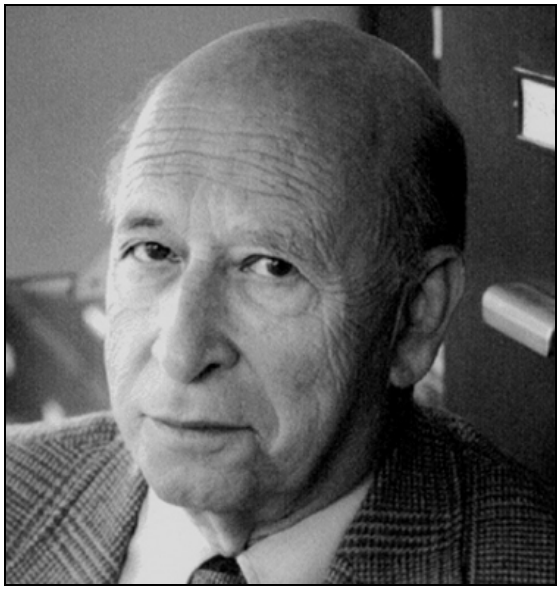

VALENTINE L. TELEGDI
(1922 – 2006)

INTERVIEWED BY
SARA LIPPINCOTT

March 4 and 9, 2002

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

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Abstract
Valentine Louis Telegdi was born in Budapest in 1922 and grew up in Bulgaria. He took his Master of Science degree in chemical engineering at Lausanne University in 1946 and received his PhD in 1950 from the ETH (Eidgenössische Technische Hochschule), the Swiss Federal Institute of Technology. Victor Weisskopf and Gregor Wentzel were instrumental in his appointment as an instructor at the University of Chicago in 1951, where he worked with Murray Gell-Mann. In 1954, after Enrico Fermi's death, Telegdi became the head of Fermi's Nuclear Emulsion Group there. In 1956, he went to the Institute for Advanced Study for three months. Later that year, back in Chicago, he and Jerome I. Friedman found parity violation in muon decay, in parallel with the work of Chien-Shiung Wu at Columbia and her collaborators at the National Bureau of Standards, and that of Richard L. Garwin, Leon M. Lederman, and Marcel Weinrich at Columbia. In 1959-1960, on leave from Chicago, Telegdi worked with Garwin at CERN on the anomalous magnetic moment of the muon. In 1966, again on leave from Chicago, he had a visiting lectureship at Harvard. In 1968, Telegdi was elected to the National Academy of Sciences and in 1972 he

became the Enrico Fermi Distinguished Service Professor of Physics at Chicago. He left the university four years later—discouraged at what he called the "decay" of the Enrico Fermi Institute since Fermi's death and the increasingly cumbersome grants process—and returned to Switzerland, where he headed a group at the ETH doing atomic physics; he also took up a joint appointment at CERN, heading a particle physics group. In 1981, he began coming regularly as a visiting professor to Caltech, where he worked with (among others) Gell-Mann, Richard Feynman, and Felix Boehm. In 1991 he was awarded (along with Maurice Goldhaber) the Wolf Prize for his work on the weak interactions and in 1995 the American Physics Society's Julius Lilienfeld Prize. In 2003, he was elected a foreign member of the Royal Society. He died in Pasadena, California, on April 8, 2006, at the age of eighty-four.

Administrative information

Access

The interview is unrestricted. A slightly different version of this interview was published in two parts as "A Conversation with Valentine L. Telegdi" in *Physics in Perspective*, 9 (2007), 434-467 (Part I); and 10 (2008), 77-109 (Part II).

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ORAL HISTORY PROJECT

INTERVIEW WITH VALENTINE L. TELEGDI

BY SARA LIPPINCOTT

PASADENA, CALIFORNIA

TABLE OF CONTENTS

INTERVIEW WITH VALENTINE L. TELEGDI

46-56

Further recollections of Feynman. Yale lecture series. Three-month visit to Institute for Advanced Study, c. 1956. Impressions of Princeton physics department, E. Wigner, J. Wheeler. Comments on changes in postwar funding for science and burdensome grant proposals. Contrasts support in Europe with that in U.S.

56-73

Social life at University of Chicago. Recollections of R. Dalitz, Bristol University, late 1940s. Recollections of his visit to Bristol. Meets Dalitz again at University of Chicago. Comments on S. Hawking. Recollections of P. A. M. Dirac. Recollections of R. Taylor, H. Kendall, and J. Friedman. Comments on A. Sommerfeld and why Sommerfeld never won a Nobel Prize. Receives 1991 Wolf Prize with M. Goldhaber; recollections of ceremony. Honorary degree in same year from University of Chicago. J. Lilienfeld Prize in 1995.

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Valentine L. Telegdi **by Sara Lippincott**
Pasadena, California

Session 1 March 4, 2002
Session 2 March 9, 2002

Begin Tape 1, Side 1

LIPPINCOTT: Dr. Telegdi, it's good to have you here. Could we start this interview by talking about your background? You were born in Budapest in 1922, but you tell me your parents were not living in Budapest at the time.

TELEGDI: My parents at that time were living in Bulgaria, and my mother came to Budapest just for the delivery.

LIPPINCOTT: Your parents were Hungarian?

TELEGDI: Yes, both of them.

LIPPINCOTT: Why did they leave Hungary?

TELEGDI: Well, immediately after World War I, my father came back from France, where he had been interned for five years. The conditions in Hungary were very, very unstable. All kinds of regimes succeeded each other in the space of a few years, and my father thought there was no real future there for a young man. At that time, all countries asked for visas in order to admit foreigners, but Bulgaria was so backward that they hadn't heard about visas yet, and that's why my father went there.

LIPPINCOTT: Were your parents scientists?

TELEGDI: No, nothing could be farther from that. My mother was a housewife. You know, in that generation, most women were housewives. My father had attended a special commercial high school, and he was fluent in a fair number of languages. In general, he worked for all kinds of shipping companies—he was a manager.

LIPPINCOTT: Did you turn to science as a very young person? Were you interested in science when you were a child?

TELEGDI: Oh, that I remember—at age fourteen.

LIPPINCOTT: Where did you go to school when you were fourteen?

TELEGDI: In Vienna.

LIPPINCOTT: And then you went to Lausanne University?

TELEGDI: Much, much later. I went to a number of places before going there.

LIPPINCOTT: Did you have your strong interest in physics by the time you were at Lausanne?

TELEGDI: Oh, I had it by the time I was fourteen. Except that I couldn't make a very clear distinction between physics and chemistry. And in fact what got me started at the very beginning was that somebody gave me, for my birthday, a chemistry set to do my own experiments. But I was reading books on physics.

Chemistry was a good science, but it also had the enormous advantage, at least from my parents' point of view, that there were possibilities for practical employment in industry. Nobody had ever heard of a physicist in my family. If you told them you were a physicist, they wouldn't have known what that was—except a high school teacher. And so it was more natural to push me in the direction of chemistry. Originally I felt that chemistry was quite interesting; in fact, I got a degree in chemical engineering [from Lausanne] before I switched to physics.

LIPPINCOTT: And then you went on to ETH [Eidgenössische Technische Hochschule, the Swiss Federal Institute of Technology] for your PhD?

TELEGDI: That's correct. Actually, it was very simple. The only courses I really enjoyed during my study of chemical engineering were those concerned with physical chemistry. I didn't like the *real* chemistry courses very much, and I thought I should become a so-called physical chemist. But I realized that I didn't know enough physics and mathematics to be a modern physical chemist, so I decided to go to Zurich. In Zurich, physics was very strong and organic chemistry was world famous, but physical chemistry hardly existed. So I wound up in a physics institute doing chemistry.

LIPPINCOTT: This would be in 1946?

TELEGDI: This was just after my master's degree from Lausanne in chemical engineering [in 1946].

LIPPINCOTT: Tell a little bit about how it happened that you came to ETH.

TELEGDI: Well, I wanted to go to ETH, as I say, because I wanted to improve my knowledge of physics and mathematics in order to become a modern physical chemist. And there was no good institute for physical chemistry at ETH at that time, so I applied to the head of the physics institute—Professor Paul Scherrer—to go there, and I was rejected. But at Lausanne, I had taken some courses in theoretical physics on my own.

LIPPINCOTT: From [E. C. G.] Stueckelberg?

TELEGDI: Yes, from Stueckelberg, who was a great genius. I complained to Stueckelberg that I had been rejected by Scherrer, and he said, "I think I can fix that. I'll call him up on the phone," and he did. So Scherrer completely disregarded the rejection letter he had sent me and expected me to come to his institute. But of course, once there, I had to do something. I had to do what's called radio chemistry; they would bombard a target with protons and some radioactive

substance would be formed. And they wanted to extract that radioactive substance for the physicists to do measurements. So it was a service job.

LIPPINCOTT: Were people sophisticated by that time in handling those materials?

TELEGDI: Yes, and the radioactivity we produced was rather weak. Anyway, so I did this chemical stuff, and I hated every minute of it. But I was so happy to be tolerated at this mecca of physics, which it was for me. I could take courses and I would be in touch with physics. In Zurich, homework problems were handed out to the physics students and the other students. Every week, they got a sheet with about ten problems. And they were very interesting, with the different levels of difficulty. The physicists had to do all the problems; the electrical engineers had to do maybe eight out of the ten; the mechanical engineers maybe six out of ten. And so on down the line to the agricultural engineers, who only had to do two.

Well, I looked at these sheets and every week I solved every problem. I have one advantage over other people—I have very, very tiny handwriting. So I could write my solutions of these problems on the margins of these sheets. And one day my colleagues discovered that I had this magic talent to solve problems. The word was passed around that this chemist could solve these problems. They thought that chemists were a lower form of mankind.

LIPPINCOTT: Really! Unlike how the physicists at Caltech feel about chemists. [Laughter]

TELEGDI: I don't know…but to physicists, chemists are a form of cook.

So this got around to Scherrer, the boss. And he promoted me; he permitted me to stop doing all that chemistry that I had to do. And I became a problems assistant—that is to say, we had problem sessions with the students. Scherrer's personal assistants took a group of students and helped them solve the problems.

LIPPINCOTT: These were physics students?

TELEGDI: No. I never rated good enough to take physics students. I got as high as the electrical engineers. But I solved all the problems. And it's not true, but I made the joke: If they hadn't

given me the job as a problems assistant, for the price of a franc I would have sold the same sheets with solutions. [Laughter]

LIPPINCOTT: So in the course of your time as a graduate student at ETH, you dropped chemistry and you became a physicist.

TELEGDI: That's right.

LIPPINCOTT: And your interest was in the experimental side rather than theoretical.

TELEGDI: That's right, exactly. Well, I was even less prepared for theory than I was for experiment, because of my shoddy mathematical background. Chemists know very little mathematics—they are expected to know very little.

LIPPINCOTT: I've read that you studied with [Wolfgang] Pauli when you were at ETH.

TELEGDI: Well, you have to be very cautious about that. It sounds very good—and it's good publicity for me. But I was not a personal student of Pauli's, I just took his courses. I had a fair amount of contact with him, but I certainly didn't study with him as a professional student of theoretical physics would. I wrote my thesis, which was only partly experimental—although that was the main body. It also contained a fair amount of theory—namely, the theory of the experiment which I had performed—and Pauli was one of my examiners, so I got to know him fairly well.

LIPPINCOTT: What were your impressions of Pauli?

TELEGDI: Well, I could talk for the next few hours about that. Pauli was very, very special—an extraordinary person, both in his appearance and his mannerisms and the way he was talking in his classes. A very strange man. A very sensitive man. Most people were afraid of him, because he was extremely biting and sarcastic in his remarks. I never suffered from that.

LIPPINCOTT: You mean he was nice to you?

TELEGDI: Well, I think he was essentially nice to everybody, as long as you didn't say anything that was stupid. Of course, I was a little worried about having him as my thesis examiner. But everything went very well. After my exam, he took me out for a glass of wine and told me about his own PhD exam with [Arnold] Sommerfeld and so forth. No, I have no complaints.

LIPPINCOTT: You were in experimental physics at that point—

TELEGDI: Yes, and I still am.

LIPPINCOTT: —but Pauli was a theoretician. So your PhD was kind of a mixture of the two.

TELEGDI: That's correct.

LIPPINCOTT: Would you like to say what it was on?

TELEGDI: I was studying the disintegration of carbon nuclei by energetic photons into three alpha particles. And I studied in the theoretical part of my thesis the possibility that the carbon nucleus indeed consists of three alpha particles.

LIPPINCOTT: And then immediately after that you got an offer from the University of Chicago? I know you went there in 1951, and that's a year after your PhD.

TELEGDI: Well, I *tried* to go to the University of Chicago. I had other offers, but I really believed in Groucho Marx's maxim that I don't want to be a member of any club that is willing to take me. So I rejected all the offers and I decided that the University of Chicago, with people like [Enrico] Fermi, was the last place in the world that would be willing to have me. So that's why I applied. And in fact, I got promptly rejected.

LIPPINCOTT: Oh, really!

TELEGDI: Yes. Well, this rejection was later reversed. But then also, we were in the McCarthy era. It was extraordinarily difficult to get an American visa. When I finally got my visa, they

rejected my wife's application, on the grounds that she had been Fascist-trained. Obviously, since she had lived in Italy during Fascism she was by definition Fascist-trained.

LIPPINCOTT: This is Lia. When the two of you arrived, Lia got a job as [Leo] Szilard's secretary, is that right?

TELEGDI: Yes, she did, after a little while. But frankly speaking, I couldn't live on what I was earning, so it was very convenient to have a wife who could bring in a salary.

