, I think it was, or somewhere, and go through the regular basic training and become a second lieutenant, and then they could use me, technically. I said, "I don't think that's a good scheme. I mean, there must be a more direct way of doing it." He said, "Well, that's the way it's organized. That's the way the Army works. If you want to make a contribution, that's the way you have to do it." So I went outside in the park and thought about it. I wanted to make a contribution, and I realized how dumb was the organizational scheme, but after all that's the way it was, and if you're going to do something you've got to do it with the way the world is, and so on. I mean, I didn't know any other way. However, I thought it was possible that there may be, and I'd better not take the first thing I think of. There may be a way to go more directly. I'll keep it in mind. Fortunately I decided not to go and become a second lieutenant. However, I want to explain — you see the level of the strong feeling of wanting to make a contribution. When I at last had gotten a job at the Bell Telephone Company for the summer of 1941 — I'd been applying for summer work for three years or so every year — when I at last got the job I wanted so much, because I liked Bell, then a man come down from the Frankfort Arsenal. Big deal — they need physicists. Physicists are needed! The need six physicists, he was interviewing men — and so on. It's the same feeling of patriotic stuff, so I figured, OK, here's my opportunity. So I talked to him, and he offered me a job, and I wrote to Bell Labs and told them I had this chance to do some war work and I felt that was more important under the circumstances and so on, and they excused me from my arrangement. They said that they did have some war work if I wanted. I was quite foolish. I would have been better off in the Bell Labs, for my own sake, and possibly even made a bigger contribution. But anyway that's what happened. So I went and I worked at the Frankfort Arsenal, there, as a sort of an engineer — I checked gears, partly — part of the time they used me to check drawings of non-circular gears and complicated things. You had to check everything, and it wasn't easy for anybody to do, but anyway it was a kind of waste of things. It was only after several — near the end of the summer — that the boss realized more than I was useful and I was getting practically to the point where he would call me aside, when nobody else was in the office, because he didn't want the other engineers of the same rank to get worried, and we'd talk about quite serious questions. The whole plan of the director. They were building a director, mechanical director, for shooting down things, and certain

general problems, instead of just the little things. Then, when the end of the summer came and I wanted to go back to Princeton, they made me all kinds of promises. I would be able to design my own director, and go down and get the information from so and so, and such and such and so on, and they showed me a sample of the data — and the shells wouldn't even go off for the director I was supposed to do, because they were fuses that were powered — the fuse would be timed by air burning some powder, and they were supposed to fire at high altitude out of airplanes, and there wasn't enough air to burn the powder, so the data stopped. And it was this kind of insanity — Anyway, they promised me a lot. It would be the whole job. I could tell them what data I needed. I'd be a big shot. But they'd given me the same stuff at the beginning, when I went to join the place, about what I'd be able to be doing there, and it wasn't what I was doing. And I'd learned quickly about this that it's easy to promise but they won't do what they say. I don't mean just one place. I find that the lures that are put out in industry often exceed the reality. The reason is that when they get a good man in an industrial position, they promise him something — like, to leave him alone, he can do whatever he wants. And they have somebody there, and to have suddenly a serious commercial or military problem of the company — they know this guy can help them, and there's the guy, he's hired, and the guy himself, in spite of all the promises, feels a responsibility toward the company, and he destroys himself. He can't help it. And this is very common. Anyway, I learned the lesson, a little bit, anyhow. It was a hot summer, and it was a lot of work, but I kind of enjoyed it anyway. I liked their engineering too. It wasn't so bad. But I wouldn't stay there.

Weiner:

This was in the summer of 1941, I gather.

Feynman:

That's right. I couldn't stay there, with that thesis I was working on. So I went back to Princeton, to work on some more on the thesis, and while I was working on it, one day, in the morning, Bob Wilson — I don't know how many months after this was—came into my office.

Weiner:

Was he on the faculty of Princeton at the time?

Feynman:

Yes. Yes. A very young member of the faculty. And he said, "I have something to tell you. I'm not supposed to tell you because it's an absolute secret. But since after I tell you, you'll work on the project anyway, there's nothing to worry about." So he told me that they were going to try to develop the atomic bomb in the country, and that they

needed a way to separate uranium from the two isotopes, and he had invented a scheme, a buncher scheme. He explained to me how it worked, to separate the isotopes. There were a lot of technical, mathematical, theoretical problems associated with it, space charge limitations, how current should carry a beam, and other devices, and they were going to try to develop this thing, and he wanted me to do the theoretical work to help them along. I told him he'd made a mistake, I'm working on this problem here and I'm going to finish it, and I'm not going to do that. I guess my patriotism had disintegrated or something. I said that he'd made a mistake to tell me, but that's all right because I'm not going to say anything to anybody. "All right," he said, "we're having a meeting at 3 o'clock and everybody who's going to work on it'll be there, see you at the meeting." So he went out. He's a great fellow. Anyway, I went to sit back at my desk again, but I couldn't work. I began to think about this, you know, and the importance of it, the great seriousness, and so on, and what would happen if Hitler got the device before we did, and went through these things. People said, "Don't you feel guilty?" afterwards. No, because at that moment is when I did my thinking of the seriousness of the situation in the world, of the possibilities of making a bomb, of the dangers that would result if the other side were to have done it if we didn't, and it seemed to me absolutely clear that we must really make an effort to "save the world" from the other possibility. It's true, it turned out later that they weren't doing very well, but there was no way at the time to know, because it was possible. We showed it was possible. It was not impossible, therefore, that they would do it, and if they did, that would be terrible. So, anyhow, I went through all that thinking, and decided indeed I would come to the meeting at 3 o'clock. So I took my thesis and put it the drawer. That's what I referred to later or earlier — that I had to write it down later. I put it in the drawer and that afternoon at 3 o'clock I was in the meeting. The meeting didn't last more than 30, 40 minutes. Various guys said, "We're going to do this and that," what they could do. Then they picked out a room, and they gave me a roll top desk and lots of paper and I started to figure that afternoon, right after the meeting, what the limitations of current would be. It turned out that the current limitations were very severe and we couldn't see any way around it, and there wouldn't be enough uranium separated to make it worthwhile, you see. So it was an important thing to decide as quickly as possible. But, not to waste time and wait for this guy to figure it out, they started to build apparatus to make experiments, to build arcs and so on, and it was for me — the first day was like those cartoon movies. There was some apparatus in the room, and every time I would look up it was bigger, and bigger. It was being put together like in those cartoons, you know. Fellows running in and out, putting together the parts. What they were doing was, they were raiding their own equipment. They had all decided to stop their research and to work on this problem, and they were just taking apart their vacuum systems and pumps and putting it together in a new way. That's why they could do it so fast. And I was calculating as fast as I could. Finally I figured that there was a limitation, yes, but it was at a high enough current that it was probably feasible. It was feasible to make this thing work in reasonable way. So I said, "Ok, it'll work." I was very impressed by the experiments, how they were putting this stuff together. I was at party some time, much after the war, at which the guy was describing how it looked to him at Princeton. He was one of the experimenters. He said

they'd got this plan, they were going to do it, they started to build the apparatus, slowly putting things together, getting ready to make measurements — and all the time, on the question of what charge, there was this guy Feynman sitting behind his desk, "And each time we'd come in to do something, there's pieces of paper flying in all directions, and finally climbing out of the paper, with all the paper falling on all sides of him he says, 'It'll work! It'll work!'" So anyhow, that's the way it looked from different sides. So I got involved in that project at that point. I can describe the details of that project and the rest, of finally going to Los Alamos and so on, and at Los Alamos, at some other time; there's a good chunk of material. It's sort of isolated. The two works are completely different, you see. There's also the personal life at that period. So, which do you want?

Weiner:

Let's take the personal life now, and then we'll use the other as a continuity into the later period.

Feynman:

Ok. During this Princeton period I got married, and I would like to tell about the problems that were going on, associated with that. I had met this girl, Arlene, when I was 13 or 14, and we grew up together, so to speak. There was a considerable exchange of ideas. It is, I think, of some interest. This probably happens to everybody, but anyway to me it seemed like independent and personal, that her feminine softness and different view of the world — and she was an artist, too — of what was valuable, what was beautiful, and so on, were things that I didn't ordinarily have direct interest in — like the lack of interest in humanities, in a way. But because of her interest in these things and the love that was developing between us, I paid a lot of attention to these matters, and softened up. I became a better guy as a result of the relationship and of listening to her ideas. I think also, vice versa. She was an ordinary girl, as far as her views about what she should do or say is concerned. I felt that honest, straightforwardness — that one shouldn't worry about what people think. As a matter of fact, they're not really thinking, they're just giving some dumb opinion, and what you should think out is, are you hurting anybody or are you not hurting anybody? And you decide whether you do it or you don't do it on that basis. And if you're hurting them because they're foolish in thinking something, then you judge whether you want to pay attention to their silliness or not. You understand what it is that's happening, and don't act by some rules always, but think about. Not by some rules. And so on. We developed our views of looking at the world together that way, and I was considerably modified, but also she was modified by this kind of scientific character that she was associated with. There has been much talk about the way scientists look at love and so on, and I think it isn't really quite right, that science is not a dull, hard, cold business, but as a matter of fact I believed then, and I still believe, that if used right it gives you a way of looking at the world and at the meaning of things that are happening to you that gives you some control and calmness in otherwise difficult situations, and so on. We tried to develop this way. So that's why I mention

these matters. It's interesting in some respects. Anyway, our relationship grew, and we loved each other, and we got engaged somewhere along the line. Also, to show you the absurdities of the scientific mind, however — sometime after the engagement — I felt I was engaged, I'd asked her to marry me and she'd said "Yes" — and sometime after, I was saying to her, "How did you feel when I asked you to marry me?" Dumb questions! She said, "Which time, the first or the second?" And I didn't know that there were two times. I only remembered one. Then she said to me, "No," and she told me about the two times. It turns out, the first time, I had said to her, "I would like you to be my wife," and what I meant was, "It would be a pleasant idea." The second one was a proposal, which she considered the second time I asked her — it was, "Will you be my wife?" — which is quite different to the scientific mind! But I can hardly blame her, now that I've kind of grown up a bit, with the first error. I thought at the time, "But that's not what I said, I didn't ask you, I was just dreaming about how nice it would be, it would be nice —" Well, anyhow, that's the kind of trouble that we had, some kind of silly trouble. At any rate —

Weiner:

Did you become engaged at 19, I think you said?

Feynman:

I believed I got engaged — 7 years — 20, when I was 20 years old, roughly, and I don't know when that is. We were engaged for six to seven years without getting married. She had visited sometimes at Princeton, stayed at Wheeler's house. Wheeler was very kind and so on. These things went on a long time. The question is why we were taking so darned long. Because today fellows marry easily, relatively easily, and don't worry about interference with their life and so on. However, at that time the view was very different in the world, and especially in my father's mind, and then also with the authorities of the schools and so on, that marriage would only interfere with the career. I don't believe now that that was correct, but I did at the time, and so we waited a long time, till everything was straightened out. During this time she got some kind of illness. She got some bump on the side of her neck and so on, and she got ill, and the doctor said it was typhoid fever. So she was in the hospital and we wore all these gowns and so on. What I would do was go to the medical library at Princeton, get a book, a big book out, that tells all about typhoid fever, all the details, how you make tests for it and everything else. I learned about it. When I went to the doctor then, I asked him, did he make a Whitehall test, or whatever it was. He said, "Yes, but it was negative." I said, "Then it's not typhoid fever, is it?" So he complained to the parents. I said, "Why are we wearing all these gowns? To protect us from the bacteria that you can't even find when you make a test from some intimate material. What's going on?" Also there were very strange symptoms that she noticed, that had nothing to do with typhoid fever — a swelling of the glands, bumps in the neck here, and swellings somewhere else, this would change and get on the other side. It was all crazy. It had nothing to do with the disease. So I asked the doctor if

he — you know — knew what he was doing, so to speak. I just said, “How could it be typhoid?” The response I got was that the parents, who otherwise liked me very much, her parents, who were in charge — because I was not married to her, I was only her fiancé — got very angry with me, and said not to interfere with the doctor and not to say anything to him or bother him in any way, and it was none of my business, and so on. This was a difficult position for me.

Weiner:

She was in a New York hospital?

Feynman:

Which hospital was it? In Far Rockaway. The doctor was an incompetent, but the parents had confidence in him. But I knew he was just an absolute incompetent. At any rate, she got better, temporarily, from the so-called typhoid fever, and then got ill again. This time she went to another doctor, who at least had the sense — he felt around under the armpits and this other place and found other swellings besides the one on the neck, and said it was swellings in the lymphatic system, and he doesn’t know what it is, very hard to figure out. It’s the lymphatic glands that are doing these things, because he found them in different places. At least, that — the next step of brilliance is to find out. I went to the library again this time. This doctor was trying to help her. She wasn’t in bed now, she was walking around, but she felt uncomfortable from time to time and had these swellings. So I read about lymphatics — these symptoms — you look up, “effects of the lymphatic system, swelling of the glands.” So it listed the causes of this, and the first one listed was “local infections, like a bad tooth or some other such thing.” But this was obviously not local, because it was running all over the place. The second was “tuberculosis of the lymphatic glands,” and I looked that up. It said immediately that this is easy to diagnose, it’s just like ordinary tuberculosis, and this and that. I knew the doctors were having so much trouble. In fact, excuse me, by this time it was more complicated. That’s why I knew the doctors were having a lot of trouble. This doctor sent her to the state hospital, and there they had trouble diagnosing it. In fact, they brought her into a room with 30 doctors, a big thing, to try and figure it out. Nobody could figure it out. But under the lymphatic system things it said, “Tuberculosis, easy to diagnose.” Then I read the other ones — “lymphodynema, lymph adenoma,” all kinds of stuff — and every single one of them was a fatal disease. Hodgkin’s disease and so on. So I deduced that she had a fatal disease and wouldn’t last very long. I remember reading this, and then going to have tea at the Fine hall Library, and thinking, while everybody’s milling around, “It’s rather interesting what’s going on in one mind, while the others don’t know.” It’s an interesting problem. Anyway, I wasn’t sure, but it was very, very likely. So then I went and I discussed it with her. I didn’t have any doubt that I should tell her what I’d found out. You see, this is funny, because we’d come to it, from lots of experience, that we should tell each other everything, absolutely, and that we could face any reality. The thing you can’t face is not knowing what somebody’s cooking up. That’s

hard, not knowing. But any real thing, you just sit there, take it for reality, and see what you do under the circumstances. So she was this way; in spite of everything that people said, we started to figure it out. So she suggested to one of the doctors, "Could it be Hodgkin's disease?" and he wrote that down, "Hodgkin's disease?" with a question mark. After that, it got to be Hodgkin's disease, if you know what I mean. Everybody looked at it, they agreed. So that disease had — until they finally decided — now, how did it work? Oh, when they finally decided it was (that's right) the doctor of the hospital came to me and said, "Listen; now we know what it is. It's Hodgkin's disease, and it's a fatal disease. There are remissions for a while. You can live outside the hospital for a while, then you have to go to the hospital for a while, then you go away again, and so on. Ultimately it's fatal." "Now," he said, "I don't want you to tell her this. We are going to tell her that she has glandular fever." I said, "No, I'm not going to tell her that she has glandular fever, I'm going to tell her what she has" — you know, so we could solve the problem of how to live. They say: "No." The parents were against me again — they think I'm crazy and so on. The doctor says, "I know everything that happens to people, you don't know what happens to people like this!" I said, "I know that girl. We've talked about" — because we'd already talked about the possibility, when we weren't sure, you see, and we knew what we were doing. Finally they said... You know, I began to think about it. I went home. My parents talked to me. They said, "We're older, we know — you're just young, wild, you don't understand. You can't tell her a thing like that." My family doctor, whom I liked very much, my own, came over, a special trip, worked on me, and so on. Everybody worked on me. When a lot of people work on you, you know, you could think, "Maybe I'm wrong." So I thought, "Maybe I'm wrong." So I said, "Ok," finally. And there was really quite a difficult experience, because I went to the hospital — she's still there in the hospital, and there's people standing around there, and I come in, and she turns to me — nothing could have been built worse, you know — she turns to me and says, "Ah! They're all telling me I'm all right, I've got glandular fever. I'm glad you came, because I know from you we'll find out what the real situation is!" Ah! But I had decided not to tell her. I said, "Yes, it's glandular fever." She was immediately and completely relieved. There was not a bit of doubt in her mind. But I thought, if she ever discovered that I had done this to her, we were finished. That's what I thought, because I had done such a — we had built this up for so many years, and it was my idea to be honest and straight-forward, and that that was the true value and so on, and that it was really important, so you had someplace to hold onto — and I went against it, under all this pressure. So I wrote a letter explaining what had happened, how I did it, excusing myself, and saying good-bye, so to speak — I had to write something — which I kept in my pocket, against the day she ever discovered. Then I would give it to her and go away. One day she called me up on the phone. She had gotten home now. She was home for a while. She called me up: "Come over, I want to talk to you." So I came over, letter in pocket, as usual, and she said, "What — sit down on the end of the bed — now, tell me: what disease do I have?" So I said, "You've got Hodgkin's disease and it's a fatal disease." I knew she had found out somehow, see. She said to me, "All right then, don't worry. I know that you must have been under terrible pressure, for you to lie like that, but just never do it again." The first thing that was in her mind was the

trouble been through, see — not that she was in difficulties. This was the kind of person. She was a great woman, you know. Really great. She had found out. She had lived in this house with her parents, who had all this lying, trickery game, you see, which I had trained her against. So she knew how to do it. She crawled down the steps one night, when she heard her mother crying to the neighbor, weeping, talking about it, you know, downstairs, very low voices, little sobs. She figured, "This thing can't be so simple." She went down and listened in a little bit. Then she realized that I had done her in, so to speak, but that I must have done it under great pressure. I showed her the letter, but she said, "It's all right. Just never do it again. You were right, and you must stick to it," you see. So anyway, when we knew what it was, we decided we must make plans. Instead of going to the Telephone Lab, where I had this job set up, instead of continuing in my work — because it was only going to last a few more years — I would stop working, we would get an apartment together, when she was well we would be as happy as we could be. Then she would go to the hospital, and so on, until the end, and I would temporarily interrupt my career for this purpose.

