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ALVIN V. TOLLESTRUP
(b. 1924)

INTERVIEWED BY
DAVID A. VALONE

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Abstract
An interview in two sessions, September and December 1994, with Alvin V. Tollestrup, who joined the Caltech faculty as a research fellow in the Division of Physics, Mathematics, and Astronomy in 1950. Dr. Tollestrup received a BS in engineering from the University of Utah (1944) and after a stint in the U.S. Navy became a physics graduate student at Caltech (PhD 1950), working with William A. Fowler and Charles C. Lauritsen in the Kellogg Radiation Laboratory. He became assistant professor of physics in 1953, associate professor in 1958, and full professor in 1962. In 1977, he joined the staff of Fermilab, where he had spent the preceding two years on sabbatical developing the superconducting magnets for the Energy Doubler/Saver machine that became the Tevatron. There he also played a key role in creating the CDF [Collider Detector at Fermilab], work leading to the 1995 discovery of the top quark.

In this interview, he discusses his early interest in science, his wartime radar work, and his career at Caltech, where he helped develop the Caltech synchrotron and later conducted important and innovative experiments, including the photoproduction of pions. He recalls his 1957-58 sabbatical at CERN, helping to plan and execute the first experiment at its 600-MeV cyclotron, on pion decay. He discusses the history of particle accelerators, and particularly of Fermilab's Tevatron, noting the contributions of laboratory director Robert R. Wilson and his successor, Leon Lederman; the competition with Brookhaven National Laboratory's ISABELLE project, and the search for the top quark. He concludes by commenting on the future prospects for high-energy physics.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH ALVIN V. TOLLESTRUP

BY DAVID A. VALONE

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
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Pasadena, CA, and Batavia, IL

Session 1 **September 30, 1994 (Pasadena, CA)**

Session 2 **December 23, 1994 (Fermilab, Batavia, IL)**

Begin Tape 1, Side 1

VALONE: Let's begin by having you tell me a little bit about your early life and education.

TOLLESTRUP: Well, I was born here in Los Angeles in 1924. The Depression came along when I was about six years old, so we went up to Logan, Utah, where my grandfather was a professor of psychology. He had a tenured job, so he had some money; he essentially supported our family during those years. My father had been an architect, but at the beginning of the Depression he got fired from the Edison Company, one of two hundred people who got laid off. So we had to move, and I went to school in Logan, Utah, until I got to high school.

That was an important period in my life, because I had trouble learning. I really didn't learn how to read until I was in the sixth grade. Then my mother tried giving me twenty-five cents for each book I'd read. The first one I picked was *Who Killed Cock Robin?* [Laughter] She objected to the twenty-five cents for that book, but I think she paid up. That was well into the Depression.

VALONE: Had you not learned to read because of the nature of your schooling, or were you just not interested?

TOLLESTRUP: I don't know what it was. I guess maybe it was a traumatic time. I remember in the fifth grade when you had six-figure numbers to add up. There would be maybe twenty or thirty of these on a page, and you had to add them up. I added by counting. But it just happened that my sixth-grade teacher noticed that when we were talking about science, I seemed to be more interested. She gave me a book on science experiments. Half the pages had the pictures cut out, and the other half had the picture and the experiment all together. I remember that that really motivated me to understand, because I wanted to read about these experiments and do some of them. I don't know whether she was smart enough to have done that on purpose, or whether it was just a book she had in the library that somebody had gone through and cut pictures out. Anyway, I loved that teacher. She was very important. It goes to show how you can get influenced at an early age. The first six years of education, I think, is a really important thing.

That was the sixth grade. Then in Logan, in seventh grade, you went to high school. That was a disaster. I remember not understanding. We were starting algebra, and I didn't know what the hell was going on. [Laughter] It was very discouraging. Then we moved down to Salt Lake City. They tested me down there and said, "This guy doesn't belong in high school. He goes back to grade school." But in Salt Lake, they had a thing called an articulating unit. It was seven grades for the first, and then they split; and it was three and three in the two high schools. So they put me back in the seventh grade, which was divided up into an A, B, and C section. They put me in the C section. Somehow that was labeled "the dummies." That made me mad [laughter], so I really started to work. At that point, I was really into science, because before we left Logan I'd already built a little two-tube radio and things like that. My father was very good at getting me things but not at helping me. Somehow he got me an old radio to take apart, and I built a little two-tube receiver.

I also remember the first model airplane kit I got; that was in fourth grade. It had plans. It was one of the kind you build with balsa wood and glue. I wanted my father to help me with that, but he was too busy trying to see his way through the Depression, so he didn't have anything to do with me. That made me mad, too, so, I worked my ass off to figure out how to put this airplane together.

I also had a basement laboratory before we left Logan, where I had a chemistry set. I got an old transformer and enough wire to run a wire down the block to a friend's place, where we had a little telegraph. I was really into science very early. I don't know why; it just happened. It always seemed natural. I never wanted to do anything else.

VALONE: Although you said some of your interest was spurred by that book your teacher had given you.

TOLLESTRUP: The book is the thing that got me started on things like building a telegraph line. Also, the Logan library had lots of books written around the time of Edison—books that told you how to make batteries and all kinds of wonderful things. They were written for grownups, but anybody could understand them. At that point, at least, I was reading something. Those books were great. They weren't modern technology, but they were just right for a boy experimenter.

VALONE: You had that science lab in Logan—

TOLLESTRUP: Yes, I was twelve.

VALONE: —so when you moved on to Salt Lake City, did you get a new science lab?

TOLLESTRUP: Yes. As it turned out, that year back in grade school was really quite good, because I really started to work academically. It made me mad that they put me in the C unit, and so I worked myself into the B unit. It was nice; you could work at your own speed at the math problems. So I got way ahead in math and finished up all the math beforehand. By that time, I'd overcome my difficulties, but before then it was hard. I may have had some kind of learning disability. I've always had trouble remembering how words sound; I had a lot of trouble with French when I went to Europe. There may be some dyslexia, or something like that. We had twins, and they had reading troubles that were very reminiscent of mine. We took them to an expert, and he fixed them up in about six months. He knew just what to do. Had they been left alone, though, I think they would have come out just like I did. We took them [when they were] in the fourth grade, which is when I had started to have trouble.

VALONE: You said you moved up the rankings in your school from the C level to the B level. Did that progress continue in high school?

TOLLESTRUP: Yes, in high school I was what nowadays I guess you call a nerd. [Laughter] I got all A's, and I was really into science at that point.

VALONE: So by then you felt you had found what you were really interested in, and you were driven to work harder?

TOLLESTRUP: Yes. I put all of my energies into it at that point. I had a ham radio, I built a machine shop, stuff like that. I wasn't groping anymore. I had a lot of drive in one direction, when I look back at it. But it wasn't toward physics. I wasn't sophisticated enough yet to know that there were all these different branches [of science]. I was just fascinated by electrical things, and I loved chemistry—things like that.

VALONE: Did your parents encourage your interests in science, once they found that you were doing well in it?

TOLLESTRUP: Yes. Since my grandfather was a professor, it was always assumed that I would go to college and somehow turn into something, although the prospects must have looked pretty dismal at the early stage. [Laughter] There was real encouragement from my parents, but not help. My father was very good with his hands, but we never actually did anything together.

VALONE: Did your parents have a higher education?

TOLLESTRUP: I'm not sure whether my father actually graduated from agricultural college in Logan. Mom, I think, graduated and had a master's degree in art. When I was in graduate school, they moved back down here, to Pacific Palisades, and my mother took up art again. In fact, some of her paintings are over in Lauritsen [Charles C. Lauritsen Laboratory of High Energy Physics]. When she died, [Caltech physicists Richard P.] Feynman and Carl Anderson and Bob [Robert F.] Bacher went over to a show she had hung in Santa Monica. They picked out a series of pictures, which my dad then gave to the institute. We had just finished Lauritsen at that time. There are at least five of them still there. I think there were ten of them altogether—watercolors that came out of that show—but the ones we put in the classroom have all been stolen. Three of the remaining ones are opposite the elevator on the third floor. One of them fell down in the [Northridge] earthquake, and my wife just got new glass put in it. There are two by the entrance of the old synchrotron lab on the second floor—both oils. Actually, they're spectacular paintings, and those are in good condition.

VALONE: At what point did you think you would pursue a scientific career? By the time you were in high school, were you fairly confident that that was the direction you were going in?

TOLLESTRUP: Yes, senior year in high school, I took a chemistry class from a spectacular teacher. He didn't know quantum mechanics, but he taught a thing called the Lewis-Langmuir theory of how atoms go together. So he didn't teach in terms of wave functions, but it was a way of talking about wave functions that the chemists had invented. Well, the names of the two guys are both famous people—[Irving] Langmuir and [Gilbert Newton] Lewis. So they had this picture. That was the first really good science course I had. One of the things he did was to have the students do a science project and write a report on it. At the end of the year, you had to turn this thing in. It could be on anything, as long as it was science—it didn't have to be chemistry. I picked cosmic rays. [Robert A.] Millikan had a book—*Electrons, Protons, Neutrons, Mesotrons and Cosmic Rays,* or something like that.[1] There are several editions of that book, and this was not the first one, because mesotrons were already in the title. I took that book, and it was just like the electrical stuff I read in Logan when I was a little kid. These were things that a normal person could understand. A high school kid could take that book Millikan had written and could understand what was going on. The latitude effect, which [H. Victor] Neher and Millikan did, you could really understand. So I wrote my report on that. And I knew that Millikan was here, at Caltech.

I graduated from high school probably in 1940 and immediately went to the University of Utah—I don't remember exactly when we got into the Second World War.

VALONE: December of 1941 is when the U.S. got in.

TOLLESTRUP: Well, I was in the University of Utah at that time. This chemistry teacher I'd had in high school also didn't know that there were all these different brands of science. He knew I was interested in ham radio, so he said, "You ought to sign up for electrical engineering." And that's what I did. Then I looked at all these courses—"heat power engineering." I didn't give a crap about heat power engineering. [Laughter] Finally I said, "No, that's not what I want." But then there were a lot of neat courses on AC circuit theory and things like that. So I started to

[1] *Electrons (+ and –), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays* (Chicago: University of Chicago Press, 1937).

throw out all those courses I didn't like, and I put physics courses in place of them. Actually, I was able to skip the freshman physics and go into the sophomore physics course, because I knew enough physics at that point. And I taught myself calculus. The big hurdle at that time was going from freshman physics, where you didn't have to know calculus, to sophomore physics, where you had to know how to integrate. So I got into that. I also got a class in modern physics out of a book by Richtmyer and Kennard,[2] which was a classic text in those days on what modern physics was all about. I guess I had two years at the University of Utah, doing this whole thing, when I wrote down to Caltech and said, "Look, this is terrible. I really want to do physics. Please, can I transfer?" I got a letter back. I wish I'd saved it, because I don't know who it came from—it may have come from [Earnest] Watson [In 1942, Watson, a professor of physics, was not yet dean of the faculty or chairman of the physics division—ed.]. Anyway, the advice was—and it was good advice—it said, "Stay where you are, graduate and get your undergraduate degree. Try to keep from getting drafted until you get done. Then come down here and apply to Caltech after the war."

So I did that. During the war, the United States had a very forward-looking policy, both the army and the navy. They had these schools in the universities where they would put people through. And then, after you got out, you'd join the service. But they felt that they needed educated people. We were going all year at this time, because there was a real pressure to get people through—so it was twelve months. Then they all of a sudden decided they needed more people. Guadalcanal happened, which was a very bloody fight down in the South Pacific. All of a sudden they decided they needed people, and they didn't need them educated, they just needed them. [Laughter] So they closed down all these specialized training programs.