LIPPINCOTT: Did you go there as an assistant professor?

TELEGDI: No, that was too high a rank. I came as an instructor.

LIPPINCOTT: So you taught for a while. And one of your students was James Cronin, is that right?

TELEGDI: No, he was a student of Sam [Samuel K.] Allison.

LIPPINCOTT: Well, he thinks you taught him. He said something about it in his oral history. He might have taken a class with you.

TELEGDI: Well, it's true. He took more than one, and I think I played an essential role in his getting his first job.

LIPPINCOTT: I'd like you to talk about some of the people with whom you worked at Chicago. Of course, one of the outstanding ones is Murray Gell-Mann, who got to Chicago maybe a year after you did.

TELEGDI: Six months after.

LIPPINCOTT: And you did some work with him.

TELEGDI: Yes, we did some theoretical work together. That theoretical work had its beginnings, its foundations, in some observations that I had made experimentally. It is quite a significant piece of theoretical work.

LIPPINCOTT: And that resulted in a paper?

TELEGDI: Yes. ["Consequences of Charge Independence for Nuclear Reactions Involving Photons," *Phys. Rev*. 91:169 (1953)]

LIPPINCOTT: Did you like working with Gell-Mann?

TELEGDI: Yes, I did. It wasn't easy, but it was a good experience. He was seven years younger than I. I always used to joke that with his being seven years younger, I still wonder who the senior author is.

LIPPINCOTT: Tell me a little bit about Enrico Fermi. Did you interact with him very much?

TELEGDI: Oh, yes. It was very easy to interact with him. The first level of interaction, of course, was in the weekly seminars. The seminars essentially consisted of Fermi giving answers to questions put to him by other people, solving various kinds of problems on the spot. I also could interact with him in Italian, but that was something Fermi didn't care for particularly.

LIPPINCOTT: He wanted to speak English, now that he was in America?

TELEGDI: Yes, especially about physics. It came more easily to him. I was also frequently at his home.

LIPPINCOTT: You lived in Hyde Park.

TELEGDI: Yes, and so did everybody else who worked for the university.

LIPPINCOTT: Did you see very much of Szilard?

TELEGDI: No. I'd see him now and then. I had a very distant connection with him through my family.

LIPPINCOTT: Is he Hungarian also?

TELEGDI: Yes. My father, as I mentioned before, was interned in France for five years as an enemy alien. He arrived in France in 1914 and was promptly interned as a subject of the Austro-Hungarian Empire. There were many Hungarians in Paris—quite a few painters—and my father was interned with a man who later became the husband of Szilard's sister. And so I had some indirect connection with Szilard.

LIPPINCOTT: Had you known him before you came to Chicago?

TELEGDI: No, I knew *of* him.

LIPPINCOTT: Some others who were there: [Edward] Teller.

TELEGDI: Teller was there very briefly. You see, when I first tried to go to the University of Chicago, I applied to Teller to help me go there, and Teller answered that he couldn't help me because he himself was leaving. But by the time I got there, he was back again.

LIPPINCOTT: After your first rejection, who helped you get the job?

TELEGDI: Oh, that's a very, very simple story. When I was a graduate student, we had a visiting professor from MIT—Victor Weisskopf, quite a well-known man. I followed his courses, and he asked me what I wanted to do once I'd gotten my degree. And I said, "Well, I want to go to the United States." And he was astonished. He loved Switzerland. He said, "Why do you want to go to America? It's very nice here." And I said to him, "Look, if Switzerland is too small for the Swiss, it's obviously even smaller for a foreigner." I was living in Switzerland as a refugee, and the police there couldn't wait to see me depart. So he said, "All right, I'll help you. When I go back to MIT, I'll try to get you some junior appointment for the fall."

Weisskopf went back, and a month or two later he wrote me a letter and said, "Unfortunately, I shall not be able to keep my promise. There's not a single opening in the fall at MIT. So I did for you what I felt would be the best that I could do for you. I recommended you to Fermi." [Laughter] And that's how I wound up at the University of Chicago.

LIPPINCOTT: Do you think it was difficult to get a job here, just because it was right after the war and there were a lot of European scientists coming over?

TELEGDI: Oh, no—by that time the Europeans had already come. For young people, it was difficult because of the GI Bill. Universities were full of young people at that time, and of course the full development of physics as we have seen in the late fifties was not yet in full swing.

LIPPINCOTT: You were at Chicago—

TELEGDI: For twenty-five years.

LIPPINCOTT: I have here a note to ask you about the work you did with Jerome Friedman—looking for parity violation in muon decay.

TELEGDI: Well, "looking" is an understatement—finding!

LIPPINCOTT: That was in 1956. And I guess this was just after [Tsung-Dao] Lee and [Chen Ning] Yang had found it at Columbia, is that right?

TELEGDI: They didn't find *anything*—they proposed that it could exist. I mean, they are two theoreticians, and among the many ideas that they threw out, [this was] just one of them. This is now forgotten; people think Lee and Yang said, "This is the solution to the tau-theta puzzle," but they proposed other solutions as well. I read their paper in preprint and instantly decided that I should work on that. And I can tell you that my senior colleagues were all against it. They said, "Why should you waste your time on such an unlikely thing?" It looked like a completely outlandish idea.

LIPPINCOTT: What did they think was outlandish?

TELEGDI: Well, after the experiment was done, I talked to somebody from *Time* magazine. And I told them, "Oh, it's like saying in geometry that two parallel lines meet at infinity and then you discover that they can actually meet earlier than infinity." It was contrary to everything you had ever learned in quantum mechanics. It was a very bold statement. That's why very few people wanted to do the experiment—because they felt that it was so unlikely. And others thought, "Well, maybe parity is not conserved, but the effect will be so tiny that it will be too hard to detect." I had my own arguments about why I thought, *if* it was valid, that it would be a very major effect. In fact it turned out to be a maximal effect. The only merit in doing this experiment was in deciding to do it; it wasn't a hard experiment and it didn't require very clever ideas. It was done in an extraordinarily simple way.

LIPPINCOTT: After you and Friedman did this, did others do it, too?

TELEGDI: No, other people did it in parallel. Those people who did it were under the direct influence of Lee and Yang, which I was not. And in fact, because of these circumstances, some of my competitors got into print before me—which was really very bad.

LIPPINCOTT: Is that—

TELEGDI: Miss [Chien-Shiung] Wu, at Columbia—the Dragon Lady.

LIPPINCOTT: Did you know her?

TELEGDI: And how!

LIPPINCOTT: She was a dragon lady?

TELEGDI: Well, only for us. For the Chinese, the dragon is a symbol of good luck.

LIPPINCOTT: Were you head of the Nuclear Emulsion Laboratory [at the University of Chicago] at that time?

TELEGDI: That was the Nuclear Emulsion Group; Fermi had such a group and when he died [November 1954] I took it over.

LIPPINCOTT: What about Murph [Marvin L.] Goldberger [Caltech's president 1978-1987]? Did you work with him at Chicago?

TELEGDI: I never worked with any of the other faculty members except for Murray [Gell-Mann]. That was not the style at Chicago. Once you were on the faculty, you were supposed to be completely on your own. Some of the younger faculty who tried to attach themselves to the senior staff got fired.

LIPPINCOTT: Why was it that way? Were faculty members in competition with each other?

TELEGDI: Well, I don't think they were in competition, beyond any normal behavior in our field. But I think the idea was that once you were appointed faculty, you should not depend on others for your work; you should create your own ideas. It's an excellent thing. I'd like to say that there was no pressure on us—except that every week the secretaries stuck their heads through the door and asked, "Nothing to publish this week?" [Laughter]

LIPPINCOTT: Now, you knew the Goldbergers when you were there.

TELEGDI: Murph Goldberger was one of the first people I met when I arrived. Let's say I arrived on a Thursday, and Friday morning I started to talk to Goldberger. By the way, Goldberger also has some Hungarian ancestry.

LIPPINCOTT: How about Richard Garwin?

TELEGDI: Richard Garwin was a phenomenon—completely comparable to Gell-Mann. When I arrived at the University of Chicago and looked around, I wasn't impressed by people like Fermi

and Teller and Harold Urey. You see, if Fermi had walked on water, I wouldn't have been surprised. I anticipated being in the presence of somebody of totally unusual intellectual powers. But I had never even imagined that young people such as Gell-Mann and Garwin existed. Garwin was just as smart—and is still just as smart—as Murray, except he's an experimental physicist and not a theoretician. They are the same age—they're both seven years younger than I am. And Garwin also has some Hungarian ancestors.

LIPPINCOTT: Really! Do you think there's something in that?

TELEGDI: Pure accident.

LIPPINCOTT: [Laughter] Well, it seems incredible!

TELEGDI: Well, you might just as well argue that they are both Jewish. I just don't think that's relevant.

LIPPINCOTT: OK, I have a note here that in '56, when you were doing the work with Friedman, your father died and you had to interrupt your work and go back to Italy.

TELEGDI: I had to interrupt *my* part of the work. Jerry went on, on his own. Probably if I hadn't gone to Italy, where my father lived and where he died, we would have finished the real work, let's say, two weeks before, at most. And then we would have gotten in print either earlier or at the same time as our competitor, Miss Wu. And I must add, one of the other competing groups was Leon Lederman and Garwin and a graduate student at Columbia.

LIPPINCOTT: Was Garwin at Columbia then?

TELEGDI: Yes and no. Ever since he left Chicago he has been on the staff of IBM. But he was an adjunct professor at Columbia. He had never heard about parity violation. He was completely out of, let's say, the academic type of physics, being at IBM. And during the famous Chinese lunch, he heard the discussion—[T. D.] Lee telling Lederman that someone should

really do an experiment. And on the spot, Garwin invented the most ingenious way to do it, and they did the experiment in one night.

LIPPINCOTT: Is that different from the way you pursued it?

TELEGDI: Well, Jerry and I did it in a way that took many, many months. It consisted literally of observing tracks in an emulsion. You cannot observe several thousand events in one night. They did it by electronic techniques. And Richard Garwin figured out how they could do it, with just a piece of apparatus, which Lederman already had. The man is a great genius—there's no doubt about it.

LIPPINCOTT: Do you see Garwin now at all?

TELEGDI: Yes. I have maintained good relations with all the junior people I knew at Chicago. And through these last fifty years, we have never given up contact.

LIPPINCOTT: The next watershed in physics in your career was this Bargmann-Michel-Telegdi equation.

TELEGDI: Well, I don't think it was a watershed of any kind, but it certainly was a piece of work that got into all the textbooks. It's a piece of theoretical work. It's quoted in most textbooks. That preceded a very interesting experiment on the magnetic moment of the muon. I don't want to go into technicalities—it's called the g-minus-2 experiment, a name I invented. That equation was able to describe the experiment in a very simple and transparent way. And that was an experiment on which I worked during a leave of absence from Chicago—'59-'60—and believe it or not, I was working *with* Dick Garwin there. As long as he was at Columbia and I was at Chicago, we were competitors, but when we were both at CERN [European Organization for Nuclear Research], in Geneva, we joined forces.

LIPPINCOTT: Was this your first time at CERN?

TELEGDI: Yes. Well, CERN was not very old at that time, only three or four years old.

LIPPINCOTT: So you were there for a year. And Garwin also?

TELEGDI: Yes.

LIPPINCOTT: You took time off in the middle sixties to go to Harvard. Do you remember that? The Morris Loeb Lectureship? Who was at Harvard then that interested you? Anyone?

TELEGDI: There were two men in experimental physics there. There was Norman Ramsey and Ed [Edward M.] Purcell. And at MIT there was Weisskopf. There were other people, but they were not as impressive as these men. I remember that [Julian] Schwinger was there at that time, but I had no contact with him.

LIPPINCOTT: Schwinger was quite young then, but kind of a star, wasn't he?

TELEGDI: Well, this was a man who started publishing [at the age of] fifteen or sixteen. Like Pauli, he was a child prodigy. He was a very reserved person, or at least [it was hard] to have any real contact. We could discuss a physics problem with him.

LIPPINCOTT: Anything more you'd like to talk about, with regard to Chicago? You left Chicago in '76. When you left, you were the Enrico Fermi Distinguished Service Professor of Physics.

TELEGDI: You know the first thing I did when I got that sumptuous title? I wrote a letter to Fermi's widow and apologized. I said, "I had nothing to do with it."

LIPPINCOTT: Why did you decide to go back to Europe at that time?

TELEGDI: The reasons are numerous, but there were two main reasons. First of all—I put it once in a piece of writing [in which] I started out by saying, "Ever since the death of Enrico Fermi, the institute now named after him has not ceased to decay." And I felt that I had done everything that I could to stop the decay. I was very vociferous in faculty meetings, trying to block what I called second-rate appointments and promoting good ones—and I was completely ineffectual. I

saw the place crumbling in front of my eyes. That was one reason. I saw it and I couldn't do anything about it.

The second reason was the way research was funded. I spent far too much time telling the agency how good I was and how interesting my projects were. I said, "Look, I've been doing good work for twenty-five years. Believe me, next week what I do will also be of some interest." But no, you have to start all over again. I didn't like this proposal-writing business.

LIPPINCOTT: Who funded most of your work?

TELEGDI: I think the largest part of it was funded by the Office of Naval Research, and much, much later, NSF [National Science Foundation]. Naval Research was much more generous. The cyclotron [at Chicago] had been built with ONR money.

LIPPINCOTT: Fermilab [Fermi National Accelerator Laboratory, Batavia] started up in '68. Did you have anything to do with that?