Weiner:

You'd sort of take a leave from Princeton?

Feynman:

Yeah, in some way, you see. That was the idea that I got. I tried not to take a leave from Princeton. I wanted to stay at Princeton. I went to the authorities, to the dean; I told him the situation, that my wife — that my girl — has an incurable disease, and will last only so many years, two years at most or three or something, and that I wanted to marry her; could I be married to her and stay in the university? "No." I couldn't keep my scholarship or whatever it was, and be married to the girl. It was very severe. So there was no way to continue at the place with the scholarship. I have a financial problem, see, that I had. And be married to the girl. It was quite serious. I was surprised at that cold answer. So we cooked up a different scheme, that we'd live in Long Island, I'd work at some — I think the Telephone Company, the lab, somewhere — I can't remember the details, but anyhow, I had it all figured out. Well, in the hospital they took a sample from one of the glands, in an operation, biopsy, and they hadn't got a return on it. So a few days after this, when we got everything figured out — or maybe it was a week or two — I got a call from her, and she said, "I have some news from the hospital. I want to talk to you." I said, "Is it good or bad?" She said, "I don't know." So I went over and talked to her. She said, "It turns out, they've found tuberculosis in the glands, and it's not Hodgkin's disease. I have tuberculosis, and I'll last at least five to seven years and maybe in fact get better." So I said, "What do you mean, you don't know if it's good news or bad news?" She said, "Well, we had it figured out so good, that we were going to get married and all, and now we won't for a while." So it shows you, you see, that even under the worst circumstances, you can solve a problem to the point where — I had to convince her, it was good news. Anyhow, she stayed in the state hospital, and I visited

her, about once every two weeks or once a week, something like that, on weekends, going all the way from Princeton to Long Island, also from Philadelphia to Long Island. She got gradually more ill. Then the war began, in December, and I got this war project business, and I wasn't working anymore on my degree. I'd got it and there wasn't any reason now not to get married. I was getting money from the war work. So we got married. I borrowed Bill Woodward's station wagon, and we put mattresses in it. I had found a hospital in New Jersey which was closer to Princeton that she could stay at. We transferred her from her house all the way to New Jersey. While she was sick we got married, in Staten Island. We crossed on the ferry: that was our honeymoon ship. And we got married, and I took her to the other hospital.

Weiner:

When was this?

Feynman:

We were married in June, 1942.

Weiner:

After commencement?

Feynman:

Yes, that's right. She ended up in a hospital called Deborah Hospital, which is a very good one. It was by the Garment Workers or something — the money supporting it — and I made contributions to it. But the man who ran it, the doctor — he liked me. He didn't want me to make the contributions, but I did. I didn't have much money, but I saved what I could, and I felt they were doing a good thing. So she stayed there all during the time when I was at Princeton. I'd visit her every weekend. That was the way it was. We wrote letters all the time.

Weiner:

Was she in the hospital, then, from the start of your marriage?

Feynman:

Yes. It was rather sad, in a way. She tried very hard to get better and did everything they told her, all kinds of things, like having some kind of a weight, a sack of lead shot on her shoulder or something, lie still, all day long. It was quite a thing. She worked hard to get better. It didn't work. To finish the story — maybe I should just keep on telling it? When

we ultimately went to Los Alamos, I had to take her to Los Alamos, and they had a little problem of finding a hospital there. Oppenheimer helped, and we got a hospital. When they went to hire me, I told him my problem, and he found something. So everything was set to move out to Los Alamos, and we packed all her stuff in boxes and so on and everything, and put her on a train. I went with her. We got a nice suite on the train, and we went all the way across the country — it was a nice ride — got her out to Los Alamos. And then she was in a hospital there. We'd moved to different hospitals a time or two — it doesn't make any difference. Every weekend I would visit her down there. She was in a pretty good state there. That was a happier time. It was less severe, the hospital rules, and life was more pleasant. She had many things in her hospital room, books to read, a record player, and all kinds of stuff. When I would visit her — we'd write letters, and have all kinds of games that we would invent, to play, and she was very ingenious in cooking things up. Then she got interested in studying things like Chinese calligraphy, which she would do with brushes, and so on, so she had quite an interesting time. It wasn't so bad there. I would come down on weekends and cook steak on the hospital lawn, on a grill, and so on, and we'd have the steak to eat. And it was kind of fun, it was Ok; we had a pretty good time — under the circumstances. Well, she gradually got more ill, and finally died, just before we made the test at Alamogordo. She never did know exactly what I was doing. But she didn't desire to know, I don't think. I mean, she knew it was secret stuff. The situation at the time of her death, and what happened to me on account of it, is interesting, but it's connected to work at Los Alamos. I knew she was getting sicker. So I knew that sooner or later I would have to go down there. She was in Albuquerque, which was 100 miles from Los Alamos, and I used to hitchhike and use various ways of getting down there. But I figured, in the emergency I'd have to get down there somehow. So I had some friends who had automobiles. One of them was Klaus Fuchs, another was Paul Olum. I had ahead of time arranged with them, could they lend me their cars under the emergency situation? When finally I got a telephone call from the hospital, I should come down there; her father was visiting at the time because he had heard of this trouble. I borrowed Fuch's car, and I drove down. It was a little old rattletrap thing, and the darned thing got a flat on the way to Santa Fe. I had picked up some soldiers to hitchhike. First they fixed the flat on the road. Then we went a little further and we got another flat. We went into a station to get it fixed, because we didn't want to get another flat, and we had no spare. There was somebody else getting a tire fixed. The guy was going pretty slow and one of the men who was hitch-hiking with me went up to the guy and said, "This fellow's going down, his wife is ill in a hospital and is dying, see." I didn't even think to do that. It was a special situation. Well, the guy immediately stopped on the other car, explained, and fixed ours right away. I'd never thought to ask him. Anyway, on the way down to Albuquerque, we got two more flats in succession, running out of the spare, not being able to continue. And I left the car about 20 or 30 miles from Albuquerque and myself hitch-hiked. Somebody picked me up, and I got into Albuquerque, and then went to see my wife. Up to this time, she had to take oxygen through the nose, and she was very still, and her eyes would follow — so she knew it was near the end. Anyhow, I got some company to take care of the car, put new tires on it, get the car and so on. Finally — well, there was a little period.

I had a little time, some time to walk around and think about it. I knew what was happening. But because it happened slowly, I was completely adjusted to it. I knew it was going to happen, and it happened. It was very realistic. No trouble. It was to me, in fact, almost — this is terrible to say, but if it had to happen anyway, it was interesting to watch the, phenomenon, which I had never seen before, in this particular. Anyhow, finally she died. I was in her room there, and she died. They left me alone for a few minutes. I went over to kind of kiss her, like, you know — and I got the shock that most people get under those circumstances. It smells exactly the same as if it's alive. You'd think more would happen — you know? And it's the same. It's just crazy. Also, a rather curious thing I noticed — I had given her a clock, when she just got sick, the first time, and a clock with numbers that turned this way. The numbers would change. You could read it quickly, not like a regular face. The clock was with her all this time. It was getting old. I had to repair it sometimes, and fix it up. It was a little wobbly, but it was Ok.

When she died, and the nurses wrote on the paper the hour of death, 9:22, I noticed the clock — it was 9:22 — afterwards, stopped. The clock stopped at 9:22. I recount this only for the record. There are so many phenomena of this kind recorded that are mysterious, but at this particular time, I was wise enough to have remembered and noticed something. The nurse picked up the clock to see what time it was, because the light was dim in the room, and this clock was wobbly, you see — I had repaired it a couple of times — so that stopped the thing. And it was easy to explain. I say this because you always hear these doggoned stories, because there's always somebody who doesn't notice something. You say, my God, what a thing! — you know — it's documented! Well, this is absolutely true, but it's explainable. Anyway, I went back to Los Alamos. It turned out that during this period we were doing an extremely complicated calculation, a very big moment, trying to calculate what would happen — big deal — and I was in charge of this group that was making the calculation. When I came back, I started to walk into the room where they were calculating, and they said, "Get out. It's too complicated." I mean, they had a lot of things. They didn't want me to ask them any questions, they were nearly confused, they were doing something quite wonderful and complicated; they wanted to keep going and they didn't want to get confused so they kicked me out till they could straighten it out. But the thing that bothered me, more personally, when I came back, was what people would say. I didn't know how to face people. They'd say, "Oh, I'm so sorry to hear that!" Well, I was sorry to hear it too, but I knew it was coming and everything and I just wanted to keep on going. Well, as soon as I got back, I gave Fuchs back his car and told him. He saw right away how I felt. Anyway, I ran into some friends of mine, very good friends, that were working in the computing group — they're still very good friends, Metropolis and Ashkin — and they said, "What happened?" I said. And they made a long face, and I tried to make some joke about how many tire flats I had, and they saw immediately, they understood me immediately, and they just kept close to me. And it turned out that somehow or other, through the whole place at Los Alamos, everybody knew how I felt. Nobody made a long face. Everything was all right. It wasn't as I'd thought. There was only one guy, who was away, and came back, and he gave me the usual, "Oh so terribly sorry" and so on. But only one guy. So that was quite interesting. Another thing that

struck me as very interesting was, Fuchs knew what I wanted, so he tried to keep something going, so when I came back he said, "Let's visit Peierls, he's sick in bed," so we went over to see Peierls and said, "Hello, you ought to get better," and so on. Then we went to visit some people called the Deutsches, and they were very interested in psychology and psychoanalysis and all this stuff, and she was sort of a psychologist at this place and so on. I was sitting there thinking to myself — no, I was sitting there eating grapes, talking. Fuchs brought me over there. We just talked about this and that, different things. All the time inside it's going, "My wife just died, the one I love so much," and so on. But I wondered, you know, if I can act calm enough so they don't even notice anything's the matter — these great psychologists that observe all this stuff — So I'm thinking, how it's possible to have a mind which is so completely involved in some other thing, such deep experience, and yet, nobody can know what's going on. And they didn't know what was going on. But you can imagine my interest in the fact, when I later discovered that in that same room there was my friend, Klaus Fuchs, sitting there, within his mind the same thing going on. I mean, not the same situation, but the same idea. That he's leading a natural life —

Weiner:

— a double life —

Feynman:

— a double life, and I was, for a short time, leading a very double life. I remember definitely thinking to myself; is it impossible to really know, even by an astute person, like these people were, what somebody is feeling, if he doesn't want you to know? And at the same time when I'm thinking that, the de Maupassant twist of the thing, you see, was the delight later to discover that, while I'm thinking proudly how they don't know what I'm thinking — Fuchs was thinking — God knows what they were thinking. Anyhow, that's true, the story of that. I don't know how relevant that is, but you can figure it out.

Weiner:

Oh, I think it's part of the whole thing.

Feynman:

But it's an important thing on the personality of the character. I had a great faith in that way of looking at the world — scientific — make sure what the reality is, don't get mixed up. I mean, everybody cries because somebody dies. There's no reason to cry. Why should you cry? I cried anyway, maybe a month or two later. But it's not — it doesn't make any — you can do it if you want, in other words. You don't have to make a rule, because other people expect it. That's what worried me — they'd expect me to have a

long face, I don't want to have a long face. Actually, my friends were excellent friends, and they all understood me.

Weiner:

Do you think it was a part of this special situation of being there anyway that made them more sensitive?

Feynman:

Well, we were there a long time together. We were very good friends. People were friends of each other. Everybody knew the love I had for my wife, and that she was ill, and that she might die. Everybody knew the situation. And they knew — they liked her. See, many times we'd go down and visit her, and she was always playing some kind of trick on me, from down there, which they loved, and they would always get a kick out of, and she would use them to help her. Like, one day, for example, it was my birthday — and every box in all of Los Alamos, all the boxes, you know, the mail, everybody's got a newspaper. We open it up and it says: "Entire Nation Celebrates Birthday of R. P. Feynman!" In the whole town of Los Alamos! This was her idea. She found out a place where she could write to get these newspapers printed the way you want. She made up some stuff for the newspaper. Then she asked some friend to get her a list of as many names as he could think of. That was a little troublesome because, you know, censorship — I don't know how they did it, maybe he wrote them down, I don't know what. But anyway, she had quite a long list of the names of people up at Los Alamos that she'd heard me mention, in one way or another. So there were a tremendous number of these newspapers. And so on. There were all kinds of crazy things. So everybody liked her that knew her. It wasn't that the situation wasn't known. So they knew me, they knew her, and they understood me enough that there weren't long faces.

Weiner:

We're back after a break. Returning now to Princeton, for more of an account of the transition between Princeton and Los Alamos, what occurred after graduation, or would the Wilson project?

Feynman:

I worked on this project for Wilson. There were a number of technical problems that developed. There were problems of behavior, what is now called plasma physics, the behavior of mixtures of ions and electrons. There was a theory of the analysis of this function operation, and there was calculating electric fields from systems of grids, and so on. I think we did, with Paul as an assistant; we did some fairly clever things.

Weiner:

Who was your assistant?