It turned out that because of all these extra classes I'd been taking, I had enough credit hours to graduate, but there was a problem because I didn't have any of these electrical engineering courses I didn't want to take. I couldn't just change my major, because if I changed over to physics, it was going to be another year. So I was stuck, with no way out. I went and saw the dean—I had all A's at this point, so I felt I had something going for me. [Laughter] And I said, "Hey, look, I don't want to be an engineer. I wanted to go into physics. But I need to graduate. If I graduate, I can become an ensign in the navy and go to radar school," which I really wanted to do. Also, I'd be all set up for graduate school. So he said, "I understand your

[2] F. K. Richtmyer & E. H. Kennard, *Introduction to Modern Physics*, 3rd ed. (New York: McGraw-Hill, 1942).

problem, but we have our reputation to think of. You haven't got these courses; we can't certify you as an engineer. But let me think about it." So he went and looked back in the records and discovered that in 1905 the University of Utah had a degree in general engineering. I had passed all the requirements for that. [Laughter] So they graduated me with a degree in general engineering [1944]. But at that point, I really wanted to be a physicist.

I was lucky I got into radar school, which was a very, very well-run program. It was the only way you could learn the modern techniques that hadn't come down into the schools yet. Radar was all new; pulse techniques were all new; microwaves were all new. And all of this stuff was still classified. As a result, the navy had an incredibly good school. So for about a year I got trained in electronics, which was very important for me. Because after the war, when I did come here to Caltech, the one thing I knew that Willy [William A. Fowler] and Charlie [Charles C. Lauritsen] didn't know anything about was electronics. [Laughter] So that helped an awful lot.

VALONE: Where did the navy send you for radar school?

TOLLESTRUP: There was a pre-radar school at Bowdoin College in Brunswick, Maine. I think that was almost six months, something like that—it was really intense.

VALONE: There's a big air base near there. Was it related to that?

TOLLESTRUP: No, it was actually at the college. These little colleges had real trouble during the war, because all the guys just disappeared who normally went through them. The last thing you need is to give deferments to little rich kids who can't think. They all got drafted. [Laughter] So these little colleges were really strapped. I think they went out and they got these training programs put in, in order to hold them over. We used the facilities there, but the teachers and everything were all people within the navy.

VALONE: So that must have been 1944, or '45?

TOLLESTRUP: That would be '44. Because I went from there down to MIT, where I had a practical course on real radar—how you maintain it and install it, and all that kind of stuff. Then they flew you out to Honolulu. I was out there for reassignment, because that's where they did it. They didn't tell you where you were going to go until you got there. My reassignment was on a battleship that was up in Bremerton, Washington. It was the *Massachusetts*; she was in there for all new radar. That was a fun job. A battleship is an incredibly interesting place, and the technology at that time was just right at the cutting edge.

VALONE: So you were installing radar on the *Massachusetts*?

TOLLESTRUP: The naval yard was installing it. The guys who were on the ship, of course, all wanted to get out fast as they could. They didn't give a damn whether the stuff worked or not. I was this funny guy who thought that maybe the stuff that was being put on should actually be turned on and made to work. So I was looked at as a little bit of a nut, but we got it all working.

VALONE: So you sailed in '44-'45 into the Pacific?

TOLLESTRUP: Not exactly. I was on the *Massachusetts* for about probably six months, something like that. But after they got all this radar on it, they decided to mothball her, and that was going to happen some place on the East Coast. So I applied for a teaching assignment to teach radar at Treasure Island—the navy had a big school there—in San Francisco. So I sailed down with the *Massachusetts* to San Francisco, and then I was assigned to that school. I was there for a few months and then got discharged. I must have gotten out along about August or something like that. Because I then wrote to two schools—I wrote to Berkeley and to Caltech—to apply.

It was crazy, in retrospect, you know, because four million guys were getting out of the service. [Laughter] All those people really wanted to get back into school, and there was the GI Bill to support you. So the schools were just absolutely jammed at that point with guys who really wanted to do something. So I was lucky. I don't know what happened with Berkeley. I can't remember whether I ever got accepted there or not, but Caltech said, "Come." So I came down here at that point.

VALONE: You said you had written to Caltech while you were still at Utah. Was that because you knew Millikan was here? Or was it the reputation of Caltech?

TOLLESTRUP: Yes, it was because of the book, and Millikan was here, and Neher was here; I knew who they were. There was a big spread on Einstein—I'm sure you've seen the pictures of Einstein on the Olive Walk and down at the Athenaeum. Boy, if they had Einstein around, I was going to go there, too. [Laughter] That was the place for me.

Then my chemistry teacher was all excited, because at Caltech they had been able to bombard lithium with protons and got out 16 million volts' worth of energy. That blew his mind! I didn't know about Willy [Fowler] and Charlie [Lauritsen] at that point, but I knew that there was exciting stuff going on. But it was terribly naïve of me. People are much better educated now about graduate schools. I was a little kid from the boondocks and had no idea, other than this rather limited thing, about making a choice.

VALONE: So when you arrived at Caltech, then, for graduate school, was it a shock—the kind of level of your introductory graduate courses?

TOLLESTRUP: Yes. I had a Tau Beta Pi [engineering honor society] key from Utah, and soon I discovered that that was what you put under the corner of your desk to keep it level. [Laughter] There were classes at that time that Millikan had set up, and he had various people teach them. There was one in mechanics that had been derived from [Fritz] Zwicky's early mechanics class and was taught by Leverett Davis. There was a class in Mathematical Analysis, which was taught by Morgan Ward—that was the first rigorous math class I'd ever had. Then there's Electricity and Magnetism, which was taught by [William R.] Smythe.

I always wondered why Smythe had never done actually any experimental work of any kind. What I found out from people was that he had this enormous book of problems to which he'd really dedicated himself. Millikan had told him to set up a course that would be a filter for people, and Smythe had really committed his life to it. I think there is basically no other research that came out of the guy. Originally, he had done mass spectroscopy of some kind. But he became a serious person to get past, because of this course.

VALONE: Was there quite a lot of weeding out of students that went on in that first year?

TOLLESTRUP: I felt I was flunking out the first year. I was in over my head. I went in and saw [professor of physics] Carl Anderson and told him I felt I had too much course work. I'll always remember: Carl said, “Hang in there. A lot of these guys aren’t going to be here next year. You’re doing OK.” He was right [laughter]; a lot of those guys didn’t come back.

Smythe had three classes at that time. His class went three days a week—Mondays, Wednesdays, and Fridays. There must have been thirty people in each one of those sections. That means there were a hundred people going through that filtering section. I don’t know how big the graduate class was, but it certainly was a long way from what it was after this initial shock wave went through there.

VALONE: After the first year, then, things settled down and you felt like you were getting along well?

TOLLESTRUP: Well, we were going to school on my GI Bill. My wife was working over in the Biology Division for Dr. [Henry] Borsook, who was working on drying carrots and various vegetables to feed poor people in the world. She was making $90 a month, and we saved all of that. We lived off my GI Bill. [Laughter] It’s incredible. But during the summer, I wanted to get a job. Actually, it was before the summer; I wanted to get in the labs. So I went around and interviewed with [associate professor of physics] Jesse DuMond—I heard he was looking for somebody. I’ll always remember that when I talked to Jesse he was really enthusiastic, and I talked a long time with him and told him what I’d done in my laboratory. His eyes lit up, like any professor’s—he was thinking, “Here’s somebody I can really use.” [Laughter] I was so thrilled that anybody would even let me near any of the stuff like this. So I gave him the impression that, yes, I would come to work for him.

Then I went and talked to Willy [Fowler], and he showed me all these accelerators and stuff like that, which I thought was even better. So I decided I had to work in Kellogg [Radiation Laboratory], and I worked there for the summer. Then I had to go back and tell Jesse that I was going to work for Willy. That wasn’t easy, because there was a lot of competition between those two laboratories at that time. Jesse had had a terrible time with Millikan [head of Caltech 1921-1945]. He had enough money so that Millikan didn’t have to pay him, and Millikan knew this. I think Jesse was the equivalent of a senior research fellow for a long time. Meanwhile, Charlie and Willy were sort of the anointed ones—particularly Charlie, I think. So there was real competition between them. So losing me was a hard thing for Jesse. But it was the right decision for me, because those guys in Kellogg were really where things started. They started the high-energy physics program.

VALONE: Did you have the sense at that time that high-energy physics was the place you wanted to go?

TOLLESTRUP: That's an interesting question. You see, what I did in Kellogg was electronics, which was what I knew about. Those guys there didn't know about it. So Willy stuffed a paper in my hand about scintillation counters.[3] Before the war, the French had put a photomultiplier tube to look at a zinc screen with alpha particles shining on it and actually saw the DC current from the photomultiplier increase when they put the alpha source up. But they didn't have any of the pulse techniques that came out of the war. After the war, there was a guy called Fitz-Hugh Marshall at Westinghouse who took the photomultiplier, which had been developed by the navy and army as a noise source, because when you shine a light on it you get discrete electrons coming out, and when that's amplified you got a beautiful white spectrum of noise. Then they would modulate radar with this, for jamming. So I knew all about photomultipliers.

So after the war, Fitz-Hugh Marshall had taken one of these things and looked at the light from alpha particles shining on a zinc screen, and what he saw were the flashes as current pulses. Something like this had been done a long time ago, because [Ernest] Rutherford had used a visual counting technique with his early work. But the French hadn't understood the counting aspect of it—that if you could get discrete pulses, then you could do quantitative measurement. They only saw the DC current.

So Willy gave me this paper, and he told me some of the problems they were having. They had built a beautiful double-focusing magnetic spectrometer, probably the first in low-energy physics. This was a magnet that bent the particles in a circle. But if they weren't pointed exactly along the median circle, if they were a little up or a little down or a little to the side, it would focus them, just like a lens. I think they built that to do a cloud-chamber experiment on neutrinos, showing that the neutrino actually behaved like a particle in lithium decay. They had a cloud chamber there and everything, and this magnet was the only way to get some of the

[3] Fitz-Hugh Marshall et al., "The Photo Multiplier Radiation Detector," *Rev. Sci. Instrum*. 19, 744 (1948).

particles out. What they wanted was the scintillation counter to put on the output, because if this little detector could be mounted within the vacuum system, then you wouldn't have to get the particles through a vacuum window. This was important, because most of the particles at that time were fairly low-energy, and they had these very, very thin films that were only a few microns thick that had to support atmospheric pressure. Either these things would break or the particles wouldn't go through them because they were too thick.

So they had developed a technique, which was to drop plastic dissolved in acetone on water, and it would spread out into a film. And then you'd take a little hoop and lift this up, and then you'd put it on a little support. It was all Old World craftsmanship. [Laughter] With the multiplier, however, you could put the zinc sulphide inside the vacuum, so that the particles didn't have to come through a window. What I did was to arrange the light so that it would come through a little plastic window. That way, the photomultiplier tube was on the outside and the flashes were on the inside. This was a big improvement. It was really my first contribution to the group. It came directly from my previous training—I had all the right tools at that point to do it.