TELEGDI: Well, I worked at the Argonne accelerator and later out at this new lab. I did a lot of work out there. I was very fortunate. We first had a good accelerator in the basement of the institute [Enrico Fermi Institute of the University of Chicago]. Then came Argonne. Then came Fermilab. In other words, I never had to travel in order to do my work; it was a great advantage. I could always be home in the evening.

LIPPINCOTT: Nevertheless you decided, because you thought the institute was crumbling, to go back to Zurich to the ETH?

TELEGDI: That's correct. People asked me, "Well, don't you have any idiots at the ETH?" And I said, "Of course we have, but I didn't appoint them." [Laughter]

LIPPINCOTT: By the time you returned, Pauli was long dead.

TELEGDI: Yes, he died in '58.

LIPPINCOTT: Who was there then, when you went back?

TELEGDI: There weren't any particularly remarkable people. Pauli could not be replaced—just as Fermi could not be replaced. But the research conditions were absolutely ideal. There was very little of this begging for money. They supplied you with instruments and money. CERN was well funded. It was extremely easy to get funded. From that point of view, it was an enormous advantage.

LIPPINCOTT: Did you have to do much teaching there?

TELEGDI: Not much—a little more than in Chicago, I would say. Well, Chicago is really fundamentally a graduate school—just the opposite of Harvard. Harvard is essentially an undergraduate school, with a few graduates added, and all the love is lavished on the undergraduates at Harvard. Chicago hardly ever talks about their undergraduates. The university recognizes that your personal teaching of graduate students is the most important form of teaching. So you get a lot of credit for that. At ETH I taught a little more. But I must say, I enjoy teaching.

LIPPINCOTT: Did you teach undergraduates there [ETH]?

TELEGDI: Yes.

LIPPINCOTT: What was the quality of students when you taught in the seventies?

TELEGDI: The question is, compared to whom?

LIPPINCOTT: Well, compared to the students at Caltech?

TELEGDI: The Caltech students are much better. The Caltech undergraduates are better than any I've ever seen. This is not a statement about their intelligence or their knowledge—that's not so superior—but about their motivation. When you teach them, they are generally interested in

what you're talking about. I remember teaching at La Jolla—no such thing. All they wanted was credits.

LIPPINCOTT: This is at UCSD [University of California at San Diego]? You were at UCSD for a year or two.

TELEGDI: Yes, and I was disgusted. The students take a course for credit. If you teach a course that is not required or for credit, you will get very few people to listen to you.

Well, I would say twenty percent of the students [at ETH] are of the caliber of Caltech students, in terms of their motivation. This is not so much—because the Swiss high schools are a hell of a lot better than American high schools, and so the Swiss have it easy. Anyhow, I did my teaching and I enjoyed it.

Another thing they did at ETH, which I *didn't* enjoy, is that people teach the same course for five or six years, and repeat the same stuff. Most of the professors there love that, because it takes very little labor to rehash a course. I can't stand that! At Chicago, you could rarely teach a course more than two times.

LIPPINCOTT: Well, you learn something when you're working up a new course.

TELEGDI: Yes.

LIPPINCOTT: Did you spend a lot of time at CERN during that period, too—the late seventies and the early eighties?

TELEGDI: Well, from the first day I got back to Switzerland—in '76—I lived in Geneva and not in Zurich. I commuted.

LIPPINCOTT: How long did that take?

TELEGDI: Well, I spent two or three days a week in Zurich. I had a group doing atomic physics in Zurich, and I had a group doing particle physics at CERN, and I was commuting. So they really got something for their money—they got two groups for paying me one salary.

LIPPINCOTT: Whom do you remember in particular working with you, who was important to you there? Anybody?

TELEGDI: No.

LIPPINCOTT: No? Did you do much collaborating at that point?

TELEGDI: No. I was running my own groups, surrounded by young people. I didn't much associate with other senior people.

LIPPINCOTT: So you became emeritus in 1989.

TELEGDI: I had to leave. They have no Gray Panthers in Switzerland.

LIPPINCOTT: What is that?

TELEGDI: I mean that there is no law in Switzerland that a person cannot be dismissed because of age.

LIPPINCOTT: But by that time you had been coming to Caltech as a visitor.

TELEGDI: Well, technically that's correct. To my great astonishment, one day I was offered the opportunity to be a Fairchild Scholar here.

LIPPINCOTT: That was in '81?

TELEGDI: Yes, probably. So I came here and I asked them if I could be a Fairchild Scholar in two installments—I couldn't be away from ETH that long. So I came here two times for a quarter. And they liked me here. They found me a house that I could buy if I wanted to—which we did. And I kept coming back as a visiting professor. I taught courses here—that's where my experience with the undergraduates comes from.

LIPPINCOTT: Was it Murray Gell-Mann who was instrumental in bringing you here as a visiting scholar?

TELEGDI: Murray? No.

LIPPINCOTT: But he was still here then.

TELEGDI: Of course. But I don't know who engineered it and I never asked. You know, well before that—in all the years I was in the United States, from '51 to '76—several times the possibility [arose] that I'd be appointed at Caltech. I remember very well when Carl Anderson, during a meeting in Washington D.C., offered me a professorship here. There are reasons why I never took it.

LIPPINCOTT: What are they?

TELEGDI: There was no accelerator of any interest to me here. I would have had to travel, and I didn't like that idea.

LIPPINCOTT: And you had a good berth at Chicago, and a lot of wonderful facilities there.

TELEGDI: Yes, and I had very, very good graduate students. You know, I had to give a lecture at ETH, a terminal lecture—it's the last one we give, and it is given to a large audience in the largest auditorium. In my lecture I said that I could live without colleagues but not without students. The intellectual ferment of my life has always come from the students.

LIPPINCOTT: When you came here to Caltech in '81, did you teach undergraduates?

TELEGDI: No. When I was a Fairchild Scholar I was spared such activities. I had to do nothing—just hang around.

LIPPINCOTT: Did you hang around in Downs [George W. Downs Laboratory of Physics]?

TELEGDI: Yes. Various places. I had contacts also with people in Kellogg [W. K. Kellogg Radiation Laboratory], and so forth. But actually my first visit to Caltech was in the summer of 1953, almost fifty years ago. I worked in Kellogg for one summer.

LIPPINCOTT: Oh, that's interesting! Whom did you work with?

TELEGDI: There was a French visitor there at the same time—a man called Jacques Thirion. We joined forces.

LIPPINCOTT: Charlie Lauritsen had a group in Kellogg at that time.

TELEGDI: [William A.] Fowler, too. But there wasn't anybody around in the summer. It was hot. The only faculty person I remember seeing at that time was Charlie Lauritsen himself. He looked like a crafty old seaman.

LIPPINCOTT: What kind of work did you and Thirion do?

TELEGDI: Well, first I wanted to study a certain nuclear reaction that Murray had pointed out to me as being very interesting. In fact, that's the reason I came to Caltech—to look at that reaction.

LIPPINCOTT: Could you describe it?

TELEGDI: No, it doesn't matter. Anyhow, we didn't do the experiment. We could not induce that particular reaction. So we did something else that occurred to us would be a nice idea. In those days, you know, you would come up with an idea and do it in two or three months, alone or with one other person. This was the two of us working together.

LIPPINCOTT: What kind of experimental facility did they have in Kellogg that you would—?

TELEGDI: Oh, they had good facilities, for that time. They had very good facilities for studying light nuclei—that was the Van de Graaff accelerator. I think they had several of those. Yes, it

was a very well equipped lab, leading the world in that particular area of physics. That's where Fowler's group did all these important experiments, which had some importance for astrophysics.

LIPPINCOTT: Yes. And you didn't come back to Caltech again until the early eighties?

TELEGDI: Yes. Well, I had been there to visit with Murray, to give talks, to be interviewed for a job or whatever. I liked Caltech very much.

LIPPINCOTT: There was a kind of a flirtation with you, then, to get you to come here from Chicago, and maybe from ETH also. You said they interviewed you for a job, so they had their eye on you?

TELEGDI: Yes.

LIPPINCOTT: When you got here in '81, did you do much work with Murray? Or was he not bothering with experimental physicists by that time?

TELEGDI: No. In some of the years that I've been here, I've had very, very frequent and rather strong interactions with him in a certain area of theoretical physics.

LIPPINCOTT: What area was that?

TELEGDI: Well, we might, with some arrogance, call it the foundations of quantum mechanics.

LIPPINCOTT: After that, you became a rather regular visitor at Caltech.

TELEGDI: Yes, I used to call myself the only permanent visiting professor. [Laughter]

LIPPINCOTT: Were these visits for one or two terms?

TELEGDI: Generally only one term—I couldn't be away for more than a term from ETH. You know, there was all kinds of finagling that had to go on so that I could be away. I would take a

sabbatical and cut it up into three pieces and all sorts of mumbo-jumbo so that I could get away for a while. It wasn't good for the physics I was doing in Switzerland to be away, so I fudged it. At the age of sixty-seven, I became emeritus and I added another year on my own, until I had found jobs for all my collaborators. I dissolved my group in '89 and I didn't leave until such time that I found jobs for all the young people who were with me.

LIPPINCOTT: That was good of you.

TELEGDI: Well, I think you have a great responsibility. For instance, I was very careful not to take on any graduate students, who then would not be finished by the time I had to go. At Chicago it was the same thing—I left nobody hanging.

LIPPINCOTT: That's good! In 1987, you had a festschrift, called the Festi-Val.

TELEGDI: I invented that name.

LIPPINCOTT: [Laughter] I see. And Viki Weisskopf was the emcee?

TELEGDI: That's true.

LIPPINCOTT: Where was this?

TELEGDI: At CERN.

LIPPINCOTT: I see that Murray [Gell-Mann] was there and gave a talk.

TELEGDI: Yes, he did.

LIPPINCOTT: Do you have any other memories of the Festi-Val?

TELEGDI: Well, it was a fairly nice occasion and I met a lot of old friends who came there. The book contains a number of really good articles. Very often, these festschrifts contain some junk,

people giving talks they wouldn't publish otherwise. But there are at least two or three articles that will survive.

LIPPINCOTT: Murray's was on the theory of superstrings or something like that?

TELEGDI: Yes. There was another very interesting article by [Subrahmanyan] Chandrasekhar. And there was an article by Carlo Rubbia, an experimental article.

LIPPINCOTT: And then I want to talk about Murray's festschrift here at Caltech, a couple of years later. It was January of 1989.

TELEGDI: The year of his sixtieth birthday, but not the month. I made a joke, because he was born in September. I said that we must be celebrating his conception, not his birthday.

LIPPINCOTT: [Laughter] Well, it's important when somebody like that is conceived, don't you think?

TELEGDI: Yes.

LIPPINCOTT: And you gave a talk called "For the Birds?"

TELEGDI: No, the talk was entitled, "Is Quantum Mechanics for the Birds?" [Laughter]

LIPPINCOTT: Is that because Murray's an ornithologist?

TELEGDI: Well, I wouldn't say he's an ornithologist, but he's a bird-fancier. [Tape ends]

Begin Tape 1, Side 2

LIPPINCOTT: We were talking about the festschrift for Murray Gell-Mann in January of 1989. And you gave a talk, "Is Quantum Mechanics for the Birds?" And I remember some of the other people who were there. I think Murph Goldberger spoke, and John Schwarz.

TELEGDI: Yes. Abdus Salam is one I remember. And Francis Low. And there was a mathematician from MIT called [Isadore M.] Singer. A few people I remember. T. D. Lee was there.

LIPPINCOTT: Yes, he gave a talk a day or so later, in Beckman. I'd never seen him, and I was just very impressed with him, because he had on a beautiful blue suit, and he had a kind of wonderful little swagger to him.

TELEGDI: T. D.? I think he has a son who is on the faculty of Caltech—in Chinese history [James Z. Lee, professor of history]. Do you remember some of the slides I showed at Murray's festschrift?

LIPPINCOTT: Yes. There was a picture of Murray in a cage full of lions, or something.

TELEGDI: Yes, taming tigers. Well, he always thought of himself as a Renaissance man who could do everything, and I wanted to ridicule that. [Laughter] So I showed him doing the most improbable things, like taming tigers.

LIPPINCOTT: And there were other slides.

TELEGDI: Oh, yes, there were quite a few. He was seen in a kitchen with a *sous-chef* preparing some elaborate dish. And then a great balancing act, with glasses he was balancing on his head.

LIPPINCOTT: Those photographs are in *Engineering and Science*, the Caltech journal. They were published along with excerpts from some of the talks, and that's where I saw them.

TELEGDI: They were published? They are not published in the book that has the lectures, because I didn't allow that. I bought the original pictures—which I then modified—from some press agency. And when I bought them I had to specify how and where they would be published, and I said that they would not be published, I would use them in a talk. I would have had to pay much more money for them if they were to be published. That's why I didn't include them in the printed version of my talk; I'd spent enough money.

LIPPINCOTT: So how did Murray take all the fun that was poked at him at that festschrift?

TELEGDI: From me, he took it very well.

LIPPINCOTT: And from others?

TELEGDI: Less gladly. [Laughter] There were people in the audience who were laughing so much—my talk was such an enormous success, from a humorous point of view—that he had no alternative but to laugh, too.

LIPPINCOTT: Was he annoyed at some of the others?

TELEGDI: I don't know about that. Let's say he's not a person who enjoys being criticized.

LIPPINCOTT: You told me that George Johnson's biography [of Murray Gell-Mann], *Strange Beauty*, is—

TELEGDI: A masterpiece.