Feynman:

Paul Olum. He was a mathematician and he joined the group also. Meanwhile the experimenters were making tests, and they obtained results which didn't agree with the calculations, as far as I could see, and we had a lot of trouble trying to figure out what was the matter, how it was really working and so on. I learned something there, in this experience, because I felt that I held the project up quite a long time by not noticing what was causing the things that were happening — by not being able to figure it out. As a matter of fact, I had figured it out. See, they kept getting bunching at currents much bigger than I figured everything would be all right, and yet the bunches were not simple. They were mixed up and complicated. They weren't clean and everything. They never did the experiment at the very low currents that I had the thing — thought no effect of space charge would occur. What I didn't do — I never really trusted theoretical physics enough — it turned out, when we finally did understand it, that the things were doing exactly what the equations would say they would do, if that was the amount of current — namely, the bunch that was formed was springing back from their electrostatic repulsions and forming another bunch. It's like the wave that slaps back and forms a secondary wave. So we were getting bunches, all right, but not the original bunches. But it turned out I could use at the end all the equations that I'd worked out for high current densities, but didn't quite believe the thing. I was always skeptical of theoretical physics. I'd like to tell you a little dumb side story that shows my attitude. I always had a feeling, some kind of an idealistic belief that the practical man knew something, and that the theoretical business was not necessarily — a belief I don't hold at all, any more. One day I was eating at a restaurant, and a workman came by, a painter, from the nearby building. He was painting there. We got into a conversation about how a painter has to know his business in so many ways, and so on. I agreed with him. He said, for example, "How would you paint this restaurant? What colors would you use?" And I made some suggestions. He explained it should be darker lower down, because your customers make marks on the walls with their elbows, and things touch — these ideas. A very smart painter. Then he said, "And mixing paint is very important. For example, how would you make yellow?" I said, "I don't know how to make yellow. I would have to make yellow by putting yellow paint in." He said, "Oh, no, you can make yellow by mixing red and white." Well, I thought that by mixing red and white you would get pink. I was very surprised to hear that you would get yellow. Actually, I didn't think that you would get yellow, and we had quite a discussion of this. I thought maybe it was a special chemical reaction with special paints? No, it was any ordinary red and white you would buy at the 5 and 10, a red paint and a white paint, mix them together, and it would make yellow. I said I thought it would make pink. And the painter — "Listen, sir," he says to me, "I've been painting all my life. I've been working with paint, and I know what's going to happen." I said, "So all right, we'll go down to the 5 and 10, and I'll buy some paint,

and we'll try it." Here was the restaurant man. After the painter went upstairs, he said, "Listen, the man has been painting all his life and he says it's going to be yellow." I said, "Listen, I've been studying light all my life and I think it's going to be pink." Or some dumb remark. So I went out and got the paints and I brought them back, and needless to say, I was really happily pleased to discover that he was, as I expected, unable to make yellow paint at all. He finally said, "Well, you add a little tube of yellow to this pink and it'll be yellow." I said, "Of course then it'll be yellow," and we went back into the restaurant, and the painter went upstairs, and the restaurant man says, "Imagine, add a little tube of yellow to make it yellow. Of course it gives pink. He has nerve, talking to a man who's studied light all his life!" But I actually never had — it's surprising, I realize now—a real faith in the theoretical thing. I always felt that in the real situation in nature, the complexities might be beyond the idealizations of the analysis. It always bothered me, and so I didn't trust it. The same in this project. I'd tried every calculation I could think of, but I probably hadn't thought of everything — I mean, some way, electrons were doing something else, that there was some effect — I always had the feeling I'd left something out, and when the thing didn't work at first, I just simply figured, well, I'd left something out. But as a matter of fact, I hadn't. And that was the way that I kept the project back, in a way. At any rate, we did get some separation, and we had samples that we had to take back to Columbia to get tested and so on. But ultimately it was decided that this project was behind the project for separation that had started at Berkeley, which was electromagnetic stuff, and that they were going to build the plant for separation based on the other project, and that they didn't have to develop this any further. So a decision was made not to continue this one. (I forget, my order isn't right) — During this time when the project was running, from time to time, there was a group of men who were in charge of deciding what to do next, how the project is going, to make such decisions as, which project is worth following. They had Karl Compton, I think Urey — no, I don't know — Rabi, very important men. This is the first time I met these great guys. I guess Urey was in it. And they could come to Princeton to hear about the project, and the project boss, who was Wilson, and the head of the physics department, Smythe, would tell them about the project. But they would call me in because I understood it, the physics of it, theoretically, how it was working, and I would have to describe to them how it was working and so on. It was the first time that I ever saw a group of very high class men operating in a kind of committee, really great men. I was very, very, impressed. Because I would say something or other, you see, to explain some part, and then they'd start a discussion, and one man would say he thought it ought to be the other way, you know, we ought to do this, or it should be something. Then, let's say, Compton would say in a few words the opposite, and give a logical reason. Then after that everybody else would be on the other side, opposite Compton, and nobody ever said the same thing as the guy ahead of him. And then they would say, "Well, what do you think we should do?" I think Compton's argument was best." Meanwhile I was sitting there thinking, "But they forgot, he'd said the sensible thing, Compton, then they're not paying any attention to him!" And I was all upset. I was used to the kind of discussion where you repeat and repeat and repeat in order to convince the guy of what you have to say. But it was very impressive to me to hear these men discuss something. They would put out the

various points of view, one after the other, and never repeat anything, and at the end say, "Well, look, the most impressive argument was Number 6." I was very impressed with the way they did. Oppenheimer was on it too, and Tolman — these men, you know, who made these decisions. For a young fellow to hear this kind of talk going on, that was a very impressive thing.

Weiner:

Let's review the committee — it was Rabi and Oppenheimer, Tolman, Karl Compton, and Smythe?

Feynman:

Smythe wasn't on the committee; he was there because he was from Princeton. I don't know whether Urey was on or not. I don't remember. These things remind me of things — I would mention something completely irrelevant that has to do with Urey. When I was in high school, we had very little contact with scientists, not like today. It was very difficult, hard to get the books and everything else, but there was big excitement — Urey and Picard were going to give a lecture somewhere in Brooklyn. So I went with some friends to this special lecture. I was very impressed. Urey talked on heavy water and Picard couldn't make it, so his wife talked about going up in the air in a balloon. I listened to Urey on the heavy water and was extremely impressed to see a real scientist for the first time whose name I had read in the paper. His lecture was good and technical, and it was fun. And that was my first contact with a real scientist.

Weiner:

Was this at the Polytechnic Institute of Brooklyn?

Feynman:

It's possible. It's possible. It was — but also, again connected with Urey, which is probably irrelevant but I'll tell it anyway and we'll waste the tape — I had to take a sample to Columbia, because only there do they have a way of measuring the proportions of the isotopes. We thought we had some separation, and I took a sample to Columbia. I was in a great hurry, so I put on my old sheepskin coat that looked like hell and went to Columbia with this sample. When I got there, there was another man from Yale or something who had some kind of a separation system. He had a sample. He was impeccable, with a nice little briefcase and so on. I looked like the janitor. We came to Columbia and tried to find the guy to tell him, "Here's our sample from Princeton," and so on. This other fellow was talking to him about the sample from Yale. And they didn't pay any attention to me. Nobody paid any attention to me. But I knew Urey was there, and I had met him somewhere, somehow, and I went down to his office to say something, and he overheard me outside and said, "Oh, Feynman, come on in." I said,

"Ok." I came in and hung this old sheepskin up. So then I told him, I was having trouble, they were not paying any attention to me, and I had this sample. "Oh," he said, "we'll fix that," and they were very impressed. They were surprised and disturbed when they were told to pay attention to this funny-looking guy, he's got a sample of separated uranium from Princeton. He was a very friendly man, a very nice man. Anyhow, these men decided that this project would be terminated, in spite of the work, of course. I mean, if it isn't any better than the other thing, it doesn't make any difference that we did an awful lot of work. Now came a new period, in which we knew that we were going to go to this laboratory at Los Alamos, but we were terminated ahead of time. The laboratory wasn't ready. Wilson and the other young men were not the kind of guys to sit around. We couldn't sit around, see. We had it in our blood to work as hard and as fast as we could, we can't sit around. So Wilson thought that what we ought to do is to build equipment for measuring neutrons, for doing research that we would need to do associated with the bomb. But we didn't know anything. We didn't know what they were doing, what would be needed, and so on. So Wilson decided to send me to Chicago, where they knew something about it, where Teller and other people had been working, big names, Pyle and everything. See, we weren't working on nuclear reaction in any way. To find out what the — was going on, so that I could come back and tell them the whole story. See, they didn't know anything about the bomb. They didn't know any of the details. The whole story of the bomb and everything else, what we needed to calculate, what we needed to measure, what the problems would be when we got to Los Alamos, so we could start building apparatus for the future experiments to take with us to Los Alamos. Therefore I was sent to Chicago. Wilson said, "The way to find out — see, you go there to Chicago, and you say, you go into any section and you say, 'I am going to stay here to do some work. Do you have any problem that you think I could help you with?' And have them describe to you in every detail the problem, to such a point that you really could sit down and work on it without asking any more questions. Then turn around and go into another office and do the same thing." I said, "That's not fair." He said, "That's all right, that's what we're going to do, and that way you'll know everything, because we need the details, so we can make instruments and know exactly what we're talking about, no missing parts. So you must in each case know exactly what you would do if you had to calculate it. Then come back." So I did. I went to Chicago, and there I learned about the atomic bomb, how the action was supposed to go, all the details, how the pile went from one...

Weiner:

We're just beginning the third reel of tape, first track; we ran out of tape on the last reel just at the point where we described your arrival in Chicago to gather information.

Feynman:

I was sent to Chicago to gather information about all the studies — you see, they had been studying the problem in Berkeley, theoretically, to some extent, but they — or, the

bomb itself — and then they had all the problems associated with the nuclear reacting pile that they were developing in Chicago. So all the business with neutrons, fission, all this stuff, was known at Chicago but not at Princeton, and I was sent there to imbibe all this stuff and come back and report it. I talked to Teller, for example, all day long. He explained things, explained things, and so on. Then I went to one office after the other, as Wilson had told me, and he'd told me to stay there as long as necessary. I went — you see, I asked him to tell me problems, and then I would get so I knew all the details and go out again. This way I learned all of the things that were known, all about what the problems would be in Los Alamos, how the bomb worked, roughly how much energy they expected, how much uranium they expected to need, what things they had to measure, what things depended on, things like reflecting material for the neutrons. We had to measure the cross section for bouncing neutrons back. We had to measure the probability of fission, the number of neutrons that came out of fission, and all these things, what was known and what was not known. And so on. One interesting thing — I felt uncomfortable about going in these offices and having these men spend so much time to explain to me the problem and then walking out, but in one office they did spend time, explained to me the problem—it was more mathematical, a certain integral that they needed to have done, and they'd worked on it for months, in some way or other. So I just looked at it. I said, "Well, you can do that integral A if you can do integral B, directly in terms of integral B, but integral B is a Bessel function so this should be easy." So the guy looked at me. He said, "My God, how do you do A if you do B, first? And how did you know B was a Bessel function?" Because as a matter of fact, in trying to do it, somebody noticed if they changed it to B, which is not what we wanted to do, they could do it, and they found out in fact it was a Bessel function, and we had done it. We had all the tables for it for the case B. I said, "It won't take you more than half an hour to get case A." The method I used was differentiation with respect to parameters on the integral side, which is something I had learned well in high school from the book by Mr. Woods. I thought it was a delightful invention and a great idea and I used to do it all the time. I learned that trick in high school. It's the same business. I can do integrals because I've been doing them so long. They were very impressed with this. It was a man who was working under Wigner. And so they decided to take me out to lunch for this thing. And at lunch the man said to Wigner, "How can we be so silly that we didn't notice it, so stupid that we didn't notice this, all this time we've been working and we didn't notice it." Wigner turned to the fellow and said, "Don't feel bad. Feynman is a very clever man." It was hard for me to understand why they didn't think of it. It was a rather simple idea.

Weiner:

How long were you in Chicago?

Feynman:

I can't remember. It may have been two weeks, I suppose. Anyway, I felt, because I'd

done that, and it was such a hard problem, they'd spent so much effort on it, and it was done so neatly, that I'd at least paid back the efforts in time that they spent on me. Anyhow, I came back then with a little briefcase full of all kinds of information, notes and everything else. I guess I wasn't too worried about secrecy. I don't remember how I handled it. I came back to Princeton, and they got everybody together in one of the classrooms, about 30 guys associated with the project, maybe 20 important guys and I came in and told them the whole business, you see. Said, "This is the way we're going to make a bomb. This is going to be —" All this kind of stuff and business, the problems they had — "This is what we have to measure." I had it all figured out. I remember this moment, because after, Paul Olum said, "Someday when they make a history of this and they make a moving picture of the dramatic moment at which the men of Princeton learn about the bomb, and all this stuff from Moran, and the representative comes back from Chicago and presents the information, it'll be a very serious situation, with everybody sitting in their suit coats, and the man comes in with his briefcase in a suit" — he said, "Look, everybody starts talking, there are jokes, there's —" he said, "It's very interesting how real life is different than one imagines it." I therefore put this in — because it was informal, in a way. I mean, everybody was serious, we all knew what it was, but it isn't in the same kind of seriousness that the spy movie would have you think, you see — scientists working... Anyhow, that's the way it was. Then they began to build equipment to go to Los Alamos. There appeared to be some delay. I believe — although you'll have to check it because I only heard it from underwater — the delay was caused by arguments between Oppenheimer and General Groves or somebody on the question of secrecy in the place. Oppenheimer was trying to maintain that the various scientists within the laboratory should be able to talk to each other about anything, where the other was trying to get them separated. He said, "We won't do anything unless we do it this way," and so on, and there was some delay. At any rate, for one reason or another, things were going fairly slowly. Wilson was straining at the leash. We were all straining at the leash. He went down to Los Alamos to see what the devil there was there. They were building down there. He came back to complain (a) that they were building a theater before they finished the laboratory and (b) that there was nobody down there to tell the guys, the men who were building, why, for example, they needed so many gas lines or how many water lines in different laboratories, and these fellows were making changes on a lot of things. It wasn't designed right. There were lots of questions which weren't being answered. They were kind of slow on account of that, and he was upset about that, and so on. He spent a few days there straightening them out, telling them what to do, then went back. Again, still, there were little bits of delays. So Manley, another young fellow from Chicago who was head of a group that was going to go to Los Alamos, called Wilson on the phone, and the two of them arranged that they would simply go. So the groups went. We went before it was ready.

Weiner:

How many in your group?

Feynman:

Well, there were probably 20, 30 people. So we started to go. We just went. They went crazy at the other end. The housing wasn't ready and the laboratories weren't ready, mostly the housing, and they went all over and they rented ranches and so on. So when we first got there, we lived in ranch houses off the site and had cars to go there to the site. The first time, when I got there, when I arrived, I stayed at the ranch house. It was a beautiful thing, of course very interesting for anybody from the East to go to the West was a very interesting experience. Then you went up this big road up onto this plateau — you know Los Alamos and how beautiful it is and interesting — mysterious Shangri-La. We went up on this road. For an Easterner to see this Western scenery, under such circumstances, for the first time — where we were going to live; what we were going to do — it was very exciting.

Weiner:

You were all very young? The group was characterized by its youthfulness?

Feynman:

Yes. My age was about the usual. When I arrived, I arrived a few days later than the others (that having to do with transporting my wife and so on), and when I got there, up to the site the first morning, there was Paul Olum, my assistant, with a clipboard and paper, checking the trucks of dirt and boards that were coming in the gate, you know, checking them off — how many loads of lumber. Then I went into the building just finished, one of the few buildings that was finished, and somebody said, "This is John Williams — Dick Feynman" — you know. And I had heard of John Williams. He had his name on papers in nuclear physics. I'd heard of many people but this was when I first met them. He was somebody I respected a great deal, big scientist, you know. His job — he was in his shirt sleeves, sitting there with big blueprints all over the place in front of him, like he was a building inspector or contractor. He gets up, "Hi, glad to meet you," and sits down — some workman comes over and he says, "Now, you put a line in here, you put this in there." In other words, we went up there, and the experimenters who had nothing to do because there was no laboratory, finished the building. They helped the contractor. They just went in there and they checked the trucks, they carried the blueprint information over, you know, they did everything that the contractors would do, to make it faster, to help them. So it was a very exciting interesting thing. Dust from the trucks, half-finished buildings — you have to get a picture of it. We were there ahead of time. And we helped to make it, you see. I don't know if this appears anywhere, but it was an exciting business. Anyhow, after some time, after rather a short time, it was decided that — you see there were some old school buildings. There was a school there originally that the theoretical physicists should live up there and not have to drive, because they could work. The others had nothing to work with, but there was a building that was finished, and what do we need? Nothing and we had nothing. There were no

chairs in the rooms. There was one blackboard which was on rollers, for a lecture, you know; it would be rolled from room to room. We had many conferences, where we all got together, and we would outline the bomb problems, like Oppenheimer or someone there thought about it in Berkeley and they were trying to tell us all the things that they thought. So people like Oppenheimer and Segre and so on would be explaining these things. And we would sit on the floor, and borrow a blackboard to operate. Of course, it gradually improved, rather rapidly — we'd get chairs the next day — I can't remember exactly, but it was a very exciting beginning. We started working immediately, first, but learning what the others were thinking about different problems, and I had set myself a problem, from before I arrived, from stuff I had heard at Chicago, and possibly with Wilson's help. It was to make what they called a water boiler, which is to make, when they got stuff that was separated, a reaction with slow-moving neutrons with the water around it with enriched uranium, to design it and figure it out how it should be and how we could do it and what we could measure with it, and how much material we'd need and so on. So, on trains, for instance when I had to wait at the guard gate for some time the first time I came up or the second, because something was wrong, I sat in the truck cab and calculated. All the time — you see, we didn't want to waste any time and so on. There was a terrific excitement — not to waste anything, you know. So we worked very hard at this. Living up at the site, in this school place — actually it was worse than I described it. They had a lot of beds on a long balcony, and they had a list downstairs, which bed number is yours and which bathroom you're supposed to change in. Well, I couldn't find the bed number, and I found out there as no number for where I should change, and so on. It was quite a mess. But I met for the first time men like Serber, Christy, and so forth.

