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Richard Feynman - Session V

February 4, 1973

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 crystalize; publishes "A Space-Time View," 1948; Shelter Island Conference (Lamb shift); Poconos Conferences; relations with Julian Schwinger and Shin'ichiro Tomonaga; nature and quality of scientific education in Latin America; industry and science policies. To California Institute of Technology, 1951; problems associated with the nature of superfluid helium; work on the Lamb shift (Bethe, Michel Baranger); work on the law of beta decay and violation of parity (Murray Gell-Mann); biological studies; philosophy of scientific discovery; Geneva Conference on the Peaceful Uses of Atomic Energy; masers (Robert Hellwarth, Frank Lee Vernon, Jr.), 1957; Solvay Conference, 1961. Appraisal of current state of quantum electrodynamics; opinion of the National Academy of Science; Nobel Prize, 1965.

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Weiner:

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

Feynman:

I can't remember anything.

Weiner:

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

Feynman:

It was '68.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Have you got all that?

Weiner:

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

Feynman:

You didn't write anything down.

Weiner:

No, I want to talk with you about it.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

That was the first time, OK.

Feynman:

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

Weiner:

Let's do it. Is it here?

Feynman:

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

Weiner:

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

Feynman:

I actually did the work on the paper.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

Sure.

Feynman:

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

Weiner:

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

Feynman:

Right.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

You know that you didn't though.

Feynman:

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

Weiner:

In the Wheeler Festschrift.

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

That's the first problem you set yourself.

Feynman:

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

Weiner:

This follows remarkably soon after.

Feynman:

Yeah, a few days

Weiner:

Were you still at Aspen?

Feynman:

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

Weiner:

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

Feynman:

Yes, this is all at Aspen.

Weiner:

Did. You have the time to do all that?

Feynman:

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

Weiner:

This is still Aspen?

Feynman:

Yeah, it's always Aspen.

Weiner:

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

Feynman:

We can tell the dates by months.

Weiner:

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

Feynman:

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

Weiner:

Why do you call them seagull terms?

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

You had everything you needed there in the library?

Feynman:

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

Weiner:

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

Feynman:

This date is wrong. It must be '66.

Weiner:

Take a look at the next item and see —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Bethe on nuclear matter, '66.

Feynman:

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

Weiner:

Some of this is '65 again.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

In terms of some specific problem though?

Feynman:

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

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

Weiner:

Right.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

But he invited you.

Feynman:

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

Weiner:

So you had to make it right.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Or “are you fooling yourself?”

Weiner:

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

Feynman:

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

Weiner:

So that was the theoretical paper which you had —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I don't know. I have no idea.

Weiner:

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

Feynman:

Right, right.

Weiner:

The literature is full of addenda.

Feynman:

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

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

Weiner:

How much of this got out in the same way —

Feynman:

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

Weiner:

Well, maybe you set yourself a different —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I don't know.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Feynman:

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

Weiner:

All this time, was it possible that there —

Feynman:

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

Weiner:

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

Feynman:

— to students, to Murray, we discussed it.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I was just there during the summer.

Weiner:

Was it June, July, and August?

Feynman:

Something like that.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Yeah, Uhlenbeck wasn't there that year.

Weiner:

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

Feynman:

Shall we eat lunch, maybe?

Weiner:

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

Feynman:

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

Weiner:

Oh no, I wanted you to talk to me.

Feynman:

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

Weiner:

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

Feynman:

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

they.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Zacharias and Koichi(?).

Weiner:

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

Feynman:

No.

Weiner:

And they didn't look for you either?

Feynman:

No.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

Well, let's talk about that course.

Feynman:

I don't remember what year it was.

Weiner:

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

Feynman:

Yeah, I remember the whole story now.

Weiner:

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

Feynman:

I've lost all that — it's marvelous.

Weiner:

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

Feynman:

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

Weiner:

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

before.

Feynman:

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

Weiner:

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

Feynman:

I can remember that cold very well.

Weiner:

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

Feynman:

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

Weiner:

I thought she was at Syracuse.