LIPPINCOTT: I mean to read it.

TELEGDI: You will read it with the eye of an editor. Maybe from an editor's point of view it's poor, I don't know. But I'm looking at it with different eyes. I'm just a reader.

LIPPINCOTT: Well, I think George Johnson's a good writer, and he probably got Murray down pretty well.

TELEGDI: I think so.

LIPPINCOTT: Did you ever visit Murray at the Santa Fe Institute?

TELEGDI: I've never been there. I don't particularly enjoy Los Alamos, but the city of Santa Fe is a beautiful place.

LIPPINCOTT: I wanted to ask you if you ever were at Los Alamos? Did you do any work there on the—?

TELEGDI: No, no. But I can tell you that in times of peace, I would have refused. I think that when there's a war, anything you can do can be interpreted as defense.

LIPPINCOTT: You never visited the National Laboratory there?

TELEGDI: Of course I did. To give lectures.

LIPPINCOTT: We haven't talked about von Neumann.

TELEGDI: He's one of the celebrated Hungarians whom I have not really known. I've seen him once. He once gave a talk at Chicago. So I physically was in his presence, but I didn't exchange two words with him. It's not like Szilard, or Teller, or Wigner—people with whom I had extensive discussions.

LIPPINCOTT: What about [Eugene] Wigner? We haven't mentioned him. He was at Princeton?

TELEGDI: Yes. He also once spent a quarter at Chicago—I think it was in '57. He was a very interesting person, but I have not much to say about him. The thing he was known for and is always mentioned is his exaggerated sense of courtesy. He was very polite. But it was fake. In Hungarian, he was completely different. He thought that this kind of behavior was appropriate behavior for Anglo-Saxons, so he adopted it. [Laughter]

LIPPINCOTT: Along with your experimental work in particle physics, you're interested in the history of physics, isn't that right?

TELEGDI: That's true.

LIPPINCOTT: And you've done a lot of writing. But you've never written a book, except I think, one physics textbook, right?

TELEGDI: Not even that. I wrote, with two graduate students, a book of exam problems [*University of Chicago Graduate Problems in Physics, with Solutions*, Jeremiah A. Cronin, David F. Greenberg, Valentine L. Telegdi (Chicago: Univ. of Chicago Press, 1967)]. It's a successful book. Most people think the problems are too hard, but that's tough luck. The book was translated into Russian.

LIPPINCOTT: So you write your history pieces for *Physics Today* and other publications?

TELEGDI: Yes. I have always been interested in the history of physics—for as long as I've been in physics myself. To me, that's the cultural side of physics. On the other hand, I tell all my students that knowing the history of physics is completely irrelevant, and they shouldn't be mistaken by the fact that I'm interested. I tell them that they could be the greatest physicist of their own age without knowing when Newton lived. It's completely irrelevant. It's a cultural activity.

LIPPINCOTT: Yes. On the other hand, don't you think that it's important to know the context in which you're doing your problems?

TELEGDI: No.

LIPPINCOTT: You do not. Don't you think that if you don't know these things, you might reinvent the wheel?

TELEGDI: Well, Dick Feynman reinvented the wheel, but it turned out to be a much better wheel.

LIPPINCOTT: [Laughter] OK. Tell me about your Sommerfeld talk. You've given that quite a bit.

TELEGDI: Well, I'm going to give it two days from now, here. I have been working on that thing for many, many years. Not that I was working hard, but I interrupted my work.

LIPPINCOTT: He was way before you. You would never have met Arnold Sommerfeld?

TELEGDI: I did. Yes, and that shows two things—he lived for a very long time, and I'm very old. [Laughter]

LIPPINCOTT: Where and when did you meet Sommerfeld?

TELEGDI: Sommerfeld was, in a way, my grandfather. He was the teacher of Pauli. He once came to Zurich and paid a visit to Pauli, and I met him then. He was about as old then as I am now.

LIPPINCOTT: I wanted to ask you about [Werner] Heisenberg. You told me you'd met Heisenberg.

TELEGDI: Several times.

LIPPINCOTT: Where did you meet him?

TELEGDI: I met him at CERN; I met him at international conferences.

LIPPINCOTT: This was when he was quite a bit older?

TELEGDI: Yes.

LIPPINCOTT: And he went off a bit on the wrong tangent, did he not?

TELEGDI: Well, I can't pass that judgment, but that's what everybody else says. I haven't read his papers of his last ten years. The point was that he was barking up the wrong tree. But I think it's not an uncommon phenomenon among aging theoreticians to want to invent the formula of the whole universe.

LIPPINCOTT: He was so remarkable when he was very young.

TELEGDI: He was *unbelievable*—totally exceptional. He's also a great writer of the German language. His books are wonderful—just from the literary point of view.

LIPPINCOTT: Would you be able to get that impression in translation? Or do you have to read them in German to—?

TELEGDI: Well, I've never tried reading them in translation. [Laughter] Why should I?

LIPPINCOTT: How many languages do you speak?

TELEGDI: Five and a half.

LIPPINCOTT: Five and a half?

TELEGDI: Yes, because one of those languages is not really a bona-fide language. The five languages are all modern languages; the half language is Swiss-German. Swiss-German is to German like hillbilly is to Oxford English. The Swiss don't talk good German to each other—they speak this hillbilly German.

LIPPINCOTT: When you were a child, what was your natural language?

TELEGDI: Hungarian—and also, in parallel, Bulgarian, because that's where I was living. But I've forgotten Bulgarian.

LIPPINCOTT: Well, during World War II, you bounced around, you told me.

TELEGDI: During the war, I was in three places. When the war broke out, I was in Belgium. I stayed there for a little while. And then I went back to Italy, where my parents were living at that time. I lived in Italy from 1940 to 1943, which was no picnic, because Milan and the other cities were constantly being bombed. The fall of 1943—after the Allies had landed in southern Italy—I escaped with my mother to Switzerland. So, a little bit of the war in Belgium, three years in Italy, and the rest in Switzerland.

LIPPINCOTT: Where did you meet Lia? In Italy?

TELEGDI: I met Lia in Zurich. She had a job there. I think her big aim in life was to get away from home, and so she got herself a job in Zurich.

LIPPINCOTT: She wasn't a student, then?

TELEGDI: No, no. She was a secretary. She also knew several languages. Not as well as I, because she hadn't lived in those countries. We met each other and we married a year before we left for the United States.

LIPPINCOTT: So, a year before you got your PhD?

TELEGDI: No, I wouldn't do that. It was after. You didn't marry in my day if you had no security. You were supposed to be able to support the wife and possibly even children. So unless you were wealthy, you had to wait until you had the degree.

LIPPINCOTT: Let's get back to Caltech. You're visiting here every year from 1981 up through 1995. Pretty regularly. Whom did you interact with here, principally?

TELEGDI: As long as he was here, I interacted a lot with Murray [Gell-Mann], as I told you. I also had a fair amount of interaction with Dick Feynman. And then in nuclear physics, there was a man who retired rather recently, called Felix Boehm. He was a fellow graduate student at ETH, and he came here—he was already here in 1953, when I visited. I had lots of professional contact with him. I also had contact with a few other people.

LIPPINCOTT: You wrote a piece about Feynman for *Physics Today*. I read it; it's nice.

TELEGDI: It's very short. You actually read it?

LIPPINCOTT: Yes, I did. You call him a teacher's teacher and a physicist's physicist. He had more to do with the experimental physicists than, say, Murray did—isn't that right?

TELEGDI: Well, Murray's contact with experimental physicists was very rare. He would have some contact when those people were doing something of immediate direct interest to whatever Murray was involved in on the theoretical plane. But in my opinion, Murray had no feeling for experiment whatsoever. That's true of most theoretical physicists. But Feynman would read experimental papers and he could figure out where they went wrong. He had a real knack for understanding. I remember one day he showed me a paper and asked me what I thought of it. And I said, "Well, there's three mistakes in that paper." And he sort of hit his head with his hands and said, "Oh, I'm so dumb! Those three I could have figured out myself."

LIPPINCOTT: Why do you think Murray wasn't good at understanding experimental things?

TELEGDI: Simply because he didn't care. You know, he also never cared about any kind of physics outside particle physics. About all those other problems he felt, "I just would solve them if I cared to, but the real problems are these and I can't waste my interest."

LIPPINCOTT: He got interested a bit in superstring theory.

TELEGDI: Yes, but that's part of elementary particle physics, so that's natural for him. But Feynman was really very sharp when it came to experiments.

LIPPINCOTT: So you would talk with him maybe about partons—his version of quarks?

TELEGDI: Well, that wasn't a subject I particularly discussed with him, but there were others. He once paid me a very nice compliment, which I'm much prouder of than those honorary degrees and stuff. He went to the blackboard and said, "You know, Val, you've got such simple explanations for almost everything. Can you explain this one?" Coming from him, that's quite a compliment. [Laughter] Generally, *he* was the man who gave the simple explanations for everything. I'm very proud of that. He was tired that day, and he had made a mistake.

LIPPINCOTT: What did you think in general of the physics division at Caltech? You enjoyed coming here?

TELEGDI: Oh, yes, I enjoyed coming—although the place just couldn't ever be the same without Feynman and Gell-Mann. But I think that the real strength nowadays is not in physics. I think it's in astronomy and astrophysics. I think they are unsurpassed at the moment. The fact that the president of Caltech [David Baltimore] is not a physicist, contrary to tradition, is a clear indication of where we are moving. I think biology has probably made bigger strides in the last fifteen years than physics.

LIPPINCOTT: Well, these things are cyclical. I wanted to ask you more toward the end of the interview, but I'll ask you now, because we're talking about it. What do you think of the future of particle physics? Do you think that it's at an impasse at the moment?

TELEGDI: Yes, I think it has entered a fairly Byzantine stage. Up to now, we probably could get away by multiplying whatever means we had by a factor of ten and building something ten times bigger. [Laughter] But I think we have reached the end of the rope, unless somebody invents an entirely new method of accelerating particles.

LIPPINCOTT: Well, do you think that when the Large Hadron Collider comes on line at CERN—I think that will be in a few years, won't it?

TELEGDI: Supposedly in 2006.

LIPPINCOTT: Well, are you anticipating some really important new—?

TELEGDI: No. I anticipate that that accelerator, like almost every accelerator, will win its glory on things that nobody expects. If they just find what people say now that they should be looking for, it will be dull. There are very nice theoretical predictions of what one should see with this machine, and if that's all we do, it's boring. You know, the prospectus for the Chicago synchrocyclotron was written by no lesser man than Enrico Fermi, and that prospectus didn't anticipate the two major discoveries that were made with that accelerator.

LIPPINCOTT: Well, it will be interesting if we can find more experimental validation of superstring theory.

TELEGDI: What do you mean, "More?" There is zero, strictly zero. There's no experimental verification at present at all.

LIPPINCOTT: But aren't there little indications?

TELEGDI: I don't know of a single thing in physics that anybody would take as an experimental indication.

LIPPINCOTT: Do you think there ever will be, or is just too much energy required?

TELEGDI: I'm not psychic. [Laughter] I don't know whether there will ever be or not. Even if that theory were to be true, the question is, What is the energy scale if it is true? You miss the energy scale, it may be true and you may not see it. I don't know. I mean, it certainly has turned an enormous number of people at the present moment away from concrete physics. For instance, when you watch what's going on in astronomy, they have these telescopes which are more and more and more powerful. That's the area that's moving like particle physics was moving thirty years ago.

LIPPINCOTT: And now particle physics is at a standstill, more or less.

TELEGDI: More or less, yes. I'm very lucky. I lived at the time when you could do significant experiments with a small group without spending millions of dollars. Parity violation must have cost a total of $1,000, at most.

LIPPINCOTT: Really? That is fantastic! [Laughter]

I hope you don't think this is a stupid question.

TELEGDI: There are no stupid questions—only stupid answers.

LIPPINCOTT: What's your personal opinion of superstring theory? Do you think it's going to be fruitful? Do you like it as a theory?

TELEGDI: I have no opinion on that theory. I just know its barest outlines. What is very important to me is two points: A theory should be internally consistent and it should have some contact with observation. Well, I'm told by all the experts that this theory is internally consistent, although they think up new interpretations every time I turn my back. But contact with reality? Nobody's given me anything. I just watch. I'm somewhat unhappy that so many people are working on it. To me, as a physicist, it's sort of sad that so many people at the same time work at something that doesn't seem to have any contact with experiment. But that, to some extent, is due to the fact that we don't have any great experimental puzzle to be thinking about. We have to supply the puzzles, and there aren't that many puzzles right now.

LIPPINCOTT: Did you have any interaction at all with Ed [Edward] Witten when he was here?

TELEGDI: Very little. Ed Witten is, again, one of those people to whom you can talk only about one subject—and that is whatever he's working on. Plus I think he is so fanatic about Israel. I did discuss that point with him, and I think he's a fanatic—very concerned. He was very shocked when I walked up to him and said, "You know, you and I have something in common." [Laughter] His eyes popped out of his head and he said, "What do you mean by that?" I said, "Well, we both have Italian wives."

LIPPINCOTT: That's right. It's too bad that Caltech lost him.

TELEGDI: They never gained him—so how could we lose him?

LIPPINCOTT: Well, he was here for a couple of years, and I think they wanted him to stay.

TELEGDI: Oh, I know that. Yes, I regret that, too, because the man is certainly exceptional.

LIPPINCOTT: I went to a talk of his. I only understood two percent of it.

TELEGDI: He always sounds a little messianic.

