



Photo taken in 1993

CHARLES W. PECK
(b. 1934)

INTERVIEWED BY
SHIRLEY K. COHEN

October and November 2003

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California



Subject area

Physics

Abstract

Interview in five sessions, October-November 2003, with Charles W. Peck, professor of physics (now emeritus) in the Division of Physics, Mathematics, and Astronomy.

He recalls his early life in South Texas and his interest in radio; first year of college at Texas Arts & Industries; three more years at New Mexico College of Agriculture & Mechanical Arts. Recalls graduate studies at Caltech with Murray Gell-Mann, H. P. Robertson, Robert Walker, Richard A. Dean, W. R. Smythe. Works on increasing intensity and stability of the Caltech synchrotron, with Walker, Matt Sands, and Alvin Tollestrup; 1964 thesis on K-lambda photoproduction. Joins the faculty as an assistant professor in 1965.

Discusses his various teaching assignments, including an embarrassing moment when Richard Feynman attended one of his freshman physics lectures. Discusses his research at the Stanford Linear Accelerator Center and Lawrence Radiation Laboratory's Bevatron. Collaboration with UC Berkeley and SLAC on "crystal

ball” detector for SLAC’s SPEAR storage ring. Taking the crystal ball to DESY, in Hamburg. Works with Barry Barish at Gran Sasso laboratory in Italy, on MACRO; search for magnetic monopoles.

He also discusses his administration work at Caltech, as executive officer for physics (1983-1986) and as PMA division chair from 1993 to 1998, when he immediately had to deal with the troubles plaguing LIGO [Laser Interferometer Gravitational-wave Observatory]. Detailed discussion of the LIGO contretemps and how it was settled, and of turning Big Bear Solar Observatory over to the New Jersey Institute of Technology. Advent of David Baltimore as Caltech president; attempt to recruit Ed Witten.

Administrative information

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Preferred citation

Peck, Charles W. Interview by Shirley K. Cohen. Pasadena, California, October 1, 8, 15, 30, and November 12, 2003. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Peck_C

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH CHARLES W. PECK

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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Charles W. Peck, 1993

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Charles W. Peck
Pasadena, California

by Shirley K. Cohen

Session 1	October 1, 2003
Session 2	October 8, 2003
Session 3	October 15, 2003
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COHEN: Let's go back to the beginning. I'd like you to tell me about your parents.

PECK: I was born in a little town in South Texas. Every time I have told anybody where I was born, they say, "Huh?" With one exception, and that's our trustee Ms. [Shirley] Hufstedler. When I said I was born in Freer, Texas, she said, "Freer? Oh, I know Freer." I said, "How do you know Freer?" I was absolutely amazed! And she said, "Well, when I was a little girl, my father was a lawyer in the oil business."

Freer was in fact a center of oil discovery in the late twenties, early thirties. My father moved there and started in the oil fields but quickly discovered that being a grocer was in the long run a better way to make a living. So I was born in 1934, in this little town called Freer, Texas, which is just 100 miles south of San Antonio, straight south. It was an oil town; it was wild—though it didn't seem wild to me.

COHEN: Were your parents from this town?

PECK: No, my father was born in Louisiana. He was a Cajun—that is, he's part of the Acadians who came there, but his father was unable to live in the moist climate of Louisiana because of

asthma, and he was advised to go to a drier place, so he went to South Texas and that's where they eventually ended up. South Texas, in this case, means just a little north of Brownsville, really right at the very bottom of South Texas—in the Rio Grande Valley. So my father grew up down there. Laredo is part of his range, as it were, and then he worked in the oilfields as a carpenter and a “roughneck,” as they're called in the business.

COHEN: Now, was your mother from this town of Freer also?

PECK: No, but she came from South Texas, from a little town that was even more remote, or unknown, than Freer, called Fashing—as in the *Fasching* [Mardi Gras] in Munich. I assume that's where the name came from. Fashing was a little town south of San Antonio, and it had three or four houses in it. Freer was a metropolis; it had 1,000 or 2,000 people; it was a big place.

My mother came from a farm near Fashing. How she and my father met I have no idea; it was before my time. [Laughter] But at any rate, they got together, and as far as I know, they started their married life in Freer and that's where my father lived the rest of his life.

COHEN: So he didn't particularly have a university training?

PECK: No, there was no university training. My mother made it to third grade. She was the eldest daughter in a big German family, so her heritage is German. Her grandfather, I think, was the immigrant—my great grandfather. He and his wife came from someplace around Hamburg, in northern Germany. They migrated to South Texas to farm.

Then my father came. He had a long heritage on the French side, starting with Acadia to Louisiana, and then he migrated, because of his father's health, to South Texas. My grandfather—my father's father—was a grocer. There's no scientific or educational or intellectual connections at all, in either my mother's or father's families. On the other hand, my father was highly motivated toward education.

COHEN: He was probably a very smart man.

PECK: Yes, he was—oh, I'm sure he was. He read all the time. Not intellectual books, but he did a lot of reading, and in his youth he got the Harvard Classics series. I have them all, the whole series. I have no evidence, however, that he read them. [Laughter] I can't tell if they were ever opened or not, but at any rate he had them.

He didn't want me to go to school in Freer—it was too small a place. He felt I should go to school in San Antonio. So I went to a Catholic boarding school in San Antonio called Mount Sacred Heart, starting in the first grade.

COHEN: Well, that's interesting!

PECK: [Laughter] He thought I should have a better education than you could get in Freer. I'm not sure that's true, but at any rate it was his view, and he also wanted me to have a Catholic education. He was very, very religious, coming from the French tradition—more generally, the Acadian tradition, in this case. So I went to Mount Sacred Heart from the age of six. I well remember first going there and being so embarrassed because I was wearing short pants and all the big boys—you know, the guys in first grade [laughter]—were wearing long pants. It was referred to as military school: There was marching; there was an old retired sergeant from the army who ran things. So we had drills and a drum and bugle corps.

COHEN: It sounds like you don't have a bad feeling about this school.

PECK: No, no, it was fine. I left there when I got to eighth grade—it only went up to eighth grade. It happened to be adjacent to a Catholic seminary called De Mazenod, a seminary for the Oblates of Mary Immaculate. Eugène de Mazenod established this particular Catholic order—OMI. At any rate, the brothers would come over and take care of our PE [physical education]. So, they threw balls for us little kids to chase and catch—

COHEN: There were only boys in your school?

PECK: Oh, it was only boys. It was a big place! There were sixty kids when I first went there, and when I graduated I was one of three. [Laughter]

COHEN: That says something for your tenacity.

PECK: It had three classrooms. The first was for first and second grade; and then the other classrooms were third, fourth, and fifth; and six, seven, and eight. And as you progressed, you moved farther back, to the back of the room. My last year, I was at the very back of the room, and there I could pull books off the library shelves and put binders around them and read them when I was supposed to be reading something else. So I read lots of Hardy Boys books and many other things.

COHEN: But you must have been a very good student.

PECK: Yes, I had no problem in school. But at any rate, the only culture I knew was Catholic culture. And these young men—the brothers, as they were called; they were pre-priests—the brothers would come over and play ball with us. That defined the milieu in which I lived. So the only natural thing to do for a young kid who's pretty smart was to go to the seminary. So in the eighth grade, I left Mount Sacred Heart and went to what was called a junior seminary—that is, for high-school-level kids—at another place in San Antonio, called Saint Anthony's. It was actually a five-year place; at the end of five years, the young boys who managed that then went to what's called a novitiate, which was close to where my father grew up, down in the Rio Grande Valley of South Texas, in Mission, Texas.

However, by the time I got to the fourth form—what we would normally call the senior year in high school—I had discovered girls, and they were rather interesting creatures.

COHEN: And they weren't there.

PECK: And they weren't there. [Laughter] At which point I decided I didn't wish to go back to Saint Anthony's. I was aware enough to know that I had finished high school.

COHEN: And did you have a good training there?

PECK: Oh, yes, I think so. I had the run of the place in some sense. I'm a little puzzled as to why I had as much freedom as I did. Because, for example, I was so bored by all that religion

stuff that I managed to—I was interested in radio, and I managed to set up a ham radio station in a room that was behind a big chapel on the second floor. I had this little room to myself. I had a Tesla coil that I'd built in there; I set up a transmitter radio; I built radio receiver that I would listen to in bed at night—all illegally, of course.

COHEN: But this was very positive, because these men evidently realized that they had someone they should let do—

PECK: I guess so. I always wondered: It must have been obvious that I was not at the rosary or whatever was going on down there, because I would always skip that and be upstairs in my special room doing interesting things. But nobody ever called me on it. It always amazed me. Still does! [Laughter] Of course, when I left, they were a little bit distressed. One of them came to my house to see me; there was some attempt to keep me. But, as I say, the world was broader than what I had seen in Catholicism at that stage in my life.

COHEN: Well, it's interesting that you saw even this. Did you have siblings?

PECK: I was an only child. My mother and father divorced when I was about ten or so, in third or fourth grade. Complicated—it was all very complicated, but at any rate I do have two sisters and a brother, with several fathers involved. [Laughter] It's complicated.

At any rate, I didn't know what to do at that stage. I decided I'd like to join the navy, because they had better food than the army and I could learn more radio there—because radio was wonderful. I loved it. I had a ham station set up in my home, my mother's house in San Antonio. Of course I brought it from school, along with all my things, my Tesla coils and my big motors. I had all sorts of stuff that I had developed.

COHEN: They must have helped you get this stuff?

PECK: No, I don't know how I got it. It wasn't through the school. I would work in the summers in order to make money, in order to buy things, and that's what I would buy. My Bible was the *Radio Amateur's Handbook*. I didn't understand much of it, but I struggled like mad to try to figure out what they were talking about. I remember the wonderful experience of learning

how to solder, for example. I would try to solder; at first, it wouldn't work very well. And one day I managed to get the material hot enough and the solder suddenly flowed. Magnificent! Eureka! [Laughter] Not a very deep discovery, but there was no scientific environment—there was nothing! It was just me and a few books. I read everything I could in the library and magazines. They had *Popular Science* there, so I would read those things.

I also had free run of the chemistry lab. The guy who ran chemistry let me make all the hydrogen I wanted until I blew something up. [Laughter] I managed to break a Kipp generator—I think it's called a Kipp generator; some kind of generator. Anyway, I managed to explode it.

Nobody else did these kinds of things. There were three of us who were always at the top of our class—a fellow named Greg Leville, and me, and a third person whose name I don't remember. I suspect that Greg is a monsignor or cardinal or something nowadays. I suspect that he stayed on. He was very smart. He and I always vied for number-one position.

Well, at any rate, I decided to join the navy. My mother, who had had no education, had no reason to mind—if I wanted to do it, that was fine. But I always went in the summers to see my father. It was complicated. I'd go down to Freer to spend my two or three weeks with my father, and when he learned I wanted to join the navy, he was horrified. He said, “No, you should go to college.”

“College? What's college?” I asked him, “What do you do in college?”

“You learn how to run a business.”

“Business?” [Laughter] But I somehow learned that you could also do engineering in college, and being an electrical engineer sounded pretty good. You get to play with more radios.