But to get back to your question, high-energy physics was just starting. The cyclotron was working up at Berkeley. I remember when I was a graduate student, pi mesons were discovered. **[Tape ends]**

Begin Tape 1, Side 2

TOLLESTRUP: This period was an important one for Caltech. We were doing all this great stuff, making discoveries. We did the first nuclear mass scale, where you got the mass of all of the light elements from looking at the nuclear reactions. Up until then, people had only been able to use the mass spectrograph. We were more accurate. And I discovered that I could do this. One of the first articles I wrote and published was in this area—*my* first discovery. We wrote an article.[4] And [Kenneth T.] Bainbridge [of Harvard], who was an old mass-spectroscopy expert, wrote an article in reply saying that we didn't know what the hell we were doing; why didn't we mind our own business and stick to nuclear physics. [Laughter]

[4] A. Tollestrup, W. A. Fowler & C. C. Lauritsen, "Nuclear Mass Determinations from Nuclear *Q*-Values," *Phys. Rev.* 78:4, 372-4 (1950).

Someplace, probably along about my third year, Charlie and Willy started to have Friday night seminars. They were a very important part of Kellogg life. All the wives hated it. You went at seven o'clock. There was a seminar for an hour and a half, or something like that. Then we'd go over to Charlie's or Willy's place afterwards, and we would drink beer until twelve o'clock. Then you reported in Saturday morning for work at eight o'clock, but they would turn off the Van de Graaff [generator] at noon and go home. [Laughter]

Anyway, they started a series of seminars on electron synchrotrons. The first idea was to have something like a 70-MeV [million volt] machine down in the basement of Kellogg. I was talking with Willy the other day and trying to get him to reconstruct this. Unfortunately, it's gone. I know more than he does right now, I think. What fascinated me was why these guys who were interested in nuclear physics were reaching out in a high-energy direction. You see, the whole thrust of Kellogg at that time was given by Willy and the nuclear synthesis. My thesis was on the mass of beryllium-8, which wasn't known at the time. All of my friends got theses on carbon plus protons—all part of the carbon-hydrogen cycle of the sun and of the light elements, the nuclear synthesis of the light elements.

But the high-energy electron synchrotron didn't have anything to do with that kind of stuff. So I don't think it came from Willy, although he supported it. It must have been Charlie. That's sort of what he [Fowler] told me the other day, when I pressed him on this; he said, "Well, it must have been Charlie," because he [Charlie] always felt that energy was an important thing. And that's true. He [Lauritsen] came through the high voltage laboratory; that's basically why they hired him, I think, because he got a million volts very early. Nevertheless, those guys decided there would be a synchrotron down in the basement. Then people started to worry about radiation and stuff like that. Quite a few universities had 300-MeV machines—there was one at Urbana, there was one at Berkeley, there was one at MIT. The only one that actually ever did any physics, I think, is the Berkeley one.

At some point [1948], they got Joe Langmuir—his real name was Robert, but there were too many Roberts, so everybody called him Joe. They got him from GE [General Electric], where he had built a 70-MeV synchrotron. It grew at that point. It was clear that 70 MeV was no good; there were already 300s around, so you should go for more. So it became a 600-MeV machine, which actually, although limited, would have done a very good job. Someplace, if you can find them, there are some beautiful drawings that [Russell W.] Porter [associate in optics and instrument design, d.1949] did of that machine. Porter also did the drawings for the 200-inch telescope—beautiful things. And there was a drawing of the 600-MeV machine that was going to go over in the synchrotron lab, in the old optics lab. Bacher was here at that time [chairman of the Physics, Mathematics, and Astronomy Division, 1949-1962] and was pushing for it, too.

One day E. O. Lawrence [of the Radiation Laboratory at UC Berkeley] called up and said, "Hey, we've got this magnet up here. Why don't you use that for your machine?" As it turned out, it was a completely different design. The 600-MeV machine would have been what was called a resonant machine—it would have been working at some multiple of 60 cycles, a very rapid cycling machine. The Berkeley laboratory had a quarter-scale model of the Bevatron magnet that they had built. They built it one summer, because the Bevatron, unlike most of the machines up to that time, was not circular. The Bevatron was so big that they had to open it up and put four straight sections into it, where there was no magnetic field, because they had to have enormously big vacuum pumps in it. The theory was so poorly understood at that time that they felt they should build a model and see if it works. So over a period of about one year, they did! We got the log books from this whole thing. It was an incredible performance. They built a quarter-scale model of this damn machine. They only wanted to test the concepts, so all the magnet coils in the field only went up to 500 gauss or something like that. Can we put protons in this thing and have them circulate and behave like we think? Or does something bad go on? It was a crude model, but it was there. They tested it, and it worked. So when they were done with the testing, this thing was just sitting around. So Lawrence called Bacher up and said, "Do you want this?"

We all went up and looked at it. It was a completely different concept for a machine. Instead of being a small one that would work at 15 hertz—a high cycle rate—it was a big thing that turned out finally to take one second to go through its cycle. But it also had the opportunity of going to higher energy, for the same reason that the Bevatron could. They were so unsure of the Bevatron, how much aperture, how big a field space they needed, that they had a space that was 4 meters wide by 4 feet high—an enormous aperture in that machine. That was phase one, which would only go to about 1.5 GeV. Later on, they put in pole tips, and it would go up to 6. And the crucial point, and the whole object of building that machine and picking an energy, was to make antiprotons. So phase one wouldn't have done that. On the other hand, with that enormous aperture, it would be almost impossible for the thing not to work. Anyway, they got a lot of confidence from the tests that they'd done in this small model, and theory was coming along very fast. So they put pole tips in the Bevatron right from the start, so it went up to 5.5, 6 GeV when it was first built.

On the other hand, the model that we had didn't have pole tips in it. And it was quarter-scale, so it was a meter wide by a foot high. You could crawl around inside the magnet and actually measure it. That would get us up to 500 MeV, which was the same as the machine we had been talking about. When we put pole tips in it, we figured it would go up to three times that energy. There's another problem that comes in with electrons. They radiate, so you can't go four times in that machine. The electrons radiate so much energy in synchrotron radiation that you can't put the power in.

We got the machine down here; we made it work at 500 MeV, which was very, very good energy, because it was over what's called the first resonance in the proton. The quarks in the proton are normally in the ground state, but if you put enough energy in to get to the first excited state, instead of having a spin of a half, it has the spin of three halves. It takes about 300 MeV to reach that excited state. In the ground state, the proton weighs 938 MeV. So you add 300 onto that, it's up around 1,238 MeV in this excited state. Now, that excited state can decay, because a pion only weighs 140 MeVs. So it can decay back into a pion and a proton, and the pion has some energy left over.

Other people were investigating that same channel by studying pions plus protons. On the other hand, two things were happening. They didn't have high-enough-energy protons to really go over the resonance thoroughly on the high side. And all of the 300-MeV electron machines would just get up to the peak of this resonance. Cornell was also very active in this field. They were the other place that had a synchrotron, and they could also get up to 300 MeV. At that energy, you see the pions getting made. They would rise. We were lucky enough to have 500 MeVs, so you could go way over the resonance and see them come back down on the other side. So we were instrumental in pulling together this whole picture of what that first resonance was like. We were able to take the measurements we made and the measurements that had been made on the pion-scattering, and relate those things together and show that it made a consistent picture.

VALONE: Was this the late forties, or the early fifties you're talking about now?

TOLLESTRUP: By that time, we're in probably '52 or '53. We skipped over a chunk there. [Before that,] I was this happy little graduate student working away, and Willy came down one day and said, "You've got to graduate. We've got a job for you." [Laughter] That was neat, because I hadn't even thought about graduating, and I hadn't even thought of ever leaving Caltech. This was the place I was going to stay, but I didn't know quite how. So when he said, "We've got a job for you," that was great. So I stapled together a bunch of papers I'd published for a thesis—it was only about a quarter-inch thick—and submitted it.[5] It wouldn't get past anybody nowadays. [Laughter] A lot of the research was good, but the write-up was pretty elementary. So I went to work on this project.

And then Matt [Matthew L.] Sands, who was back at MIT, got chased out of Massachusetts. He'd been married, and he was getting divorced. His wife was going to sue him and take away all his money, so he wanted to leave the state in a hurry. Bacher got him to come here [1950]. Then there was Langmuir and Bob [Robert L.] Walker. So there were four of us now that started to work on this project. Then, for an engineer, we had Bruce Rule, who had designed the 200-inch telescope. So we had a pretty good start. I built the million-volt pulse transformer for the injector on it, and I built part of the RF [radio-frequency] system for the machine. I don't know when our first papers came out—but it must have been a couple of years after I graduated, which was 1950. Things happened pretty fast in those days. I don't know when those first pion papers came out; it must have been '52 or '53.

VALONE: So the job that Willy Fowler had for you was as assistant professor?

TOLLESTRUP: No, it was as a research fellow. Assistant professor came later [1953]. I think I took probably one of the longest times to go from research fellow to assistant professor—or was it from assistant professor to associate professor? I've forgotten which. I think Leverett Davis had a record that was almost as long as mine. [Laughter] It was a pretty long time, because I didn't go to associate professor until after I came back from CERN [European Organization for Nuclear Research]—and that was '58 or something like that.

VALONE: So this is the period, then, that you were doing experiments on the photoproduction of

[5] Tollestrup, Alvin (1950) "Precision determination of the energy released in nuclear reactions in the light elements." Dissertation (Ph.D.), California Institute of Technology.

pions.

TOLLESTRUP: Yes. That, as I say, was the first resonance. It was just this beautiful structure of all of the particle physics that we know about, just starting to open up. Quarks hadn't been invented. Willy and Charlie had hundreds of resonances in the nuclei. They had charts—I don't know whether you've ever been in Kellogg or not, but they have resonance levels on charts along the wall. I don't know whether they're still there; they were last time I was here. They would kid me: "You know, you've only got one resonance. What are you going to do with it?" [Laughter]

The photoproduction work I think has never been recognized for the real importance that it had, because it was a completely different channel to come in and excite this thing. So you had the pion-plus-proton work being done at Columbia and Chicago and Berkeley, where they had the big cyclotrons. And you had the photoproduction work here, which was a photon plus a proton. Both of these things were tying into the same resonance. The theorists connected those things up very rapidly and made a consistent picture of the whole thing. It was an exciting and important time. And K mesons had been discovered, so there was work on finding those that came out of the lab here. But that came later. Like 500 MeV, we didn't have enough energy for that, so that part was later. But there was also photodisintegration of the deuteron and pion pair production, where you made two pions and not just one. Those things were all sort of predicted theoretically, and we were struggling to try and measure them.

VALONE: It seems that this was a period where the experimentation was running ahead of the theorists.

TOLLESTRUP: In a way. We weren't measuring predicted things. After we were measuring stuff, then the theorists could tie it together. We had a guy named Ken [Kenneth M.] Watson [of the University of Wisconsin] who came here. Here's one of the papers we wrote with him, which tied together the photoproduction in the pion-scattering experiments in a very nice way.[6] It wasn't that the theorists had this great curve that you should go out and measure. People were still very much feeling their way through this whole thing.

[6] See for example J. C. Keck, A. V. Tollestrup, & R. L. Walker, "Partial Wave Analysis of the Experimental Photomeson Cross Sections," *Phys. Rev.* 101:3, 1159-72 (1956).

VALONE: I wanted to ask you a little bit about the funding for all of this. Did you have a sense, at this point, of where the funding was coming from?

TOLLESTRUP: No. But I do have one memory about funding. It happened after I got back from CERN. I wanted twenty photomultiplier tubes that were $500 apiece—which is $10,000. I was worried about spending so much for an experiment. I talked to Bacher about it. He said, "Your job is to do physics. My job is to worry about the money." You know, I wanted to worry about the money; maybe I was using up all of the budget or something like that. [Laughter] But that was all taken care of by this club of guys that had more or less come through the war. Bacher would call up his friends back at the AEC [Atomic Energy Commission] and say, "I need another $10,000 or $50,000." And it would come. I think Charlie was the same way with Kellogg. You never heard about money.