LIPPINCOTT: What else can we say about Caltech? Did you do any teaching in those years?

TELEGDI: We talked about that already. When I was a visiting professor here, after the Fairchild was over, I taught various courses.

LIPPINCOTT: Did you teach undergraduates or graduates?

TELEGDI: I taught both.

LIPPINCOTT: What kinds of courses did you teach?

TELEGDI: Electromagnetism, nuclear physics, particle physics. They give it to me, I do it. Well, I don't teach solid state physics, because I don't know enough. But I like to vary what I'm teaching.

LIPPINCOTT: So they would just ask you to teach one or another course, and you complied? That was nice of you.

TELEGDI: I like to do that. And they paid me.

LIPPINCOTT: And you have a high opinion of the students?

TELEGDI: Yes, in terms of their motivation.

LIPPINCOTT: Did you ever work with any of the postdocs or the graduate students here?

TELEGDI: No. I had a couple of graduate students over there in particle physics, so I had a fair amount of contact, but I never did any work with them.

LIPPINCOTT: What about the heads of the division? [Thomas A.] Tombrello is the head now.

TELEGDI: I barely know him. I knew Robbie [Rochus E.] Vogt very well because he was a student at Chicago. In fact, I'm the person who persuaded him not to go back to Germany. He felt a great urge to serve the fatherland, at which point I said to him, "What has the fatherland ever done for you?" He thought about it and had no answer.

LIPPINCOTT: This is in the fifties. We're not talking about the Nazis.

TELEGDI: No. [Laughter]

LIPPINCOTT: Did Robbie Vogt ever take any classes from you at Chicago?

TELEGDI: That's something you'd have to ask him. He was a very bright student. Very Germanic and very bright. He's not all that Germanic, because after all he has a French wife.

LIPPINCOTT: Charles Peck came later as division chairman [1993-1998].

TELEGDI: I had some contact with him. And Maarten Schmidt was chairman when I first got the Fairchild—I remember he signed the letter.

LIPPINCOTT: Can you think of anything else you'd like to say about Caltech? About your experiences here and how you feel about them.

TELEGDI: Well, I really like Caltech. I was very tempted on a couple of occasions to come here and join the faculty.

LIPPINCOTT: By the time you came here, Murph Goldberger was here. And then [Thomas E.] Everhart [Caltech president 1987-1997] came.

TELEGDI: Yes. I had no contact with Everhart. Nobody seems to have such fond memories of Everhart, I've found out. I had no contact with him.

LIPPINCOTT: He was an engineer, as opposed to all the physicists who had run the institute before.

I think we've covered almost everything. Is there anything else?

TELEGDI: Well, as I say, Caltech is small. Caltech, by and large, has a very good faculty. It attracts excellent undergraduates. I'm not sure about the graduate students; I think the graduate students of Chicago in physics are just as good, or better. And Caltech has this wonderful

student-faculty ratio, which is almost unique. And it's physically small. Everything's within walking distance. So there is a big concentration of intellectual abilities.

LIPPINCOTT: So you're glad to be here.

TELEGDI: Yes, I'm very glad to be here. Although, I must say, I'm not the person I was ten years ago and Caltech is not the place it was ten years ago. [Tape ends]

VALENTINE L. TELEGDI

SESSION 2

March 9, 2002

Begin Tape 2, Side 1

Lippincott: Dr. Telegdi, let's talk a little bit more about the University of Chicago, and when you got there [1951]. I'd like to hear some of your recollections of Fermi.

TELEGDI: Well, Fermi, of course, was the main attraction at the University of Chicago as far as physics was concerned. Fermi spent a fair part of the war, first at Chicago and then at Los Alamos, on the so-called Manhattan Project. But after the war, the University of Chicago made some kind of package deal involving a group of very distinguished scientists, with Fermi, Harold Urey—I'm not going to list them all. And in the space of six months or so, Chicago became what I call the mecca of physics. There was no place anywhere in the world of similar concentration of talent in physics—both at the senior level, which was this package surrounding Fermi, and some of the junior people who had worked with Fermi and the others on the Manhattan Project. And extremely brilliant students that Fermi attracted. Fermi, in addition to being a great physicist who could do both theory and experiments—which was very, very exceptional; there are only two or three such people in this century who could do that—was also a very great teacher. We always speak about him as the man who built the first working reactor. This is true; he was leading that effort. But that, I think, is not the way he left his mark on the United States. I think he left his mark on the United States primarily through his teaching—the way he was teaching, his style, the way he trained graduate students both in theory and experiment. And the Fermi approach to teaching physics has pervaded all the great universities of this country. And I think that was the biggest impact he had. If he hadn't built a reactor—well, I'm sure somebody else would have built it.

LIPPINCOTT: That was ten years before you got there, roughly.

TELEGDI: That was on the Manhattan Project, not at the university. He was not a professor at the university at that time; he was working on that wartime project.

LIPPINCOTT: At the Metallurgical Lab at Chicago.

TELEGDI: Yes. And after the reactor worked well, he left for Los Alamos. And in 1946 he accepted an appointment at the University of Chicago and brought with him a large group of exceptional people. Edward Teller was one of them.

I think I mentioned to you that I didn't get immediately to Chicago—that I was first rejected by Chicago. And then through a very fortunate combination of circumstances, I was accepted. At that time, they had wastepaper baskets full of applications from bright young men from all over the world. And there were, of course, very brilliant students who had come there—either directly from the army, like [Murph] Goldberger, who worked as soldiers assigned to the Manhattan Project. Well, I wanted to go to Chicago because, as I said before, it seemed to me the most unlikely place that would take me. There were two things. First of all, this letter from [Victor] Weisskopf, who recommended me to Fermi. But that's not the whole story. They had a committee for selecting postdocs, and one of the people on that committee was a man who until shortly before had been a professor of theoretical physics at the University of Zurich. He knew me, and he supported my application simply on the grounds that he had met me before and formed some idea about my talents. I think that was very important, because I really hadn't accomplished anything up until then. I mean, I'd never done anything.

LIPPINCOTT: You're being modest.

TELEGDI: No, no, no. I did a good thesis, but not something that would stand out. The only remarkable thing about it at that time was that I had done my own theory corresponding to the experimental part—not the usual approach at all. Which actually—now that I think of it, fifty years later—is very much a Fermi approach. Fermi never wanted to separate experimental and theoretical physics so sharply—especially not during the time that students were being trained. So this must have been moderately attractive.

Now, there's another little detail. When I had to fill out the form, applying for this postdoctoral fellowship—at Chicago it would be called a research associate—there was a little asterisk, and the note at the bottom of the questionnaire said, "The University reserves itself the right to transform a research associate appointment into a junior faculty appointment."

Well, when I filled out the form I wrote that I intended to pursue an academic career, therefore I would prefer right from the very beginning to be on the faculty. And maybe that made a big difference to me later in life. Maybe if I hadn't started out as an instructor—which is the lowest degree of faculty appointment—maybe I never would have made an academic career, which was what I wanted to do.

Now, this was not an entirely platonic arrangement. Because if you had a fellowship, you paid no taxes, but if you were an instructor, you did. You see, a fellowship was an award and an instructorship was just a form of employment.

LIPPINCOTT: Tell me, what was the name of that professor at the University of Zurich who was on the nominating committee.

TELEGDI: Gregor Wentzel. He had occupied a very distinguished chair [at Zurich]—he was the successor of [Erwin] Schrödinger. Zurich was like Cambridge [Massachusetts], with two sister institutions—the University of Zurich and ETH. You could go from one to the other, listening to lectures. Pauli was at ETH, and Wentzel was at the university, and they both had been students of the same professor in Munich, so there were very lively relations there.

LIPPINCOTT: Was that [Arnold] Sommerfeld?

TELEGDI: Yes, that's right. And as I was progressing in physics at Chicago—as I was developing, doing more interesting things—Wentzel, who was a rather formal German person, often pointed out, [adopts German accent] "You know, I zupported your appointment, unt I did *right*!!" [Laughter]

LIPPINCOTT: Now he was on the faculty of the University of Chicago?

TELEGDI: Yes. Everybody was shocked [when he left Zurich]. He arrived as a young man in Zurich in, I don't know, 1928, and suddenly in 1948 he decided, "I've had enough," and left for the United States. He would spend the rest of his life here. For him also, Chicago was the mecca of physics. He was a very great teacher, very much appreciated at Chicago. But he certainly was not in the same class as Fermi—but then nobody was.

Well, I was telling you about not being impressed by Fermi and all those people because no matter what they did, I expected them to be able to do it. But I was very much impressed by my contemporaries, by the young people at the instructor, assistant-professor level.

LIPPINCOTT: You mentioned [Richard] Garwin.

TELEGDI: Garwin and [Murray] Gell-Mann—both seven years junior to me. I mean, I just couldn't imagine that such people existed! And the rest of the crowd were pretty talented people, too. You know, people like Murph Goldberger, et cetera. I once gave a talk about those Chicago years, and I summarized them in the following sentence: It was a place where you could be proud to be the dumbest one.

LIPPINCOTT: [Laughter] That's like what you said when you got tenure.

TELEGDI: Yes.

LIPPINCOTT: You said—

TELEGDI: "The university slowly sank to my level."

LIPPINCOTT: [Laughter] Yes. This was in 1972?

TELEGDI: No, I got tenure much earlier.

LIPPINCOTT: Oh…maybe it was the Fermi chair you got in '72.

TELEGDI: The Fermi chair was, so to speak, created for me. There hadn't been one before. But it was a little bit phony, because generally these named chairs involve some particular privileges. There wasn't anything; there was just a title. But it was a very fancy title. It was actually fancy to the point that it embarrassed me.

LIPPINCOTT: So it didn't entail your getting extra research funds, or anything like that?

TELEGDI: No, no. There was one little thing I got. When the president called me up and they said to me—maybe it was the provost, I don't remember—anyhow, when the great news was announced to me, I thanked him, and then I said, "Well, what are the advantages? What are the privileges?" He was a little embarrassed. I said to him, "You know, another university"—I shall not name it now—"offered me a named chair, but there was something going with it—$10,000 a year that I could spend without the supervision of any dean or any other person, on anything I want." Unrestricted. And I could travel. Well, when he heard that, I got that—a yearly sum that I could spend.

There was a joke that the General Electric Company wanted to endow a chair in physics at the University of Chicago, but they gave up the idea, because nobody wanted to sit in the General Electric Chair of Physics. [Laughter]

LIPPINCOTT: [Laughter] Is this true, or is it something you made up?

TELEGDI: Well, if it's not true, it's still a good joke. Anyhow, there was this unbelievable congregation of people [at Chicago]—probably almost as great an assembly of people as at Los Alamos itself.

LIPPINCOTT: I want to go back a little. You worked with Murray [Gell-Mann] in the fifties at Chicago, and then he left to go to Caltech—I think that was in the middle fifties.

TELEGDI: '54. You see, after Fermi died, he simply didn't come back. He was on leave at that time—I think at Columbia. He had met his wife, Margaret. And Fermi was dead. So he decided that he needed to change his lifestyle.

LIPPINCOTT: I wanted to talk about his discovery of quarks in '63 and the parallel work done by George Zweig at CERN. Were you interested in that?

TELEGDI: I was interested.

LIPPINCOTT: Murray had left for Caltech by then—

TELEGDI: And Zweig was in Europe, at CERN. And he was also at the summer school in Erice, in Sicily, and that's where I heard him talk about it. I was one of the first people invited to Erice. Dick Feynman was there—but not at that particular meeting.

LIPPINCOTT: This would have been in '63.

TELEGDI: Yes, or '62. Anyhow, Zweig had a problem—namely, that the head of the theory division [at CERN] didn't allow him to publish his work in the *Physical Review*. I think the man was unable to understand how important it was. He missed a lot of other points—that head of the theory division—so this is no news. At any rate, I must say I didn't get much out of Zweig's lecture. He wasn't very good at selling his idea.

LIPPINCOTT: Zweig called them "aces."

TELEGDI: Yes, but he just didn't sell it very well.

LIPPINCOTT: No. I think all he did in the end was to circulate a preprint instead of publishing. But you first heard about this notion of quarks from him?

TELEGDI: Yes.

LIPPINCOTT: Did you have any back and forth with Murray about it, after—?

TELEGDI: None whatsoever. Personally, I must say, one has to be very careful, because Murray has a certain tendency to rewrite history. He is of course now of the opinion that he considered quarks as physical objects, and I don't think that is entirely true. I think he considered them as mathematical objects. If he had considered quarks as physical objects, then after the Stanford experiments, he, Murray, would have invented the parton model and not Dick Feynman.

LIPPINCOTT: You mean the Kendall-Taylor-Friedman experiments [1966-1978 experiments conducted by Henry Kendall, Richard Taylor, and Jerome Friedman at SLAC, the Stanford Linear Accelerator].

TELEGDI: Yes. [Jerome] Friedman was my postdoc.

LIPPINCOTT: Yes, well, I think Murray acknowledges Zweig's accomplishment, in any case.

TELEGDI: Yes, Murray is very fair to Zweig, but there's another debate among historians as to which way Murray thought about these quarks—as real or just as mathematical objects. He rewrites history very successfully.

LIPPINCOTT: So now he says that he had always thought of them as physical objects?

TELEGDI: Yes.

LIPPINCOTT: Do you know whether Zweig thought of them that way?

TELEGDI: I'm convinced that he did.

LIPPINCOTT: That's interesting. So, tell me about Erice. Were you one of the people who founded the summer school?