Now, South Texas is a very political part of the world. We all know the famous Landslide Lyndon story; well, it happened right there. Landslide Lyndon [Johnson] was so-called because he became a senator by virtue of about 100 votes, and they all came from one area of South Texas run by a man named [George] Parr, who was the local *padrón* of all of the local Tex-Mex people. And it's curious that the last thirty or forty names in the voting register were all signed in green ink, in the same handwriting, and most of the people were long since dead. But nonetheless, Landslide Lyndon managed to become a senator from Texas in 1948. And anybody who wins an election by 100 votes obviously has a landslide. [Laughter] So, it's a highly political part of the world, and my stepmother—my father's wife, a woman named

Rena—managed to do the right things to get me into the local community college. It was called Texas A&I—Arts and Industries. It was a school that was the shining jewel in the constellation of Governor [James Stephen] Hogg. He had a sense of humor—he named one of his daughters Ima. So, as you can see, it's a little bit of a seedy part of the world. [Laughter]

At any rate, I managed to get into Texas A&I, although it was well past all registration times, and all the legalities were taken care of.

COHEN: This was just your father keeping you out of the navy?

PECK: That's right. And Rena helped in that and got me in. She's the one who did all the work, I'm sure, although my father was behind it all. But at any rate, I suddenly found myself in Kingsville, Texas, right in the middle of the King Ranch—the ranch house was just up the road a little piece—and I was suddenly a freshman majoring in engineering. All engineering was the same the first year, so I was taking all sorts of wonderful things like mechanical drawing and the Monge method of descriptive geometry, which is like drawing; learning how to use a slide rule. It was wonderful. I even won a prize, because I figured out how to multiply 1.002 times something, and I was the only one who figured it out—it was only a *tiny* little bit at the end of the slide rule. At any rate, that was my first year in school at Texas A&I.

But I didn't like the idea of being, as it were, my father's ward. I wanted to take care of myself; I didn't want to depend on anyone else. I've always been a rather independent cuss. One of my friends at Texas A&I had discovered somehow a place in New Mexico called the New Mexico College of Agriculture & Mechanical Arts [now New Mexico State University—ed.], located in Las Cruces, which happens to be, as you may know, just over the Organ Mountains. Well, it's just over the border from Mexico on one side, but it's also just on the other side of the Organ Mountains from White Sands Proving Ground [now White Sands Missile Range—ed.]. White Sands Proving Ground was at that time where the army was firing V-2s they'd taken from Germany and creating the first military rocketry in the United States. And the college and the proving ground offered a program, for those who were interested, to be co-op students, which meant that you worked half the year at White Sands for a salary and you went to school the other half year—blessed by both the college on the one side and the proving ground on the other. So that's what I decided to do.

COHEN: That made you financially independent.

PECK: Yes, that would make me independent. I could then pay my own way and do what I enjoyed doing anyway. So I and two people whose names I no longer remember drove up to New Mexico in a little Ford coupe across the desert of West Texas, where I had never been. We had to go there to take civil-service exams, because we were going to be hired by White Sands. So I did the civil-service exam, and I must have gotten a pretty high mark, because they took me and they didn't take the other guys. [Laughter]

At any rate, I spent half of that first summer going to summer school in Kingsville, at Texas A&I, doing organic chemistry, of all things. Then I left in the middle of the summer to go to New Mexico. I decided to start in school. It was a full-year program—you didn't have summers off. So I started in school and at the same time I was learning the beauties of the sandstorms in the middle of the summer in New Mexico. [Laughter]

COHEN: Oh, how terrible!

PECK: But actually it was wonderful for me, because it was the first time in my life that I was in a place with low humidity. I didn't realize that it could be dry and hot but not oppressive. It was wonderful! I found it wonderful, actually. It would rain, and I'd call it dry rain, because by the time the rain was on the ground it had practically all evaporated, and you had only a sort of mist coming down. It was remarkable! Dry rain.

So I decided, as I said, to start the first half-year in school, and then I would start working whenever the half-year was up. But I took my first physics course. I'd never had physics in my life at this point; I'd only had chemistry in high school, and general science, and no such thing as calculus. When I was at Texas A&I, my first year there, I had to take algebra. I took algebra and trigonometry—stuff I'd never had at seminary—and descriptive geometry. I was taking descriptive geometry when I went to New Mexico, and then while I was taking physics for engineers I was also taking calculus. So my second year in college was the first time I'd ever seen physics or calculus or any of those things. [Laughter]

I came to the attention of the physics teacher, a man named Ralph Dressel—who in fact spent a year at Caltech many years later. At any rate, he took me under his wing, and in lab he

would give me special things to do. “Here, do this instead. Don’t worry about all of that. Here do this experiment.” [Laughter]

COHEN: A little niche for you.

PECK: That’s right. He took care of me. He said, “Well, you don’t want to be in the co-op program. You come work for us at PSL, the Physical Science Lab, which was sort of the analog of JPL [Jet Propulsion Laboratory], except much smaller. Its fundamental function was to analyze data that was taken at White Sands Proving Ground and to develop various things appropriate for rocketry. They in fact developed the Corporal missile there. But Professor Dressel was interested in electromagnetic radiation and making these spot antennas—antennas that were adapted to the geometry of a rocket, for example. So he put me to work in the lab for him, and one of the first things he explained to me that I was going to do was measure some microwaves. He was looking at the rotation of microwaves as they go through a piece of magnetized ferrite. He wanted to do that for adjusting polarization, and he was having me do some measurements. He told me that I could measure the wavelength of the radiation by measuring the peak signal on a little probe that was in a rectangular piece of wave guide. And at the end of it, there was a little cavity with a knob on it that could tell you what the frequency was. So I could read the frequency and I could measure the wavelength, and I thought that was pretty neat—I could calculate the velocity. I calculated the velocity, and it was greater than the velocity of light! So I did it over. This was the first time I had ever been in a lab and I’m making this tremendous discovery! [Laughter] I kept doing it over and over again, and the numbers always checked. It was going faster than the speed of light. Of course, I didn’t realize the distinction at that time, between phase velocity and group velocity and signal velocity, and all these various things. Phase velocity can be faster than the speed of light, no trouble at all. It goes to infinity in various cases, for example. And that’s what I was measuring—I didn’t know I was measuring it. But I was very puzzled by this.

COHEN: Now, were there any graduate students there?

PECK: No, it didn’t offer PhDs. It was just a college, and its purpose was agriculture and mechanical arts.

COHEN: So this man doing these experiments and having you do them—that was really his own thing?

PECK: That was his own thing, that's right. I don't know the technical details, but he probably had a contract. He was working in PSL—the Physical Sciences Laboratory. It was a laboratory that offered employment for professionals as well as for students. Students could come and read film of rockets going up and make measurements on them in order to work out accelerations and velocities. This was in 1953 or '54, I guess. So it provided employment for students. It provided employment for me, since Dressel said, "Here, you're going to come work here; you're going to drop that co-op stuff. I want you to get through college faster than that will let you." And he took me under his wing and gave me a job, told me what was in the real world, had me do various kinds of calculations that he was doing. I remember calculating tables of Legendre polynomials. Tables existed up to some number, but he needed them much higher for the work he was doing. So I was calculating these tables with an old-fashioned calculator. It was great when we finally got one that you could just hit a bar and it did the arithmetic electrically.

COHEN: Now, did you take other courses besides physics?

PECK: Yes, I took [engineering] courses. Then I changed from engineering to physics, at the appropriate time. The trouble with going from engineering to physics was that physics was in the arts and sciences part of the college, whereas engineering was in the school of engineering. And if you're in the arts and sciences, you have to take things like literature and languages and sociology, and various other things. At any rate, I hadn't taken these kinds of things at the appropriate time, because I was in the engineering school. So I had to take them all in one year, one semester, or something. And I didn't have *any* science courses that semester. It was *awful!* It was so hard! I was just struggling so much with this damn sociology—excuse me, with sociology. [Laughter] And the only attractive thing about sociology was the young lady who sat beside me. [Laughter] So it came time for the exam, at the end of the sociology course, and people were filing out of the room. And I was walking out, when the teacher called me over. I said, "Why did you call me?" He said, "You did so well that you don't have to take the exam." [Laughter] Oh, my! What a wonderful feeling! [Laughter] Anyway, I passed the course, and I

was so pleased that I didn't have to read any more of that—I found it so boring. I could finally get back and take relativity, for example, and classical mechanics, and good stuff like that.

COHEN: You were living full time in Las Cruces now?

PECK: I was living full time in Las Cruces. We were living in what were called barracks, which was where the co-ops lived. They were World War II army-surplus barracks. Whenever the wind blew, it found every crack, I assure you. I had my radio receiver there. I would go to sleep at night listening to KSL radio from Salt Lake City—music through the night. Had my earphones on so I didn't disturb my roommate, who slept above me. There were two people to a room, with bunk beds.

But eventually, when I was no longer in the co-ops, I guess I wasn't allowed to live there anymore. I moved to a permanent structure then, and I had a roommate there also, who was an engineer. Somehow he managed to find a pinball machine and we had a pinball machine in our room. Needless to say, it was a popular room. [Laughter] We decided we would liven it up, and we decided to make some beer. So we made ourselves some beer and put all the bottles underneath the sink. Then, a couple days later, I was coming to my room and I smelled something. [Laughter] And the closer I got, the stronger the smell was. Until finally I got to the room, and there was a lake of beer all over. The bottles had burst, obviously. [Laughter]

COHEN: Well, you make school sound like a real lark. But you must have worked quite hard some of the time.

PECK: School was quite easy. I loved the science I was working on. It was exactly what I had always wanted.

COHEN: That school has a reasonably good reputation.

PECK: Yes. It's now called New Mexico State University. It's independent of the University of New Mexico in Albuquerque and Santa Fe.

COHEN: It's probably part of the state system?

PECK: Yes, it's part of the state system. And of course it was a land-grant college. The plains needed to be filled, and you needed colleges that would find out how to do the things you needed to do in order to make cotton grow. This is all in a very rich agricultural area, the Mesilla Valley. And it was there for quite a while—I don't know when it was formed.

COHEN: You spent four years there?

PECK: I spent three years there, because I had spent my first year at A&I. And then Professor Dressel said, "Apply to the NSF [National Science Foundation]. They're starting to have fellowships. They started last year giving fellowships to graduate school."

"Graduate school? What's graduate school?" I didn't know what any of this was. I mean, it hadn't occurred to me. I just figured I'd enjoy doing radios for the rest of my life. And he said, "No, you need to go to graduate school. You should apply for an NSF fellowship, and you should go." So I applied to Princeton and to—

COHEN: Had you been out of New Mexico or Texas at this point?

PECK: No, never. I'd been to Mexico, but I'd been nowhere else. I had gone to Mexico City as a boy with my father, and I went across the border at Laredo and Juarez many times—it was just down the road. Certainly when I was in A&I, we would get together, big groups of boys, and go down and check out Nuevo Laredo, of course, and stop at my father's businesses—he also owned and ran a restaurant.

Dressel was telling me I should apply, so I decided I'd apply to Princeton, to MIT, to the University of Illinois—which is where Dressel came from—and to Caltech. The first place I got admitted to was Princeton, and then I got admitted to Caltech, and then I got admitted to Illinois. But MIT decided they didn't want me, so I said, "To heck with you." I was pretty happy with the Princeton offer. But as I thought about it, I found I preferred the West. So I chose Caltech—and not for any deep reason. I had also taken the NSF exams and I got an NSF fellowship. So I started graduate school with a three-year fellowship.

