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# Richard Feynman - Session II

March 5, 1966

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Interviewed by: Charles Weiner  
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## **Abstract:**

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

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**Weiner:**

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,  $\text{SiO}_2$  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-H<sub>2</sub>O, 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

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**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 decided 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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