Weiner:

Had you met Oppenheimer before?

Feynman:

I had, because when we were trying to get arrangements he had come. I had also seen him when he came to the conferences, I believe, at Princeton. Anyway I did meet him when he came to get the men, to talk to each one individually into going. He in fact called me at one point long distance from Chicago about something involving going, and my wife, and I was very, very impressed. I had never got a long distance call from such a distance and so on. That I remember — yeah — going into it, for the first time, all these things are really quite exciting. If somebody bothers to call you from Chicago to tell you that they've found a hospital for your wife — you know, and so on. Really, they were working on it. I was sorry to bother him with relatively trivial problems when he had so many people that he was trying to get to the thing, you see. Anyhow, we did start to work and things improved a little. We got chairs and desks and filing cabinets and this and that, rather rapidly, and blackboards and so on. But in the first few weeks — there were many men I had heard of, you know, like Teller, Weisskopf — I don't know how

many I had ever met, exactly, but if I met them all I met them only from a distance or quickly, like Bethe was there. I'd only met him a little bit before, you know — not really met him, seen him — and there were all these great minds and great names that I knew of. They were great people. When I got up there, in the second or third week (I can't remember exactly) there was a kind of accident, that all of the important men had to leave. Weisskopf had to go back to check something, he was selling his house or something. Teller was out because of something. Everybody was away except Bethe, who was the head of the theoretical division, and Bethe apparently needed somebody to talk to when he had an idea to make it was Ok. He wandered around. He went into my office. We'd never met before, but he couldn't find anybody, and be started to explain his idea. I'm kind of dopey — just like it happens in the lecture that I gave where I was nervous but the moment I started to talk physics, I'm only thinking physics —

Weiner:

You mean that lecture in the first colloquium at Princeton?

Feynman:

Right. The same thing always happens to me when I'm thinking physics. I'm 100 percent involved. So he started to talk a little bit, and when he would tell me something, I'd start thinking, and I'd say, "No, that's crazy, you see —" Without thinking, who am I talking to? or anything. "Crazy" and he'd say "Why?" and I would explain — he'd say, "No, you see, you're wrong," and he'd explain back, and of course I was wrong. This went on again and again, and I kept saying these things and he'd point out I was wrong and so on. Finally he went out of the office. Then I kind of woke up, you know. I said, "My God, what am I doing? I've told him he was wrong a million times and I was wrong every time?" But apparently that's just what he wanted. He wanted someone who, he felt, was checking, really checking the thing. And none of these guys are really worried if you tell them they're crazy. They argue only on the physics, not on the human. So apparently he was very happy with this, and he kept coming into my office. Then when the other guys come back, we had a good relationship, Bethe and I. He would still discuss things with me a lot. So I kind of was lucky in that respect, you know. We discussed many things of this kind. At the beginning I was always wrong. After a while, once in a while I would catch him out, but usually not. People used to say that to hear the two of us talking was to watch a battleship and a mosquito boat, because he would plow through the subject slowing, uniformly, correctly and so on, not deflected in the one direction or the other, working something out, while I would jump to conclusion — "No, no, wait a minute, that's wrong, let it go like this," and so on. Once in a while I'd bump into something that he heading for, but usually he was going all right, you know. It was amusing. Anyway, I wanted to mention that relationship, which was quite close, and we always discussed many things together. We had lots and lots of problems. I don't know whether I should discuss all the technical problems with things. It was very interesting. Sometimes I felt that we were doing some kind of work which was new and

different and had never been done before. First, it's a pleasure to work on a problem of this kind where there's an application, aside from the war and all this sort of stuff, where there's an application, for a person who's trying to work on fundamental problems all the time. The reason is that in a fundamental problem, like what's wrong with quantum electrodynamics, great minds have already worked and not gotten an answer. Right? And it's also obvious that this is the problem, so other people are working on it. In other words, a problem is hard by elimination, you understand? So you can work very hard and get no success. If you get success you get an enormous gold ball, Ok? But the odds are against it. Whereas, on a problem that has an application, like this, nobody's done anything yet, much. It's a new kind of problem. There are a lot of new problems generated by this implicational subject. So in this engineering, this bomb building business — that hasn't been worked over. The problem, so to speak, comes off for the first time, and there you happen to be standing. So it turns out that all the problems are easier. They're all easier. And there are many of them, and you get a large number of small successes. You get perpetually little bangs, you see, with the happy result that you can work this thing out. You work that thing out, then you work this thing, that's pretty clever; then you get this, and you do all kinds of — so that Los Alamos was for me, as far as the scientific work is concerned, a very happy time, without the very long periods of frustration and confusion which appear when you're working on the more important problems.

Weiner:

You thought you were making steady gains, then.

Feynman:

Well, I was solving this problem, then I'd turn to another one. It's not the same thing. There wasn't just one problem. There were many side questions. How can we design an efficient fission counter? The fission counters that we designed had this, that and that difficulty, so — “I've got an idea” — boom, it's a great idea. Then you turn to another problem. You see?

Weiner:

I know what you mean by that, the different problems, the excitement, the many small successes. But when I said making gains, I meant on the total application, nearer your goal —

Feynman:

— oh, of course, yes —

Weiner:

— so there was some satisfaction in that, I imagine.

Feynman:

Maybe. But actually I think the satisfaction of solving the challenges directly, not entirely in terms of their environment exactly, was also a kick. See, I got certain pleasures out of just this matter of designing a fission counter. I had never designed equipment for apparatus, for experiment. I designed it. That was the first time I found out how it was to design for an experiment, equipment, and how to estimate ahead of time how many counts you were going to get and all this. I learned an awful lot at Los Alamos. That was an early problem. Then as we went on there were many problems. One that I felt we really contributed to — we did discover a way of solving — see, I got into the problem. We got groups after a while, small groups, and then there were group leaders of these groups. I was a group leader and had a group of four or five fellows, which included [Fred] Reines and Ashkin and Welton; people who knew each other and knew me, so we worked together. Welton and Nicholas Metropolis.

Weiner:

How many in the group?

Feynman:

— the leader of the small group —

Weiner:

— was it broken down?

Feynman:

Oh. The whole laboratory was broken down into different divisions — experimental division, bomb physics, experiments on explosives, experimental chemistry, metallurgy, and theoretical physics. I was in the theoretical physics division, but like other divisions, that division was divided into groups, and the groups would have somewhat different problems assigned to them, depending on how things were going, what the next problem was. This group business came a little later than the first few months. It settled down. Our group had a number of problems, and the one that I felt we did the best on — we had a relatively hard problem for the era, and that was — see, other people could make approximations; all the neutrons have the same energy and so on. We had the problem of the stuff, say, mixed with hydrogen. For instance — in which the neutron energy would vary considerably, and there was no easy approximation, they were all slow or

they were all fast. Then the collision between one atom and the next — there's no approximation — if it hits it comes out equally in all directions, because when it hits hydrogen, it goes more likely in one direction than in another. Furthermore, those which are going forward have the most energy, more than those that are going at an angle on a lower end, so everything depended on everything. It was no easy approximation. Nevertheless, we invented, to work out how much uranium or uranium hydride one would need, mathematical techniques which would bracket the answer on each side — the answer was between this and this — and the two brackets were extremely close together. I felt it was very good. It was one of the hardest problems that I did there, and I invented this technique. I invented a whole lot of other gadgets and things, and a large number of small inventions, every one of which I'm rather — well, kind of proud of. There were some rather beautiful solutions given to a large number of small problems, the best of which was this rather not so small problem. But I haven't seen it used anywhere else, nor do I know where to apply it, but I'm sure that the technique was worthwhile.

Weiner:

Let me interrupt at this point. For the first time now you have responsibility over other people.

Feynman:

Yeah, that's true.

Weiner:

How did you react to this?

Feynman:

I didn't feel it much in the group, because there were five guys, all of them friends, very nice fellows, and there was no problem of any kind. Welton would go to sleep, as he always had the habit, but we all knew it and we let him sleep, and if other people were coming around, if we could we would keep them from coming in his room, because to see him sleeping in there with everybody working so hard in the middle of the day always looked kind of funny to people. But we knew it was a five minute thing. He'd sleep for five minutes and then, zang, you know. So... Other than that there wasn't anything. I don't remember any problems at all at this level, because the people were all equivalent. I mean, although I was the leader, we were all good guys working hard. I don't mean good in a moral sense. They were all very, very, capable fellows. We all worked together on these things, and there was no problem. It wasn't as if there was a boss. The relations were very good, the same as relations of me to Bethe and our group to Bethe — there was no problem, "The other group is doing this, why can't we do this?" The whole of

Los Alamos, because of the terrific pressures involved, was extremely easy — or anyway, it was so well administered that there was not, as far as I know (at least as far as I know) a great deal of biting and clawing. There was just a complete exchange of problems and so on, and all the time a lot of cooperation between the groups, etc., etc. I don't think I will describe the special problems, I don't think that it's — Of course, it is, in a way. If it's physics, it's interesting. See, everything fits together. For example, I was very good at doing integrals, and, for a reason that I don't know, especially good at doing numerical calculations. I knew how to organize arithmetic so that you did a minimum amount of work to get the answer, and apparently that talent is not very widespread. I didn't know that until I got there. Because, on the way home from lunch often, I would walk through the computing division, the computing department. See, the theorists in different groups would give problems to the computing department. I would walk through the computing department and look over the shoulder of a girl and say, "That's wrong, that number." Things like that, you see. Or I'd go through and say, "What are you doing?" and they'd explain. I'd say, "That's not the way to do that problem," and I'd go to the guy who gave it and explain to him a way of doing it five times faster. You'd think a guy like this would be annoying, but no, everybody liked it. Anyway you could improve was all right, it didn't make any difference. There were no personal difficulties, you see. If I'd say "It's wrong" it was to help, and everybody knew it. It was no problem. So I used to be able to do this, go around the computing department. One of the (for me) most amusing things was, a man was trying to work out an integral differential equation, complicated thing — the third root of something is a complicated integral, with a kernel and everything, an integral equation with derivatives, and he was integrating this three times, because the third derivative by Simpson's rule — but he had to first calculate this integral kernel, many integrals — a long and elaborate thing. I looked at the kernel and I noticed that that operation was the one-half derivative. You remember I told you that I'd worked out that from before? I'm just telling you the connection. I looked — one-half derivative — so I figured, his equation is a non-linear $3 \frac{1}{2}$ order differential equation; it said, "the $3 \frac{1}{2}$ derivative of U is U squared," that's all there was to it. So I figured: Now, look, there's numerical ways of doing one integration, Simpson's rule — of doing a double integral, doing triple integral, see. Is there a way? Or at least there was of single integral and maybe double, you could invent them. What about inventing a numerical method of doing half an integral? So I cooked up a numerical scheme for doing half an integral in one step. Then to do three integrals, the three integrals which he did by Simpson's rule in succession, I made up a new numerical rule to do three integrals in succession and extrapolate to the next point, and it turns out by some freakish accident that the numerical method is one order higher or two orders higher than it ought to be. You see, in any numerical method you are doing some polynomial to approximate the curve. Then the error is the first degree of polynomial that's higher than the number of points that you've taken. You can't fit. Well, there's an error that comes when there's one higher order derivative. It has a coefficient, such as Simpson's rule, the 4th derivative times 1 over 180 times the integral to the 4th or something. Well, the thing that would correspond to 1 over 180 for this problem was zero. It was accident that with this particular method, the coefficient of the error was zero, and it was much more accurate.

So that was cute, that three times integration extrapolated can be done so accurately, and that was not known before. And the numerical way of doing the half integral was very amusing. I got a terrific kick out of that, and ended up inventing a numerical method to do the problem, a special problem, but this is the kind of thing that's not generalizable. But it was so much more efficient that in spite of the work I did to find the method, develop it, explain it and do it, I got way ahead of the guys that were doing it slowly. And they just stopped, because they had this other scheme.

Weiner:

Do you consider this as play, really?

Feynman:

Yes, a great deal of it is play, you see. I mean, I look for problems and I do things. I know, but play that was contributing, you see, and not fiddling around. I never fiddled around there. I played a lot, but I always played in a way that was directed. I could always explain the play as not useless, you see. There was a tremendous amount of play. That's really what it was — so many problems — I'd look for them, because I liked all these crazy things. Yes, very much like play. But always with a purpose in the end. Now, along that time a problem developed. They had had trouble. We began to get — let's see, how did it work? They began to be able to produce a little bit of separated isotopes from the plant in Tennessee, sort of on an experimental basis. And they had made surveys, I mean tests, of the proportions, of the degree of separation. They had calibrated the degree of separation at Tennessee. Then they would send us the samples, and Segre and the others who received them would measure the proportions, and it didn't agree. We were just getting tiny little samples to do experiments with that they had separated in their attempts to test their equipment before they put 200 of them together, you see. That was the stage. The plants were getting ready, but sort of pilot experiments were being done with the equipment, you see. And it didn't check. They had many things back and forth that didn't check. Finally Segre and company said, "We can't straighten this out unless we go down there and find out what they're doing to make the test. They're doing something wrong, and we can't do it by mail," and so on. This was very much against the rules. See, the Army's rules, or Mr. Groves' or something was one department like Tennessee does not have to know what's going on at Los Alamos or anything about the bomb. They just separate the uranium. And the other side, we'll tell them anything they need to know, the other side, Los Alamos, doesn't have to know how the plant works, what it looks like or anything. It was the secrecy. It sounded like a good idea. But there was this problem of communication. So finally it was broken down. Oppenheimer or somebody helped to break down this so that they would be able to go to Tennessee to talk to those guys, rather than the other way, because the secrecy was much more important. So they went, Segre and two or three other guys, went, and as they were walking through the plant, they see a little bit of what the plant is like. And the guys are practicing already. They haven't got the thing separated yet, but they're practicing with

the chemical process and so on, where it was partly built, and they're partly going through the motions of the operations. And they see great barrels of bluish-green water being carried on dollies and so on, boxes, and cardboard boxes with salts of various kinds stored in a room — and while they see it they say, "What's that stuff? Is that uranium?" "Yeah." "Well, when your plant gets operating, you're going to separate it, you're not going to handle it like that, are you, partly separate it..." They said, "Sure, why not." "Won't it explode?" they said innocently. And this caused a terrible excitement, you see. Well, to make the story shorter, they didn't know, in Tennessee. They had been told that there was no danger whatsoever. Segre saw these bottles of water with this stuff in it — realized, of course, as we all do, that when you put it in water, because it slowed down the neutrons and made them much more effective, you'd need very much less stuff, and so on. He realized that there was a danger. And he didn't think that they didn't know that there was a danger, you see. The Army's first reaction was: "It just shows you we shouldn't let these guys in." But their second reaction was to wake up. The point was, they had been told that it wouldn't explode because presumably, my guess is, that they had been told how much stuff we needed for a bomb, which would be worked dry and very efficiently to get as much energy out as possible, not to get the reaction to go at all, and so on — not just to get the reaction to go. The Army, then, hearing that number, simply said, "It's so big they're never going to get all that in one barrel, there's no danger." But the fact is that with water solutions and other chemical solutions, you could accumulate stuff to explode. So there was a great moment of excitement just prior to the plant beginning to operate, when it was discovered that a new thing had to be worried about, the safety. Ok? I just set the situation up. Well, Segre was then authorized by Oppenheimer to go through the entire plant and make a list of all possible accumulation points where there might be danger. So he and his cohorts (I don't remember who they were) made this thing and sent back to Los Alamos. See, it was an emergency problem for us, and we were set. I remember getting this thing and looking it through. Then we had a division of labor. Christy and his group calculated water solution, what the critical limits are, in the various circumstances, like in a plant — you know, what would happen if you mixed, how to do it, if you put cadmium in how much you would stop it. In some cases there was carbon tetrachloride. Well, the chlorine will absorb the neutrons and that's OK. And all these questions of liquids. I was to calculate, in my group, the dry solids, the boxes full of salts, you see, against the brick walls — what were their limitations. You can get a lot more in a dry solid than in a liquid, and it was much harder to calculate, but anyway, that's what we did, and we did it as fast as we could. But unlike calculating for the bomb, we took safety limits. It's easier to calculate something that's safe, than exactly what it is, you know. I mean, I can't say 657 so and so's going to explode. I can say, I know it isn't going to explode with 302. You know? It's much easier. You don't have to be so accurate. So anyhow we got this all prepared. An emergency business — all work at Los Alamos, a lot of work in theoreticals, was stopped for a couple of weeks or so while we did this as fast as we could. We had to do it fast because the plant was getting ready to go, and they were not allowed to go until this thing was looked into, you see, so it was a very great and interesting emergency, very exciting. So Christy is going to go tell them about the thing,