Feynman:

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

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

Weiner:

The data that you were talking about here —

Feynman:

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

Weiner:

OK.

Feynman:

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

Weiner:

Great.

Feynman:

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

Weiner:

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

Feynman:

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

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

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

Weiner:

And you had the confidence to tell him that.

Feynman:

Yeah, I was just joking.

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

Oh yes.

Weiner:

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

Feynman:

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

Weiner:

You went up to SLAC for that?

Feynman:

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

Weiner:

The same summer?

Feynman:

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

Weiner:

That's where he is.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Well, you must have been excited.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

That's right.

Weiner:

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

Feynman:

That particular one has never been checked.

Weiner:

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

Feynman:

None.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

— how it looked.

Weiner:

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

Feynman:

Sure.

Weiner:

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

Feynman:

Yes, very definitely.

Weiner:

Where especially was this done?

Feynman:

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

Weiner:

Didn't anyone in the audience see this?

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Well, I certainly wouldn't bother to learn —

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

It was from them that you —

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

What was their status? Who were they?

Feynman:

Students.

Weiner:

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

Feynman:

No.

Weiner:

Post-docs?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

Ravndal and Kislinger.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

That was those guys.

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

About '62 or something.

Feynman:

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

Weiner:

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

Feynman:

No.

Weiner:

It would show up in his notebooks.

Feynman:

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

Weiner:

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

Feynman:

That's right.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Right.

Feynman:

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

Weiner:

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

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

Feynman:

No.

Weiner:

It's been this kind of notebook solitary work?

Feynman:

I always work alone.

Weiner:

Except when someone else draws you into their problems?

Feynman:

Yeah, right.

Weiner:

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

Feynman:

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

Weiner:

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

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

Feynman:

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

Weiner:

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

Feynman:

Yeah.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Well, what do you mean?

Feynman:

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

Weiner:

So the burden on the experimental works now in this?

Feynman:

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

Weiner:

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

Feynman:

That's not what I'm talking about.

Weiner:

I know but —

Feynman:

No you don't know.

Weiner:

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

Feynman:

No, you don't understand it.

Weiner:

OK, well, let me —

Feynman:

Just let me say something.

Weiner:

Please.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

It's such an abbreviated kind of thing.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Weiner:

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

Feynman:

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

Weiner:

Well, you're still working on it.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

No, I got my problems too.

Feynman:

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

Weiner:

Maybe change the garden too.

Feynman:

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

Weiner:

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

Feynman:

No, no.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

No.

Weiner:

Once you had gotten the Prize, I mean.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

No different than in the past then.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

You mean with the money from that?

Feynman:

Yeah, that part isn't so ridiculous.

Weiner:

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

Feynman:

Right, exactly, exactly.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

Yeah, you said — I mean the paper —

Feynman:

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

Weiner:

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

Feynman:

I do. I still do.

Weiner:

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

Feynman:

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

Weiner:

In this notebook.

Feynman:

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

Weiner:

It's not bad paper.

Feynman:

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

Weiner:

They're folded over loose leafs.

Feynman:

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

Weiner:

That should have been admitted to evidence.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

She selected it?

Feynman:

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

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

Weiner:

Yes, Ofey, right.

Feynman:

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

Weiner:

A waitress, wasn't she?

Feynman:

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

Weiner:

Your first one-man show.

Feynman:

Yeah. It was really great.

Weiner:

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

Feynman:

Sure. They found out who it was.

Weiner:

Do you still sign it Ofey?

Feynman:

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

Weiner:

So therefore they want them.

Feynman:

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

Weiner:

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

Feynman:

Usually through me.

Weiner:

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

Feynman:

No, they're not good enough for galleries.

Weiner:

Well, for an exhibition, anyway.

Feynman:

An exhibition is kind of fun.

Weiner:

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

Feynman:

I don't do as much as I did — now.

Weiner:

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

Feynman:

Probably. That's probably right.

Weiner:

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

Feynman:

No.

Weiner:

Did you travel for entire summers or anything like that?

Feynman:

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

Weiner:

And you'd work then while you were there?

Feynman:

On and off.

Weiner:

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

Feynman:

No.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

No.