COHEN: Did you come out here before you were admitted?

PECK: No. I came to Southern California, where I had a job at North American Aviation in Inglewood, working overtime doing nothing—doing calculations of rockets and things like that. But there was nothing to do. Nonetheless, they were paying us overtime. I never understood that. But it was clearly supported by the military. You could go out and watch the advanced fighters take off. It was quite exciting. At any rate, I worked there for the summer, and then I found an apartment to live in, in Arcadia. I started in, it must have been around October 1st, in 1956, and I was admitted to graduate school here in physics. Of course I had come to the institute shortly after I arrived in Southern California. The first thing I did—just as I would do in New Mexico—I marched into the chairman’s office and said, “Here I am.” And [Robert F.] Bacher looked at me and said, “Yeah? Who are you?” [Laughter] He was the chairman of PMA [the Division of Physics, Mathematics, and Astronomy]. So I went to the top, I went to see Bacher. We chatted a little while. He told me his daughter had just gotten married. He was a very jovial chap, but he seemed a little bit formidable. I’d never seen anybody quite as formidable. The office—it’s the same office I was in when I was chairman [1993-1998], except it looked very different. It was dark. It seems to me, from my memory, that it was a dark room. He was sitting behind a dark desk. You go through a little corridor from the secretary’s office to the chairman’s office, and there at the end was this imposing desk that was all dark, with Bacher sitting behind it, with a little table lamp. [Laughter] That’s how I remember it; it probably wasn’t that way.

COHEN: You just came in to introduce yourself?

PECK: I came in to introduce myself: “A new graduate student. I’ll be starting in the fall. Thought I’d come say ‘Hi’.” At any rate, we had a short but fine little conversation. I remember he was slightly formidable; that’s all I recall.

COHEN: Well, he probably talked a lot.

PECK: [Laughter] But as I said, I finished working at North American, where I got the summer job to make some money to buy furniture and stuff like that. And then I moved to Arcadia, and I started graduate school, with an NSF fellowship. I didn’t have a GRA [graduate research

assistantship], so I didn't have to work in the laboratory or do any teaching or anything. I had all my time to study, so that's exactly what I did.

I had quantum mechanics from Murray Gell-Mann. I had classical mechanics from H. P. Robertson. I had methods of mathematical physics from Bob Walker. I had Math 108—which was at that time called Analysis—from Dick [Richard A.] Dean. And of course, I had the *pièce de résistance*, the course that separated the sheep from the goats. [Laughter] And that was Static and Dynamic Electricity, by Professor [William R.] Smythe. This was the famous Smythe course, which you may have heard of if you've talked to other people who were graduate students here; I can't imagine anyone would forget that experience. I did quite well, I think—with one exception. I got the lowest grade I'd ever gotten in my life—in anything related to science, at least—in Smythe's electricity course. I got a C+ at the end of the first term. And I was devastated with a C+. And the next term I got a B+. And the last term I got an A-. [Laughter] I was challenged.

COHEN: But there must have been a sea change of difference to come to Caltech from where you came from.

PECK: Oh, certainly. People here were a lot smarter. I was very impressed, for example, with the undergraduates. Now when I read it, it looks rather unsophisticated, but *The California Tech* then seemed so sophisticated and dramatically intellectual compared to anything I knew about. As I say, the courses were wonderful. It was hard. I happened to run into all my course notes the other day, moving some stuff, and there were my little spiral binders, with all the notes and all the Smythe problems worked out.

Then the summer came, and of course I didn't have to do anything, but I wanted to get involved in research. People who come from places that don't have an experimental research program—all they know about physics are theoretical things, and that's all I knew. So I wanted to be a theorist, of course. That's what most young people do—everybody wants to be a theorist. That's still true. We have the fourth floor full of theorists. But then they learn about the real world after a little while.

COHEN: [Laughter] And they come downstairs.

PECK: That's right. Exactly. [Laughter] But I decided I wanted to be a theorist, so I decided to work for Bob [Robert F.] Christy [Institute Professor of Theoretical Physics, emeritus]. I think that was the first year—I might be wrong. At any rate, I worked for Bob Christy. He gave me a problem to do and I didn't have the faintest idea what it was. This actually might be my second year—but at any rate, at some stage I was formally a graduate student working for Bob Christy, but I couldn't do what he asked me to do. Some problem—I can't remember exactly what it was. So I kind of drifted over to the synchrotron. It sounded like there were interesting things going on over there. And I started working with people who were building a bubble chamber there. I could do anything I wanted; I was under a fellowship; I was not restricted from anything I wanted to do. So I started working over there. I remember vividly being underneath the bubble chamber, working on something. And who do I see just beyond, sort of looking to see who was underneath, but Bob Christy. [Laughter] “Oh, hello.” [Laughter]

COHEN: What did he say?

PECK: Well, there was nothing formal about it. This was before taking any exams or anything, so it was completely informal. But I was working with the bubble-chamber guys at the synchrotron and that was where I rather soon found a home. In those days, there were no written qualifying exams. You had an oral exam as a qualifier for admission to candidacy. It was an oral exam that covered all of physics. You had to take it in your third year. So the summer before, I spent the entire summer doing nothing but reviewing every bit of physics I had ever learned and writing it all out in a book I had, about this thick, three inches of handwritten papers. I reviewed everything I knew and put it all together.

And so finally it came time for my exam. H. P. Robertson was going to be on it, but he couldn't be there; he was always off to Washington. In fact, whenever he was teaching the course in classical mechanics, he would walk in with his briefcase and he would put it by the door. In fact, the place where it is is exactly where Hal [Harold] Zirin [professor of astrophysics, emeritus] has his office now; it was a classroom in those days. He would put it by the door, and then he would start lecturing on the blackboard, at its far extreme, and the lecture would then proceed. Always, somehow or other, there was a chalk line in the middle of the blackboard where two boards came together, and that was always the Z-axis for something or another.

[Laughter] And as the lecture progressed, he gradually moved to the right, until two minutes before the end of the lecture, he would pick up his briefcase, march out the door, and go off to Washington. [Laughter]

COHEN: Were they good lectures?

PECK: They were wonderful lectures. He was a great lecturer. H. P. Robertson was a man I admired enormously; he was very good. Murray [Gell-Mann] was fabulous. This was the year in which parity was discovered to be nonconserved in the weak interaction. And Murray, of course, was intimately involved in this and was telling us in the class what was going on. We didn't know much about it, but it was all very exciting, I assure you. He called Professor Wu—can't think of her first name—Chien-Shiung Wu; he just called her “Wu.” [Laughter] And he talked to her about the latest experiments she was doing and talked to the guys in Chicago.

COHEN: So, there was a real sense of excitement.

PECK: Oh, it was very exciting! And Murray was fabulous. He was a *great* teacher. When I was executive officer for physics [1983-1986], I tried *so hard* to convince Murray to teach, because he's *so good* at it. But he refused to. But I had the wonderful experience of the young Murray—he's only five years older than I am—the young Murray in full throat, teaching quantum mechanics to all of us who had never heard of it before. **[Tape ends]**

Begin Tape 1, Side 2

PECK: One person I did feel left behind by was the Nobel laureate Ken [Kenneth G.] Wilson. Ken Wilson and I were in the same class; he and I started graduate school here in the same year. He probably took his PhD in three years, or maybe it was four [Wilson received his PhD in 1961.—ed.]. I took eight years—I took a long time. But at any rate, Ken was the guy who left us all in the dust. He could just do those Smythe problems like *pow!* I mean, they were like little explosions. He was fabulous. We all aspired to be a Ken.

COHEN: About how many people were in Murray's class?

PECK: In Murray's class— It was called Physics 205; the course number still exists, but it's a different course now. Let me think about it a minute. This was a quantum mechanics class. In those days, quantum mechanics wasn't something you taught to undergraduates; it was a graduate course. This was a first-year graduate course—probably it was a class of about twenty. We were probably all physics graduate students, is my guess. There may have been an occasional astronomer who managed to wander in and get lost. [Laughter] I don't know.

COHEN: Or an electrical engineer?

PECK: Or an electrical engineer, possibly.

COHEN: At that time, electrical engineering may have been part of the physics division?

PECK: No, it was no longer part of physics, but there could have been some electrical engineers; those guys were getting interested in quantum mechanics at that time.

COHEN: How did you spend your eight years as a graduate student?

PECK: Oh, well, I decided I wanted to do a certain experiment. The main thing that had been done at the synchrotron was the photoproduction of pi mesons. You shine high-energy gamma rays on hydrogen, and what comes off is hydrogen plus a pi-zero, or a neutron and a pi-plus. And what was going on in that part of the world was one of the motivations for the building of the synchrotron here. Because at the time, at Chicago, [Enrico] Fermi had noticed that whenever protons collide, you make pi-p. The cross section kept getting bigger and bigger for making pi-p's as a function of energy over the energy range available at that time. And one way to get insight into what was going on was to do photoproduction. That is, to make pi-p systems from photon-p systems. And so that was the motivation for building a photon machine here, so they could do these experiments.

The experiments had shown that the cross section for this just seemed to be rising and rising, and nobody had seen it go over the top. I mean, it kept going. One of the first things that was discovered here was the fact that it did rise and then it came back down again. And it went by the name the "3-3 resonance," for technical reasons—we now call it the "delta 12-38." That's

a particle, but at that time it wasn't so interpreted. It was referred to as a resonance; something that's inside protons and pions was resonating—we didn't know what. It was a wonderful sense of the unknown, and that's what really attracted me to that field—all these new kinds of matter. It wasn't like different elements; it was something else, something completely new that you find only in these circumstances. That was the most exciting thing I could imagine. A synchrotron is a nice, big, wonderful machine to play with.

It took me eight years to get out of graduate school because I wanted to do the same kind of experiments in which you shine gamma rays on protons and you make K mesons and lambda particles—these are the “strange” particles. The experiments had been done at low energies. The strange particles were completely different kinds of things from protons and pions. We didn't know what they were, but they were the first so-called “strange” particles. Murray Gell-Mann, prior to coming here, had postulated the existence of a new quantum number that solved some of the puzzles. The idea was purely phenomenological—the notion of adding a new quantum number, which he called “strangeness” because these things were very peculiar. They were very strange indeed. [Laughter] So they were referred to as the strange particles. I wanted to do experiments with strange particles, but the synchrotron was such a low-intensity machine compared to modern ones that it would take a very long time to do such experiments. I'm astounded, when I think back, at how primitive things were at that time. It would have taken me the rest of my life to do the kinds of measurements I wanted to do. And the only way to do it practically was to get a new injector for the synchrotron. The injector I'd been using—the machine that gave the initial burst of energy to the electrons before they went into the ring and circled around in the magnetic field—had been built by Alvin Tollestrup. It was a 1-million-volt transformer. You put a pulse on one side, you get a million volts on the other end, in which electrons get accelerated. And at that energy, the required magnetic field to hold the electrons in the synchrotron was lower than the so-called remanent magnetic field that you have in a piece of iron whenever you magnetize it with an electric current and then you turn off the current. You try to get iron which has as little of that remanent field as you can—so-called “soft” iron. But even so, 1 MeV [mega electron volts] was such low energy that it was necessary to “degauss” the magnet—i.e., reduce its field to as small as possible—so that it could then be increased just a little so the electrons would stay inside. But it was below the natural remanent field of the iron, and therefore—because it was a rather complicated thing and the energy was so low—almost any

little thing could come along and disturb the beams. The number of particles we accelerated successfully varied dramatically from pulse to pulse. And in fact Matt [Matthew] Sands had built a beeper—as he called it—which made a beep whose frequency was monotonic with the intensity; the higher the intensity, the higher the tone of the beep. So *beep, beep, beep, beep, beep, beep, beep, beep* was the music that came from the synchrotron, which we all learned to enjoy and love. The moment you walked in, you knew how the beam was. [Laughter] If the beam operator was a gentleman named Al, who was one of our beam operators, it was usually beeping high. If it was somebody else, it was usually beeping low. There was a whole panel of switches and knobs that you could adjust. And there was a feel in moving around and adjusting the size of S1, or maybe S3, or C2—all these referring to sines and cosines of angle around the machine.