and I give my stuff to Christy, all my numbers, explain everything to him, and breathe a sigh of relief. Then he got pneumonia — Christy — and was in the hospital, and I was going to have to go. So Christy gave me all his information about water and gave me my stuff back about solids, and said, "Good day." Then I was sent across country to tell them about this thing. Oppenheimer said to me — before I left he called me up and said, "Now, about the safety thing, I want you to make sure that the following men are in the meeting when you first tell them the problem, because they're the men there that know physics enough to understand. You tell them, the situation, what to do — but don't directly tell the Army. Make sure that it's not you telling the Army and the Army is going to be responsible, because they don't know enough. I mean, they're nice, but you've got to get somebody there who knows physics." They gave me names, Webb and a few others, and so on, to do it. So I said: "Well, suppose they arrange a meeting and these fellows aren't there?" "Well, you ask for them," says Oppenheimer. "But suppose that they say no for some reason, secrecy or something?" He said, "Then you say, 'Los Alamos cannot then accept the responsibility for the safety of the Oak Ridge Plant.'" I said, "You mean me, little Richard Feynman, is going to say that?" He said, "Yes, little Richard Feynman, you're going to say that." Growing up, yeah. So I got on the airplane to go across. I went by air. I'd never traveled by air before. (Just giving you the level, you know, the way it looks to the human end of it.) It was very exciting. You see, in that day we had to have priorities to fly. Then at one place, we'd fly, and we had to land — in those days you had to land in many places, like a bus trip, you know, as you went across. I think it was in Kansas City somewhere that we got off the airplane for a while, and then a lot of big important looking cats, some generals, important looking businessmen — and some guy's standing there, swinging his gold watch round on a string, and he's talking. I look like a kid, you know. And he says to me, "It must be extremely hard to fly without priorities in these days." So I said, "Well, I don't know. I have a priority." So he's still swinging. I mean, he was such an important what do you call it — the way he treated me, you know. He keeps swinging away, you know, he says, "Well, you know, some of us Number 4s are going to get bumped. I hear there are some generals getting on here." Then I kind of leaned — I said, "That's all right. I have a Number 3." He probably wrote to his Congressman, "What are they doing? They give a priority to some kid." Anyway, with all this information, I got to Oak Ridge. (I tell this just because it is very interesting to me.) I go to Oak Ridge, and the first thing, they asked me a lot of questions. "I'm not saying anything. I want to go through the plant. I want to see with my own eyes what I got on the report from Segre," and so on. "Ok." And I went through this plant. I discovered it was worse than they thought. There were a few things, like they would describe a room that had boxes of something; then they would describe another room that had barrels of something and another room that had bottles of something. Well, they had confusion going through the plant, because they were following the process. And it was the same room that they would go in several times. The boxes are on one side, the barrels on another. I am convinced that if they had simply started to separate the uranium, they would have had an accident. I don't mean an explosion, but they would have had a nuclear reaction in some accumulation somewhere, relatively fast, and it would have made neutrons and radioactivity all over the plant, and

there would have been a terrible calamity. I'm convinced of it, from the circumstances. Anyway, I went through the plant. I kept my trap shut. I didn't go and say, "Oh, ah!" — nothing. I recorded everything in my mind, and that evening, I spent the whole night — I was practically awake the entire night — preparing for a meeting the next morning in which I would tell them the situation. I went through everything. I worked very hard on it. I have a fairly good short term memory, but not a good long term memory. When I work very hard on something I can remember it, and in all this stuff I remember the building numbers and the equipment numbers, you see, the tank No. 16 and building 9206 — because that's the way they would tell it to me. "Now we'll go into building 9206," "This is tank so and so" — all this junk, this useless stuff, I would remember. I remembered it — for one day, is all — but when I was making my calculations and figuring and analyzing and so on, I thought in terms of this tank and that number and so forth. So I was very impressive the next day, when I could tell them that in tank no. 74 in building 9206 requires this, and can be repaired, and so on. It worked out very nicely. I made a big effect. While I did it, I prepared a kind of a speech in which I'd explain how the uranium underwent fission, how the neutrons came out, about slow neutrons and fast neutrons, what the effect of water was, why cadmium would slow the reactions, and so on — in order that they could understand how to be safe. I don't believe — I didn't believe it was possible to make the plant safe, under the circumstances, because it was a complete — It's like you build something when nobody even knows there is such a thing as fire, and it evidently could have burnt up because there's a flame standing there and there's a piece of silk hanging over it, you see. You have to understand something about it to make it really stick. Well, the higher-ups had to understand — not just a series of arbitrary rules concocted by an expert from Los Alamos, but an understanding, for real safety. So the next day I came to go to the meeting. I had a lieutenant. They gave me a lieutenant to take me around all the time, Zumwalt or something, his name was. At the beginning of the meeting, Colonel Nichols said to me, through Zumwalt — he said to me that the Colonel said that he doesn't want them to know anything about — it's not necessary to tell them anything about the way nuclear reactions go, or something; just tell them what's safe. So I reported. I said to the Colonel: "I do not feel that's the way to do it, that it would be safe that way. It is necessary to give this information in order to make the plant safe, in my opinion." I was ready, of course, for the next operation: "**WE AT LOS ALAMOS CANNOT ACCEPT THE RESPONSIBILITY FOR THE SAFETY OF OAK RIDGE**" — but I didn't have to make that. Now, I was very impressed with these guys, colonels and generals. Very hard decision. The meeting is starting in five minutes. He goes to the window and looks out. They had never had this kind of exchange of information before. He asked for my opinion and I explained it. I explained why I had the opinion. He explained that it was dangerous that they should know this information. I explained that it was also dangerous if the plant didn't work. You know? Not a long argument. I misrepresent it. It was three minutes. He goes to the window and looks out the window. He comes back. He says, "All right." He makes the decision. I don't know how they do it. Anyway, then the meeting started. I went through the meeting. I told them that the plant would explode. Why? I explained about neutrons, how everything worked, how it would explode, how it had to be redesigned, but it wasn't

very difficult. In the water solutions they could put a cadmium salt, if it doesn't disturb the chemistry, and in this part of the chemistry it probably wouldn't. That is a special problem. In that case we could surround it by a cadmium sheet. In this stuff we put boron solution, because cadmium would have an effect. "In Building 9216, in Tank 74, we can do it by doing this," you see. "In such and such circumstances in the store room, we just have to get a bigger store room, and pack the boxes separated from one another, definitely, by building wooden platforms and so on, the way you put the boxes, so they can't be stacked next to one another so you get too much in one place" — etc. So I told them the trouble, I told them the solutions. I told them, "some places I haven't worked out the solutions," and so on. It was a very exciting moment for me. This was the first time when I was telling anybody anything really, you know. It was a very important thing. So I was in a great and important position at that moment. After that I had to return to Tennessee from time to time, every month or so, to give advice, you see, to kind of confirm. Like, they had started some man to calculating himself. I had given him rules and formulas, so he could figure his own things out, and he wanted his hand held. I mean, they wanted his hand held, to make sure that he was doing it right, and this and that, so I had to go back and check. In addition, the company was building a new plant. It wasn't ready yet. They were designing a plant for handling enriched material (a future plant) and with this stuff, the problem was even more serious. They had to separate things, and they had all these matters to take into account in design. So one time I returned — next time I returned to Tennessee, this company was ready with their new plans. They wanted me to check their plans, if they were safe. And one of the things we had to worry about was if valves jammed or something, like, say, an evaporator is evaporating the liquid from some uranium, so it keeps accumulating uranium, or if a valve gets stuck somewhere and stuff begins to pile up — you had to worry about all that. So they showed me these plans. They took me into a room, a room with a very long table — it must have been 15, 18 feet long and 5 or 6 feet wide — stacked with blueprints. The designers, the blueprint men, you know, the company men — they brought me into this room and they said, "Here's the design. Now, we have designed this thing so that if any valve gets stuck — not one valve getting stuck alone would allow any accumulation. We always have a safety way," and all this, it had been carefully worked out and so forth. Well, I had taken engineering drawing at MIT. I didn't remember it too well. And here are these blueprints. Well, they got started fast, because I was so impressive the time before, and they thought I knew everything, you know: I knew all about neutrons, so I knew everything. Although lots of people knew about neutrons, for them, I was like a god. So they thought I knew everything, they started right in explaining about the plant — and here's millions of lines on these things," and so here, the plant goes down, and carbon tetrachloride goes up on the second floor," and then they flipped a lot of paper up and they climbed down into the sheets of the blueprints — "Here's the second floor." They go up and down. I'm trying to follow as fast as I can, and I notice — most of thing I understand, more of less — but all over the paper there's a rectangle with a cross across it on the diagonal, and I don't know what that is. So I'm thinking to ask, you know. Did you ever —? You must have gotten into this situation: you think to ask, and then you hesitate — maybe I can figure it out — and

then the later it gets, the more they've told you, the more embarrassed you are to ask after they've told you all this stuff and it shows you weren't understanding anything — you know? So I got in deeper and deeper. I got in more and more in trouble with this. I couldn't ask. So finally I got an idea. I thought, "maybe it's a valve" — I was guessing. (This is absolutely true — I'm just telling you, this is absolutely true, incredible but true.) I put my hand on one of those crosses to find out if it's a valve, and I say, "What happens if this valve gets stuck?" — you see? To see if it's a valve. And they would say to me, "That's not a valve, Sir, that's a pyaaa..." — you know? No. It's a valve. It was a valve! I say, "What happens if this valve gets stuck?" So they say, "Well, then it backs up over here," and they go through blueprints, up to the second floor, down to the first floor, and these two guys are going up and down, they're talking, talking, talking, very fast to each other, I don't know what's going on, all mixed up — They finally turn to me. "You're absolutely right, Sir," they say. Absolute luck! I always have luck like that. I've always got crazy luck. "Absolutely right." Zumwalt, this lieutenant who'd taken me around everywhere, as a kind of, you knew, security guard or something — he just sat there with his mouth hanging loose, you know. After we came out, he said, "Feynman, I know you. I've been to see you a lot. But that performance is physically impossible! How did you do that? It's impossible!" I said, "I did it by luck." Anyway, that was part of this thing. So I got involved in general safety problem for anywhere else. I had ultimately to go to the plant at Hanford about safety. Wheeler was there and I talked to him about the safety and checked his calculations on safety and so on, but there was no particular thing. I became a kind of a safety expert from Los Alamos on these other plants, although it wasn't so much, especially at Hanford, that I was from the outside, but to make sure, and then leave. With a like Wheeler it was OK. And the men that I had taught in Tennessee were OK. So gradually there was less of this. Then, with regard to certain safety problems at our own Los Alamos place — because I became kind of an expert on this matter and would give advice — but I didn't get involved, and didn't want to get involved, with the safety of the experiments whose purpose it was to make a reaction. It was a different kind of safety. See, safety when the purpose is only to handle materials is a safe matter. Because my whole mental attitude was to be on the low side, you know. It's a wholly different problem as to how to design experiments. So I did not have anything to do with the safety of the experiments whose purpose it was to get near critical, but only handling in the metallurgy division, handling in the chemistry department, what the trucks should look like, what the safes should look like and the shelves of boron in between the blocks and so on, so that the stuff wouldn't explode in storing. That kind of stuff. I did do that. (I'm just telling you all the different things.)

Weiner:

This is independent of your group, though?

Feynman:

Well, it was, temporarily — I had to go and do these things from time to time, that's

right. That I had a new problem. Shall I go on with this?

Weiner:

Sure.

Feynman:

We had at Los Alamos — gradually it became apparent, we did a lot of calculating, and we did this on Marchant Monroes and so on. Incidentally, I used to repair those machines; it's an amusing story, but never mind. We repaired our own machines — it took too long to send them out and bring them back — so I was repairing them all the time. That was fun. Anyhow, it became clear that sooner or later we would have to compute how the implosion worked — that is, how the bomb, when it was put together, when it would explode afterwards, when it was exploding, how much energy would come out. This required analysis of the outgoing waves during the explosion, development of energy, outgoing waves, the speed at which they work. It was too complicated for analysis. It had to be done numerically. It was a differential equation in time, as the thing developed, and one variable — partial differentiation — the radical distance, and matter would be moving in this, as a function of density, a function of range. We assumed this for every explosion. It wasn't the neutron calculation, just the explosion of the bomb. Question, then, of how fast it exploded determines how much energy's coming out — it was important to make this. Then, this calculation had to be done over and over, with different kinds of design, theoretical design — with heavier materials outside, lighter materials, better reflectors, and so on. So we realized that this was coming up, and Stanley Frankel particularly saw a possibility, which was that the IBM Company had computing machines, business machines, which in those days were nothing but gear wheels electrically connected with switches that would go off from the cards. It's not transistors, it's not tubes — electromechanical, it was called. Very slow compared to nowadays. His idea was to get a whole bank, a whole set, of these machines, and to carry the cards from one to the other, and do what would be for business calculations, but instead to calculate the solution of this differential equation. That was a very important project that developed. I got involved in that because of my fooling with numerical calculations — questions of the stability of the method of calculation, how errors would propagate, the way the — whether the particular plan that they used to calculate was efficient and would operate... So I worked out all this stuff with Frankel. And the machines were on order, and they started to come. In addition, in order to test it — I always felt that a calculation should be first tested before you send it off to do — in order to test it, Metropolis and I set up a series of girls to imitate the adding machines. Each one would have a Marchant. It's like mass production. Instead of the usual way, where a girl goes through a whole sequence of operations on her Marchant, we had like an assembly line, sitting around a big table, each girl with a Marchant. The card would be sent from one to the next. The girl would cube the number on the card and write down the answer and send it on. The next girl would

subtract the cube from the previous cube and get the difference and write it down and send it on, and so on, and each of these operations would go around the cycle. So we would imitate exactly what one machine would do, you see, one step of the machine — so that we would be imitating the thing that ultimately the machines would do, so that ultimately when we got the machines and we started to put the cards in, we'd have the numbers that they ought to be giving. All bugs could be eliminated — you know. We found many interesting bugs, already, in the computation — things having to do with oscillations. Well, it's an interesting subject, and we got this group going. We got them going pretty fast, at a certain rate, that they could do the calculations. This is interesting — this was the first kind of mass production calculation ever done in this particular way, so far as I know, where you would send the stuff around like it's on an assembly line. But we did this to this to imitate machines, and we were ready. The machines came, but the repair man (that we were going to get from somewhere in the Army) who knew the machines hadn't arrived. The crates were there. But he was supposed to come in a week or a few — you never know when it's going to be, you know. So Stan Frankel and I opened the crates, and the machines were partly taken apart. We put the machines together, and we tested them. We wired them and we worked on them to make them go. By guess, by looking at the machines and the blueprints and everything, we put them together. We had some trouble. One of the multiplying machines didn't work quite right, and I was trying to fix that one. Finally the repair man came and he worked two days trying to fix that multiplier. I said, "Oh, by the way, I noticed that there was a thing that seemed to me to be bent a little bit, but I was afraid to straighten it, I might snap it off." Because, as a matter of fact, there had been some criticism: we shouldn't be allowed to play with the machines, we would break them. So I was very careful — "I was afraid to bend it, it might break off." He says, "Where?" I say, "Back in there." "Oh," he says. He bends it straight, and it works like a charm. Yeah — well, he knew that it wouldn't break. Anyway, we got the machines going, and they started to calculate the problems. I was not then involved with that program. However, after nine months —

Weiner:

— where does the nine months take you into? Sometime in 1943?