TELEGDI: No, but I was among the few people who were invited to the earliest sessions. And that is due to the fact, I believe, that I knew the man who founded the school—a physicist by the name of Nino [Antonino] Zichichi, who is Sicilian. He organized this school, and he managed also to get NATO support. He had been one of the junior members of the team that Garwin and I led [at CERN] in '59-'60. So he knew me—in a way, he also knew me as a lecturer from talks I had given at CERN. Plus I had the advantage that I also spoke Italian.

LIPPINCOTT: When you worked with Garwin, this was at CERN?

TELEGDI: Yes.

LIPPINCOTT: And what kind of work were you doing?

TELEGDI: We were measuring what is called the anomalous magnetic moment of the muon. The muon is a heavy electron, but it wasn't so clear then that it was a heavy electron—one suspected it. Well, if it really was a heavy electron and different from the electron only in its mass, being about 200 times heavier, then you could predict its magnetic moment to be essentially a certain unit, the same as that of the electron. So Garwin and I organized this experiment to measure the so-called anomaly. I invented the name for the experiment, which now everybody uses for the last forty years. It's called the g-minus-2 experiment, because g is the gyromagnetic factor. That g is supposed to be first approximation equal to 2—exactly 2—but it is not. There's a small correction, about 1 part in 1,000. So let's say it's 2 plus 0.001. So g minus 2 is then equal just to this little extra piece, the anomaly. The experiment does not measure g but it directly measures the anomaly. It's just the difference between two large numbers, but it measures the little difference directly. [H. Richard] Crane and collaborators, I must add, at the University of Michigan, had laid the foundations for such an experiment, and they developed a brilliant method for measuring this anomaly in the case of the electron. And what we did in Geneva was something analogous, although totally different in detail, for the case of the muon. While the electron is a stable particle, the muon lives only millionths of a second, so the methods are not the same.

LIPPINCOTT: So that was successful work?

TELEGDI: Very successful! Done in record time.

LIPPINCOTT: And that's what prompted Zichichi to invite you to Erice?

TELEGDI: I don't know that—but he worked with us. A few remarkable people were in the junior group that Garwin and I had. One of them was Georges Charpak; not so long ago he won the Nobel Prize [1992]. He was a Frenchman who was with us. Another former student of mine was called Hans Sens, a Dutchman. I used to joke and say that I had two students, Sens and Nonsens.

LIPPINCOTT: [Laughter] At Erice, was that the first time you met Feynman?

TELEGDI: No, I had met Feynman many years before—it must have been the very late forties—when he came to Zurich to give a talk.

LIPPINCOTT: What did he talk about?

TELEGDI: He talked about his spacetime approach to quantum mechanics. And I remember it like it was yesterday. I told the story to Dick many times, years later. He came in and the other professors were sitting in the first row.

LIPPINCOTT: Was this at ETH?

TELEGDI: At a colloquium at ETH. I don't know how he was dressed, but he was his usual self. There was a professor at the university there—also a very famous man, [Walter] Heitler. And Dick acted like a graduate student. I remember all his gestures. He kept the chalk between his two hands, and he was rolling the chalk—his usual self. His lecture, of course, was brilliant, and at the end of the lecture Professor Heitler got up and started talking very politely to Feynman. He said, "What you did doesn't sound to me exactly like orthodox quantum mechanics." And Feynman said, "Look, Heitler, this ain't orthodox quantum mechanics. This is something I figured out." [Laughter] I'd never heard a man in a colloquium behave in this fashion. But, of course, he was right.

After the colloquium, I went to him and asked a question at the blackboard. And he very kindly answered me right away. So that was my first meeting with Feynman. And then he came to Chicago. He came to give a series of lectures on liquid helium.

LIPPINCOTT: Liquid helium?

TELEGDI: Yes, because he had done some work on liquid helium. That was the second time I saw him. And from then on I saw him very frequently.

LIPPINCOTT: Would you say that you became friends?

TELEGDI: Friends? I think so. I can assert with some pride that we were good, close friends.

LIPPINCOTT: How long is the summer school at Erice? Is it just a week or two?

TELEGDI: About two weeks. Nowadays there are many summer schools in Erice, on different topics. But you see, those summer schools gave the Europeans an opportunity they had never had before—namely, to be in daily informal contact with professors. This just did not exist—the professor you met in the lecture hall.

LIPPINCOTT: It's more formal than it is here?

TELEGDI: That's the understatement of the year. Just yesterday, I was telling a friend here at Caltech a little anecdote about the famous Hungarian physicist, one of the few that people remember—[Baron Roland von] Eötvös. He was landed gentry; he was a baron. It's very unusual for landed gentry in Hungary to go into science. But anyhow, he was covered with honors. He became minister of education and president of the academy—and everything else that you could be.

LIPPINCOTT: When was this?

TELEGDI: Let's say at the turn of the century. He was very formal, and he was examining a student—probably a medical student. The student was very mixed up during the exam, and he said to him, "Well, now, Professor Eötvös, so-and-so and so-and-so." Well, Eötvös had the right to be addressed as Excellency, so he looked at the student and said, "Now come on, my young friend. If you reach this point, you might just as well call me Uncle Roland." [Laughter]

LIPPINCOTT: Are you saying that Hungarian full professors were addressed as Excellency?

TELEGDI: No, but he was.

LIPPINCOTT: Oh, because he was a baron?

TELEGDI: No, that required other forms of address. I think it's because he had been minister.

LIPPINCOTT: Oh, I see. [Laughter] Another thing I think I forgot to ask you last time was—you mentioned that when you got the Fermi chair in 1972, that a little bit of money came with it, and that you had the wherewithal to travel. Did you take many sabbaticals when you were at

Chicago? I know you went to Harvard one year.

TELEGDI: No, I didn't go for a year, I just went for a quarter.

LIPPINCOTT: Did you ever take sabbaticals?

TELEGDI: Yes. The year at CERN that I spent with Garwin was a sabbatical. That was a little bit more than a full year. I came in the fall of '59 and left at Christmas of '60.

LIPPINCOTT: Any others that you can recall? You went to Yale, didn't you, to teach?

TELEGDI: I gave a series of so-called distinguished lectures—I've done that in many places. I have a list of all these fancy lectures. Well, for that I didn't need a sabbatical.

LIPPINCOTT: You weren't a visiting professor?

TELEGDI: No, I was a Page Lecturer. Page was some professor there a hundred years ago, eighty years ago. I've given the Schiff lectures at Stanford. There were several such series of lectures.

LIPPINCOTT: How about the Institute for Advanced Study?

TELEGDI: Well, now here we come to a kind of sabbatical. I took a sabbatical—of three months or so—to go to Princeton.

LIPPINCOTT: When was that?

TELEGDI: Early '56, I think—anyhow, it was before parity violation. I got the money—a big check—from Princeton, and I got a beautiful, elegant office at the Institute. And I always used to tell Lia, my wife, that J. Robert Oppenheimer paid for two of my suits.

LIPPINCOTT: He was the director then?

TELEGDI: Yes. And at the end of my stay, I went to the appropriate cashier's office and wanted to pay the rent for my apartment at the Institute. She just looked at the papers and said, "Well, the director has taken care of that." I didn't have to pay—so I went to a decent shop in downtown Princeton and bought myself two suits. [Laughter]

LIPPINCOTT: Einstein died in '55, is that right?

TELEGDI: I think that's exact.

LIPPINCOTT: Were you at the Institute just then, or did he die before you got there?

TELEGDI: He died before I got there.

LIPPINCOTT: Had you ever met him?

TELEGDI: No. I have to tell you one thing about Princeton. I thought the university was very stiff and very formal.

LIPPINCOTT: The physics department or the whole place?

TELEGDI: Yes—well, I never knew the rest of it. By the way, I formed the same impression of Harvard. Compared to Chicago, those places were very stiff and very formal. I had to give a colloquium at Princeton, and I was introduced by somebody—one of the junior faculty. I gave him a piece of paper to introduce me, and he looked at this little piece of paper and he said, "The speaker today is Professor Telegdi from the University of Chicago—currently a research associate here." [Laughter] So I started my talk and said, "From this introduction you can see already the great difference between the two institutions." I thought that was a pretty dumb thing to do.

LIPPINCOTT: Who was in the physics department at Princeton?

TELEGDI: Well, it was a wonderful department. Eugene Wigner. Murph Goldberger was there. There was an experimental physicist called [Robert] Dicke. John Wheeler. I've stayed friends with Wheeler. In fact, he wrote me recently.

LIPPINCOTT: He's an amazing person, isn't he?

TELEGDI: Very amazing. I can tell you a wonderful anecdote, if you'd like to hear it.

LIPPINCOTT: Oh, please do!

TELEGDI: I'm full of anecdotes. You won't have enough tape for my anecdotes. Anyway, Wheeler was living next door to the famous art historian called [Erwin] Panofsky. Very famous! And he has a son who's an outstanding, brilliant experimental physicist—Wolfgang Panofsky, the man who built SLAC. Every physicist knows that name. It was just because of the stupid rules of the Nobel Prize—that they can't give it to more than three people at a time—that he didn't get the Nobel Prize. But anyway, that's beside the point. So Wheeler's neighbor was this famous art historian. Well, Wheeler was always up to his ears involved in secret work—

LIPPINCOTT: You mean classified work?

TELEGDI: Yes. So he had all the clearances, and these clearances had to be renewed. And you know, when you have these clearances to be renewed, these FBI characters are going around asking questions. So an FBI man came to see the neighbor, Professor Panofsky, an aged German professor. And he said, "Do you know your neighbor, Dr. Wheeler?" Panofsky said, "Yes, I know him." The FBI man said, "Do you think that Dr. Wheeler is controversial?" And Panofsky said [adopts German accent], "Iss *he* controversial? *I* am controversial! He iss a varmonger!" [Laughter]

LIPPINCOTT: How about Freeman Dyson? Was he at the institute then?

TELEGDI: He was not there, because if he had been there I wouldn't have had an office. I occupied Freeman Dyson's office. He's a wonderful writer of the English language.

LIPPINCOTT: What did you give your colloquium on at Princeton?

TELEGDI: It was a very interesting point, a piece of work that we had done at Argonne National Lab on the beta decay of the free neutron. It had some substance.

LIPPINCOTT: Who attended the talk? Wheeler? Eugene Wigner?

TELEGDI: Everybody came. Princeton has that tradition, that the whole faculty in physics comes to talks.

LIPPINCOTT: Did you have any interaction with the mathematics department there?

TELEGDI: No. I never had interaction with the mathematics department.

LIPPINCOTT: You don't care for mathematicians?

TELEGDI: No, I just don't understand anything about them. I don't understand what motivates these people. For me, they are a different brand of people. You know, they invent their own problems. We don't. Nature invents our problems.

LIPPINCOTT: And also, you have kind of an experimental cast of mind.

TELEGDI: Yes, yes. But I mean, I can never figure out how these people come up with the questions they want to answer.

LIPPINCOTT: OK. So then you decided you wanted to go back to Switzerland in 1976. You felt that the University of Chicago had fallen off—

TELEGDI: No. That's a statement I would not make. For instance, I knew that in anthropology, they were number one in the United States. But not in physics. I had watched it decay. At one point, I wrote a five- or six-page memorandum, and it started out with the following ominous sentence: "Ever since the death of Enrico Fermi, the institute now named after him has not ceased to decay." And I advocated a certain number of measures to be taken. Some of them

were actually taken, but I was not happy with the faculty appointments. You know, I have a theory that every second-rate appointment is later used as an argument for a third-rate one.

LIPPINCOTT: That was the mechanism of the decline.

TELEGDI: Well, if mediocre people become the majority in a department, then you can be sure that it will perpetuate its mediocrity. I also was not happy—not with the University of Chicago but with the American system of funding experimental research.

LIPPINCOTT: Well, by that time, the military wasn't doing as much funding as it had been. Is that what you mean?

TELEGDI: The military did a lot for science—in particular, the navy.

LIPPINCOTT: And they didn't really have an agenda—they just gave money.

TELEGDI: No, they never told you what to work on. No, no, it was very liberal.

LIPPINCOTT: And then what happened?

TELEGDI: Well, then that decreased and the NSF came along. Mostly the support was, in my book, too short-range. And I didn't like the idea that I had to write every two years some propaganda material about how good I was and all that. I said, "Look, I've been doing good stuff now, nonstop, for twenty years. Why don't you just trust me?"

LIPPINCOTT: There was too much emphasis on grant proposals?

TELEGDI: That's right. I think that this way of funding research ultimately corrupts the university.

LIPPINCOTT: Why?

TELEGDI: The university should lose money on every piece of research; then they would be much more selective. But the scheme in Chicago—and at any other university—was that they would support the humanities out of the overhead charged to the hard sciences. If you asked for $100,000 for your research, you had to pay the university $60,000 or $70,000 for the privilege of spending the $100,000—it's called overhead.

The next step in the corruption was that the university would be inclined to hire those people who brought in the most money.

LIPPINCOTT: You think it's that they were interested in patents?

TELEGDI: No, not at all. Look! If for every $100,000 you get $60,000 for general purposes, then of course if you got $1 million you would get $600,000 for general purposes.

LIPPINCOTT: Are you saying you have to turn over some of your NSF money to the university?

TELEGDI: Sure! That's included in the very proposal you write. Well, they'll tell you, whether you do research or not. . . . I never figured out whether we even got letterhead paper from the university.

LIPPINCOTT: You're talking about Chicago?