VALONE: Were there any concerns about secrecy issues during this period?

TOLLESTRUP: No. Well, I should be careful. There were funny things. I recall when we were building the synchrotron, occasionally we would go up to Berkeley to talk to them about things, and you couldn't go through some places in the lab up there. They were building an enormous big linac [linear accelerator] for making tritium or something like that. You could fly an airplane through it, apparently. Just a huge machine! There were pieces of that lying around, and you could never find out what the heck they were for. Before you could go into Berkeley, you had to have a Q clearance. So there was a little bit of security concern, but not in the sense that it interfered with anything that you wanted to do or there was some technology you couldn't get ahold of because it was classified technology. All of this stuff was completely open.

VALONE: Did you have a Q clearance that was from the war?

TOLLESTRUP: No, I got a Q clearance when I got on the synchrotron here, just because we had to go to Berkeley.

VALONE: I also wanted to get some of your impressions of some of the older generation of physicists, like Lauritsen, and Millikan, who was still around here when you were in graduate school.

TOLLESTRUP: The talk I gave yesterday was just forty-eight years after the first talk I had heard here. It was in that same room. I don't remember what it was about, but I remember—here's a guy getting up talking about science. I'd never heard that before. The University of Utah never had any seminars like that. Millikan would come to these seminars, and he would sit down in that corner seat on the front, on the left-hand side if you're the speaker. [Lee A.] DuBridge [Caltech president 1946-1969] came that same year. There was some kind of a transfer of power—I don't know what it was—but Millikan still had his office in Bridge [Norman Bridge Laboratory of Physics] in that corner, a big office.

I don't know when he died, but I remember that after he quit, he started to go downhill very rapidly. Finally, I remember the big black limousine driving up in front of Bridge. It would come up, the driver would get out, help him out, and he would wobble upstairs and sit in a chair and go to sleep almost immediately. I was always sorry that I didn't take a class from him—he was still teaching, if I remember right—a class on the history of physics or something like that. I could kick myself. But, you know, I had my plate full and I was trying to stay here, not get a cultural education.

When [astronomer Fritz] Zwicky was around here, he was always a lot of fun and a complete terror. I didn't have him for a teacher, thank god. I had [Paul] Epstein for a teacher in thermodynamics, which I just hated. It was the worst class. I really disliked that subject. He had a book, and when the time came for your final orals, you went and saw him and he'd give you a list of ten questions that he was going to pick from. The technique would be, he would say, "Yes, let's talk about entropy." So you'd go and do the entropy question. So you'd start to write down something, and he would say, "No, no. Let's go on a little further." [Laughter] You know, you'd just memorize that section of the book. Finally you would get to the point where he wanted you to start it, and then you'd go through the little derivation.

Charlie, I remember, I had a class with that was called Atomic and Nuclear Physics. I'd already mastered a lot of that stuff when I was still a sophomore—from Richtmyer and Kennard's book—because it was the same stuff. The book he used was a simpler book. You would get up there, one at a time, at the board, and go over describing what was in the book. He would sit there. I just felt that he didn't know it. When I started to work over in Kellogg, all of these guys revered him—"Charlie says this, Charlie says that." It took me a couple of years before I realized that he was really the wheels behind a lot that was going on over there. He just hated teaching; he didn't give a damn about it and did a terrible job. But we had great times together. He was a fantastic guy. He was so good with his hands; he was the guy who ran the Van de Graaff.

I had two professors: He was one, and Willy was the other. Charlie would run the Van de Graaff; Willy would sit there—he was the mastermind for the experiment—and the graduate student was the data taker. Willy's job was to use a little quartz-fiber electroscope to measure the charge in the capacitor, so that you know how many protons you've used. But we replaced him with the vacuum tube shortly after that. [Laughter] Charlie was always just exceedingly good at getting Van de Graaffs to work and getting the experimental conditions right. He had a real feeling for how things go. He always taught me that you never started out making anything from scratch. He said the first thing you always do is go over to the junk pile and see what other people have built for you and how much of it you can salvage from what you can find.

VALONE: It seems like you did that for Fermilab [Fermi National Accelerator Laboratory] to a certain degree. Do you think that attitude was one of the important things you got out of Caltech?

TOLLESTRUP: There was a thing that came out of Kellogg—and if you talk with more people, you'll get it from almost everybody—the two things that Caltech gave you were these courses like Smythe's: incredibly complicated problems that you would solve analytically. It gave you the feeling that any time you walked into a problem, you had the feeling you could solve it, because you'd done these damn Smythe problems, which looked so impossible to start with. [Laughter] So you had this innate confidence that you could do it. The second thing was you always built stuff, but you built it to measure something with. You weren't just building things to be building things, but you were building things because you wanted to do physics with it. Technology was the thing that you sit on the cutting edge of, because it lets you sit on the cutting edge of the physics that you wanted to do. At the time I went through, almost everybody had to build some kind of a high-voltage machine. [Laughter] You know the Van de Graaffs were all built by the students. That was an important lesson, and I've seen it in students of my own that have come out of Caltech. I've heard it from other people who have been through the system. And it basically came out of these early guys. As a physicist, you got involved in apparatus. When I first came to Kellogg, I stayed in the shop for one whole summer, learning how to run lathes and milling machines. My first job was to sort out about four billion nuts and bolts that had been collected over the war period. You know, every time somebody used something and it didn't fit, they'd take it apart and throw the nuts and bolts in a bin. They wanted those all sorted according to size. I got very good at that. [Laughter]

VALONE: When you first got here, did you get other senses of the way Caltech was making that transition from really concentrating on war projects to moving back towards basic science?

TOLLESTRUP: I don't know what went on here during the war. But there was something, because as I understood it, the Van de Graaff had all been stored in the corner of the third floor of Kellogg and was reinstalled after the war. All that was done by the time I got here. There may have been something, because they were heavily involved in rockets and things like that. But there was no sign of that in 1946, when I got here.

ALVIN V. TOLLESTRUP

SESSION 2

December 23, 1994

Begin Tape 2, Side 1

VALONE: We left off last time just when you were finishing up your graduate work at Caltech [1950] and moving into your own first research projects. Could you describe a bit what you were working on in the early years that you were at Caltech as a researcher, especially your work on the synchrotron and Caltech's high-energy physics program?

TOLLESTRUP: Let me tell you about my work on the 500-MeV synchrotron. We had a small group of people. There was myself, Bob Walker, a guy by the name of John Teasdale, Vince [Vincent Z.] Peterson was there, and Matt [Sands], and Bob Langmuir. So it was a very small group of people that put that machine together.

In order to use it, we had to inject into it with 1-MeV electrons. That was my first job, to build a pulse transformer that would do this. That was my baptism by fire, I think, because it was crucial for the machine, and it had to be ready on a certain time schedule. I started building these things. It was a design—I guess we got it from either Berkeley or Stanford—for a 400-kilovolt transformer. We wanted to get it up to over 1 MeV for injection. We would build them and they would break down for some reason. So I was trying to understand why this was. We had a big tank of transformer oil that we would put these things in. So I studied the transformer oil, and I plotted the fields and everything, but we just couldn't see any reason why these things kept breaking down. You'd turn them on, they'd go up to 1.3 MeV or something like that. You'd run them for a little while, and then they'd short.

Finally, what turned out was that the things were wound like a roll of toilet paper, with cellophane insulation and a little piece of aluminum foil, just wound up in a spiral. There were two pieces of plastic film, because plastic film has pinholes in it. This transformer worked at about 1,000 volts per turn. So if it was going to work, and you had a pinhole there between the two layers of aluminum, it would short. What I did was actually to put in three layers, so that the pinholes wouldn't line up. Then we tested it with high voltage as we wound it.

It turns out that in putting together these three layers of cellophane, I had two razor blades that cut it to the right width, because this had to have a special shape. It was narrower on the outside than it was in the first turn. So these razor blades were holding it in and trimmed the film to the right width. When they trimmed the film, they also welded the edges of the pieces of insulation together. So that then when you would impregnate it, you would evacuate this thing with a big pump and let oil come in and fill up all the spaces.

What finally turned out to be causing the problem was that there was air trapped in between these films. I built about twenty of these things before I finally found out what was going on. The air that was trapped was in a big-enough gradient so that it would ionize. Then after it ionized, it would chemically attack the plastic film. After a few hours, it would eat through it, and then you would get a short. Afterwards when we would take them apart, we'd try to understand what had happened to cause the short, of course. But the transformer oil was every place. Nobody noticed in the dissection process that the oil hadn't gotten into the place where it had to get.

So after I discovered that this was the problem, it was easy to fix it. We just arranged it so that the three different pieces of insulation were a different width, and then they weren't sealed by the razor blades. Then everything worked fine. We built one and installed it. And as far as I know, it worked until it got replaced with the linear accelerator later.

VALONE: At this point, did you have a conception of a group structure? You said Bacher was bringing people in. Was there a feeling that everyone was working on a single project, or were you all doing individual projects?

TOLLESTRUP: It was a funny group. I'd been used to working in Kellogg, where everybody was very supportive. That was a very warm group. On the other hand, Matt was a very hostile personality. [Laughter] There were things like, you could get the award of "shit for the day" by doing something wrong. So it wasn't a lot of fun. It was a highly creative and very effective group, but it wasn't a fun group to work in at that time. Later on, after the machine worked, there was still a lot of tension.

Bacher was not a machine person, and he was worried about the machine working. So we would have weekly progress meetings, where each of us would describe what we were having trouble with. We called them weekly no-progress meetings. But eventually we got the problems solved, and it actually worked. For the first beam we commissioned, I organized a bunch of the graduate students and postdocs. Matt had the concept that we had this big aperture. The idea was that if we could get all the fields adjusted right so that you could start electrons at three different radii and go around in circles, that then you'd had the field adjusted. We went through a long time where we would adjust, say, the outside circle, get it right. And then the next day, we'd come back and adjust the next circle, the central circle. But in the meantime, the outside might have drifted off. So finally we got graduate students and we put each graduate student with a batch of knobs underneath the machine. We tied everybody together by telephone, and we just stayed there until we had the whole thing adjusted. And then all of a sudden it worked. And that was exciting, that was a lot of fun.

VALONE: How long did the construction process take—this period of tension about getting the machine working?

TOLLESTRUP: I'm not sure. I graduated in '50. I remember Willy came down and told me, probably in something like January, that my time was up. I had no idea that I was going to graduate. I was having a good time. I'd been there four years, and he said, "You've got to graduate this spring. We've got a job for you." So already things were starting to happen, and that was in '50. So I had to take my language exams and write my thesis, all in a very short time, to graduate. I don't know when we got our first beam. I would guess it was in '52 or something like that—so there were perhaps two years to get the whole thing going.

VALONE: The 500-MeV level, you said, was significant. What kind of science came out of that?

TOLLESTRUP: It was crucial, in that if you take a proton and hit it with a gamma ray, it has a first excited state at 150 MeV above the ground state, plus a pion. So it takes 300 MeV to excite this. And if you shine gamma rays on a proton and you change the energy, all of a sudden you'll go through this enormous resonance, where the energy is just right to excite this state.

The 300-MeV machines, like the machines at Cornell and at Berkeley—although Cornell was the major competition here—got just up to the peak of this resonance. And the cyclotrons had made pions. Now, there is another way to get at this resonance, and that's to scatter pions off protons. You can also sense the final state of these things—they're both the same; it's a proton plus an excited pion, if you can find this resonance that way. So people had been doing the pion- scattering experiments, working very hard. And there was the p($^3/_2$) phase shift going through 90 degrees. Nobody had enough energy to actually pursue this thing and show that it was really a resonance. But when our machine came on, it had enough energy to go up over the resonance and come back down. This was the first high-energy quark resonance. It wasn't put at all in those terms in those days, but it was a quark—it's a structure resonance that a proton has, the first indication that the nucleons, which we'd thought of before as fundamental, had structure down inside of them. So it was the start of the opening up of this whole thing. People were very reluctant to accept the idea that you could have a resonance here at these high energies.