Feynman:

No, it's 1944. You'll see, because I add three months — maybe 5 months to the 9 months — and we have the explosion in Alamogordo. Roughly. Anyway, they'd been working for 9 months, and had done three problems, you see. Bethe came to me and said, "I'd like to know —" Oh, in the meantime I'd gotten another job, which is liaison between theoretical physics and all the other experimental things, all experimental laboratories. I would go to all the laboratories, see what problems they had, what things they wanted calculated, bring back the need for calculation — or if we had something worked out, explain it to them at the other end. So I would go all over Los Alamos. I knew everything that was going on. I was the only guy besides Oppenheimer who knew

what was going on in every division. Anyway, while I was doing that, besides a little bit of group leading of the group, and the safety work (but that was slowed down a great deal by this time; that was nearly finished, because they were working on their own and I merely had to check them a little bit) — Bethe asked me if I would be leader of the calculation group down there. They'd had a lot of trouble and they were going very slowly, they only did three problems in nine months. (This is a little personal, because it concerns certain people who didn't do too well, but I'll say it anyway, Ok?) I said, "What's the difficulty? Why do you need me?" He said, "Well, you're good in numerical calculations, and in the project they're getting worried about it —?" So I said, "What's the difficulty?" He said, "Personalities." I said, "Ok. Well, if you go down and find out by asking that the people that are there working would be satisfied to have me as a group leader — I don't want to get into a rat's nest, and they don't want the new director either —" So he went and checked, came back and told me "Yes." So I took up the direction of the computing machines, the IBM machines that Mr. Frankel, who had organized it in the beginning and got the idea, began to play. "The machines are fascinating!" He was very clever. The machines are fascinating because you can think of a way to use some little pulse that comes from some switch somewhere to control what the next card's going to do. Gradually, by knowing more and more what's going on inside, reconnecting and fixing selector switches and everything else, it's amazing what you can get them to do. In fact, the IBM Company didn't realize the possibilities. They have things called selector switches, but Frankel knew that they were valuable, and had many more ordered for his machines than for other machines. I found out later, when I tried to help the IBM Company themselves do a problem for us, that they never knew — none of the officials knew — what this thing could do, what logical flexibility was available. Anyway, he discovered this, but in discovering it, got fascinated to the point that he was playing. You see. He was making the machines calculate the arctangent of X and make a table and list the numbers and do all kinds of things, because he could. He'd gotten away from it. He only worked — there were many problems — he only worked in the daytime, while they worked three shifts. There wasn't any supervisor who knew what the problem was about at night. When the fellows at night would get into difficulties, something would go wrong, some little something, they'd try to fix it first, but then they would fix something wrong — like numbers are going over the range of the digits, like 9, a front digit, it would go to 0 and spill over. So then somebody may have enough intelligence to move the digits over, but he forgot to move them on the rear of the next machine, you know. Things like that. So every time they fixed anything by night, they discovered by morning that they'd just wasted their time. So it turned out then that what was happening was, every time at night something would do wrong, they would simply not do anything. They would just sit and drink coffee until morning, and so on. There wasn't any supervisor to help them out. Stuff like this. He asked me if I would be the director, and I took over from Frankel. I would describe this, because you asked me. I never led people because. This is the first time I really led people. There were fifteen people involved, three shifts of guys, and I can describe details if you want to, about my first — and only, as a matter of fact — experience administratively. The first thing I thought was this — that the men whom they had there were SED5. This is Special Engineer Detachment, selected from

different schools for being clever in scientific things, and inducted into the Army. They had them up there. They had to do some drilling, no matter what, and then they would work on this stuff. What they were doing, as far as I could see, was punching holes in cards. They were not told what the thing was about. They were not supposed to be told. It seemed to me that their talent was not being used, because they didn't know what they were doing. So the first thing I did was so say — I was introduced by Oppenheimer, by the way, to the group. See, it was very important, this transfer, and they hoped that the morale would be improved and everything would go. It was an important thing and they tried to straighten it out. So Oppie introduced me and said, "Now, Mr. Feynman will tell you that he's going to change." I got up and said, "I'm changing nothing. I want you to do everything in the same sloppy way you were doing it before, and I'll just watch. Then I'll see what needs to be changed." Because I didn't know what to do. So I went down there, and first thing I wanted to change was that the guys should know what they're doing, because I realized that they were intelligent but had no information. So I went to Oppie. I said, "In my opinion they ought to know what they're doing. Well, this was against the principles that such and such a rank should know, but he said, "Ok." I went to the Security officers. They said, "No," but then through the higher-ups, it was "Yes." So I brought them into the transformation in the morale of the whole group was fantastic. These guys had been inducted into the Army because they clever. They were drilling up in all day. Yeah? You know the kind of feeling you get, "what's this all about?" Now they were fighting the war. I explained to them why we had to make the calculations fast — because we had to make these designs, and we were going to have an experiment at such and such a time, we had to design and so on and so forth, you see. They were very excited. I also got another idea. I took one of the machines out of the line, the line of calculators. I put one of them to one side: "Play Only." Not on hours, but any time any guy wants to fiddle around with a machine, plug in things, fiddle around, he can, see? Then I did something else. Everything that I was worrying about I would write on a blackboard on a long list: "Problems I Am Trying to Solve: 1) Is it possible that in the interpolating operation so and so we could do the two operations at once? It seems to me then the machine..." And so on. You know? And I redesigned the computing cycle so that more was done on each machine, and certain standardizations were possible, from problem to problem, so that decimals changing wouldn't have to be made so much, and a few other minor things, plus some major things. It turned out — I didn't expect them to do it, but they invented, in playing. They would solve some of the problems I couldn't solve, and invent schemes. These guys therefore, many of them, had in their machines their own inventions for special plug boards and so on. Also, guys came to me — two people came to me and they wanted to quit. You know, they wanted to quit, they were waiting. Someone said that she had been promised before for several months "that I could transfer to another job." — some secretary, a WAC or something. She kept saying, "But they're getting new men and he wants me to help train 'em — I gotta go. I'm going." I said, "Well, why are you going? Because I might change something. I want to know, only because I might change something that's bothering you." She said, "Because I can't go overseas if I work on this, because it's secret stuff, and I want to get transferred to something and then from there I can go overseas. I want

to go overseas." I said, "I can't change that. Go." She said, "What are you going to do when the new people come?" I said, "I don't know, but the man's been promising you for six weeks," and so on. She said, "I'll stay, till those people come, and then I'll go." I said, "Ok." And so on. It turns out, you treat a guy right, they treat you right, see. So I treat everybody absolutely right. I saw the light wasn't good. It's stupid, but I got better lights in the room, you see. And so on. Then everybody changed. The whole place was changed. The excitement in the work was completely different. At first, the first time, when it was set up — see, I had to set up a whole new plug board system. I changed the method so I changed the plug board system to be more efficient, and I worked 17 hours at a stretch there — no, I guess it was 36 or something — I don't know how long it was, nearly two days, or a day and a half, solid, at a stretch, and then slept for 17 hours. When I first took over I worked solid. Also I picked out myself to be the one on the night shift. I made sure there were supervisors on every shift, and I put myself on that, at first, with a little box, with sandwiches. At first I worked 36 hours; then slept 17. When I got back in 17 hours it was in the same condition I'd left it when I went to sleep. Because there were bugs and troubles but they were working hard. Anyway, as time went on it turned out we no longer needed supervisors at night — very soon — because the fellows knew enough by this time. There were always enough good guys in the crew of operators that they always knew what the hell they were doing, and they wouldn't get mixed up like that. Then, we had one very great improvement. We could do more than one problem at a time. The way it was, then were many machines in succession, so we'd take a deck of cards. It would have first the cubes, you know — take the cube of the radius, subtract the two radii, do something else, find the pressure and so on. We'd go from one machine to the next machine to the next machine. But because we had everything standardized, we could take another problem, which means different proportions of matter and density and pressures and speeds, and start another problem, because after all it went through the same machines. So we just used different color cards, and followed the other problem around, sometimes passing it if one deck was bigger than the other. But you could still go around in the same equipment. So we did approximately three problems at a time, which was an improvement, too. So it as more efficient and we did several problems at a time. One of the greatest efficiencies I can't count, and that is when people would come to me with a new problem from somewhere, that we've got to do this case. I'd say, "You've really got to do this case? Look, we've got a case of aluminum here, and we've got a case of lead here and this somewhere in between. How accurately do you need the answer? Can't we get the information from this or that? It's not an importantly enough different case." And they would agree, see. If they didn't agree, I'd do it. But I would talk them into it. So I saved a lot of trouble by not doing a lot of problems, which is important too. Anyhow, to make a longer story rather shorter, we did nine problems in three months — compared to three in nine months — and that was pretty effective, pretty efficient. In addition, we had another thing. Machines made errors. Or people made errors — say they had to fix some balmy condition on a shock wave and they make a mistake. Or usually a machine. A digit would go wrong. Something would go wrong. The way the thing worked, if you made an error on one card, say the radius and the cube of the radius, had to subtract the radii from two

adjacent cards for the next cycle, that error would appear on two cards, because that radius was used for the front and back of the subtraction — next time three cards, then four cards. In other words, errors would grow. An error on one card would grow through the stack of say 50 cards, gradually. Well, we invented a way to correct errors without slowing down the problem. We continued to do the problem with the error in it, and we'd discover an error, say, 5 cycles late, so it's over 5 cards. We'd take a little deck of 15 cards, copy everything and get it right, the central cards right too, and go around with that deck doing the central part of the calculations, just fitting the edges from the old calculating. And we went around much faster with the small deck than with the big deck. So while the main problem was going around propagating this error, we're catching up with it, with the little one, with the fixing, repairing the fault. So as we'd go around we'd repair the fault. We're inventing a lot of things.

Weiner:

You would apply those results of the smaller deck.

Feynman:

Then when we caught up with the other one, would just take out all the wrong numbers and put the right ones in, and then go on with the 50 cards, see. Now, let's see, is there anything else to tell about those days, managing that? I had no managing troubles after that. I didn't need supervisors. Everybody was cooperating. It was very easy after I got things set up there. It was just fine. I think the secret was, just natural — I'm not good at administering, but I was just lucky — instinctively I knew what to do. I have to show I'm doing a lot of work myself. Second, everything that they want, I mean as is honest, you give them. I mean, there's no way out. You can't fight. Then, also, consider how it looks from their point of view. And it worked out pretty well. At any rate, we were going as fast as we could. We did about nine problems. The time was coming that they were going to make the shot at Alamogordo, and a very important question was: what do we think the energy is from that shot? So exactly the Alamogordo design has to be calculated. It was suggested we ought to. But it was brought in late. I mean, they were going very fast. Christy came around and said, "We have to do this problem and it's gotta be ready by such a time." I can't remember now the time, but—I don't know, it must have been, maybe we had to do it in two weeks or something crazy. I said, "But no, Christy, we can't do that. It's impossible. I cannot do it. It takes us three months to do a problem." What do you mean it takes you three months? You've done nine problems in so and so —" I said, "Yeah, but we do them simultaneously, you see. We have a lot of production and we do it simultaneously. It takes a long time to put through all the machines in one deck. I can't go any faster. We do so many problems because —" So he explained the problem and I said it was impossible, "We can't get it in time," so he went out. And I began to sit and think about it. Well, we don't need the last moments after the explosion. We don't have to carry it so far in time, because we get a very good idea from the other case. If we get that far — this kind of stuff, you know. And I gradually realized

that it's almost possible. Probably impossible but worth a try because it's important enough. So I write on the blackboard — big thing, you know! "EMERGENCY! All other problems stop, because there's no use having this other stuff going around." This thing was a first priority, see. That's the only way we could ever do it. "All problems stop. Need volunteers for a week for Sunday" — because we didn't have to work full shift Sunday, or something. "Will pay you back by the other shift," and so on. Explained what we had to do, why we had to do it, and outlined how I expected it might be barely possible, though probably impossible. These guys were very full of juice by this time. They all volunteered and they started working. And that was when I got the call that my wife was dying down in Albuquerque — just then, in the middle of all that. And they're going as fast as they can. Just got it set up, and I had to leave. But it shows how good these SED guys (and everybody else) were — they kept the doggoned thing going. When I came back from Albuquerque and went in, I was upset. It looked to me like they were doing everything wrong. First of all, there were different colors. There were four different colored cards. Right away I said, "Hey, why are you doing more than one problem?" They said, "Please go out, and we'll tell you when you come back." I said, "All right," because I trusted them. Well, it turned out, here's what happened. They decided to do this problem. They were doing it as fast as they could. You know, everything was organized, everybody working on it, everything was getting ready, the pressure calculators were a little bit anticipating — you know everything was as fast as they could possibly go, going round the machines. And naturally, as usual, they had an error. So they got a deck of a different color, to follow the error around. In that deck they got an error. If that ever happened to us ordinarily, we got so mixed up that we would stop and get everything cleaned up and start over. But they don't want to stop. So we made a still smaller deck. They had I think three or possibly four, one inside the other, errors, chasing each other around. And they had been doing this during all the time I was in Albuquerque. Every time there was an error, and error within error, they never stopped. They kept this thing going. They had practiced this stuff. They had sometimes two and three, and now this particular time they had three in a row. But what was their big worry was: confusion. You see, to keep track of which deck goes inside of what deck, and which one is the wrong one and which one is the right one. And they didn't want to have to stop in the middle of this explain to me what the hell they were doing. They wanted me to get out. Then when it was unconfused, one of the guys came out and said, "I'll tell you what we've been doing," and then he explained to me, this terrible mess — I couldn't follow, you know, because there were so many wheels within wheels. But they had kept themselves from getting confused, and had gone around, wheels within wheels. One of the machines had broken down, and they used the "Play" machine. It was very exciting. Here was a battle that wasn't in the Army. It's a similar kind of battle. It was a battle — they were trying to get something, to find out about that design. You see the problem — it was important to calculate, to compare to how it worked, to see if other calculations of design would make any sense. It was vital to have a calculation to see if it agreed, to understand.

Weiner:

Did you make it?

Feynman:

Yes, we made it. We made it very satisfactorily and well. Yes, it was very exciting. They were great guys. And as a matter of fact, I went down to their officers and told them that they'd done such good work that I thought they ought to have a leave, or a pass for a couple of days — you know, these fellows. The guy gets up in his chair and he says: "You cannot grant them a pass. Only I." I said, "Sir, you're absolutely right. I'm absolutely wrong. Of course, naturally, I had no intention of granting them a pass. I just wanted to report to you the excellent work that they were doing, and you can decide what you want to do about it. But I wanted to tell you, Sir, that your men are very good. It's probably from your training." I just turned it around. So he said, "You know, I have an idea. I'll give them a pass." Just crap — the world we live in! Anyway, it was great. That was a very exciting time for me. We did pretty well. Right after that, then, the test was getting ready, for Alamogordo, and there was a short period — my wife had just died — when I was granted a leave to go home. They told me I ought to rest a little bit from the exertions of the adding machines, or something, because I had finished getting the problem ready and so forth — or some reason. Probably they felt that I just had this trouble. So I went home for a short time. Incidentally, amusing enough, my father, who was home — he knew I was doing some secret work at Los Alamos, but not what it was — said to me, (he was in the uniform business), "You know, I have to go down to the Navy Yard in Virginia. I also went down to Oak Ridge. There are some guards there. They've got a secret project. They need uniforms, so we supply them with uniforms." He says, "You know, the crazy rumors and crazy stories they tell about what's going on in there!" I said, "Like what?" He said, "They say that that's an atomic energy plant. They're going to make some kind of a bomb that liberates atomic energy," he says to me, "but it's so stupid, because I know that atomic energy is the energy that you get like in gasoline when it burns in air, and so on, it's the changing around of the atoms, and the energy of an atom is in chemical reactions, and they just don't even know what it means!" Well, that's right. "Atomic energy" is a misnomer. "Nuclear energy" is the right thing. And he concluded from his knowledge of science that the rumor was absolutely wrong, but it was a dead right rumor. I kept a straight face through all this, of course. At any rate, I got a telegram when I was home — something like "The baby is expected on such and such a day," which was a prearranged signal from Hans Bethe. I flew back to Los Alamos and got a car from the airport and zipped up to the site and there were the busses getting ready to go to Alamogordo. Without even changing my clothes, I jump on a bus and out I go to Alamogordo. I was at the place 20 miles away from the main thing, on a mountainside. At any rate — can I describe what the bomb looked like to me, or you got enough descriptions of the bomb?

Weiner:

Oh, yes, that's from your own personal reactions.