COHEN: Who were you working with at this point?

PECK: Well, at this stage, I was probably working with Bob Walker. I had decided I didn't want to continue in the bubble-chamber work. I wanted to do various other things at first, but I decided that I would work with Bob Walker, and I decided I wanted to do K-lambda photoproduction, in the hope that maybe there would be some new phenomenon that arose there—like the delta 33 that appeared in pi-p scattering. As it turned out, nothing happened. [Laughter] But to me that was the most exciting thing around.

But the machine worked so poorly, and the cross section was so small for making the K-lambdas, that to get enough events would have taken forever. Also, we needed higher energy. So I worked on the synchrotron for many years, working to improve both its stability and energy.

COHEN: At this point, you must have been done taking courses.

PECK: Oh, yes; I finished that in three years or so. I was working full time in the lab. I loved it—just loved the whole smell of it.

COHEN: By this time, you didn't have any NSF grant, did you?

PECK: No, I was then being paid as a graduate research assistant. Caltech had some loans available, so I managed to get loans. At any rate, Bob Walker and Matt Sands and Alvin Tollestrup—the triumvirate, as we graduate students referred to them; they were the guys who ran the place—had managed to get monies to buy a 10-MeV linear accelerator that would provide a much more intense and much better-controlled higher-energy beam to inject into the synchrotron. I did calculations to see how that would work. I then made measurements around the machine as we got it working, to get the beams all coming out at the right places. So I worked on machine physics and the kinds of things that you do in order to make a synchrotron work, and finally I did my thesis experiment. It probably took about a year of data-taking, and then I finished the data-taking in approximately December [1963] and I had my thesis ready to go by March or so.

COHEN: You obviously must have been very good at this machine, and they wanted you. Because, as you know, they don't like to keep people so many years.

PECK: Yes, that's right. I remember walking once with Alvin Tollestrup. He was a very cryptic fellow, and he made some cryptic remark to several of us—that there was only one graduate student that had gone to the synchrotron who he felt was good enough to stay. None of us had any idea who he was talking about. I have a suspicion he was talking about me, because I was the only one who stayed. [Laughter] Nonetheless, that little remark he made—something to that effect—suggested that he didn't think much of most of the people who came here. [Laughter]

COHEN: What did you actually do for a PhD thesis?

PECK: I did photoproduction of K-lambdas on protons. So gamma-p goes to K-lambda, K+ lambda-0, and analyzed that data. Then it was time to get a real job, so I applied to a number of places for jobs. I'd already started working for SLAC [Stanford Linear Accelerator Center] at the time.

Pief [Wolfgang K. H.] Panofsky at Stanford was starting to build what was referred to then as “the monster.” This was the idea of building a two-mile-long linear accelerator. It's now called SLAC. When I spent my first summer there, it was still being referred to as “the monster.” I had gotten a bicycle and I was living in a hotel in Palo Alto and bicycling up to the

laboratory. There was a sign—"Monster"—with an arrow pointing to the left; so I knew I was on the right track. And there were barracks, army-surplus barracks. I went up there in the summer before I finished my PhD. I finished taking my data in December and I finished my thesis in March, so this was probably the previous May or thereabouts that Alvin and I and Jerry [Jerome] Pine—who at that time was a relatively new hire at Caltech, in the particle physics group, and had come from Stanford and knew his way around Stanford—went up there. Alvin and Jerry wanted to get involved in what was going on at the new machine that was being built there, and so I went along. Then I guess I spent some months in the summer, as I recall it, working in the offices of the monster—not yet named SLAC.

Then, I guess the next summer, I worked there yet again and designed a magnetic spectrometer that went up to the highest energy of 8 GeV [gigavolts], called BeV in those days. It was, in fact, subsequently built. And another one was built that was 20 GeV. These two were used by—well, by me eventually—but by Henry Kendall and Jerry [Jerome] Friedman of MIT and Dick Taylor from SLAC, all of whom subsequently got the Nobel Prize some years later [1990] for doing deep inelastic scattering, so-called, which yielded the first really strong indication of the existence of quarks inside protons and neutrons.

COHEN: But you didn't move up there? You just went up there to work while your family stayed here in Arcadia, or Pasadena, or wherever you were.

PECK: Yes, that's right. I guess one summer my family went up there. Although I was hired to work on the Caltech synchrotron—in fact the synchrotron was going up to higher energies then—in fact I never worked on the synchrotron again after that. I got involved in doing experiments at SLAC—first things being experiments on using the spectrometers that had been built, whenever SLAC started operating. I can't remember what year that was now [1966—ed.]. It was sometime after I had first been there. When I was first there, they were still laying the tunnel. My god, two miles is a long way. [Laughter]

COHEN: I wanted to get to the point where you have a regular appointment at Caltech.

PECK: Yes. So by the time I was finishing my PhD—around March 1964 or thereabouts—I immediately took a job as postdoc at Caltech. I didn't look for a professorship at that stage. But then, during that year, they told me, "You know, you should be an assistant professor."

"Oh, what's that?" [Laughter]

So I applied to SLAC, and to Cornell, and to the University of Washington, and to Caltech. And I had an offer from Pief Panofsky at SLAC, as well as from Bob [Robert R.] Wilson at Cornell. And from people at the University of Washington, and then Caltech. I thought about it a long time and decided to stay here. The real reason was that at Caltech I could still work at SLAC just about as easily as if I were there, and I also had the option of going to Berkeley or to other labs that might be developed in the future—as they, in fact, were.

COHEN: Well, you know, that's almost unique. At Caltech they don't usually ask their own students to stay on.

PECK: Yes, that's right. It was more common then than it is now. It's become less common. But even then, it was relatively infrequent.

COHEN: Some people go away for a while and then come back.

PECK: Yes, but I never left; I was always here.

[Tape recorder turned off]

CHARLES W. PECK**SESSION 2****October 8, 2003****Begin Tape 2, Side 1**

COHEN: We have you launched—you got your degree, you got a job. And there are three tracks that I want to go to. I want to talk about your teaching; I want to talk about your research—which doesn't matter where you are in your career, it goes on and on; and then I want to talk about your administration. So, I think maybe we should start with your teaching.

PECK: That's fine. I never taught as a graduate student, because I didn't want to be distracted. And as soon as I became a postdoc, I immediately took a section of freshman physics and I found it a wonderful experience.

COHEN: Who was lecturer at this point?

PECK: I can't remember who was lecturing. Jerry Pine was one of the lecturers, I think; there might have been several, actually. But at any rate, I had a section all year. As you perhaps know, the way freshman physics is organized is that there are two lectures each week—at least at that time; and I think it's still that way—and then the students break into groups of between twenty and thirty, of which there are about ten. These are called sections, and they meet independently with an instructor twice a week.

COHEN: And very often they're taught by professors.

PECK: Oh, yes, they're often taught by professors—half the time, maybe more, they're taught by professors. In my case, I was a postdoc. But I found it wonderful because I had all these malleable little creatures that I could tell the right things to. [Laughter]

COHEN: So you didn't look at this as a chore, you really enjoyed it.

PECK: Oh, I really enjoyed it; it was great fun. And then, probably the next year [1965-66], when I was an assistant professor, I then lectured. I guess I managed to do one lecture to the whole class that first year. Later in my research career, I got to know and work with, very well, a person who had been in that first class. He said, “Well, we didn’t understand anything.”

[Laughter] I talked *way* above their heads. Of course, that’s a characteristic problem of new teachers. They always want to impress their students, and in making sure they impress their students, they lose them. That’s common—that’s not unique to me and my experience.

Then the next year I lectured. I had one terrible experience. This was one day when I was doing electricity and magnetism. I had worked out my lecture the night before. I wanted to explain something in a physical way. It turns out that that particular day Dick Feynman came to me before the class. Of course, we were teaching from his book [*The Feynman Lectures on Physics*, R.P. Feynman, Ralph Leighton, & Matthew Sands (Addison Wesley)]. And he said, “Can I sit in on the class?” Of course I said yes.

COHEN: He made you a little nervous?

PECK: Not really. I had no problem, I gave the lecture, I wasn’t paying any attention to Feynman. But at a certain point in the lecture, I heard later from the students that he started shaking his head “No!” [Laughter] Oh, god! I didn’t know that. I had given my argument about how electromagnetic waves work. And then, as I was getting ready to go have lunch, it occurred to me, “You know, that argument was wrong. That’s not the way it works.” I had it wrong—I suddenly realized I had it wrong. Oh, my god! Feynman was there! I was just devastated.

So that afternoon, I gave him a call and said, “Look, I know you were in my class this morning. I realize that my description of how electromagnetic waves propagate was incorrect. For the situation I gave, I should have had waves going both ways and I had them going in only one direction.” He said, “Yeah, yeah, yeah! I know all that. Look, but that’s one of the hardest things to teach—to do it physically. So don’t feel too badly about it. There are two other things that are about as hard.” I forget what they are right now; I haven’t thought about this in years. But at any rate, he put me at my ease. He said, “OK, you made a mistake. Tell them next time that you made a mistake and don’t worry about it.” He was a very kind man. He could have

squished me. [Laughter] But of course, the students knew that Dick Feynman had been up there shaking his head.

COHEN: They caught that. Even in their student days, they knew—

PECK: Oh, well, they were all watching Feynman, of course, out of the side of their eyes. And I'm sure they saw him shaking his head. As I said, I learned it from someone who was in the class.

COHEN: Look how you remember this, after all these years!

PECK: Oh, man, I assure you! There are certain things you never forget. [Laughter] And getting that wrong with Dick Feynman in the classroom was a bad thing.

So I lectured. Probably in those days we lectured maybe either one term or one-and-a-half terms—I don't recall. You didn't have to lecture for the whole year. I probably did that for a couple of years, and then I was assigned to teach Physics 106, Electricity and Magnetism, and I guess it was a full-year course at that time, at the junior level. I've taught that course, or modifications thereof, many times since. I got attached to it, and indeed I taught it two years ago. I taught an advanced version of it last year, and I'm teaching it this year. [Laughter]

COHEN: Do you enjoy the subject?