TELEGDI: Yes, but Chicago was no worse than other universities. For instance, you couldn't even get a secretary from the university. I had a terrible thing happen to me. I never had enough work for a full-time secretary and I didn't want a woman to sit out there and knit all day long. I never had more than a half-time secretary, because that's all I needed. So my arrangement was to share a secretary with another colleague—half her salary would come from my grant and half from his grant. Well, comic ending: the colleague lost his support and I lost my secretary. I found that horrible. But that wouldn't happen in Zurich.

LIPPINCOTT: How does it work in Zurich?

TELEGDI: In Zurich, it's just the opposite of the United States. You come there, they want to appoint you. And you can debate the conditions. There's only one point in the conditions that you cannot debate and that's your salary. That's the only thing that in America you *can* debate. But in Zurich you can say, "I want so many square feet of lab space. I want to have such-and-such-a-style secretary. I want three postdocs. I want two technicians." All that stuff. "And if you don't grant it to me, I don't come."

LIPPINCOTT: Did you get what you wanted from ETH?

TELEGDI: Exactly! As long as you live, as long as you occupy your chair, nobody can take that away. There are research grants in Switzerland—of course there are. But for a certain minimum of activity, you are guaranteed for life. They have excellent mechanical shops. You don't pay a penny for their work. That's a large factor of your support. In other words, I could have a life of total security and, if I want, do extraordinarily expensive things. When I arrived at ETH, they were astonished at my modest requirements. I asked them for a million Swiss francs to install my lab, and they gave me a million Swiss francs.

LIPPINCOTT: And you began working at CERN, too, right away?

TELEGDI: Yes. They got plenty out of me.

LIPPINCOTT: You said that you commuted between the two places. But your principal place of residence was Geneva.

TELEGDI: Yes, because the principal place of residence, at least from the income tax point of view, is where you have your wife.

LIPPINCOTT: How did Lia like going back to Switzerland?

TELEGDI: She hated it.

LIPPINCOTT: She wanted to stay in America?

TELEGDI: And particularly in Chicago.

LIPPINCOTT: Because of friends?

TELEGDI: Mostly because of friends—friendships she had built up. Also because social relations in the United States, both inside and outside the university, are much simpler and more straightforward than in Switzerland. You know, in fourteen years I was in Zurich, I think I was invited to the home of a colleague maybe twice. [Tape ends]

Begin Tape 2, Side 2

TELEGDI: At Chicago, everybody lived in Hyde Park.

LIPPINCOTT: Were you particular friends with the Goldbergers? I know you see them now.

TELEGDI: Yes, we were very close friends. Well, you know, my wife's is, of course, a somewhat particular point of view. When she talks about Chicago—it was like this, it was like that—there's a hidden factor.

LIPPINCOTT: What do you mean?

TELEGDI: It's not that it was Chicago. It's that we were both twenty years younger then. [Laughter] She'll say how wonderful it was for her in Chicago, and this, that, and the other. But we were both twenty years younger and that makes a big difference.

LIPPINCOTT: Yes—that's when you tend to make your friends.

TELEGDI: Exactly. It gets harder later in life.

LIPPINCOTT: And then you were away half the time while you were living in Geneva, weren't you?

TELEGDI: Yes.

LIPPINCOTT: Tell me more about the Goldbergers. You showed me a clipping from the *New York Times* describing how Mildred Goldberger worked up a skit for the American Physical Society that you were in. She did a lot of that kind of thing, didn't she?

TELEGDI: Well, she's very literate and very witty. Sometimes her wit is acerbic. Even when he was president here at Caltech, she must have made some cutting remarks.

LIPPINCOTT: She had a lot of fans here, though.

TELEGDI: Yes—well, I'm sure she did. I think she's about the most intelligent faculty wife I have ever met—but that doesn't say much, because many of my colleagues are married to very stupid women. [Laughter] No, she's very smart. She would write stuff for comedians. There was a wonderful group of comedians in Chicago at the time. Mike Nichols was one of them. Elaine May also. She wrote stuff for those people. And she asked me to produce an act for this [APS] meeting. She was so good as an editor. I lined up all the jokes and stuff.

LIPPINCOTT: That was '76.

TELEGDI: Yes.

LIPPINCOTT: Who were your other friends in Hyde Park in Chicago?

TELEGDI: One of my friends was Roger Hildebrand. He came about the same time. Roger Hildebrand was the son of a well-known chemistry professor who lived to be 100 and was scientifically active almost to the end.

LIPPINCOTT: How about Dick Dalitz? Did you know him?

TELEGDI: Oh, yes, Dick Dalitz I knew. That was during a period in my life that we never discussed: I also spent some time in Bristol, England.

LIPPINCOTT: When was that?

TELEGDI: I think it must have been in 1947. Bristol was very famous at that time.

LIPPINCOTT: 1947. So you had graduated from Lausanne.

TELEGDI: Yes, I was at Zurich already. Dalitz was then a very young man and he was interested in the experimental work that went on, and he came down [to Bristol], so I met him in I think '47 or '48. But I stayed only three months at Bristol. A very famous physics group existed there in the forties and very important discoveries were made there. I went there from Zurich for three months.

LIPPINCOTT: At that time, you must have been working on your PhD?

TELEGDI: I interrupted that in order to go there.

LIPPINCOTT: Is that a usual thing to do?

TELEGDI: No, but I'm not a usual thing, either. But it had an interesting consequence. They liked me there. The head of the lab was Professor [Cecil] Powell, who later won the Nobel Prize [1950]. He wrote a letter to my boss, saying that…

LIPPINCOTT: Your boss being—?

TELEGDI: Professor Scherrer in Zurich. No, it was not Powell, it was [Nevill] Mott. Mott was the head of the whole physics section [at Bristol]. Mott wrote a letter saying that it was not their habit to steal each other's assistants. But Scherrer didn't mind. When I had finished my thesis Mott would have been very happy to give me an appointment.

LIPPINCOTT: And that's where you met Richard Dalitz.

TELEGDI: That's right. I didn't like life in England at all.

LIPPINCOTT: Why not?

TELEGDI: It's a lifestyle I don't like. There's nothing about it I liked.

LIPPINCOTT: You didn't like the food?

TELEGDI: The food? I was too poor to appreciate that, in particular. Maybe if I had had more money, I would have eaten better.

LIPPINCOTT: You hadn't met Lia at that point?

TELEGDI: I met her shortly before I went there. And let me tell you one thing, she was very kind to me. Every second day she sent me a letter containing twenty cigarettes. You couldn't buy any cigarettes in England unless you were an old customer.

LIPPINCOTT: Really! Well, this was right after the war. Was it because of that?

TELEGDI: Yes. It was austerity. It was real austerity, so the tobacco man would sell cigarettes only to his old customers. There was a lot of hypocrisy, which I didn't like. I think, in those days, it was part of good education in Britain to be hypocritical.

LIPPINCOTT: And very class conscious.

TELEGDI: But that didn't make much difference in Bristol, because Bristol was considered a red brick, secondary place. The *real* arrogance comes from Cambridge and Oxford. Perfectly decent people get transformed into snobs there.

I didn't travel in England at all—I was really, really, very poor. I had a very low salary in Zurich, very low—let's say, the equivalent of $100 a month or $150. My professor said, "We'll let you go to Bristol because you'll learn certain techniques"—I never learned those techniques, because I already had them—"and you can keep your salary. But now, about the travel money—"

LIPPINCOTT: You had to pay your own way there?

TELEGDI: Well, I didn't have to, but I couldn't. So Scherrer said, "I have no money for your travel. But here's an idea. You know the student lab over there? You may remember that when the students don't clean up the place where they work after they finish with their lab exercises, they have to pay a fine of one Swiss franc"—which in those days must have been the equivalent of a quarter, I don't know. And there was a wooden box. A student who was penalized had to put a franc in the box for being disorderly—the Swiss are big on law and order. Everything is neat. So Scherrer got hold of that wooden box, emptied the contents of it on the table, and he said, "This is the only money that nobody controls. You can have it."

LIPPINCOTT: To travel to Bristol?

TELEGDI: Third-class railroad. [Laughter] Third class no longer exists.

LIPPINCOTT: And then they ship you over to Dover, is that right?

TELEGDI: Yes.

LIPPINCOTT: So Richard Dalitz then ended up at the University of Chicago?

TELEGDI: Much later. I think Dick arrived in '57. We overlapped for many, many years. I'll tell you something about Dalitz. He is a very, very earnest person—very matter-of-fact. Never smiles. He rarely talks about anything but his work—completely focused.

LIPPINCOTT: I met him once at a dinner party, and he didn't talk to the women. He just talked to his colleagues and in an undertone, so that you couldn't hear him. [Laughter]

TELEGDI: Yes. He was thinking about pions all the time. I invented a name for him—"Richard the Pion-hearted." [Laughter]

LIPPINCOTT: Did he like that?

TELEGDI: I don't know, and I also don't care. But everybody who knew him liked it. We were very good friends—and we are to this day.

LIPPINCOTT: Is he at Oxford now?

TELEGDI: He's retired. He was a Royal Society Professor.

LIPPINCOTT: Did you know Dennis Sciama?

TELEGDI: I've met him. He was a teacher of this unfortunate handicapped person.

LIPPINCOTT: You mean Stephen Hawking?

TELEGDI: Yes.

LIPPINCOTT: Stephen Hawking is going to be giving a talk at Caltech.

TELEGDI: I'm not going to go.

LIPPINCOTT: Now, is he going to give a talk for the physics department, too, before he gives the public talk at Caltech?

TELEGDI: He does that, too?

LIPPINCOTT: He does, and the unwritten rule is that they don't publicize the departmental talk, so the Caltech community isn't invited to it, but if you can find out where it is, you can go.

TELEGDI: Well, you can probably ask him.

LIPPINCOTT: He's here now, I take it.

TELEGDI: With a whole retinue. Which must cost plenty.

LIPPINCOTT: Is he going to work with Kip Thorne?

TELEGDI: I have no idea what he's going to do. I just don't like this idea of his giving lectures.

LIPPINCOTT: You don't?

TELEGDI: He's an exhibitionist. I mean, if I want to know what Hawking is thinking about, I would much prefer reading a paper by him and maybe later asking him. But to listen to this electronic mumbling in public I think is painful. He likes it!

LIPPINCOTT: But he's trying to make the outside world conscious of how important this kind of thing is. And I think that's good.

TELEGDI: That's one rational explanation. I personally think he's an exhibitionist. He likes to display himself.

LIPPINCOTT: Yes, he probably is, but it has a good effect overall.

TELEGDI: Do you know the story about Murray's book, *The Quark and the Jaguar*? Do you know the history of that book? Simply, Murray was very impressed by the enormous amount of money that Hawking made on his book. So he said, "If he can do that, I can do better." So he wrote a book, which cost him enormous pain and effort, and it was not very successful. Anyhow, when he told me about this plan to write a book, I said, "But Murray, you have a shortcoming." And he said, "Me? A shortcoming? What?" I said, "You're not handicapped." [Laughter] The public is moved by the infirmity of this poor man. I'm not criticizing the audience.

LIPPINCOTT: Yes. But is that a bad thing?

TELEGDI: No. It's just a statement of fact. I'm saying that if this man looked like Robert Taylor, he would have much less success—with the public, not with the science.

LIPPINCOTT: I've been to a couple of those talks. One was for the physics department, which I stumbled onto. And then I went to one of the public talks—this was a couple of years ago. The thing I thought was good about them was that because he has to deliver them in the way that he does, they have to be succinct and well thought out.

TELEGDI: I don't think that's right. No, I think that's his nature.

LIPPINCOTT: You think it would be that way regardless?

TELEGDI: Of course. That's his mental structure. You know, [Paul] Dirac…. Now, Dirac is the strangest man I ever met in physics.

LIPPINCOTT: Where did you meet him?

TELEGDI: I met him in many places, but most importantly I met him at Princeton, in Wigner's home. Wigner, of course, is his brother-in-law—Dirac married Wigner's sister. Dirac speaks in very few words, has driven logic to its ultimate extreme. And supposedly when he writes a paper, he just sits at his desk—or props up his feet on another chair—and thinks. And after being in this cataleptic state for two or three weeks, he takes a pen and writes a paper and doesn't scratch out a single word. He delivers lectures that you can instantly print; every sentence is finished. That's the way he is, that's the way his mind is organized. I'll tell you a little story about him. When he was at Princeton, he wrote a paper on general relativity, and somehow he got obsessed with the idea that the press—you know, the daily newspapers—would get ahold of his manuscript and write some stupid things about it, not understanding it. He didn't like the idea of the newspapers getting ahold of his stuff, because they would misinterpret it.

LIPPINCOTT: When was this?

TELEGDI: This was in '58. So he asked another physicist for advice. He said, "What should I do?" So the man said, "Well, Professor Dirac, why don't you write on the upper right-hand corner of your manuscript, 'Not to be published in any form.'" And Dirac looked at him and

said, “Don’t you think that ‘in any form’ is redundant?” [Laughter] You see, that’s his approach. Extremely logical.

LIPPINCOTT: An editorial mind. So you met him for the first time in ’58 at Wigner’s home?

TELEGDI: Well, I’m not sure it was for the first time, but it was an extensive meeting. I had a long conversation with him. The conversation lasted for about an hour, during which he must have pronounced about twenty words.

LIPPINCOTT: Really? So you did most of the talking?

TELEGDI: Well, there would be a question put to me, and then I would answer. And then I would ask him something and he would answer “Yes” or “No.” Apparently he lived for three months in Germany, and he knew only three things to say in German: “Yes,” “No,” and “I don’t know.” That took care of all his conversations. But a great genius—the greatest England had since Newton. You know he is Swiss by extraction.