Weiner:

Why not?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Well, even philosophical questions.

Weiner:

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

Feynman:

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

Weiner:

No, I don't know about that.

Feynman:

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

Weiner:

Without your coat.

Feynman:

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

Weiner:

It was five.

Feynman:

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

Weiner:

Amaldi?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

We'll see. You never know what happens.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

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

Feynman:

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

Weiner:

No, I'm just trying to remind you.

Feynman:

I know what the story is.

Weiner:

Well, let's start from the beginning then.

Feynman:

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

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

Weiner:

Yes, I saw those.

Feynman:

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

Weiner:

Yes.

Feynman:

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

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

Weiner:

Do you know what they did? They applauded.

Feynman:

Did they applaud?

Weiner:

The demonstrators did.

Feynman:

I didn't know that.

Weiner:

And then they left.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I don't know what I said.

Weiner:

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

Feynman:

It was crazy, I think.

Weiner:

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

Feynman:

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

Weiner:

I heard them, yes.

Feynman:

And so I said yes, or something.

Weiner:

Yes, but it was your tone —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

We have lots of time. We have until three.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

That's right.

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

I see. Was this published?

Feynman:

Yes.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

I want to ask about Hughes in a minute.

Feynman:

I give lectures every week.

Weiner:

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

Feynman:

Yeah.

Weiner:

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

Feynman:

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

Weiner:

The guys are good — they respond?

Feynman:

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

Weiner:

At what level?

Feynman:

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

Weiner:

What about the biology? Who do you use?

Feynman:

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

Weiner:

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

Feynman:

No.

Weiner:

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

Feynman:

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

Weiner:

You haven't done any of it since then?

Feynman:

No.

Weiner:

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

Feynman:

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

Weiner:

Well, the subject's not old.

Feynman:

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

Weiner:

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

Feynman:

Only for a historical purpose.

Weiner:

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

Feynman:

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

Weiner:

You mean from physics to biology to —

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

No, they're just, interested in culture.

Weiner:

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

Feynman:

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

Weiner:

But they brought people like yourself. Telegai was involved.

Feynman:

Yeah, lots of guys from outside came.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I broke my kneecap.

Weiner:

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

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

Yeah, it's awfully frustrating.

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

I see.

Feynman:

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

Weiner:

In the early sixties?

Feynman:

Yeah.

Weiner:

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

Feynman:

Anyway, this Thornbill work is a continuation.

Weiner:

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

Feynman:

Yeah, that's it.

Weiner:

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

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

Feynman:

All right, so were finished.

Weiner:

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

Feynman:

I don't think so.

Weiner:

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

Feynman:

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

Weiner:

Do you still think so after looking at those notebooks?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

The ones you don't even know yet.

Feynman:

That's usually what happens.

Weiner:

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

Feynman:

Well, maybe Murray — his way of thinking.

Weiner:

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

Feynman:

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

a way that I think is sensible except Murray.

Weiner:

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

Feynman:

All right.

Weiner:

OK, great, a good point to stop.

Weiner:

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

Feynman:

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

Weiner:

Yeah, I think it's fascinating.

Feynman:

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

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

Nothing very interesting -- advanced quantum mechanics.

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

I don't answer many letters.

Weiner:

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

Feynman:

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

Weiner:

When was this?

Feynman:

1953.

Weiner:

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

Feynman:

Have you got that thing still running?

Weiner:

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

Feynman:

That might be it.

Weiner:

Let me check it out.

Feynman:

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

Weiner:

I think you gave it to me.

Feynman:

I probably gave it to you.

Weiner:

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

Feynman:

That's right, that's on gravity.

Weiner:

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

—

Feynman:

— get these notebooks from me somehow.

Weiner:

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

Feynman:

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

Weiner:

How long a period of time did it take?

Feynman:

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

Weiner:

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

Weiner:

That you still have somewhere?

Feynman:

Uh huh.

Weiner:

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

Feynman:

I think he's in America now.

Weiner:

But he went back to Japan for a while?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

'64 or '70?

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

Feynman:

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

Weiner:

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

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