When Bob Walker and I went to Chicago on our way to a Rochester conference meeting, we stopped in and showed [Enrico] Fermi our results. He said, "I don't believe it; you just got a threshold from making pions that starts to go up. And then you've got one over lambda bar squared falling off on the wrong side, on the high side. So of course you get a peak." But later on, all of the pion-scattering experiments and the photoproduction experiments were put together in one very nice theory that showed that, indeed, this was a resonance.

Then there followed, of course, the additional resonances. Cornell had evidence for another one at around 1,500 MeV. The total energy, including the rest mass was around 1,500 MeV, and they had evidence for that. Then later on, the higher-energy pion beams came along—the bubble-chamber guys at Berkeley, who started to find resonances all over the place. So the whole field just blew up.

VALONE: What was the original motivation for going up to higher energy—say, in the 500-MeV machine? Fermi didn't think that you were seeing anything.

TOLLESTRUP: You know, it was the quest at that time that's still driving people—higher energy. Now we want to find the Higgs [boson], or we want to find supersymmetry or something. There's always been more excitement coming as you go to higher energies. That frontier has been the one frontier that you could always count on—it's sort of the Holy Grail. Charlie and Willy realized that, and that's why the machine came out at the higher energy. If we had built a 500-MeV machine, we would have been much more limited in what we could finally do than we were with that machine. Finally, it got up to 1.2 GeV, which was the highest energy any place in the world for a little while.

VALONE: Tell me a little bit about your CERN fellowship in 1957, and maybe a little bit about what was going on in Europe and the competition and cooperation between the European program and what was going on in the U.S.

TOLLESTRUP: I'm not very good for that. I know that there had been a lot of conversations from people in this country, pushing for a collaborative European laboratory. But that was really beyond my level of interactions with people. I'd been at Caltech for seven years and, you know, everybody went to Europe. So I decided, OK, it's time to go to Europe. We had four children at that time—we had twins and a little girl and a little boy. So I looked at CERN and I knew absolutely nothing about it. It was the most uninformed decision I've ever made. So I said, OK, I'll apply for a National Science Foundation fellowship to go to CERN. I got it. Then—it's sort of a funny thing—they didn't send me enough money for the tickets. I had to do spherical trigonometry to show them the distance between Los Angeles and Geneva. So finally they sent us enough money for the tickets, and we wound up over there.

The laboratory was just starting at that time. Actually, it was a good decision that I had made—not with any knowledge. They had a 600-MeV cyclotron that was just being finished. Now, that wasn't a very exciting machine, because there were cyclotrons in this country—the one at Berkeley and the one at Chicago and at Columbia—that had done an awful lot of interesting physics at that point. So the problem was, What were you going to do with this machine? Gilberto Bernardini was there; they'd gotten him to be director of the physics program of the cyclotron. The first thing he did was to start a series of weekly meetings where we discussed what experiments you could do. The machine hadn't worked yet, which was sort of fun—I got to help actually bring out the beam. I'd never worked around a cyclotron before, so it was a completely different experience.

We had these weekly meetings for the experimental people on what experiments we could do. Then the theorists gave us lectures on theory. [MIT physicist] Victor Weisskopf was visiting there. And Victor Weisskopf listened to their theory lectures and said, "That's a bunch of crap." So he gave a series of lectures telling us what the theorists were talking about.

[Laughter] So it was an extremely stimulating atmosphere there.

One of the things I studied at this time, while the theorists were giving us talks, was Robert Marshak's book on mesons, called *Meson Physics*. In there, there was a little section on the decay of the pion, which normally goes to mu plus a neutrino. But it should go to an electron plus a neutrino one time in 10^4. And there had been an experiment done by Herb [Herbert L.] Anderson at the University of Chicago that showed that this didn't take place, and the limit was one part in 10^6, which was less than one percent of what it was supposed to be. But I read Marshak's book and it was very convincing. It looked to me like Anderson had done a bad experiment. In any case, if it was really true, then there was something new and exciting there that we didn't understand.

VALONE: Why did you think the experiment didn't look good?

TOLLESTRUP: Well, I'd done a lot of work in electronics at Caltech. In fact, we probably had some of the better electronics at that time and a better understanding of phototubes than other places. I think we were quite ahead. So I really understood how phototubes are supposed to work. I felt that from the way that Anderson described things, he was running his phototubes in a way that would obscure the result he was looking for. So when we set up to do the experiment at the cyclotron, we had a two-phase approach. One, we were going to do a very quick, dirty experiment, because the machine was starting to run. We wanted to have some fun and get our hands dirty. Then we were planning a very good experiment that would push the limit down below the 10^{-6}, if that was true.

In the process of setting up that experiment, I got the phototubes to be in a linear mode and very fast, which was crucial for what we wanted to do. We set up the experiment, and the first thing we saw was that we started to get events. By the time we were through with our run, we had some twenty events, or something like that. I was very cautious. I said, "Look, this is happening a hundred times more, it's true, and it agrees with theory, but a very famous guy said it didn't happen. So we should be very careful."

On the other hand, it was the first experiment that had been done at CERN. So they were anxious to publish it. It was about the time my year was up, so I was getting ready to come home. But there was going to be a meeting in Geneva between the Russians and the United

States where the thermonuclear program was starting to get declassified. CERN wanted to present these results at that meeting. I was nervous. But finally, we did all the checks that we could think of, so we went ahead and we published the result.[7]

When I got back to Caltech, it turned out that this was a crucial experiment for the V-A theory [of the weak force], because one of the things that had been bothering Feynman and Gell-Mann was that Ti→eν decay hadn't been observed, and the limit was one percent of what it should be according to Herb Anderson's experiment. So it turned out that our experiment was a good thing, and that was a lot of fun. It may be why I got made an associate professor finally. [Laughter] I don't know.

The other thing that went on at CERN that I continued to work on later at Caltech was pi-zero lifetime. Bernardini wanted to measure the lifetime of the pi, so I got interested in that as well. There were all kinds of ways to do it. He kept looking for things with very short lifetime. I knew that an experiment had been done on an MIT synchrotron, showing that the lifetime, using the Primakoff effect, had to be longer than a certain amount. The nice thing about the Primakoff effect is that if the lifetime gets very short, it becomes a very easy thing to measure. So the guys there, like Lou Osborne, had looked for it and set a limit on what the lifetime could be, using this Primakoff effect. Meanwhile, I kept telling Bernardini, "Hey, look, you don't have to look so short. It's longer than that." But I had no effect on him.

Just before I left [Caltech] for a year at CERN, we'd put pole tips in and commissioned the synchrotron. We thought it was going to be 1.5 GeV, but we got only 1.2 GeV. With high energies, this was an ideal place to look for the Primakoff effect, because it gets stronger as you go up in energy. So we set up an experiment to do that, and it worked. We got the first measurement of the pi-zero lifetime. In retrospect, this was very interesting, because the pi-zero lifetime is one of the things that gives you a very clean handle on telling you that the quarks have three color states to them. We had the right answer, but unfortunately nobody knew what to do with it at that time. So it wasn't an important experiment in that way. Later on, it was done better at Frascati and then finally it was done very well at Hamburg, and then the work at CERN finally paid off. They had other ways of doing it with their Proton Synchrotron, and they managed to get good numbers measured for that. Later, when the three-color theory of quarks came along, it was realized that pi-zero lifetime was just where it should be.

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[7] T. Fazzini et al., "Electron decay of the pion," *Phys. Rev. Lett.*, 1:7, 247-9 (1958).

VALONE: Could you say a little more about the way theory and experimental practice were working out in this period? It seems, again, that the experiments were running ahead of the theorists.

TOLLESTRUP: Experiments were way ahead. One of my graduate students—George Zweig—went off to CERN. He invented quarks at the same time that Gell-Mann did. But at that time, when you had an enormous number of experiments—there wasn't very much work on resonance done at Caltech. I don't think that you could say honestly that work at Caltech had a very big impact. At one point, we did have—on the first resonance; that was important. But later on, the energy wasn't high enough, and the major impact was coming from the Bevatron and the Cosmotron [at Brookhaven National Laboratory]—the hydrogen bubble chambers were dominating things. So they were turning up states like mad. You had a lot of these states, and no effective theory for it at all. So experiments were way ahead of the theory at that time.

Then the theory came along with the quarks, which helped, but a lot of things still didn't fit. We didn't understand all the statistics and things like that. Then finally the color for the quarks came along, and that helped the electron-scattering stuff. At SLAC [Stanford Linear Accelerator Center], Feynman and [James D.] Bjorken understood experiments in terms of partons, or quarks, whatever you want to call them. So, slowly this structure inside of the proton unfolded. And I think the many states that the bubble chamber turned up, the structure that you found from electron scattering at SLAC, those things were way ahead of theory and were sort of leading the way. But finally, the quark theory got more sophisticated, with more colors, with more flavors, and then charm and strangeness were discovered. Finally, we have the Standard Model [of Particle Physics] which is sort of the thing that everybody bows down to. [Laughter] And it says, "The Standard Model predicts this or that." But we're stuck right now. If you say, "Is theory ahead?" the answer is no, because there is no guidance. The theory is not good enough to say, "All right, this is what the next step is going to show." So it's waiting for experiments. The top quark has turned out to be enormously massive; nobody predicted that. But nobody knows how many Higgs there are. You don't know whether supersymmetry is right. None of these things. You have hundreds of different theories now, and one of them may be right. But it's not leading the way, because you have no way to pick out which one it is.

Begin Tape 2, Side 2

VALONE: Could you say a little bit more about Gell-Mann and Feynman and their interaction—the kind of environment they generated at Caltech, the kinds of sparks they generated in the physics department?

TOLLESTRUP: It was different. I learned an enormous amount of physics from Feynman, but it was done both by going to classes and lectures. Also, he would drop by my office and you could ask him a question. The thing that always impressed me about Dick was that you could ask him a question, and if it wasn't a very good question, he would take it and turn it around and answer maybe another question that *was* a good question. You'd just learn an enormous amount from him—very different from Gell-Mann. You'd ask Gell-Mann a question, and if it wasn't a good question, he would say, "That's a dumb question" and walk off.

Murray [Gell-Mann] generated both a good and a bad atmosphere. He made it very difficult to hire new people. He was never sure that anybody was good enough. He was also somewhat of a sucker for flattery—almost a sycophant, you might say. Dick [Feynman] was also not good concerning new hires, because he always felt that he didn't understand how to do it, so he just wasn't going to do it. As a result, it was very, very seldom that he would come out and really push hard for somebody in a faculty meeting. I don't know what went on between him and Gell-Mann, but certainly it was a curious thing. Because everybody—all the young theorists—wanted to come to Caltech. Shelley [Sheldon] Glashow was there, Fred Gilman. You can go through a list of everybody, except [Steven] Weinberg, who spent time at Caltech, and all have done important work. I'll always remember that one year Murray had decided that maybe he was going to leave. At least that's what we all heard—that he had accepted a job at either Harvard or MIT. He'd also accepted a job at Berkeley. All the young theorists, of course, wanted to follow him. So they were applying all over the place, and they didn't know what the hell was going on. Then finally he decided to stay. Bacher is probably, in a reserved way, a better source for what was going on there. But Murray was always looking for someplace else to go. I'm not quite sure why. I don't think either Dick or Murray felt that they should build up the physics at Caltech. I don't think they had the same drive that Willy and Charlie had, looking ahead and saying, "What do we want? Who are the good guys? How can I help stabilize things and keep good guys here?" It was much more volatile.