PECK: I very much enjoy the subject. I've never taught the same class twice—well, maybe I have once; I shouldn't say that so generally. But generally speaking, I change what I do—certainly if there are several years of it. I like to have a pattern of teaching the same course three times: First, to learn the material; the second time so that I can then feel comfortable about teaching it; and then the third, I get bored with it. So I typically have taught a course three years in a row. Over the years, I've taught classical physics—which is classical electricity and magnetism. Nowadays, in the first half year it's classical mechanics. At the junior level, it's Lagrangians and Hamiltonians—that formal formulation of Newtonian mechanics. And then the second half is electromagnetism through relativity and Maxwell's equations.

COHEN: Now, you had just physics majors in that course?

PECK: Oh, it's intended for physics majors, but it's taken by graduate students, who come here from other places as first-year graduate students. It's taken by our juniors. It's taken by a few sophomores. It's taken by people in astronomy. It's taken by some applied physicists, and occasionally somebody else. But [physics majors are] the main clientele. As it turns out, it's always been a very inhomogeneous group—and so rather hard to teach for that reason. You have some people in there who, if you don't dot the "i" right, they yell at you, because they know everything about it. Then you have others who are completely lost; they don't know what's going on. So it's hard to do. Efforts have been made these last couple of years to change that—to create a new course called Physics 196, which is supposed to attract those who are better prepared. And it worked last year. Instead of having the usual seventy in that class, it was a more reasonable thirty or so.

COHEN: Now, do you share the lament that the students aren't as good as they used to be?

PECK: No. As far as I can tell, there are some who get it immediately; there are some who have troubles—I find the whole spectrum. Starting at some point in my teaching career, I used a particular grading scheme, based on the assumption that I always made exams of the same difficulty. The grading scheme was eighty to a hundred is an A, and sixty to eighty is a B, and forty to sixty is a C, and then it goes down finally to a fail. And the distributions over the years with that particular scheme have always stayed about the same.

COHEN: So you would say the students are as good as they ever were.

PECK: As best I can tell, they're as good as they ever were.

COHEN: Because you're always hearing this, you know.

PECK: Yes, yes. But that just proves the professor is getting older. [Laughter] I mean, we all worry about the next generation, our replacements. Clearly they're inferior to us, right? But in fact, in some cases, I'm teaching more sophisticated things than I did.

COHEN: There's more to teach all the time.

PECK: But the classics are quite, quite stable. So, as I say, I've taught Physics 106 and 196 many times—I don't know how many. I taught a course that I loved enormously—Physics 102, I think it was called—which was Modern Physics. It was an introduction to quantum mechanics without doing the full quantum mechanics. It was based on Bob Leighton's book [*Principles of Modern Physics* (McGraw-Hill)]. And probably before that, on [F. K.] Richtmyer, [E. H.] Kennard, and [T.] Lauritsen [*Introduction to Modern Physics* (McGraw-Hill)], I suspect. It was an open-ended kind of course, and in fact in the third term I always taught it differently than the others. Often I would have the students give lectures. They would choose a subject and give a lecture. I always had students write papers in that course; they would choose some subject and write about it. I was sitting in my office a couple of years ago, and a man came to see me. He said, "I had your course many years ago, and, you know, writing that paper was really hard work but it was really great. And it turns out that Steve Koonin and I decided to work on the same subject, and you said it was OK to work together. And we finally finished it and we brought it to you. You were working down there at the synchrotron. So we came in at five o'clock in the morning, and of course you were there, and we presented you with our thesis. Do you remember that?" [Laughter]

COHEN: So Steve [Steven E.] Koonin [Caltech provost and professor of theoretical physics] was in your class?

PECK: Steve Koonin was in that class. Another instance: Mike [Michael S.] Turner, who's a professor at [the University of] Chicago—he's now running the physical sciences at NSF, for example.

COHEN: He's quoted a lot in science magazines.

PECK: He's quoted a lot because he's very quotable. [Laughter] He has a rich sense of humor and he uses it freely. He gives talks with slides, with transparencies, in which half of them are Darth Vader or something of that sort. He really holds your interest in his talks. Well, he came to give a physics colloquium here some years ago. It was a great talk, as Mike's always are.

And afterward I wanted to go down and shake his hand and say, “Hey, that was a great talk. I really enjoyed that. I learned something.” And I was coming down the steps in 201 East Bridge—you know, they’re steep. As I was coming down, he saw me. There were several people around, but he happened to notice me coming down, and he suddenly put up his hand and said, “*I’ll get it in! I’ll get it in!*” I didn’t know what he was talking about. He said, “Well, don’t you remember? I never turned in my paper in the last term of Physics 102, twenty years ago.” [Laughter] It was on his conscience. He’d promised me he would turn it in. But I said, “No, it’s OK. Your seminar today, you did a fine job, in place of it.” And I went back—I have all my notebooks, my grade books—and looked it up, and sure enough, he hadn’t turned it in. [Laughter] But he passed anyway, because he was so brilliant at everything else. In spite of his not having turned in his paper, I passed him. Probably one of the lowest grades he ever got. He was a real character—he still is.

There are a number of people I’ve had the pleasure of teaching in that course. One was Douglas Osheroff, for example, who’s a Nobel laureate. For some years, I taught the electronics course—Modern Electronics. I very much enjoyed electronics in the laboratory. Several of us team-taught—Ricardo Gomez, Alvin Tollestrup, and I would team-teach that, each one of us doing a different aspect of the subject, and that was intended for physics graduate students. But some electrical engineers would come over, because they wanted to figure out what the heck those [physicists] were talking about. So we did it in a physical way, rather than formally, as an engineer would do it: I mean, an engineer has to save three cents; we didn’t worry about the three cents. [Laughter]

Let me tell you something I’ve never taught: I’ve never taught a full course in formal quantum mechanics, although I’ve always wanted to. Or in my own subject, particle physics.

COHEN: Why is that? They didn’t offer the course?

PECK: No. The courses that I teach tend to be less popular ones—they’re rather specialized. People don’t like teaching 106, for example—Topics in Classical Physics. But I enjoy it. Teaching has always been a very pleasurable experience for me, and I continue to teach. I’m in my last year now; I’ll be retiring just at the time that I start teaching, curiously. I won’t go into

the details of that, but I'll retire just at the moment I begin teaching this academic year.

[Laughter]

COHEN: I see. Well, many people retire because they're tired of teaching.

PECK: Yes, right. But what I'm tired of is being on committees and looking at all the new rules. A whole set of rules was just published for whenever you have to buy something now. It's just unbelievable. The place is getting too bureaucratic.

COHEN: Of course, you've had good students.

PECK: Yes, of course. They keep me young. They're great. I learn more from them than they learn from me. They ask good questions. Of course, by now, I have enough experience that I can answer them. [Laughter] I've heard that question before, so I'm ready.

COHEN: In some sense, your teaching has not been aligned with your research. I mean, they've been two distinct—

PECK: Oh, I bring my research—the technical aspects of it—into the teaching as much as I can. There are lots of things about accelerators that you can incorporate into a course on electricity and magnetism, for example. There are more than I in fact have incorporated. You have to cover some of the standard stuff, but whenever I can, I try to talk about beam optics and things of that sort, and at least give people a general idea of where they would use this theory I've just given them. So I bring it in that way. But as I said, I've never taught—except in seminar courses or something like that—I've never taught a formal full-year course, or even a one-term course, in my own subject, particle physics. [Laughter]

COHEN: OK. Then let's come back to your research. You were already well enmeshed in your subject before you—

PECK: Yes, I took my PhD finally in 1964, as I mentioned, after eight years as a student here. But I managed—with a lot of help from other people—my goal, which was to get enough

intensity and energy in order to measure the photoproduction of the K-lambda system. And that was my PhD.

As I said, nothing spectacular showed up [in K-lambda photoproduction; in content to pi-p photoproduction. The reason was unclear, but the quark model that Murray Gell-Mann invented in the last years while I was a student gave some insight into the reason behind the qualitative difference between the two channels].

I guess it was during my postdoc here, and perhaps the beginning of when I was an assistant professor, that I designed the 8-GeV spectrometer for use in what's called End Station B at SLAC. There were two big spectrometers, analogous to the kind of detectors we had at the synchrotron here, except they were designed for a much higher energy. But I only designed it; I was not involved in the building of it—that was done by SLAC.

There was a hiatus between the time I finished that work and before the spectrometer would be available to do physics with. So during the hiatus, I got involved with Ricardo Gomez and others—I can't remember exactly whom now, but including Frank Sciulli—in an experiment to be done at the Bevatron at UC Berkeley, on the hill at the Lawrence Radiation Laboratory. The Lawrence Berkeley Laboratory is what it's called now. The experiment was to make a beam of particles called rho-zeroes—as many as we could. According to Gell-Mann's ideas about its quark structure—and the fact that this particle has the same quantum numbers, except for mass, as the photon—the rho-zero can turn into a photon virtually, which can then turn into an electron-positron pair. And by measuring that, you would measure an important attribute, a calculable attribute, of the fundamental interactions of the two quarks. So it was an important experiment.

The trouble with that was that people were beginning to think at the same time about building electron-positron storage rings. In fact, I remember vividly the talk given by Gerry [Gerard] O'Neill here, on the idea of making a pair of beams counter-rotating in a common vacuum chamber and colliding in two places. It seemed outrageous, but of course that's the way everybody makes their living now. [Laughter]

[It turns out that a superior way to learn the basic physics that our experiment was designed for is to collide strong—but doable—beams of electrons and positrons in O'Neill's machine. But of course there were no such machines at the time, so we forged ahead with our experiment.]

COHEN: Now, could you do all that work down here?

PECK: Yes, that was all done here. We had a collaborator at Berkeley—Willy [William A.] Wenzel was the fellow up there who was our principal contact at Berkeley. We needed that, obviously. We were going to use a great big dipole magnet. I designed a Cerenkov counter to put between the magnet's pole pieces to see the electrons going out, and so forth. And finally came the day; we packed up all of our stuff into some moving vans and drove them up the highway. I remember walking down the streets of Berkeley—I'd never spent much time at Berkeley—and there were these ads for some actor who wanted to be the governor. [Laughter] Ronald Reagan. I couldn't believe it! [Laughter]

But our stuff eventually arrived, and we started unpacking it and started to mount it. And then all of a sudden, the news came that within a couple days of our arriving, a man had been doing a manual check on the armature of the generator that created the current for driving the Bevatron. And as he worked around, he suddenly felt a crack that was big enough for his finger to go into. So it seemed that if the machine were run for any length of time, it could explode—because there was this enormous crack in it. It was clear that the Bevatron was going to be down for—you know, nobody could really guess how long, at that stage. We were just ready to go, just ready to start installing all the apparatus, and suddenly the underlying object that brought us there, the Bevatron, was inoperable and would be for many months. So, with great sadness, we put everything back into our moving vans and came back to Pasadena, feeling pretty bad. Not only bad about the fact that it was a big letdown, but also because, gosh, we're going to lose the race to these storage-ring guys. The Italians were building storage rings. At Stanford, they were busy trying to build small storage rings. That was looking more and more likely as a competitor.