Feynman:

All right. Well, I was up at this site, and we were supposed to get communication from below as to when the thing was supposed to go off. They had a theoretical hour, but we were supposed to get some warning ahead, and we had a radio system that was supposed to let us know. Guys were fooling with the radio and they couldn't get contact with the people down there, and so we couldn't find out what was going on. I began to play with the radio at some point along the line, and suddenly I got contact. Apparently they'd forgotten us or something. Just before they began to tell us when it would go off, and that we had a certain amount of time left — and then someone came around from a safety office or something to distribute dark glasses. I think this is nuts. Twenty miles away, they're going to worry about burning out your eye. I mean, that's crazy. They worry about ultraviolet light, from 20 miles — you know. I didn't think it would go. I mean, I was always a pessimist. I never believed in the theoretical — I thought it would be relatively small, most likely. Something would go wrong. Always a pessimist about our knowledge of the world. But I did have the common sense, as for the ultraviolet light — I got in the cab of a truck, so I would look through glass, but I didn't think I had to look through dark glass. Maybe I wouldn't see anything.

Weiner:

You'd miss it, you mean.

Feynman:

Yeah. Right. Anyway, we got the signal and the timing and everything — the radio — we finally made contact, just in time, and found out how many seconds, and the count was going backwards — the usual business. And then I saw a terrific bright flash, out on the desert. It was so bright that my instinct was to turn my head — you know, suddenly. I look and I see a big bright purple thing. That's the bomb? Can't be, it's on the floor of the truck. It was the after-image, you see. So then I looked quickly back, and I saw things that others didn't see, because I was looking with my eyes and they had to look through the glasses. I saw the light. It was white, turning yellow, turning orange, but during the first stages, I saw clouds form and disappear, waves. I could see the wave of motion of the cloud formations and other things. It was very interesting. Then the light began to peter out gradually, got more orange and so on — and it was a ball, and it was lifted up off the ground. It was a round ball. Then it began to flicker like an oil fire, with black. This was quite a bit later. Black and more and more flickering. Then I saw a purple halo. Most of the light went out, and there was the purple halo around it. I figured, there's another after-image, but when I looked up to one side it didn't come with me, and I realized that it was the radioactivity in the cloud or something, making the halo. It was

very impressive. It was by this time dark but still visible. And it was quite a long time, (I guess we can figure out now — a minute and a half, or a minute forty seconds, something like that) — when all of a sudden there was a crack, a loud crack like a rifle going off nearby, and the thunder — brmmmm, brmmmm — because all the rest as in silence. I've always been impressed by acoustics. Acoustics have meaning for me. Not so much as the visual. When I heard the solidity of that crack, at 20 miles away, then I knew that that thing was something, and I got excited. Everybody began to talk then. Everybody was silent during the entire period while the sound went the 20 miles — what would it be, a minute and a half? One incident was amusing. When the crack came — because everybody was so intensely watching this thing — when the crack came, I was standing next to William Lawrence of the NEW YORK TIMES, and when the crack came he says to me, "What was that?!" I said, "That was the bomb down there."

Weiner:

Have you read Lawrence's account of that? It's a classic account of that very moment.

Feynman:

I guess so — no, I haven't read it. He was standing there. Anyway, that was the moment. Then we all got excited, and there were a lot of parties back at the place and all kinds of excitement, a wild and wonderful business altogether. No — there weren't parties at that point, because it was still secret. Excuse me; I've got it mixed up with the time of the announcement when the war was over.

Weiner:

What did happen at that point?

Feynman:

Well, we knew it was good, and we were all very excited. We went back to Los Alamos. But we're not going to dance around and let everybody know. This Lawrence reminds me of something. At first they thought that there would have to be, sooner or later, an official Army description of what was going on at Los Alamos, the bomb, the work, the whole thing, somewhere, so that somebody, newspapers or what, would have some place to read about it — or maybe for historical purposes or something. So they got Bill Lawrence of the NEW YORK TIMES, (he was no longer on the TIMES, was hired off the TIMES) to come to write all about Los Alamos. And I was given the job of taking him around Los Alamos when he arrived, to show him everything, because I knew all this stuff, because I had this liaison job before and I knew what went on everywhere. I saw him around. But somewhere, somebody was not satisfied with the way he was writing things. I don't know why. Perhaps it wasn't technical enough or something. So then H. D. Smythe was the one, and I showed him around the whole thing, the place,

next, same way. So I wrote part of the theoretical section for him, explained it and wrote it and so on — very good — I knew him very well. But one story I must tell, because it's so amusing. At one stage — you see, we'd gotten finally some plutonium from Hanford, and were making some measurements on it, and had a little ball of the stuff on the end of a silver tube in a room, separated from the walls, just all by itself, so that the reflection of neutrons and this and that wouldn't make any difference. They were measuring something. It was a silver-plated ball in the middle of the room — a very kind of semi-dramatic looking thing. It was a relatively small room, but there was this small ball, oh, like a tennis ball. The door was open on the room, and I showed it to Smythe, and said, "Here we are making experiments with the plutonium we've just gotten. We're bombarding it with neutrons and seeing how many are reflected, measuring all the necessary numbers." He's kicking the doorstop, you know, while we're talking. You know how people nervously do. And he begins to talk about how here the world has never seen this metal before, we have at last achieved transmutation of the elements, and here stands this ball of metal, and so forth and so on. Dramatic. He understood the moment and he described to me the moment — of the human beings, here we are, standing in front of this accumulation of stuff, of a new material that had never — that had died 20,000 years after it was created, hadn't existed on the earth for billions of years, transmutation of the elements and so on. I said, "Yes, and I think you would find it appropriate that the doorstop for this room, that you're kicking" — which as half a sphere, about 18 inches in diameter — "is made of solid gold." He looked at it. It looked like a brass thing. "It is?" I said, "Yeah." You see, some of the question was to reflect the neutrons back from the uranium. We would save neutrons that way and therefore material. The plutonium was infinitely valuable compared to anything else. So the best reflector we could get, from the neutron standpoint, was the question. Never mind the money. So many experiments were done, reflecting from different materials. We had to get samples of different materials to do the neutron reflection experiments. One of the materials we needed was gold and another was platinum. At first they thought we would march these things around with big guards all over the place. And then it was decided by somebody, rather intelligently, that since this was a secret project anyway, the best way to do it would be to not make anything out of it, to call it brass, and to call the platinum zinc, for anybody who happened to overhear. All the scientists who were making the measurements knew what the hell it is. So we had this half a ball of gold which had been used for neutron reflection measurements and it had been treated like brass all along the line. It was used as a doorstop and so forth, while it was not being used. You know, it was stored as doorstop. But somebody had the amusing idea to use it as a doorstop for this room that had the plutonium. I thought it was very good. Incidentally, the platinum disc was amusing, because it was "zinc." I remember being called down to one of the labs — "Listen; we got a disc of zinc for measurements. Would you like to look at it?" I look at it. "Would you hold it for minute, please?" I look at it. "Would you hold it for a minute, please?" — you know they hand it to you. Uhnnn! The density of it is so much greater than you'd expect. You're not used to handling so much dense metal. It was very impressive. Very hard to hold up, though a rather reasonable-sized object. Anyway the "zinc" just sat on a table — I don't know how much, about 3/4 of an inch wide, perhaps

12, 14 inches in diameter, a circle — that's a lot of platinum, just sitting around on a table like any piece brass. Lesson in relative valves, yeah. I want to talk about the censorship they had at Los Alamos. At some stage along the way, people began to notice that some of their letters were being opened and closed. They reported it to the Security Office. Actually it was the Security Office which presumably was doing it. Anyway, after much effort they decided — censorship, which is illegal inside the country. It had to be voluntary, which meant that the letters that we mailed out, we would mail unsealed, and they would seal the letters. They didn't want to mark the letters "Censored" because they didn't want the people who received the letters to think the place was so important. The letters that we would receive would be opened by the censor, then given to us. There was something a little bit funny about it, and there were a lot of these idealistic scientists, liberal-minded or whatever you want to call it, so there was quite a tussle about it. Everybody was handling everything with kid gloves. But they did arrange to have censorship, and they said it wouldn't bother us in any way, and there would be no troubles and everything would be all right. No effect on our incoming mail. Incoming mail would come right through, only they would look at it, and outgoing mail they would simply seal up, if everything was all right. They had a lot of rules about what you couldn't say and could say. With much trouble they arranged this thing. In fact, in the description ahead of time it said that the "mail would be censored by a man whom you do not know and who is not known by you," which everybody read like it sounds, and somebody noticed meant the same thing just repeated. They were very wise and clever people, you see. But somebody noticed that little trick. They're a funny group. Anyhow, the censorship started. Now, I really bore the brunt of the censorship, because my wife was in Albuquerque and I wrote to her every day — I don't know that it was every day, at least a few times a week. So there was a lot of mail back and forth. In the beginning, when the censorship started, we could notify a few people if we wanted to that there was censorship, because that was a legal thing, by one note, but that's all. So I told my father and my wife about it. The censorship began, and immediately I was called into the office. What was the trouble? They said, "What's this?" I said, "It comes from my father." It was a sheet of paper with dots above and below the lines all over the paper. They said, "What is this?" I said, "Oh, I see, it's a code." "What's the key to the code?" "I don't know the key to the code." "What do you mean, you never saw it?" Well, it was just that I was amusing myself by trying to crack a code that my father would invent, see. And the same thing — a letter from my wife they showed me with five letters things, "wfkvgix," and so on, which was another code, which she sent me, for which I didn't know the key, see. They said, "You can't do this." I said, "But you announced that the censorship would in no way affect the incoming mail." They said, "Well, at least will you have them send the key along with the code?" I said, "No, because then I don't have any puzzle." They said, "All right. You send the key, and we will look at it. They send the code with the keys, we'll take the key out." I said, "Ok." So I started out, in other words, in trouble with the censor, and from then on they always thought I was some kind of wise guy looking for trouble. The first letter that my wife sent me had a mark taken out, something taken out by ink eradicator. I went to the censor, and I said, "Somebody's been doing something to the mail. You said you weren't going to touch the incoming

mail." This was at the very beginning. They said, "Listen, the censor wouldn't use ink eradicator. He'd cut it out with a scissor." I said, "Oh." So I wrote to my wife and said, "Did you use any ink eradicator in your letter?" And her letter came back, "No, I don't have any ink eradicator. It must have been the —" and there was a hole cut out by a scissor. So I brought it with the hole cut out and said: "I think this was your censor." "No! It was your wife!" "Listen, she doesn't know anything about conversation we had before. I haven't visited her since. This is the censor!" Anyway, the next time my wife wrote a letter, it came as a kind of confetti. Because she was saying things like, "I used to like to write to you, and I know that the censor doesn't make any difference, but to think of the censor reading the letters between the two of us bothers me, and I always think of the censor, and so on." Every remark about this was all cut, and so it was confetti. I went into the office. (This took some time, you see; I was always going into the office.) Finally the officer said, "I told you we were going to stop this," and so on. I said, "But it isn't stopped." He said, "Don't you have faith in me?" I said, "I have faith in you, but I don't think you're the man in charge." This got him very angry and he got on the telephone immediately, as I was going out of the office, with somebody. And that was the last time that they bothered any of my incoming mail. They would put notes in it to me, but they would do, you see. But they'd begun chewing on it, which is not right. At any rate — they must have censors trained the regular way, and they just started to proceed the regular way, but then — do it. We had a number of small amusing difficulties. Because they were worried about me with codes, a number of troubles. I got up — one day I went to see my wife and she said, "Where's all the stuff?" "What stuff?" I got a letter from the censor. It said that this letter had a code in it without the key, so they took it out. It was not according to our arrangement. So I went to my wife and she said, "Where's all the stuff?" "What stuff?" "I sent you a list." "What did it say on it?" "It said litharge, glycerin, hot dogs, laundry —" I said, "Ok. That's a code." She wanted litharge and glycerin to repair something and so on. It looked like a set of code words. So I always had a certain trouble. No matter what I would write, zoom, it would come back to me. See, I'd put it in the mail and it would come back, "violation of something." Once I was playing around with the adding machine. Fooling around, I noticed that 1 divided by 273 is .004115226337448 and so on. Then it gets near the end, when the 9s start carrying into 10s, with carriers and so on it rearranges itself and comes out 004115 and so on. So I wrote them a letter — I told my wife this in a letter, put it in the mail, it comes back, "violation of paragraph 17B." I read 17B. "Letters have to be written only in English, Russian, Spanish, French" — lots of languages. "Permission to use any other language must be obtained in writing from the censor. No codes." I can't figure out, where is the violation of paragraph 17B? So I look at it, and I can't see the code. I certainly can't be a code, because if you in fact divide 1 by 273 you will in fact obtain the number, so there's no more information than in the number 273, and so on. So it's not a code. "I therefore apply for permission to use Arabic numbers in my letters," and so on. We had this kind of stuff going back and forth all the time. At any rate, after they stopped cutting the letters up, my wife would still mention the censor from time to time and there would be a note in it, "This letter violates paragraph something," so I look at it and it says, "Don't mention the censorship." But the letter was coming in to me. It's

none of my business, you see. So this happened for a while until finally I got a note that says, "Please notify your wife not to mention censorship in her letters." So I wrote a letter: "I've been instructed to inform you not to mention censorship in your letters." Zip, zip — don't mention censorship. Yeah. So I figure, Ok. I wrote back to them and said: "You told me not to mention censorship, and to instruct my wife not to, but how am I going to do it unless I go down and tell her? And it's very bad practice for me to go out to tell things that I can't pass through the censor" — you know? "Furthermore I would like some information to explain to me why I am instructed by the censor not to tell my wife to tell me that there's a censor." Well, they wrote back, "Don't be a wise guy. It's very simple. In case the letter is intercepted on the way from Albuquerque up here, we don't want people to know what's going on." So they caught me on that one. They had a good logical reason. So I went down and told her not to mention it anymore. At any rate, my wife and I — she had done a number of other things. You see, she would do whatever she would have done ordinarily, like send up those letters and so on, and she had ordered, before the censorship, jigsaw puzzle blanks; for the fun of it she was going to fool me by writing a letter as a jigsaw. Well, she decided to do it anyway, even if there was a censor. So one day we had trouble. I got the letter, all the pieces, to put together to read the letter, and a note from the censor, "Please instruct your wife to restrict herself to ordinary letters. We are not in business to play games." But she did that not purposely to annoy the censor. Only then we began to get the idea that we should do things to purposely annoy the censor, because they were beginning to annoy us. I mean, I had to work so hard to get them not to cut the things out and do all these things. So we had a whole sequence of items set, of which the next one — which we never quite did — I remember the next one, but we didn't do it, because things quieted down. The letter would come to me in an envelope full of powder in it, and it would start out, "I hope you remembered to open this letter carefully, as I have included the Pepto-Bismol for your stomach as prearranged." You can imagine the guy pulling the thing out, stuff going all over the floor, having to collect it, put an apology in, you know. It was that kind of thing we were playing. Some of the things like that we did, to the censorship. But as a result of this experience, I knew exactly what could and could not be sent through the censor, because I was the one that got almost all of the trouble, you see. And people would come to me as an authority — "Can this pass?" "Will that pass?" And I'd know. As a matter of fact, later, I made bets. For example, at one time one of the boys of my group was hauled out in the middle of the night when he was sleeping, put in a room with lights and smoke and guys asking him questions — I can't remember what, something having to do with how his father or how he voted — I can't remember the details. They were discussing it at the table, and they said, "You know, the trouble is, we've got censorship. There's no way that we can send it out." I said, "Sure we can send it out. I'll send it to my wife." You see, there was one weak spot. Because of the sensitivity of people, they had written specifically: "No criticism of the administration will be censored." So I simply wrote: "You should see how they administer this place. Only the other night" — ta ta tata, I described the whole thing, and won five bucks. It went through. Yeah. It described the whole thing, in detail, but I said it as a criticism of the way things were being run. Then, on another occasion, I had discovered some holes

in the fence — big ones. See, they had an inside fence for the technical area, which was very carefully kept. They had an outside fence, for the whole region of Los Alamos. And they had people living out there who were construction workers who would come through. These guys wanted a short-cut. They cut holes. I mean, the outside fence was dealt with very carelessly. I used to like to take walks. One day I went out and I saw this thing. I was always trying to correct them, you know, not just to fool around. But instead of correcting it by reporting it in a simple manner which I knew would make no difference, I always made it more or less dramatic. So this particular time, I go out, I find this hole. I came in. You see. Then I went around and I went out again, through the same gate. Found the hole. Came in. Went out again. The sergeant at the gate took my mark down — “so and so, badge no. so and so” — every time I went out. He’s amazed. Number so and so — he looks — same badge — went out, went out, and went out, three times. So he says, “What’s the idea going out through here?” I say, “Am I not allowed to go out?” you see, and so on, went out, and came back for the fourth time. He got very upset. He puts me in the guard place there and he calls up the captain, “So and so —” I simply said to the captain, “I’m trying to demonstrate for you the absurdity of your position here. This man is standing here, preventing people from coming in, when only 150 yards over there, there’s a hole big enough for a man to walk through standing up.” I also found another hole on the other side big enough for a truck to go through. There was some trucking and so, short out, you know that nobody knew. I was describing this to somebody. They said, “Bet you can’t get that through the censor.” I said, “I’ll describe every hole in the fence, where you can go to get through the fence, and there’ll be no trouble” — same trick, you see. I again described the difficulty, the way they administered, and the lack of safety, and I described, 152 1/2 yards to the right of gate so and so (I exaggerated the precision) there’s a hole, and described the hole — and there’s another hole in another place — exactly where everything was and how to get to it. You can imagine them worrying: “We can’t send this through the mail?” On the other hand, what can they say? They can’t say to a guy, “Listen, you can’t tell us where our holes are.” There’s only one thing for them to do, damn it — send it through the mail and fix the holes. So, although it sounded like I was kidding all the time, I did have a serious purpose. I did think the holes ought to be fixed. I didn’t believe in this way of having to stop at gates and going through all this nonsense if it didn’t mean a damn thing. It was silly. So I was always annoying, for a purpose, you see. I enjoyed myself, because I was in the right, so to speak, but I would always have a little bit of a purpose behind it. Not quite the same. So that’s some of the stories of the censorship.