VALONE: Maybe because by that period the department was well established and there was less of a thought about building and more concern about getting work done.

TOLLESTRUP: I don't know. My feeling has always been that if I'm someplace, I want to make it better. So the rule is, Don't ever hire anybody who's worse than you are. I never saw that in Murray—really worrying about who should be there and what the future was. Maybe he was. He was certainly not looked to for support from the experimental group. He called us a bunch of plumbers and solderers. [Laughter]

VALONE: Let's talk some about the period leading up to the work on the 300-GeV machine and the controversy about that. How did that idea come about? Who was pushing for it?

TOLLESTRUP: That's actually been archived at Fermilab. That came about from a summer study held at Madison by a group of people called MURA, which was the Midwest Universities Research Association. They were upset that all of the big machines had been built on either the East Coast or the West Coast, so they wanted something for the Midwest. And they had developed a thing called a FFAG, which was a Fixed-Field Alternating Gradient machine—which is a rather interesting device. The FFAG has the unique property that you can send charged particles around in either direction—which sounds funny. You'd think that if a proton goes one way, if you run it the other direction, it wouldn't go around in a circle. But it does. So this was a colliding-beam machine. It was a 15-GeV proton accelerator, but you could run protons around in both directions, and so you had a 15-on-15 colliding-beam machine, which makes 30 GeV in the center of mass. If you do a little bit of arithmetic, you'll find out that 30 GeV in the center of mass is pretty close to what a 300-GeV machine does using a fixed target. In a 300-GeV machine with protons hitting a proton target, you get a center of mass energy of around 25 GeV, something like that.

When they started this study, Matt Sands and I went. The idea they had was to get people to look at all the wonderful physics that could go on with this machine. As part of it, they said, "Why don't we look at a 300-GeV machine, because that will obviously be impossible? So that will give us impetus to this way of generating high energies. We'll show that you can't build a

300-GeV machine, so the only way we're going to get there is to go this way."

So Matt took on the machine group—he headed that up. It was a cascade machine, just like we have now at Fermilab. I worked on the injector for it. Then there was a physics group that looked at the physics. The physics was supposed to be another thing that was very hard to do. You have the 300-GeV particles smashing into a proton; everything goes forward. How are you ever going to measure anything? Well, it turned out that Matt got into the design of the big ring, and it went very easily. The injector for it looked fine. It was a machine of the kind we already knew how to build. So the physics people got all excited because, indeed, everything went forward. That means you didn't have to look over 360 degrees for all the particles coming out. You could just look in this cone where you could build much smaller detectors and make precision measurements.

When Matt and I got back to Caltech, we gave some talks about this. Bob Walker was not at that summer study, I think, but Bob got very enthusiastic when we got back. So we started to think about it. At that same time, we got word from CERN that the Proton Synchrotron had started to run. This was a 28-GeV machine or something like that. And sure enough, it made these enormous fluxes. Everything did go forward, just like we had predicted. And the fluxes of pions and kaons were very high. So you could extrapolate this to what 300 GeV would do. It would make very intense beams of pions and neutrinos and all kinds of things. So we got excited about it.

We got a study put together. We got Hartland Snyder and Ernest Courant, who were the two guys who invented strong focusing. Then two other people—Hildred Blewett and John Blewett, who had also been at Brookhaven and understood strong focusing. We got them to come out to Caltech. We tried to get Berkeley interested, but they said, "We don't talk with little guys like that." We got UCLA; we got USC, [UC] Riverside, and La Jolla [UCSD]. We put it together—you must have, in the library at Caltech, a group of little reports on this machine, showing that it was possible. Those reports are also in the Fermilab history library.

Then, there was a lot of interaction with Berkeley on this, because Berkeley didn't want to join this group. They said they weren't going to join anything where SC had anything to say. Also, they said, "Well, we've been thinking about that all the time anyway. It's nothing new." So we had a meeting, and we invited them down. Carl Anderson was there. We invited them to come down and show us what they'd been doing. So Ed [Edwin M.] McMillan came down. I think [Edward J.] Lofgren was there. They brought along [Nicholas] Christofilos; he was the Greek that invented strong focusing but never got recognized for it. A very bright man.

They came down, and they had the drawings for a machine they'd been talking about. It was something like a 50-GeV machine, but it was a scaled-up version of a Bevatron. It was enormous, just absolutely enormous. It had nothing to do with what we'd been talking about. And Carl Anderson just laughed at them. He said, "You haven't been thinking at all about this kind of stuff." They were still skeptical of our whole design. Their attitude was that they knew how to build accelerators and we didn't. You know, you build them big, you build them solid. They've got to work. Lawrence had built this attitude into the lab up there: They knew how to do it, and nobody else did.

We tried to collaborate with them. At various times, we had some kind of collaborative effort going, but they never became a part of WAG [Western Accelerator Group]. They would talk with Caltech because Matt [Sands] was there and I was there. But not the rest of the group. One time I stopped at Berkeley, and Kjell Johnsen came from CERN and showed what they'd been thinking about—because they'd heard about these ideas we were talking about. They had a design for a machine that looked very much like what we turned out. [Laughter] It just flabbergasted Berkeley—that people had really been thinking about this and had gotten this far on it, and they got left behind.

So we actually wrote up and put in to the AEC, or whatever it was called then, a proposal for a complete design study of the machine. We wanted $50,000, or something like that, which was a lot of money in those days. We wanted to go further than we had in the little WAG group.

We didn't hear anything from our proposal. There was a [American] Physical Society meeting at UCLA, and Berkeley and Brookhaven guys were there. There was a big discussion in smoke-filled rooms about who should do something like this. The result was that Berkeley and Brookhaven couldn't agree, of course. They could agree that Caltech wasn't the one to do it, though. [Laughter] So we never did hear back about our proposal.

I think Bacher and DuBridge, when we talked to them, both felt that it was too big for Caltech and didn't see us leading the way in something like that. That's always surprised me a little bit, because those guys must have had a different image of Caltech than what I had. I thought Caltech would be an ideal place to lead something like that. But there was a discussion that it was too big, it wasn't on the scale of things that we were supposed to do—things like that.

VALONE: So DuBridge and Bacher didn't push for it at all?

TOLLESTRUP: I don't know that. But I suspect, through conversations with DuBridge, that there wasn't any strong push. Also, you have to remember, Bacher had very good connections with the Atomic Energy Commission at that time. So I'm sure that if he had been pushing hard, he wouldn't have been overlooked. I would actually like to know.

VALONE: Did that episode change your view of Caltech? Or were you still perfectly happy at Caltech? Did you begin to think about going somewhere else after that?

TOLLESTRUP: No. The only time I ever thought of going someplace else, when I think about it, was when I got back from CERN. With four children, I was doing consulting [for extra money], and I really didn't like to consult. So I thought, OK, I'll look and see what else is out there. So I interviewed up at [UC] Santa Cruz. They indicated the level of salary I could expect up there. It was a lot more than at Caltech, which would solve all the consulting problems. So I went and told Bacher—which had the effect of increasing my salary substantially very rapidly. [Laughter]

No, as I told you the last time, I'd always wanted to be at Caltech since I was seventeen years old, so I wasn't really looking for someplace else to go. But when I came here [to Fermilab] on sabbatical [1975], it was simply that I'd started the users group, and we were doing quite well. Barry Barish and Frank Sciulli [both assistant professors of physics at Caltech at the time] had split off on the neutrino, and so it just seemed like a good time to get out and see what the rest of the world was doing again. Barry was the one who actually got me interested in the superconducting machine. He told me one day that he had heard that [Robert R.] Wilson [Fermilab director 1967-1978] was planning a superconducting accelerator, and that DOE [Department of Energy]—or ERDA [Energy Research and Development Administration], or whatever it was called—had a meeting up at SLAC to talk about plans for the various laboratories. So I went up there, and the possibility of a superconducting machine at Fermilab was discussed. So I planned on spending nine months at Fermilab. Then, Leon Lederman had been interested in getting me to work with him for a long time; we'd been friends. He had an experiment at the ISR [Intersecting Storage Rings], so I was going over there to spend six months at CERN.

So when I came here [to Fermilab, April 1975], things were in somewhat of a disarray.

There was a guy called [Darrell J.] Drickey from UCLA who had been working on superconducting magnets with Bob Wilson, but he had died [December 1974]. The group was demoralized, because they'd built some magnets and they didn't work. I got into that. To me, it was sort of a challenge, because—this sounds terrible—Bob is very inventive, but he's not very scientific. What this whole group needed was somebody at a freshman physics level to just help with the magnet design, and that's one thing Caltech is very good at preparing you for. By the time you've taught physics there for a while, you really believe in Maxwell's equations and mechanics and stuff like that. So it was a very good match. I got interested in it, and made a big contribution. We really started to get results. The steel collars I developed solved one problem. Then it turned out there was a crucial problem with the cables. It was solved by wrapping the cables with a Kapton wrap to insulate them. That was another crucial contribution. I understood the forces, and just a lot of things about how you do stuff. It wasn't complicated, but nobody was doing it very well. So after I'd been working for nine months, it was clear that if I left, it was going to be a real blow to what was going on here. We were in a struggle with Brookhaven. They were planning on building a thing called ISABELLE. The DOE didn't want to fund the machine here because Brookhaven was supposed to get the next machine. So there was a lot of competition. I think if I had gone, the program might have collapsed, or at least it would have certainly been in serious trouble, so I decided to cancel the CERN part and stay [at Fermilab].

Then, just after I'd gotten here, Bob had gotten a couple of letters from [Carlo] Rubbia and from [Burton] Richter, wanting to collide protons in the Main Ring with protons in the Tevatron. The proposals were ridiculous. At that time, the Tevatron.... Instead of having the main rings right above each other, Bob had the Tevatron up on the ceiling of the tunnel, because it was well isolated up there. You could get at it to maintain it, and things like that, and it didn't interfere with other magnets. On the other hand, there's no way you could get any collisions between protons in those two machines. [These things were] separated by ten feet, and there was no way to bring the beams into collision.

So he asked me to run a workshop, which I did in I think January of '76. I got Bob Walker and Jim [James W.] Cronin [of the University of Chicago] to help with that. We had all these people here who wanted to do colliding beams. It became clear immediately that the two machines ought to be very close to each other, so that you could actually collide proton beams going in opposite directions with each other.

The Monday after that workshop, we came in, and I told Bob he had to get the machines together. And he said, "Well, let's put the Tevatron underneath the Main Ring. Go find out if that's possible." Well, Bill [William B.] Fowler, who'd been here when the machine was built, said, "Yes, that's possible. Because you issued instructions once that nothing was to be put under the Main Ring, because someday we might want to put a magnet there." [Laughter] So we all went over to the tunnel and got on bicycles and rode around it. He [Wilson] was a very hands-on guy; he wanted to see himself. So we rode around all the tunnel. Sure enough, basically it was clear all around there. That's when the Tevatron got moved.

The other thing that happened was that in this workshop it became clear that the high-energy collider was really a very exciting thing. I felt that we had to figure out how to do it. I started a group here to study it, so that at least, while we were building the machine, we wouldn't make any really dumb mistakes. We could do experiments. The group grew—originally it was small. We put in a proposal. It got turned down, which it should have been. We put in the proposal again the next year, and it got turned down, too. But the scheduling committee said, "Hey, this is a very good idea. You need to set up a colliding-beam support and take this very seriously." So that was really the start of CDF [the Collider Detector at Fermilab].