Thinking about what we had, and what the apparatus was like, what other physics might we do? Maybe we ought to do something else. I think it was Frank Sciulli who suggested that another thing we could do was to study another important rule—it's an empirical rule known as the "delta S–delta Q" rule, which says that in the decay of a hadron that has strangeness and charge, the strangeness will change in the interaction of the hadrons and that it will have the same sign as the way the charge changes. So $\Delta S = \Delta Q$ is favored, and from the modern point of view, with quarks it's very clear. If you draw the little quark diagrams, you can see that, yes, that's what will usually happen. If ΔS is equal to $-\Delta Q$, the reaction *can* happen, but it has to

happen by much more complicated diagrams. If so, it will be suppressed; it won't happen very often.

So we realized that we could do this experiment instead, with almost no changes. We could simply change the beam we were going to be using.

COHEN: And you did that down here?

PECK: No, no. This was using the same apparatus—the Bevatron, and the same magnet, and everything.

COHEN: But you still had to wait until it was fixed.

PECK: We still had to wait until it was fixed, but at least this was an experiment where there was no prospect that anybody would wipe us out immediately. And it was good physics. In fact, we improved the measurement of that amplitude, of $\Delta S = -\Delta Q$, by an order of magnitude, compared to what was known at the time. And it's even yet known only ten times better than we got it in 1966.

So that was great fun. Working at the Bevatron was quite a different environment than working at the synchrotron. It had hadrons instead of photons as its principal beam, so it was a very different setting.

Then follows the issue of data reduction and turning all of these measurements into physics. By then we were actually putting data on magnetic tapes. That was not something that happened when I was a graduate student; everything was written down in notebooks, and at the end of each run you'd write down fifty numbers. But now all the data was put on magnetic tapes, and we had the exciting business of how to use computers in order to turn that data into physics. But it finally happened, and we got a nice paper out of that. [F. J. Sciulli, J. D. Gullivan, D. M. Binnie, R. Gomez, M. L. Mallary, C. W. Peck, B. A. Sherwood, A. V. Tollestrup, and J. D. van Putten, "Measurement of the $\Delta S = -\Delta Q$ Amplitude from K_{e3}^0 Decay," *Phys. Rev. Lett.*, 25(17):1214-1218 (1970).]

I think the next thing was that I got interested in doing an experiment at Brookhaven [National Laboratory]. At the time, Brookhaven didn't have any beam lines that would have been suitable for doing what it was I wanted to do there. But at any rate I spent a summer there,

on Long Island, preparing for an experiment that never happened. I was preparing beams for it and designing the arrangements. At the time, Alvin Tollestrup and Jerry Pine and Barry Barish [Linde Professor of Physics] were still working on an important experiment there, having to do with another thing that now can be done in an electron-storage ring. In those days, you started with hadrons and then looked at the electrons; now you make electrons, make them collide, and then see what hadrons come out and what their characteristics are. They were doing an experiment that had, as its output, electrons and positrons also. So there were nice friends there; there were a whole bunch of us there.

But, as I say, it became clear that my experiment wasn't feasible. I can't remember precisely what the experiment was—it wasn't very important, I guess.

At the time, bubble chambers were the richest source of new physics. We decided that bubble chambers were an interesting approach. And not only interesting—they were the principal way physics was being done at that time. So we decided we'd like to get into the bubble-chamber business. "We" being, once again, Jerry Pine, Alvin Tollestrup, Ricardo Gomez, me, and then some people who are no longer here—[Alexander] Firestone, [J. D.] van Putten, [Alex R.] Dzierba, Pier Oddone, and I've probably forgotten some people. But we formed quite a large group here to build a semiautomatic bubble-chamber film-reading machine. The standard [bubble-chamber track measuring method at the time] was that you would shine an image of the bubble-chamber picture on a big table, and then a scanner—usually a woman, because only women would have the patience to do this [laughter]—would study the picture carefully and make measurements with rulers and so forth of the tracks, to measure the various points on important tracks where you'd seen interactions take place. A nicer way to do it would be to put the image of the bubble-chamber picture on a cathode-ray screen—make a television image of it—and then be able to insert a marker, which you could move about. So the scanners would see a picture of an event on a screen in front of them, and then we could, by moving the electron beam in a TV-like tube, make a little image that could move along the tracks [to points of interest whose locations could be measured and stored in a computer.]

So it was exciting, technologically, to build all this stuff. It held the possibility of doing interesting physics along with the bubble chamber. At that time, I was convinced by my good friend George Zweig—who, with Murray [Gell-Mann], was the inventor of quarks; he called them "aces"; George and I were graduate students together—George convinced me that the place

where the new physics was—it turns out he was wrong—was that one should be studying very carefully what's called “pomeron exchange,” or diffractive dissociation. Those were the buzz words that were used. He convinced me that we ought to do an experiment in which we really cleaned up diffractive-dissociation reactions. And the way to do that, I decided, was to use a bubble chamber—but not just to take a picture every time you expanded the bubble chamber, which is the usual way of using a bubble chamber: Shoot a beam in; expand the bubble chamber in; and as bubbles begin to form at the proper moment, you take a photograph of whatever happens to be there. It seemed to me it made a lot more sense, if you're looking for a particular kind of reaction, to put the electronic apparatus before and after the bubble chamber, allowing you to see if something interesting had happened at that trigger. If nothing happened, don't take a picture. If something did happen, you calculate as fast as you can to be sure—in the 30 milliseconds before you have to flash the light—you've got an answer; you say “go” or “no go.”

So we had what we called a triggered bubble-chamber experiment. It worked very nicely. The only trouble was that the underlying physics turned out not to be of great interest.

COHEN: Now, did you have students working on this?

PECK: Oh, yes, there were students. We collaborated with people, in this case from Berkeley—from the bubble-chamber side of Berkeley. Not Willy Wenzel but a fellow named Bob [Robert P.] Ely. We took a large number of pictures; I forget how many. We then started analyzing them, both here at Caltech on this machine, which we called “Polly,” and also at Berkeley—they provided the measuring facilities—and also at SLAC. So three places were involved in the analysis of these pictures, which were enriched relative to a normal set of bubble-chamber pictures, because by using the trigger we took pictures only of interesting things. So we got a very nice sample of data. As I say, the only trouble was that the science wasn't very interesting. [Laughter]

COHEN: What year would this have been?

PECK: Oh, gosh, this must be in the late sixties, early seventies—let's say '69 to '72. I remember we were analyzing data in 1974. I would spend all night on the PDP-10 in Booth Computing Center. There was a PDP-10 in there, and we had it connected. In those days that

was something you didn't normally do—have computers connected together. We had run wires from the Booth Computing Center over to the room in Lauritsen [Charles C. Lauritsen Laboratory of High Energy Physics] where we were doing our measuring. So we had computers talking to each other. We would take the data that was recorded on the little computer on the Polly apparatus and ship it over to the PDP-10, where it was then analyzed. It was a lot of fun and technologically very sweet. Nowadays, of course, it's all old-hat. We were doing the kind of thing that would not faze any fifteen-year-old now. [Laughter]

COHEN: This was new!

PECK: It was all new; all of this was new. Nobody had really connected machines. I shouldn't say "nobody," but we were among the first to connect machines and computers together in order to make all the measurements. Our goal was to automate the process as much as possible, get people out of the loop as much as possible, because people slow everything down and make mistakes.

COHEN: Machines are obviously willing to work all night—there went that problem!

PECK: [Laughter] Well, at any rate, I know this was still going on in 1974, because one morning the dawn was breaking and I was walking back from Booth, back over to Lauritsen. And one of my graduate students came outside and said, "Hey, did you hear about the fabulous discovery at SLAC?" I looked at him. He was easily excitable. But it turned out that, yes, there had been a fabulous discovery at SLAC that night. They had seen what we now know as the J/psi particle for the first time. By adjusting the energy of the electrons and positrons in a storage ring just right, the cross section for collisions suddenly went astronomical. It was very, very sharp. You had to get the energy just right in order for this to be observed.

I thought the student was probably overblowing something, so I wasn't impressed. Nonetheless, he told me I should call up my friends at SLAC, and so I did. I called up one of my friends at SLAC, and he said, "Oh, yes, it's a fabulous discovery!" [Laughter] It changed the whole of physics. We had suddenly found the next quark—the charmed quark. That's what was being seen, and it was exactly analogous to what I described earlier that we wanted to do with the rho meson—to make rhos, and then it decayed to $e^+ e^-$, and you could turn the reaction

around and make $e^+ e^-$ collisions and turn them into rhos, which decayed into hadrons. And what happened here was that you had $e^+ e^-$ at just the right energy, but in the case of going into a rho, you have to choose it at the energy of the rho. In this case, they had an energy just a little bit below 4 GeV, and that's where the resonance into a charmed-anticharmed meson occurred, so therefore there was a huge increase in cross section right at that spot. **[Tape ends]**

Begin Tape 2, Side 2

COHEN: Just let me ask one more question. It sounds like SLAC was in the forefront of all these experiments.

PECK: Oh, yes. I did my work at Caltech initially, and then at the Berkeley Bevatron and then at SLAC. I never did anything at Brookhaven. Then I did an experiment at Fermilab, which had just come on; we did a bubble-chamber experiment there. And then back to SLAC for the crystal-ball experiment. So most of my career has actually been at SLAC.

COHEN: OK, so we'll talk about the crystal ball another time.

[Tape recorder turned off]

CHARLES W. PECK**SESSION 3****October 15, 2003****Begin Tape 3, Side 1**

COHEN: We were going to start at the point at which you were getting involved with this experiment that got to be called the crystal ball.

PECK: Yes. The bubble-chamber work was continuing, but of course I realized I'd have to be worrying about what was coming next. And the interesting thing that was going on—really interesting to me and to lots of other people, was the $e^+ e^-$ collisions going on in the SPEAR [Stanford Positron Electron Accelerating Ring] storage ring at SLAC. An interesting thing that had been observed in the detector there was that as the energy increased, the fraction of the energy that was measured going into neutral particles was increasing. This is just a little bit of background, but it was particularly fascinating. It can't increase indefinitely, but there's more and more energy going to neutral things, which presumably are gamma rays—though they could be something else, such as neutrinos and other things. So that was intriguing, and I don't remember how it happened, but maybe Elliott Bloom from SLAC decided to come down here—or maybe I invited him, I don't remember. But at any rate, he came down to Caltech.

COHEN: This was in the early eighties, is this correct?

PECK: No, this was probably in '73. The November Revolution, as we called it—the discovery of the J/ψ particle—was in 1974. Well, this is before the discovery of the ψ particle, as it was called at SLAC, or the J particle, as it was referred to by [Samuel C. C.] Ting at Brookhaven. Sam Ting had made the discovery earlier, but nonetheless they announced it simultaneously. This was before that.

There was something peculiar going on in the measurements that had been made, so Elliott and I got to talking about it, and we said, "Let's make a collaboration between us and a few other people in SLAC to address this problem and see if we can make a collaboration to use the other interaction region at SPEAR." There were two interaction regions, and only one was in

use at that time. We started designing things and thinking about what to do, and eventually we ended up with a rather elaborate, complicated, expensive piece of equipment, but it was particularly designed for the measurement of high-energy gamma rays.

COHEN: Now, was this being built at Stanford or here?