Weiner:

I remember reading one, about a note in a safe —

Feynman:

That’s different, that’s about safes. That’s not the censorship. Do you want to know about safes?

Weiner:

How do you feel?

Feynman:

Yeah, sure. You got tape? This stuff is relatively easy. When I was in Princeton, I learned how to pick locks from a guy named Leo Lavatelli. It's relatively easy, with a paper clip.

Weiner:

Was he a student?

Feynman:

Yeah, graduate student. Pick Yale locks. I was surprised they were so easy. It was a surprise to me. And so I learned the thing. Because it meant that locks are not safe, ordinary Yale locks are not very safe. Now, in the beginning of Los Alamos we got very serious secret information that everybody appreciated. But because of some circumstance of supply, they had to be kept in ordinary wooden filing cabinets, which were locked often by makeshift devices like long sticks that would go through all the handles with a padlock on top, which is a Yale key kind of padlock. I thought that this was wrong, that it was not fully appreciated how poor was the security. I knew, from picking locks that these things were in no way safe. It takes a few minutes to open. And so I was perpetually annoying the system, to hurry up and get some real things to put this material into — that these filing cabinets were no good. That was the real purpose of the business of locks, and why I was doing various tricks all the time — various things, like to borrow a report by pulling it out of some guy's safe — to show, again and again, that the stuff was no good, by demonstration, not by talking. As it turned out, it was much worse than I describe. It was not necessary even to open the locks. Ordinarily you just tilt the whole filing cabinet over, say. The filing cabinet drawers have slots along the bottom to move some kind of a thing to hold the papers. And you can just put your hand in and twist the papers and pull them out, and so on. It was very easy. It was lousy, it was no good, and we had to do something about it. So I kept harping on this, and demonstrating these things to people all the time by one or another trick and so I got a kind of reputation for being annoying, but I had a purpose, because I don't think it was worth keeping our secrets that way. Just for amusement, I'll tell a story. At one of the meetings where this was being discussed, big meeting, I got up and I again said that everything was unsafe, we have to get some kind of better locked filing cabinets, it's not right — and I demonstrated something. I pulled out, I don't know. And Mr. Teller, Edward Teller, got up, and he said, "Well, how is my drawer in my desk? I keep the really important secrets in the drawer in my desk." I said, "I don't know about the drawer in your desk, but I'll look at it. I'll look at it after the meeting." So just before the

meeting was over, a little before the meeting was over — I was sitting in the back and he was in the front—I snuck out, ran down to his office, looked at the drawer in his desk. It was very simple. I didn't even have to open the drawer, which would have been easy. But there's a little space, half an inch, above the back of the drawer, where the wood slides in, so you can put your finger around, you see. I mean, the drawer doesn't fit perfectly. You crawl under the desk, put your hand up — I felt a piece of paper, and pulled it out. And like those automatic toilet paper dispensers, you pull one sheet and it pulls another, so you keep pulling. I emptied the whole drawer. And I put the stuff in my locked cabinet somewhere. That drawer was then emptied. I ran back to another floor to the meeting, which was about that time just coming out, and joined the stream of people walking along, behind Teller. Then I ran up to where Teller was and said, "Oh, by the way, let me look at that drawer of yours." So we walked over to his office, and went into his office. He says, "This is the drawer." "Oh," I say, "that looks very good; I don't think anybody could ever break into that. Let's see what you got in it." He said, "I'll show you." and he opens the drawer up, and he turns to me: "You see, I have nothing in it. I have nothing in it, since you're keeping it for me." The trouble with fooling a man as intelligent as Edward Teller is that the speed in which he could figure out is so great that there isn't a moment, there isn't time for the face to show any surprise. He just pulled it out and said, "You see there's nothing in it, since you have it" — just as fast as he noticed there was nothing in it, he understood why. So he was no fun, darn it! One step ahead of me. Yeah. But anyway, what ultimately happened is, we got filing cabinets made by the Mossier Safe Co. with a combination lock on the top drawer. And this combination lock was one where you twist the wheels. To open it, when you get the right combination, you have to turn back to the number 10, and then it pulls the bolt down and you can pull it open. This was a challenge, one of those challenges. For two years I piddled with that. I tried to figure a way to open them. I worked on it and I studied it. I thought of all kinds of things — spinning the wheels and stopping them suddenly — all kinds of things. I also had read some books on opening safes, but the usual baloney, you turn a handle and you listen for something. That's only because, the old safes, you turn a handle and it would try the bolt against the wheels, and nothing is built perfectly, so when you turn the wheels, a hole, the slot, would come from one of the discs in the right position, the bolt would start to go down even though it was held up by the other discs, and there'd be a little click, and a little feel of ease in motion of turning the wheel. But with this new device, there was no way to test the bolt against the wheels while you were turning the wheels, because you had to go back to No. 10. It was a good design. So I tried all kinds of things. Somewhere along the line, I discovered a way to testing all the combinations in a very efficient manner if the drawer was open — or rather, the other way, excuse me. I found a way of testing all the combinations in an extremely efficient manner by setting up the two first wheels and then just changing one number at a time, so that it would take me about eight hours to work the safe. But before that I found a way to get the number off the safe when the drawer was open. When the drawer was open, and the thing was at 10, the bolt was in the holes, and all the discs were lined up correctly. If you turned the wheel away and started to move the first disc, if you went to a number that still hadn't engaged the first disc and came back, the

bolt would still go back. But if you went far enough to just involve the first disc, which was the last number of the combination, the bolt would go back again. Then you could turn that other disc back because you knew the number, and so on. Anyway, I figured out a way of picking the last two numbers off of a safe combination when the drawer was open. Many people had their drawers open all the time, as they're working, and I would practice this. See, at first it was slow and cumbersome, I had to think, but it's a question of practice. I practiced just for fun. I liked the challenge. It was only a hobby. I practiced and practiced, and everywhere I went I would take combinations off the safes, the last two numbers — more and more nonchalantly. Like I'd go into Christy's office and we'd be discussing something. I'd lean against the safe and diddle the knob, nervously, like I'm doing with this little thing, I'm jiggling it in my hands, you see, just like that. Nobody ever noticed I was doing anything. I'd pick the last two numbers off. I'd go home and write them down. I kept a list in my safe, in fact in the works; I kept a list of all combinations that I'd picked off this way, all over the place. Then I claimed to be able to open safes, and I got a reputation for it. It was necessary from time to time. Somebody was out of town, or something was the matter, we needed a document, we didn't have the guy who knew the combination. So I got a reputation of being able to do this. They would say, "Can you open so and so's safe?" I'd say, "Yeah, I gotta get my tools." Not only would I get my tools, but I'd get the last two numbers of the combination. It took me a minute or two to try the first. The first number was only one. I knew that I only had to get within five to get the right answer. I'd figured that out. Every fifth number — I had to get within two, so every fifth number is all I had to try — so with 20 numbers it was easy, because I knew the last two. I'd go in and I'd close the door. I'd sit around for a while, make it more difficult looking. Then I'd open it up. I'd open the door and let people in. I'd never let anybody know how I did it, because after all I don't want this kind of secret stuff to get around, and so on. I had fun doing that, opening safes.

Weiner:

They eventually changed that? You demonstrated again?

Feynman:

No, they didn't. They didn't. I just claimed to be able to open the safe. Oh, I couldn't tell them to re-manufacture the safes. There were no other safes that were any good. I have since found, right after the war, the Mossier Co. put out another safe which I have not had a chance to study, but for which this method would not work. So they knew about it. They found out — not what I did, but that there was this weakness in their machine. At any rate, I also developed a way to try all the combination numbers in a certain order so as to be as efficient as possible, and estimated I could work the whole thing out in eight hours, guaranteed, tried everything — or in four hours average, because after all, sometimes, you have a 50 percent chance of getting the number. I tested it by having somebody give me his first number, which saved me a lot of time, and then did the

whole thing in a half an hour. It was very amusing. I had very good luck. I have a friend, Phil Staley, who also had been trying to understand these locks with me, and I was kind of telling him of my progress. When I got this method of testing all the combinations I said, "Listen, I've got a way of testing all the combinations in so and so much time, and I want to show you how to do it." So we went into the office of the computing department, which was the nearest room, and I said, "Here, let me show you how I open them." Somebody overheard me and laughingly said, "Feynman's going to show Staley how he opens safes, hah hah hah hah!" So everybody collected around. I knew I wasn't going to open the darned thing. And this is one of the craziest phenomena. My back was turned to the people and the doorway, and I explained to them, "Well, you take some number at random — suppose you're in the middle of it, your testing is 25, 15, 30; then you try 40, 50, 60, so — now, that didn't work, then you want to change that 10 to 15, you turn that this way, 15, now you try 10, 20 —" I was explaining it to them. It was only in the matter of time practically that I'm talking, just a little more, and I hear a "click." By sheer luck the few numbers I'd tried contained the combination. It was incredible. I hear a click and I open the drawer. Staley quickly looks at me with surprise, and sees that my face shows more surprise than his. So he was smooth about it. "Oh," he says, "I see how to do it, Feynman." I say, "Quite effect, quite effective." I'd recovered by that time, slammed the drawer shut and spun it. "You see, we have it worked out," and we went out, left this group with their tongues hanging out. But it was sheer luck. Sheer luck. Now, I got a remarkable reputation for these things. The most remarkable of all was, once I was visiting Oak Ridge, and it was a Sunday. It was over a weekend, and it was very important that I ok some kind of plans or something. They had a special meeting on Sunday, and they had to get these secret documents out of the safe of the man in this office, and only his secretary knew the combination, he suddenly discovered. He had all these guys meeting — me from Los Alamos, a general or two, all this junk — and it was very important. And he's stuck, this damn fool. He doesn't know the combination of his own safe and he can't get the papers out. So he starts calling up his secretary at home, making telephone calls. And they say, "Well, she isn't here, but she's probably visiting aunt so and so." They call aunt so and so — and this just keeps going on. So I say, "Do you mind if I try to open the safe while we're waiting?" Everybody laughed, you see, at the impossibility of opening such a thing, and they said, "No." So he's calling to find out she's on a picnic somewhere. They went on a picnic. Nobody knows where they are. So he's getting more and more embarrassed, and I'm fiddling with the safe — and I open it! I never told them how I opened it, because it was so incredible, they would never conceive of this possibility. You see, to open a safe cold when you don't know the combination — those safes were good, it was hard — at best it would take four hours, average time, cold. But what happened is that about three or four weeks or maybe a month and a half or something like that ago, I had been in that office, before. And I was perpetually testing my methods of taking the numbers off. I had taken the last two numbers off of that safe, just for practice. I didn't write them down or anything because I never expected to use them. I was only fiddling with the safe, my perpetual diddling, like a card sharp who has a deck that he's always practicing his dexterity with. I was always practicing my dexterity. I vaguely remembered the numbers,

vaguely, but I had them in the wrong order. First I tried one way. Then I tried the other way, see. It was a 45 10 or a 15 40 or something. But I had enough remembrance that I did get it. This I never told them. So therefore they imagined that I could open cold a safe in such a short time. But to imagine that this man had previously gotten the numbers off of this combination was beyond their imagination. Another thing that happened Oak Ridge was very amusing. On another occasion, it was very late. It was the last minute on Saturday and everybody had gone home but a certain colonel or somebody important in the Army who had to check my report. I don't remember who it was. So he was reading my report to see if it was all right before I left that evening. Everybody was away. You see, the secretaries had just about gone home and everything. Now, he had in his office a great big two door safe with those big prongs that you turn when you turn the handles and all this stuff, I'd never had any opportunity to fiddle with safes, I just did the filing cabinets, you see, and this was a big safe, so I was very anxious. I said, "Look, while you're reading that, do you mind if I look at your safe?" "Not at all," he says. So I look and I see this strange thing — that the mechanism — the wheels, the combination of the machine — is exactly like in the filing cabinet. The bolt that it pulls back is just like in the filing cabinet, but when it pulls that bolt back, then you can turn the handles, that's all. So to make sure it's exactly the same, I take the last two numbers off, standard machine, quick dexterity, you know. And he finishes the report. Then — very military — he takes his report, puts in it in the safe, and says, "It's all right," swings the two doors closed and jams the doors closed with the big handles feeling very good. I can't resist. I say, "You know, you military guys give me a laugh." He says, "What do you mean?" very angry. I say, "I can tell by the way you close those doors that you think that thing is safe. And the only reason you think it's safe is because civilians call it a safe." That didn't worry me, the illogic of it. So he got upset and said, "What do you mean?" "Well, a good safe cracker could open that thing in 30 minutes, guaranteed." He says, "Oh, yeah? Well, could you open it in 30 minutes?" "Oh, no, I —" "Let me see you open it." "No, I'm not a good safe cracker. It would take me 45 to an hour." He says, "That's incredible, that you could open that in 45. I don't believe it." You know? "You're such a braggart, I'm going to sit right here —" He called up his wife and told her he'd be late for supper. "I'm going to sit right here and you're going to work on that thing for an hour!" So he sat down and read a magazine, got a magazine and put his feet up on the desk. I took a chair, put it over by the safe — I took it slowly, I wanted to have a nice comfortable place to do my work. Then I started to just diddle at random, because I wasn't going to open it right away. You can't spoil a joke by having it go in a minute and a half, you know. So I just piddled around, piddled around for about five minutes, which is quite a time, actually. He turns to me — "Well, making any progress?" "How do I make progress? I either open it or I don't open it. I haven't opened it." Then I think it's about time. So I opened it, because I knew the last two numbers and it was easy to test the first numbers and open the safe. It was, you know, seven minutes. "Well," I said, "seven minutes. Usually it takes me a little longer." He just dropped, you know, with surprise, because it was a big safe, an expensive one, and he didn't realize it could be opened in seven minutes, you see. So I explained it to him. I said to him, "These safes are unsafe when they're open. I did this to demonstrate to you that a

person can get the combination off when they're open," and so on. That was the purpose of my demonstration. I mean, it wasn't just to tease him.

Weiner:

Let me interrupt at this point —

Feynman:

Yeah. Want any more safes?

Weiner:

No, because I'm going to have to run. So I suggest that when we get together —

Feynman:

I've got one more safe.

Weiner:

Oh, all right. Please do that, while I'm putting stuff — crack that one.

Feynman:

Yeah. Well, I have actually two more safes, but — you want this? I'll tell the rest of the safe stories next time, if you want. Ok. All right. Two more safes.

Weiner:

The next time, I think we will try to get from Los Alamos to Cornell, the wind-up of the project at Los Alamos, —

Feynman:

— Yeah, ok —

Weiner:

— and why Cornell, and how —

Feynman:

— you don't have to record all this. We'll just do it next time.

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