VALONE: Now, were these experiments that you were doing more on materials and properties of magnets? Or were these actually on the detector?

TOLLESTRUP: No. At that time, the main problem was.... We had magnets that were a foot long—model magnets. We had no idea whether that was going to apply to the full-scale magnet. There were enormous problems with forces and insulation and all kinds of things that had to be solved. So that's what was taking up most of my time. The detector, and how you were going to use this, was sort of a secondary thing that came along. When that department got set up, then I really started a group. The University of Chicago, Caltech, and Argonne—I guess they were the main players. Oh, and the Italians and the Japanese—I got both of those groups involved.

VALONE: Was this on the detector?

TOLLESTRUP: That's on just the detector. That's the core group that finally turned into CDF here and built what we have.

VALONE: What were the main problems you needed to overcome in working on the CDF detector?

TOLLESTRUP: Well, there were three big things. We didn't have a machine. When it started, we were still building short, one-foot magnets. I gave a talk at CERN about how we were going to build this 1-TeV machine. They asked "How long is your magnet?" I told them "It's one foot." [Laughter] They all laughed.

The other thing was that in this workshop I had run, the idea came up of using antiprotons. That was put forward by [Peter M.] McIntyre and picked up by Rubbia. That idea was not his; [Gersh I.] Budker had already done this in Novosibirsk and had done the experiments on it. And then [Simon] Van der Meer at CERN had discovered another way to cool protons. If you can cool the antiprotons, then it gives you the possibility of not colliding two proton beams in two different machines but, rather, using one machine and using protons going one way and antiprotons going another way. You need a source for that, and we didn't have a source. We didn't have a machine. We didn't have a detector. And ISABELLE was supposed to be the place where a big collider was going to be built. They [Brookhaven] were supposed to be the people who got the money for the detector; they were supposed to be the people who got the money for the machine. So there were a lot of troubles getting things here.

Rubbia was playing politics to the hilt. He wanted to discover the W and Z bosons. He tried to get Wilson to do the experiment in the Main Ring. Around that time, Wilson resigned [1978], because he couldn't get support or money for the place. So CERN decided to do the experiment Rubbia wanted, so that took him away from here. [Leon] Lederman came in [as Fermilab director] and said, "Look, we can't do all of these things. The first thing we have to do is get a Tevatron, and then we can talk about a collider. And the collider should not be colliding the Tevatron against the Main Ring, because that's not the highest energy. The highest energy is to use antiprotons, and the cooling experiments at CERN have shown that you could really do them." So Lederman is the one who actually put some program on the books that was reasonable. "Then we get the Tevatron. After you've got the Tevatron, then you can do the colliding experiment." When Lederman came in [1979], he also got a certain amount of support from DOE—which Wilson was unable to get—to actually make the machine. So we won on that, in the sense that they agreed to build the Tevatron here.

Then, finally, we got a plan for the source of antiprotons. That was looked at by a committee that Lederman had, and they said, “It’s not good enough. You should get a better source.” So Leon threw the first design out and went to the second one, which cost more money, and managed to get support for that from the DOE, through a fluke in politics.

So we had our source and we had our machine, finally. And then we had to get money for the [CDF] detector. They wouldn’t give us money for the detector until we actually had all of the rest of these things well under control. So the detector was delayed for some time. As a result, I went and worked on the source for a while, because that was the thing that was holding up the detector.

VALONE: So at this point, you were pretty much spending all your time here at Fermilab?

TOLLESTRUP: I decided to move to Fermilab in ’77. Caltech wouldn’t extend my sabbatical. I was away for two years; they extended me for that long. But then they said I either had to come home or.... Well, nobody actually said that, but it was clear.

VALONE: Was that a hard decision to make?

TOLLESTRUP: Yes, that was very hard. It was very emotional, because I had ties to Caltech. On the other hand, it was clear that the really exciting experiments were going to be at the Tevatron. I really wanted to be a part of that. Also, I had been at Caltech for a long time, and I found it was very stimulating to be here at Fermi. It’s a different kind of environment. I came to Caltech in ’46 as a student and I left in ’77. So I was there for thirty years. It was time for change. [Laughter]

Begin Tape 3, Side 1

VALONE: Let’s talk about completing the Tevatron and the early experimental results that were produced out of it. I want to get into some of the competition with the ISABELLE project and how Fermilab was more successful in overcoming some of the problems both of these projects were facing.

TOLLESTRUP: When I came here, they had already started—as I mentioned—a superconducting-magnet program. Bob was very independent. I don't think he gave a damn about what was going on back at ISABELLE. He didn't spend a lot of time trying to find out. He had his own idea about what the magnet should look like, and it was different. The big difference was that the outside part of the magnet is at room temperature. Then there's the cryostat, which goes between room temperature and down to the 4.3°K of the liquid helium.

Brookhaven was employing a really different technique. They took the iron and everything and cooled the whole thing. Now, that's an enormous amount of stuff to get heat out of. And so the thing that worried everybody was, with the refrigerators that were then available, how long would it take to fix something if it broke? It would take Brookhaven a week to cool one of these magnets down, because it was so massive—whereas we could cool things down in a few hours. There had been a lot of experience at this laboratory with magnets that were not very reliable. You can criticize the engineering that went into them, if you want to. But nevertheless, Bob felt strongly that if it took a week to fix something that was broken, that was bad.

VALONE: So the previous experience with having the machine go down led you into a superior design.

TOLLESTRUP: The outlines of the magnet already existed when I got here. Bob had some creative people around here, but not guys that understood freshman physics very well. So the way they were trying to make the magnets was completely impossible. I think, in retrospect, it was crazy. In place of the collars that I showed you up there, that hold the forces, what they were doing was they were putting some little porcelain rings inside of the magnet and then wrapping. Do you know the steel binding that you use on packaging? It's very strong. They would take four strips of that, half an inch wide, and they would wind a spiral around and over the coil, pressing it down against these porcelain rings. That was supposed to take the force.

Well, that design bothered me for a long time. Finally I worked out the theory of it. There are two things. If you wrap a spiral under a lot of tension to compress something like that, then what are you going to fasten the end of the spiral to? Because it's going to want to unwind. So the answer is, you wrap another one over it, going the opposite direction, and you balance these two spirals against each other. The trouble with that is, you've got no way to accurately balance the forces, and if the force is a little bit unbalanced there's nothing to take up the torque in the magnet. It'll just turn into a spiral. So I worked out the theory of this and predicted that it wasn't going to work, that these little porcelain rings were all going to crack at either one of two places—on the pole or at the equator, they were going to crack.

We finally succeeded in building the first full-length magnet and cooled it down and ran it. After we had run it for a little while, we warmed it up. We found all of the porcelain rings were broken as I had predicted. And one end of the magnet was twisted about 25 degrees with respect to the other end. You could just see it, with your naked eye. Bob immediately understood I was right on that. [Laughter] That increased my standing a lot. [Laughter]

Then, over Christmas, I came up with this idea of the collars. I didn't know how to put those things on. I invented the idea and showed that it was strong enough. But I didn't know how to put them on. The guys who were here invented that part. They were very clever at what you could do—very imaginative engineers.

VALONE: What's the theory behind the collar?

TOLLESTRUP: Each collar is a half, like this. And so you've got the coil here, that's round. And you slip one collar on this way, and then you slip another collar on this way. And they interlock—there are some little pieces that interlock, so that it becomes solid. Then you can put these things under a big press and squeeze them together. And while they're there, you go along the outside edge of the stainless steel and weld them together, so that the collars are welded right along here onto each other. Stainless steel is so resistant for heat transfer that you can weld them outside without burning the coil on the inside. So that locks them together into a rigid structure. The collar can be stamped with a precision of a few ten-thousandths of an inch. So that solved another problem we were having, of how do you do this accurately. You have to have a magnetic field that's accurate to a part in a thousand. That means the coil has to have stable dimensions of the order of a thousandth of an inch—actually, that's too much; it has to be smaller than that. We'd been trying to mill the forms for forming all of these coils out of long pieces of steel. It was a very hard thing to get the accuracy that was required on these 25-foot-long pieces of steel. In fact, we didn't succeed. But the collar, being very reproducible, solved all these problems. So it was a beautiful solution. The engineers found out how to put them together; and that was great.

That technology got carried over to all the molds and things we needed. Because, again, you could get these stampings, stack them up, weld them together, and then you had a molded shape. And it's very easy to get a flat piece of steel. So you can stack up the collars on a flat piece of steel, tack them together, and then you've got a precision shape for your tooling. All of that technology is still used. In fact, if you look at the cross section of the SSC [Superconducting Super Collider] magnet, it's directly derived from the Tevatron work here. So it really was the seminal work in how you build the magnets that was done here.

Brookhaven had a different approach. They wanted all the iron cold. So they pursued that technology, but they had a basic flaw in the cable, which they only understood quite a bit later. That's a fairly technical question. But instead of this conductor that we saw over there, where there are individual conductors, their conductor—it was still a cable, but it was actually a braid of these very fine conductors, and that, of course, wasn't very stable. So they filled it with solder—just heated it and ran it through a solder bath, so it became stiff. But the result was that the magnetic field that it produced was not very good. They never actually solved that problem. At the end of the project, when it was really threatened, they came out here and found out what we were doing, took some of our cable back, and adapted the techniques we were using to what they were doing, and actually made some successful magnets. But it was too late at that point.

We didn't kill it. The SSC killed it. The trade was from ISABELLE to the SSC. And the Tevatron had gotten past, since we were really able to build magnets and show that we actually got enough support. Although it was reluctant support. The DOE was never happy with the fact that we succeeded, and the lab has never received very much support for all the work we did on developing the technology. It was always rather hostile, because Brookhaven was supposed to be doing it.

VALONE: What's the story behind the creation of the conducting filaments? Was that a separate area?

TOLLESTRUP: That was not done here. The cable we use is called Rutherford cable. And that was because when the CERN machine was first proposed—the so-called SPS [Super Proton Synchrotron]—they thought of doing it with superconducting technology. There was a study commissioned in England at the Rutherford Laboratory to look into superconducting cable. Those guys did a lot of seminal work on developing the cable technology. So we had that to start from. The main problem in this country was that the superconductors were really developed in high-energy physics. And there were some little companies around that you would order superconductor from. It was expensive, so they didn't stockpile anything. They'd go out and buy the niobium-titanium, have it formed into rods, and then they would fabricate it into the cable. There was typically a year's wait. So you would give them $20,000 for some cable, and a year later you might get the wire you needed for your solenoid or whatever.

So the lab went out and bought a large number of tons of niobium-titanium, had it purified and formed into rods so that all of that material was exactly the same. Then they went out and they bought the copper and had that fabricated into shape. And then they gave this raw material to these small companies and said, "Process this into a strand." Then they made a contract with New England Electric [Wire Corporation] to take the strand and cable it into the form we needed.

The result of this whole thing was that these small companies became very competitive in how they would treat the cable to get the most current density out of it—the highest current density. They considered their developments proprietary, which was fine. We didn't care how we got it, as long as they had a prescription for it. Then we would take that strand and give it to New England, and they would take twenty-three of these strands and make it into the cable that we needed.

The main invention that was made here was that originally it was thought you had to have the cable itself exposed to the liquid helium in a bath. But we were having problems with shorts. What they did was, they would wrap a little strip of glass tape around it, which is porous to helium. That would space the conductors, but when you put a lot of pressure on this, the turns would start to touch each other and cause a short. After working with that problem for a while, I decided we had to have some insulation around it. But the argument was, you couldn't do that because it would keep the helium out. I calculated that the helium would get in there and that the heat could get out through the Kapton. Some argued that if we did this, then we couldn't get the heat out, but in actuality you can get the heat through the Kapton quite easily.