LIPPINCOTT: Well, I was going to say that a lot of people don’t realize he’s British. He was a British citizen, isn’t that right?

TELEGDI: He was born in England, in Bristol.

LIPPINCOTT: But he was Swiss?

TELEGDI: His father was. When I was working in Bristol—

LIPPINCOTT: But you didn’t know him when you were at Bristol?

TELEGDI: I knew of him. I mean, he had written *the* book about quantum mechanics. Anyhow, when I was there, we used to live in furnished rooms, called “digs” in local language. I was looking for digs. I was given an address somewhere in Bristol. One day I was received by an elderly lady. She was very inquisitive, by British standards—in England, you’re not supposed to

ask personal questions. In fact, if you take all the rules into account on the subjects you shouldn't touch, there remains only the weather. So the lady said to me, "What do you do?" I said, "I'm a research student in physics." She said, "Physics…physics…. When I was teaching grammar school, I had a little boy in my class who was very good at calculations and that sort of thing. I'm told that he made quite a name for himself. Called Dirac." [Laughter] Isn't that marvelous? The most famous scientist in England—"made quite a name for himself."

LIPPINCOTT: We didn't talk a lot about your later years at Caltech in the eighties and on up to now. That's a pretty relaxed time in your life.

TELEGDI: Yes. Well, you know I'm an experimental physicist, so in the truest sense of the word I didn't do any work when I was here.

LIPPINCOTT: No, because there are no big machines here.

TELEGDI: You have to travel to the machines. That's one of the reasons why I was not too inclined to take a job here. I also think I would have had difficulty getting access to the big machine in Berkeley. The kind of machine that we had in Chicago that I was using successfully—the synchrocyclotron—they had one in Berkeley, only bigger and better. But I couldn't get any assurances that I would get access to it.

LIPPINCOTT: Did you ever do any work at SLAC?

TELEGDI: Never. I served on the SLAC selection committee for four years, but I never worked there. I mean, this guy, Jerry Friedman, he still thinks that in a minor way he owes his Nobel Prize to me. Because when he finished his appointment as a postdoc with me, he asked for my advice about where he should go. And I said, "Well, I think you should go to Stanford. I think electron physics is a particularly exciting subject, and that's where you should go." And that's where he went, and that's how he got into the field where he earned his prize.

LIPPINCOTT: Yes, that was finding the physical indication that there are three quarks in a proton.

TELEGDI: Exactly.

LIPPINCOTT: Did you know Richard Taylor or Henry Kendall?

TELEGDI: I became a very good friend of Richard Taylor, and he's also a very good friend of Murray's.

LIPPINCOTT: Is Taylor still alive?

TELEGDI: I think so. He has had periods of illnesses, but he's OK. Kendall is a completely different kind of person.

LIPPINCOTT: What's he like?

TELEGDI: I think he is the richest man in physics.

LIPPINCOTT: And very political, too?

TELEGDI: You could say that, but I wouldn't use that expression. He was always very involved. I used to call him Little Lord Fauntleroy. He's against atomic energy.

LIPPINCOTT: He doesn't think nuclear power is a good idea?

TELEGDI: [He thinks] it's evil. He's quite fanatic. I'm not really 100 percent sure that he's the richest man in physics, but probably.

LIPPINCOTT: Where would his money have come from?

TELEGDI: It's what they call old money—a very old Yankee family.

LIPPINCOTT: He's not opposed to fusion power, is he?

TELEGDI: I don't think so. But he's also dead.

LIPPINCOTT: Oh, well, then—not a problem anymore.

TELEGDI: He died in a very miserable fashion. He was scuba diving in a lake and drowned. A very, very handsome man. I saw him at his coronation in Stockholm. Jerry Friedman invited me to his coronation.

LIPPINCOTT: How was it?

TELEGDI: Well, it was very exciting, because it was the first time I went to this ceremony. I have been three times altogether.

LIPPINCOTT: Do you ever nominate anyone for the prize?

TELEGDI: Now, every year, because I am a member of the Swedish Academy. So they ask me. And since I always nominate the same person, I just write on the sheet of paper, "See last year."

LIPPINCOTT: I don't suppose you want to say who that is.

TELEGDI: No. And they also use me occasionally as an expert—obviously in fields in which I have some expertise. There's a nominee and they send out letters to certain experts and say, "Could you tell us what you think of him and his work?" And they pay you for that. Not generously—it's pin money.

LIPPINCOTT: Do you still see Jerome Friedman?

TELEGDI: Yes.

LIPPINCOTT: Is he at MIT?

TELEGDI: Yes. He was president of the American Physical Society when the society had its centennial. I remember when I first met Jerry Friedman.

LIPPINCOTT: You were in Chicago.

TELEGDI: Yes, sure, he was a Fermi student—Fermi died in the middle of Friedman's thesis.

LIPPINCOTT: So then you took him over?

TELEGDI: No, I didn't; somebody else did.

LIPPINCOTT: He was a postdoc when you and he did the parity violation.

TELEGDI: Yes, that's correct. We did two experiments together. Anyhow, just to tell you how my mind was, there was a period in my life—starting age eighteen—when I became extraordinarily interested in jazz. And I had an encyclopedic knowledge of jazz musicians.

LIPPINCOTT: You were living—

TELEGDI: In Italy. I read books on the subject and so forth. So when I met Jerry Friedman, I knew where he came from; he came from the west side of Chicago, where most of the very poor Jewish people lived, some sort of a ghetto.

LIPPINCOTT: There were a lot of jazz clubs in Chicago.

TELEGDI: Yes. So the first thing I asked him was whether he knew where Austin High School was. Well, he vaguely knew where it was, but "Why would you be interested?" Here comes this guy from Switzerland and he wants to know that. [Laughter] Then I told him about the famous jazz musicians that had gone to that school.

LIPPINCOTT: Who were they?

TELEGDI: Eddie Condon, Frank Teschemacher, perhaps even Benny Goodman. But anyhow, I was particularly interested in Benny Goodman, because I knew Benny Goodman probably came from exactly the neighborhood of Jerry Friedman's. Their families knew each other. He told me a very, very sad thing. Benny Goodman—already world famous—came to Chicago to play, and his family would come and listen to him but they wouldn't talk to him.

LIPPINCOTT: Why? They thought he'd turned out badly?

TELEGDI: Well, no, but he married a Gentile woman. So they were racists, and no better than the Nazis.

LIPPINCOTT: When you were in Lausanne, of course that's when the bomb and the Trinity test and all that happened at Alamogordo. And I just wondered whether you remembered that and had memories about it. Of course, you weren't a physicist at that point.

TELEGDI: I remember one thing. I remember in physical chemistry that they had explained to us how you could predict the equilibrium in certain chemical reactions from spectroscopic data, from knowing the atomic levels, molecular levels. And I thought it was one of the best things—that purely on paper, you could predict the equilibrium for the reaction when the bomb exploded. I knew that nuclei also have levels. So I thought, Maybe it's by tricks like that that they figured out how to do it.

LIPPINCOTT: Did you know about Lise Meitner's work and that sort of thing?

TELEGDI: I didn't even know that Lise Meitner existed. But I knew about [Arnold] Sommerfeld. I had studied Sommerfeld's book when I was a high school student—on my own.

LIPPINCOTT: I think I asked you whether you ever met Sommerfeld.

TELEGDI: I did. You know, again, the guy in charge of my talk here at Caltech, John Preskill—

LIPPINCOTT: He introduced you for this recent talk that you gave?

TELEGDI: Yes, a very generous introduction. I had set him up like my straight man, and I asked him beforehand to ask me at the end of my talk whether I had ever met Sommerfeld. So he did. Then I produced some transparencies, and I said that Sommerfeld had come to ETH when I was a student and that I had met him and that I had asked him if he would be kind enough to inscribe one of his textbooks for me, which he did. So I showed them the title page of the textbook. The

inscription says, “With best wishes for your studies in physics, Arnold Sommerfeld.” And then I said, “It worked.” [Laughter]

Sommerfeld had more good students than anybody in the world. Nobody else has produced as many good students. Imagine, that at the same time, the same year, Wolfgang Pauli and Werner Heisenberg were his graduate students. Hans Bethe was his graduate student. Unbelievable! I projected the list of these people—it is mind-boggling. He was very attractive; he attracted people very much.

LIPPINCOTT: I’m sorry I wasn’t able to hear your talk. Do you want to say why he never won a Nobel Prize? In twenty-five words or less?

TELEGDI: Ten. Niels Bohr did not want him to get it. That’s not even ten words.

LIPPINCOTT: What is it that Bohr had against him?

TELEGDI: He perfected Bohr’s model of the atom in a spectacular way. And I think Bohr kicked himself for having missed that point.

LIPPINCOTT: Is this something that you deduce?

TELEGDI: This is a hypothesis. But that Bohr opposed Sommerfeld for the Nobel Prize I know essentially for sure. The reasons I can only guess.

LIPPINCOTT: Do you think Sommerfeld was aware of Bohr’s enmity?

TELEGDI: Of Bohr’s attitude? People have told me. I have a letter where this is mentioned. I projected a couple of letters, and people were just rolling with laughter. Sommerfeld writes to somebody what an unbelievable scandal it is that they haven’t given the Nobel Prize to him.

LIPPINCOTT: Sommerfeld wrote that?

TELEGDI: Yes, I have the letter—and I have the authority to use it. I met Sommerfeld's granddaughter and she gave me the authority to use those letters. You know, I've been working on this for a very, very long time.

LIPPINCOTT: You've given a number of talks on it—I wondered if you'd ever written a book.

TELEGDI: A book? On this silly subject, a whole book? An essay!

LIPPINCOTT: OK. Let's talk now about some of your prizes.

TELEGDI: There are not that many, so this will be brief.

LIPPINCOTT: [Laughter] Well, maybe we should first talk about—you were made a member of the National Academy of Sciences in 1968, right? And there are a number of academies that you belong to—the Swedish Academy, the Russian Academy, the French, and the Hungarian.

TELEGDI: Yes.

LIPPINCOTT: In 1991, you got the Wolf Prize along with Maurice Goldhaber.

TELEGDI: Yes.

LIPPINCOTT: You and he worked independently of each other?

TELEGDI: Completely. We never had any contact whatsoever. I mean, we knew each other as human beings, but we had never worked on exactly the same subject.

LIPPINCOTT: He's at Brookhaven. Did you ever visit Brookhaven?

TELEGDI: Oh, yes.

LIPPINCOTT: And you got this prize for the work on weak interactions.

TELEGDI: That's what I did for a large part of my life. I can tell you a little story about Maurice Goldhaber. He's very old and is still active. He must be about ten years older than I am, and still active. Anyhow, after it was announced that I was awarded this prize, somebody asked me whether I regretted sharing it. And I said, "That's completely wrong. Quite the opposite! You know, when I was a young lad, Maurice Goldhaber was already very well established—a fairly famous man. And he was a big role model for me; I looked up to Goldhaber when I was a student. And that the day should come when I would stand on the podium next to him, that was the best part of the reward."

Then there was another point. You know, when you get these Wolf Prizes, you have two minutes to give a little talk, to express your feelings, or whatever. Well, but there's only one talk and there are two people. So there was a little discussion who should give the talk. Well, to me it was pretty obvious that the older of the two should give the talk. So finally, we made a gentleman's agreement: I wrote the talk and he delivered it. [Laughter]

LIPPINCOTT: There's an honorary Doctor of Science from the University of Chicago, in '91.

TELEGDI: I got two more, but I don't have those citations. I have another honorary degree from Louvain University, in Belgium, and I got one from the University of Budapest. At Budapest I probably got it for being Hungarian.

LIPPINCOTT: [Laughter] Well, it must have been nice to go back to Chicago and get an honorary degree.

TELEGDI: Yes, it was very nice. You know, when you get an honorary degree, the exercise—for which I had no practice, or didn't have any at that time—you had a sort of chaperone or whatever you call it, a person who walks with you to the hall where it's awarded, takes you by the arm. I don't know what the title of that person would be. But anyhow, it was like Goldhaber and the prize, this part of getting the honorary degree. Chandrasekhar was the person who was taking care of me, in his most elegant Cambridge doctoral regalia.

LIPPINCOTT: So they must have forgiven you for your poor opinion of how the institute was being handled.

TELEGDI: Well, you may draw that conclusion.

LIPPINCOTT: Maybe we should mention the Julius Lilienfeld Prize in 1995, given by the American Physics Society.

TELEGDI: That was mostly for being a good speaker.

LIPPINCOTT: Well, the citation says it's for your "rigorous analysis of particle interactions."

TELEGDI: Well, I've never read that citation. But I can tell you one thing: I asked them how much time I would have for my lecture and they said, "Forty-five minutes." So I had an hour's worth of lecture, which I cut down to forty-five. This prize is supposedly for your great ability to lecture, so I was very careful. And then, when I got there—to Washington, where the thing was taking place—I was told I had thirty-five minutes. You know what I said at the beginning of my lecture? I said, "Here I was supposed to give you a sample of my ability to lecture. But it seems to me it's like buying a bull for raising cattle, and castrating him on the day of his arrival." [Laughter]

LIPPINCOTT: [Laughter] They must have loved that!

TELEGDI: So at least I started out with a laugh.

LIPPINCOTT: Yes, you lived up to your billing.

TELEGDI: Yes. I have a problem—now almost any joke I make spontaneously is politically incorrect. [Tape ends]