PECK: Well, at that point it was all just talk. We were in the process of trying to figure out what we'd like to build, and there were a number of other people at the same time who were deciding they would like to build some new thing that would require a significant laboratory located at the available interaction region. We were talking about multimillion-dollar stuff that would require significant laboratory commitment and commitment from DOE [Department of Energy], and so on, and our competitors were wanting to do other things. One was called, I think, mini-mag, and a variety of other names.

The method for selecting a new experiment chosen by, I guess, Pief Panofsky, who must have been director of SLAC then, was to have a "shootout," as it was referred to. Shootouts had become the approved method of deciding on competing experiments; that is, each of the proponents would stand up and make a presentation to the assembled throngs, and in particular the program advisory committee. It wouldn't necessarily come to an immediate conclusion, but that was the method—to sell the experiment, to get a recommendation from the program advisory committee as to what should be done.

So there was this shootout at SLAC. I guess Elliott gave the presentation, which by this time had been dubbed "the crystal palace," because it was really big and it was really expensive and it had a lot of different sodium-iodide crystals in it.

COHEN: The design is now what we're talking about?

PECK: Everything is design; nothing is approved; we were trying to get approval. So we made the presentation. I can't remember exactly, but I think what happened was that the committee gave us all some advice and told us to go back and think about it some more and come back and talk to them again in a couple or three months or so. Part of the advice, although I don't remember the details, was that our device was rather complicated and they weren't sure it was going to work, and we shouldn't be too sure it was going to work. [Laughter]

Well, during that time, Elliott and I got together every week here at Caltech. It was easier for him to come down here than for me to go up there, because I was teaching. So he would come down here and we'd meet for a day, typically, and discuss various things and argue about things and what's been done, et cetera, et cetera. I'm pretty sure Elliott said that one of his colleagues—a fellow named Fatin Bulos, a Palestinian—had proposed putting a sphere of detectors around the interaction region. So we talked about that a little bit, and I said, “But sodium iodide, it has to be made up of separated parts.” And there are only five platonic solids, solids in which you have each of the faces the same. A tetrahedron has them; an icosahedron is the one with the most—twenty faces, or sides. “Hey, what if we were to build an icosahedron around the interaction point!” [The trouble with this idea is that the angular resolution of the detector would be only $1/20^{\text{th}}$ of a sphere, and that is just too crude. But that was easy to fix: Just break each of the twenty triangles of the resolution into nine equal triangles so we would get an angular resolution of $1/180^{\text{th}}$ of a sphere.]

So we promptly went downstairs and got a big sheet of poster board and—because the faces on an icosahedron are triangular—drew a big triangle on it. And promptly forgetting the theorem that there are only five solids with identical faces, we immediately broke the two-dimensional triangle up into nine identical pieces—yes, nine equal triangles. On the plane, they're equal, but of course in three dimensions they will have different shapes. We didn't fully realize that at the time, but that's OK, they'd be close. [Laughter] We forgot the theorem very quickly. We then built a full-scale model with poster board. We made twenty of these triangles, cut them up, glued them together with Scotch tape and I guess staples. It was gorgeous. So we set roughly the dimensions of what became the crystal ball. The idea was to fill this whole shell with sodium iodide, the shell being made out of about 180 pieces.

COHEN: Was this granulated sodium iodide—powder?

PECK: No, about 180 prismatic crystals. You can make big crystals out of sodium iodide. There were six different prism shapes to fill each triangular portion of an icosahedron. But the six shapes are not dramatically different and they are all calculable, such that the whole thing would fit together into a contiguous piece of material. Each of the crystals would be optically isolated from its neighbors. And all of these ideas came out in that discussion. You'd have a photo tube

stuck on the end and then the center would be the interaction point. So now it's a crystal ball—no longer a palace, but a ball—and “crystal” because it was made out of crystalline sodium iodide.

So Elliott went off; he had to run to catch his plane. We folded up the model we had made. He went off carrying this huge piece of poster board—big because we made it full size, about the size we ended up building it. I don't know how they let him on the airplane; they wouldn't nowadays. [Laughter] I don't know how he got it on, but he managed to get it up to SLAC. So by the next week, a design was pretty well along. He got a number of people there. I remember distinctly going home that night and thinking hard about the geometry of an icosahedron and what it looks like if you try to break it up. So I immediately was making drawings and figuring out what to do, and my notebooks are just filled with details, pictures, and how it might look, and all the various characteristics: how long it would be, how many hadronic interaction lengths, how many radiation lengths for the photons, how it would respond to hadronic particles. We had a real flurry of invention in that short period of time, and it went from something that was not very appealing—this crystal-palace notion—to something that was simple, elegant, appealing, and probably would work with no trouble. Of course, when we finally presented it at the next version of the shootout, some of the members of the committee were worried that we wouldn't be able to build it. How would we make it all fit together? Anyway, they approved it [in the spring of 1975], so we then had sufficient resources available to build and operate it. And several other people joined us to form a viable collaboration.

COHEN: Whom did you get the money from?

PECK: DOE. I can't remember all the proposals we had to make, but the critical thing was getting the approval of SLAC management—that is, Pief Panofsky and the SLAC research director. They, of course, had gotten advice from a scientific policy committee, which all laboratories have and which always give the director advice. Of course, the director can do as he pleases, but he normally follows the advice he's given. And of course, the proponents are given a list of questions and things they ought to be sure to look at, and the views of some of the committee on whether or not the proposal is feasible. As I say, some guys thought we couldn't

build it, that it was too complicated. But there were only, as I said, six different shapes that had to be worked out, and they're just triangular prisms.

COHEN: You can't change the shape of a crystal!

PECK: That's right. [On a microscopic scale, sodium iodide falls into a specific geometric shape. Microscopic pieces, like rocks, however, can be any shape and can be machined appropriately. For the crystal ball, each of the prisms had one of six geometric shapes. The prisms had to be machined with sufficient precision so they would all fit together contiguously.]

So the committee was worried about that, of course. Anyway, they finally approved us. So we divided up the various jobs. I should say that the other major person in the collaboration by this time—I don't remember when he came on—was Bob Hofstadter, from Stanford. So there were essentially three groups. There was Bob Hofstadter and his group from Stanford, from the Mark III Linac [linear accelerator], which was on the Stanford campus. Then Elliott's group—Elliott at that point had just recently gotten a group at SLAC.

COHEN: Now, were these people also professors at Stanford?

PECK: Well, no. They're professors at SLAC. Stanford was arranged in a way that I cannot reproduce—I've never understood it. But there's a Physics Department; there's an Applied Physics Department; there's SLAC as a department. It's a whole collection of boxes, many of which can do very similar things but are treated as independent departments. So, for example, the Physics Department I think does all the teaching, and people from SLAC have no teaching responsibilities, except that if they'd like to, they can volunteer to teach in the Physics Department for a year or a month or whatever it is. SLAC is an independent department like PMA, for example, at Caltech. Perhaps a better analogy might be like Owens Valley [Radio Observatory].

COHEN: OK. But are they called professors?

PECK: Yes. That, of course is different from here. It's quite different and, as I say, I don't really understand it. [Laughter] It's a mystery to me—always has been. Panofsky was a professor at

Stanford but not a professor in the Physics Department; he was a professor at SLAC. And Elliott had been promoted to professor, or associate professor—anyway, he had gotten tenure not too long before the crystal-ball proposal was made. But he did have by this time a group, one of whom, Gary Godfrey, a Caltech alumnus—as was Elliott—was the student who told me that he had heard me lecture when I was a postdoc, which I told you about last time. And he told me, “I didn’t understand a word of it; none of us understood a word.” [Laughter]

COHEN: Nice to know.

PECK: Yes, right. [Laughter] But he was a great guy, and still is. He’s still working at SLAC with Elliott.

At any rate, the three groups then took on their own particular aspect of the building of the experiment. In particle physics, that’s the custom—that’s the way the sociology has developed. We were always building new instruments. We’d build them ourselves. Except for accelerator centers, we had relatively few engineers—almost zero—one here at Caltech at that time. Now we have zero; we’ve had zero for years. But we had an engineer at that time; he was very useful. There was an engineer at Hansen [Experimental Physics] Laboratory, which is the Mark III’s formal name. And then SLAC, of course, had lots of engineers. So SLAC took on all of the mechanical things—the building, the beams. The sodium iodide needs to be kept in a very dry climate, because it’s hygroscopic—you let it sit out in the air and it melts into a puddle of water. And here we’re talking about a multimillion-dollar piece of sodium iodide. So they took care of all of the engineering related to those things.

I took responsibility for taking care of all the photomultiplier tubes, all the light detectors and their calibration, and the artificial light sources you need in order to get signals at specified times that you can use to test all the electronics. The electronics was done by Elliott’s group—in fact, by Gary Godfrey. And the building of the sodium-iodide sphere was pretty much in the hands of Hofstadter and SLAC together. But of course the first job was to find somebody who could produce and machine suitable prisms of sodium iodide for us, and Harshaw Chemical Company could do so. I guess there were two places that could have done it, but Harshaw was the classical source.

Hofstadter was the person who discovered the scintillation effect in sodium iodide, the first scintillator that was known for particle physics purposes, many years ago. He always told the story that he was so embarrassed—he was a young assistant professor, and he wrote a paper about his discovery, and *Phys. Rev.* published the oscilloscope picture upside down. [Laughter] So the text didn't make sense, if you didn't know the picture was upside down. Of course, he was a Nobel laureate by the time he was working on the crystal ball, and so the misprint was no worry to him anymore. [Laughter]

It took us three years to build the crystal ball. Oh, I forgot an important thing. The motivation for starting all this was that there was an increase in the average energy of the neutrals as the event energy increased—the so-called “neutrals crisis.” That isn't quite right, but it had a name and everybody knew about it. So we wanted to work on understanding it. Then, on November 11th, I think, a very special day, the day I told you about when my student came running up to me when I was leaving the Computing Center at a little after dawn, saying they had just made a wonderful discovery at SLAC, and I thought he was nuts because you don't make a discovery in particle physics in one night. It's always the result of a lot of hard work that comes out gradually, and you then see something. But this *was* a one-night discovery. As soon as they got the energy right, the interaction rate just zoomed up. Move the energy a little bit, *bam!*, it fell down again. Spectacular, dramatic! And that completely changed our whole outlook.

It made us very excited, because if there's a new particle—there were lots of ideas about what it might be, but one possibility would be that it would be a particle that would produce high-energy photons, and we would then have a very interesting signal to measure. And indeed, that's what happened. The psi particle is the charm quark and antiquark bound together. And if you make the energies a little higher, you make an excited version of it, like the excited version of neon in a neon sign, which then de-excites by emitting photons—red light in the case of neon, but in the case of the psi-prime, as it's called, photons that are appropriately detected by sodium iodide. And the crystal-ball geometry has very high efficiency to catch them; they give lots of signals.

We didn't know about that possibility yet, but we knew that this was the right device to study new physics. It was what we wanted. And, of course, I presume the SLAC director's advisory committee felt similarly. And that's probably part of the reason we got approved.