After we wrapped the cables, immediately all of the magnets started to work much better. I think probably what happens is that when you wrap the cable with Kapton, it makes a little cocoon around it. If you remember, these coils are pushed together with enormous forces because of the magnetic force. If they slip a little bit, that generates frictional heat. And that heat is enough to heat the conductor up and turn it normal. If you have this Kapton wrapping around it, there's helium inside of the wrapping. And the Kapton actually forms a little cocoon that keeps the helium inside and keeps the conductor away from any frictional motion. So the Kapton-wrapped cable was an important invention here. And the collars were an invention here. And the laminated tooling was an invention here.

One other important thing that came out of the work here—again, it was something [W. R.] Smythe taught me. If you want the field to be very uniform, then you can calculate that all the conductors have to be within a thousandth of an inch of the right place. So that makes a big problem. But there's another way to turn that problem around. If you build something and measure the field, and you find out that it's a little bit wrong, then you can change the position of the conductor by putting shims in the collars so that it changes the collars just a little bit. And you can calculate what those shims need to be to fix the field. And you never have to worry about the absolute accuracy. You can build a coil, measure the magnet, and then correct the next magnet for the dimensional problems that you have. The only thing that's required is that it be reproducible from one magnet to the next. If it's random, of course, you can't correct it, because you've already built it. But if it's reproducible—which these collars were—then if you see small errors, you can fix them.

So we invented another thing, which was called the room-temperature measuring device, which measured the magnetic field just after the coil had been made. You didn't have to cool it, you didn't have to put the cryostat on—all of that stuff which is very expensive. You could immediately tell whether or not the coil was good enough. And if it wasn't, then you could immediately look at the manufacturing process to find out what it was that was changing. So it built a very tight feedback loop around the factory to make sure that what was coming out had a good magnetic field. Nobody cared whether the magnet was dimensionally very accurate. All that mattered was, Is the field good? And if the field wasn't good, then these changes would be slow, and you could modify the tooling. So that gave you a way to control the manufacturing tolerances. Because the Kapton film, the wire, all of the insulation, everything has slow changes of dimensions. You'd get one batch and it'll be just a little tiny bit different from the previous batch. So this gave a way to dynamically correct things before a whole magnet was built. By then you'd found out if it was bad and you had to throw it away. So that was another crucial invention that was here.

VALONE: And how long did it take to get all of these things put together and actually get the Tevatron running?

TOLLESTRUP: Oh, I don't remember those dates. When I came here, we were building one-foot magnets. We got our first full-length magnet probably after I had been here a year. That one was built without the collars in it. It was a disaster. Probably we had relatively decent magnets by '77, I think.

VALONE: So you spent a lot of time when you first got here working just on the construction of the magnet.

TOLLESTRUP: That's right. We had what we called a magnet factory [Magnet Fabrication Facility], which was also our R&D tool. Again, this was Bob's idea, but if you were going to make magnets, your R&D had to be in a factory that was going to make magnets. That was the way to start. So right from the first, he started building a factory. That was crucial to our success, I think. We knew we had to build a thousand of those things, so nothing was considered that was just for one magnet. That was another difference with Brookhaven. They were still fabricating their magnets with what I always called Old World craftsmanship. They would work and work and work to make a magnet exactly right. But if they had had to build a thousand of them, there's no way they could have done that kind of thing. We probably built two hundred magnets that were no good. They went into beam lines and other things; they weren't good enough for the machine. But they've all been used. [Laughter]

VALONE: What were some of the earlier results that you got, after the Tevatron was up and running? I would imagine you were happy to get back to doing real experimental work.

TOLLESTRUP: No, I didn't do any experiments on the Tevatron. I completely wound up in the colliding-beam stuff. That was the only thing that was driving me, at that point. We were behind CERN, because we were building a machine while they were building their source and detector. So we were two or three years behind CERN, at least. But we knew we had more energy coming. And we knew we had a higher luminosity—that is, more collisions per second—here because our technology was better.

I think the first results were actually rather disappointing, because it's true we made lots of Ws and Zs, but we didn't learn any strikingly new things. It was good solid physics. The jets were behaving like jets were supposed to. And the Ws and Zs were behaving like they were supposed to. Then we put in some things that were crucial to the excitement that the CDF has created. One, we had a silicon vertex detector. When you make a B quark, it will go a few hundred microns before it decays. It's unstable; it radioactively decays. We put what's called a silicon vertex detector in the CDF that sees what you call the primary vertex. You have a quark and an antiquark coming together, and they scatter and make things. Then you've got lots of particles coming out. If it's a B quark, it can go off in a certain direction and it will decay in a certain way. So if you have a detector that can measure the fact that there's what we call a secondary vertex, then you have a new way to look at things, because you can identify by the secondary vertex the fact that it's a B.

Well, CDF was the first detector at a hadron collider that had the silicon vertex detector. So that opened up the possibility for us to start to do what is called B physics, because by measuring things with a secondary vertex, you can identify those events where there's a B quark produced. So far, that had only been studied at Cornell and at SLAC. So this opened up a new field in hadron colliders that people didn't think you could do.

And then the second thing is, people had thought that you couldn't do precision measurements around a hadron collider, because there were so many particles. It turns out that the tracking chamber I showed you, which measures the tracks, is an incredibly precise instrument—much better than anybody thought. And as a result, we've been able to do an enormous amount of B physics, which was not envisioned at all when we built the thing.

The latest thing that's very exciting is that maybe you can do CP violation. There's a B factory being built at SLAC to study that. There's a good chance that by the time they have that built, we will have already made the first measurement here. Again, it wasn't anticipated.

The other thing was the top quark, which was part of the thing we wanted right at the first. In fact, Bob Walker and [Caltech professor of theoretical physics] Geoffrey Fox and a graduate student had made calculations on how many cells the detector had to have. Remember

I showed you that there's a 15-degree cell with a similar size in the other direction, where we measure the energy in that cell in the detector. If you don't have enough of those, then you can't reconstruct the top quark when you see it. It's too coarse. This is like a fly's eye; it digitizes things in chunks all over. You don't have the resolution you have in a human eye, where you can see very fine things. If what you digitize is too big, then you can't see the details enough to tell it's a top quark.

So the two things—the vertex detector and getting enough cells in the detector for digitizing, and the tracking so that we can identify some of the particles, was crucial for the top [quark]. Nobody expected it to be so heavy. In fact, I guess in 1982 the UA1 Collaboration at CERN announced that they had evidence for it at 35 GeV. Then slowly that went away. And when we came on and ran, we immediately showed that the mass was greater than 62 GeV; we still didn't have any signal. So it wasn't until a year ago that we really thought we were seeing evidence of the top [quark] production. And I think that's good evidence. I don't think the conclusions of that paper will ever change.

VALONE: What's the mass that you're estimating?

TOLLESTRUP: 174 is what we have evidence for. That mass is enormous. It's about the mass of a gold atom—just one quark. This was completely unexpected. People don't have a theory for why particles have mass. It's one of the crucial things now that you would like to attack and understand.

VALONE: But the work has to be done on the theoretical end, really?

TOLLESTRUP: No, we know there's physics at an energy level around here, or maybe a little bit higher, that's going to answer some of those questions for us. There's a richer structure than what we have, but we don't know what it is. We don't know how many Higgs particles there are—if it's supersymmetry, so-called. It's a very pretty theory, but you don't know whether it has anything to do with the real world or not. But we're starting to see some of the particles that are there. We don't know, if we go up to 1-TeV energy scale, whether there's going to be a hundred new fundamental particles that we're going to discover, like we did at the quark level, or what. It's wide open. We're just waiting for evidence.

VALONE: And without the SSC, the kind of evidence you're going to get is going to be of the more subtle nature, using the instruments?

TOLLESTRUP: The SSC, its energy was picked so that you were absolutely sure that you had the energy to answer this question. The LHC [Large Hadron Collider] is a lower-energy machine. When we looked at it in this country, we decided, "All right, if you're going to spend that amount of money, you don't want to spend it and then find out that it's not enough." There have been machines built that don't answer the questions. KEK [High Energy Accelerator Research Organization, Japan] built an electron-positron collider that has never done any new physics, essentially. An enormous amount of work, a lot of money, and no discoveries. There was a cyclotron built at Harvard that was 95 MeV that never discovered anything. It was in a barren region.

On the other hand, there's a chance that the Higgs and other particles will be available at the LHC. They have a tunnel. It's the obvious thing to do, so they're going ahead and doing it. But there's not a guarantee that there's going to be exciting physics coming out of that. If there's not, it's going to be very disappointing. Everybody will be really unhappy. But tough! [Laughter]

VALONE: Any thoughts about the future of high-energy physics, how things are going to work out without the SSC?

TOLLESTRUP: Oh, I'm very optimistic. I think it's exciting. People have tended to think that you have to find the Higgs. For instance, when we started the CDF, I was lucky if I got five people together for a meeting. Now there's a tremendous attraction to these big groups. You can come in with your university and your graduate students, you can build a little piece, and it becomes sort of a lifelong effort. And you say, "I'm looking for the Higgs," and everybody bows down and says, "Oh, Higgs hunter." [Laughter] But there's another whole class of experiments that are out there waiting to be done—the decay of the proton, does the neutrino have mass, do Ks decay in funny ways, what's the dark matter, do axions exist? Just an enormous number of things that people are going to have to look at. The frontier can't always be this knob that you turn up the energy on. Because you run out of that. That's famous last words, because when I was a graduate student if somebody had said, "You know, you'll be working on a 1-TeV collider before you die," it would have blown my mind. [Laughter] I was studying the energies of Be^8, which is 100 kilovolts. So that's ten to the fifth. And now we're studying things TeV, which is ten to the twelfth, so that's seven orders of magnitude different. I think each generation of physicists has come along and said, "We're running at the limit—this is the end of the synchrotron" or "the end of the hadron collider." Then something else comes up. You also can't imagine connecting all of Niagara Falls to one accelerator. So at some point, you're going to have to change the approach. You're going to have to look at other evidence for what's going on at very high energies, other than trying to create particles that have the Planck mass, or something like that. So people have to get to work and quit this enchantment with just the Higgs and things like that. It's important, but there are other things that will teach us stuff, too.

People are a little bit like lemmings. You know, the SSC gets canceled and the LHC starts, and all of a sudden there are four hundred people who want to rush off to Europe. It's OK, it's necessary, but it's not necessarily the only thing you're going to do. The thing to remember always is that the things we've discovered in high-energy physics have been things that people didn't expect. Just recently, people have said, "CP violation in the B system is going to be very, very hard to do." Well, you know, there are just papers coming out of CERN the other day that say, OK, there's another way to do this thing. You can't tell. There's excitement; whenever you do better experiments, you discover things that are important.

VALONE: So the issue now is really more of the experimental design and how you use the machines.

TOLLESTRUP: Cleverness! You have to think of other things. There are other clues to what's going on out there than just the big colliders. Or other experiments in the colliders that will answer it.

Again, coming back to the proton. If you can just decide what the lifetime is of a proton, that's an enormous advance. That ties down a lot about the high-energy theory. It's a real number; it's not a speculation anymore. So somebody comes up with the theory that has to fit it.

But laboratories like this one [Fermilab] also have to define what they're going to do. Maybe they're an anachronism—I don't know. I think probably not, because there's this scale of stuff that you can't do at a university that laboratories like this can do. That's what they were set up for. And you just have to try and find out what those things are. It's an important part of the community.