And then we started running the crystal ball, and it just worked like a charm. It was beautiful. So we ran at SPEAR for several years collecting data. Some of it is in the paper [Elliott D. Bloom and Charles W. Peck, "Physics with the Crystal Ball Detector," *Ann. Rev. Nucl. Part. Sci.*, 33:143-197 (1983)]. In fact, at that time, I was teaching freshman physics with President Murph [Marvin L.] Goldberger [Caltech president 1978-1987]. Murph was scheduled to give the lecture, but it turns out that Murph wasn't always there. [Laughter] Who do you suppose gave the lectures on those days? [Laughter] That's how it goes when your teaching mate is the president of the university.

COHEN: He did that only for one quarter.

PECK: That's right, he did it for one quarter, and it just was not feasible. He gave excellent lectures; he was really wonderful, but then there were some things he just didn't have time to do—preparations for demonstrations in particular—so I got the job. At any rate, it was during that time. During that period, it turns out that other discoveries had been made, similar in qualitative behavior to the charmonium. The energy was about 10 GeV rather than 4 GeV. It wasn't possible to make these particles at the energies available at SPEAR. Part of the reason for building another collider at Stanford was to investigate higher energies, because there might be more such things. As it turned out, however, the Germans did have a machine with the right energy for us, and had a place for us to put our apparatus and make studies—analogueous to what we had done on the charm system—on what we called the beauty system, although it's also called the bottom, the B quark.

The quarks come in several pairs: The charm quark and the strange quark go together, and then the B quark was discovered, and then it took a long time, in fact until about 1994 or '95, to find the top quark—or the truth quark, as it's sometimes called. People had given names to them already, prior to the discoveries. At any rate, there was one place where we could study the same things in the beauty system as we had done in the charm system.

COHEN: Did Murray Gell-Mann participate in any of these discussions? Because, you know, he gave the names to these things.

PECK: No, no, he was off doing other things. This was all ancient history to him. [Laughter] He was aware of what we were doing, of course, but he was not a part of it. Neither was Feynman, as a matter of fact. Well, Feynman was, of course, intimately involved in the first discovery. When the charmonium system was first discovered, he immediately sat down and was analyzing various possibilities. And I vividly remember his lectures. He gave us a series of lectures as to what this new thing at SLAC could be. Because there was another experiment, the Mark I, that had discovered the psi particle—charm/anticharm—and was measuring various things. Part of what we measured at SLAC when the crystal first turned on had already been measured by Mark I. The beauty of the crystal ball was that we measured photons with much higher energy resolution, and with many more events.

Arrangements were made in the early 1980s, mainly by the SLAC people, to transport the crystal ball, in its dry room, into a big military aircraft and fly it to DESY [Deutsches Elektronen Synchrotron], in Germany. It was all very exciting for all of us, of course. We had to take the crystal ball all apart.

COHEN: Why did they want to do that?

PECK: Well, the crystal ball is a unique piece of apparatus. You don't go buy one of them off the shelf.

COHEN: I see. So the Germans wanted to do this experiment in their accelerator?

PECK: That's right. They wanted to work with us to make the same sort of studies we had made at the energies of charmonium, at the energies of the beauty quarks. And we expected similar phenomena to what we saw at SLAC but at energy about two-and-a-half times higher. There could be new things going on—and there are, in fact, different things. But it's important physics to measure what these are, to see what it all looks like, with a new kind of quark, and see if it looks the same as the charmed quark. And the only place to do that was in a place that has the energy, and at that time there was only one place—the Doris II Colliding Ring at DESY, in Hamburg.

COHEN: So this is pre-CERN days?

PECK: Oh, no. CERN [European Organization for Nuclear Research] had been in existence for years. CERN did not have e^+e^- colliding storage rings. In fact, CERN has always been a place where protons, and antiprotons later, are accelerated, but they had no electron machines. The electron machines were at SLAC and at the Hamburg laboratory—DESY, the German electron synchrotron. They had built there a quite different arrangement for making a storage ring, and it was called Doris II. It has two rings in it; it was a double-ring storage ring. But as far as the physicists doing experiments at collision points were concerned, most of those details were irrelevant. We just needed the energy and the intensity. There had been an experiment done there sensitive to gamma rays, as was the crystal ball; in fact, we collaborated with the group who built it. That group recognized that the crystal ball was far superior for this application than what they had built. It's a matter of how much money and time and effort and so forth you put into it. And what they had built—the name of which I can't remember right now—then went out of business when the crystal ball showed up.

COHEN: Now, give me an idea of the size of the crystal ball.

PECK: Well, the outside diameter was about a little over a meter.

COHEN: So about four feet?

PECK: About four feet, yes. The crystal prisms were tapered from around four inches on the side at their big end to about an inch by an inch on the side on the small end with a triangular cross section. Each one was its own triangle. Then these were all individually wrapped and then stacked appropriately in order that a group of nine made up one triangle of an icosahedron. And then the overall geometry was an icosahedron, like the fountain out front here at the Beckman Institute.

COHEN: I see. That must have been a challenge, keeping that dry.

PECK: Well, the way it was kept dry was that all of the material is inside an aluminum shell, and it was all very carefully kept dry on the inside. The aluminum shell was slightly pressurized; we had verified that it was airtight by pulling a vacuum on it. Then we must have kept it under a

slight pressure, is my guess—I can't remember the details. Then we kept that inside a room that was humidity-controlled. We kept it in an environment that had almost no water in the air; in this room, the dew point was around minus-thirty degrees or something like that. So we had a rather elaborate dehumidifying system to keep that room dry, and when it flew over to Germany it was kept in the dry room all the time. I don't know if the dehumidifier was running or not, but the room was kept dry and then sealed.

COHEN: Now, were you on this trip?

PECK: No, I was not. I wasn't there to meet it, but the stories were told to me many times.

COHEN: So you feel like you were there.

PECK: That's right. I guess I must have been teaching. I remember that the first person to go over there was a graduate student of mine, Chad Edwards, who went to assist in receiving it and to start to put it together again. But then I began to spend every summer in Hamburg. I arranged things in such a way that I was never there during the miserable Hamburg winter. [Laughter]

COHEN: Well, even the summer can be cold and wet.

PECK: Well, yes. At least for one of these visits it was referred to by the Germans as the hundred-year summer. It was a wonderful, wonderful warm and pleasant summer. The first year I was there, it was really wonderful. So I spent, then, the summers—typically three months—in Hamburg. I looked forward to going and always enjoyed it enormously, and the physics went very nicely. The only problem was that you had to be sure to use the right trashcan. If you put the trash in the wrong place, or you moved the trashcan, or you did anything sensible, the German group's technician would jump all over you for being a klutz. [Laughter] He was wonderful! Oh, well. There were lots of culture things. Whenever you do any experiment in a different country, it's their country, so they of course set all of that. And this was the guy who was in charge of their version of how you take care of trash, which was somewhat different from ours. Not that we were throwing stuff all over the floor. [Laughter]

Well, the experiment continued in Germany for three to four years. And once again, as it became clear that it was coming to the end, I began looking around for the next thing new to do. And at this time Barry Barish had gotten interested in magnetic monopoles—in the possibility that magnetic monopoles might be raining down on the earth. From theoretical ideas that were current then, and still are, the magnetic monopoles were expected to be very heavy, of a macroscopic mass, and consequently they should be moving in a way comparable to other macroscopic chunks of matter in the universe, like stars or galaxies. They would have been produced in the Big Bang, and there might be a remnant of them still around, and they might be falling on the earth at a slow rate. And indeed, an observation had been made at Stanford by Blas Cabrera. It had a very well-defined date—it was on Valentine's Day, 1982. A signal was observed: just a little loop of superconducting wire about six inches in diameter, a current was induced in it, just what would happen if a monopole went through. The signal had just the right characteristics for a monopole having passed through the loop of wire. Cabrera's experiment had been running for three or four months or so—and something went through his loop of wire, six inches in diameter or something of that sort, so they must be falling all over the place. I mean, that's a huge number.

On the other hand, there was a theoretical result from the astrophysicist Eugene Parker. Gene Parker had calculated how many monopoles there could be in the universe, consistent with the observed magnetic fields and the observed rate of change of magnetic fields in galaxies. [If nature contained more than a certain number of magnetic monopoles per unit volume, then the monopoles would take energy out of the universe faster than it could be replenished by the galaxies.] And so it's possible, then, to set a limit on how many monopoles there would be. This limit was many orders of magnitude below the Cabrera result. So, an interesting puzzle. Of course, it turns out that nobody now believes the Cabrera result was due to magnetic monopoles; it is thought to have been caused by something else. There are various things that might have caused it. Other people have made the same kind of searches and found nothing. And the Parker bound—as it's referred to—for magnetic monopoles corresponds to, the way I remember it, a few per acre per year. It's just a few per 1,000 square meters in a year.

Barry had attempted initially to measure these monopoles by listening for a *ping* as a magnetic monopole went through a plate of aluminum. We have very sensitive audio detectors, and you might hear monopoles passing through a conductor. He and two students had worked

on these various ideas. But about a year before I decided to join the experiment, Barry and a friend of his—Enzo Iarocci, who is an Italian physicist—had made a proposal to a new laboratory being built in Italy under Gran Sasso Mountain. “Gran Sasso” means “big rock,” so it’s under the big rock, where the Italians had drilled two tunnels for roadway traffic. A prominent Italian physicist named Antonio Zichichi had convinced the Italian government to build a science laboratory there—unique in the world.

COHEN: Now, what year is this?

PECK: We’re in ’84 or ’85—the last time I went to DESY for the summer, making runs and writing papers. At any rate, on the last trip there, I went via Rome and was taken out to the Gran Sasso site. The laboratory was still being built. There was ice-cold water running on the floors, and raw rock everywhere, drips all over the place, very cold. [Laughter] But at any rate, it looked pretty interesting, and as I thought more about it, the science sounded fabulous. So I offered to Barry—or maybe he had asked me, I don’t remember. At any rate, we got together. He had done all the hard work of getting the experiment approved, but then there was a lot of work to be done to build it. So starting around ’85, maybe ’86, I then pretty much spent all of my time building the MACRO experiment, as it had been dubbed—for Monopoles and Astrophysics Cosmic Ray Observatory. It’s a peculiar observatory from a classical astronomer’s point of view, since we have a mile of rock over our heads. [Laughter]

COHEN: Now, this isn’t all that close to Rome?

PECK: About 120 kilometers away. You fly into Rome and get in a car and then drive like an Italian and you get up there in half an hour. [Laughter] A little more than that—it takes about an hour to drive. So then I started spending my summers in L’Aquila, which is the name of the town nearest to the Gran Sasso laboratory.

COHEN: I see. And they didn’t bother you about your trash there?

PECK: [Laughter] No, they didn’t bother us about trash there. It’s night and day to go from Germany to Rome, as you probably know. It’s amazingly different. [Tape ends]

Begin Tape 3, Side 2

PECK: [To build an experiment, many questions must be worked out.] How much money is there? Do we get a whole experimental hall or do we get only half the hall? How can we maximize our signal? So this went on for probably on the order of a year. You know, the whole thing needs definition. Usually you make proposals and they're relatively short, and typically there are only a few people on it. But once it gets approved, then the ideas have to really be fleshed out, the central physics of what you're going to do. All the technical details need to be defined in detail. In this case it was a collaboration between the United States and Italy. There were Italian groups from all over Italy, from Torino in the north, Bologna, Rome—I can't remember how far we went into the south. The curious thing about the Italian system is that once you get to be a professor, then you become a professor at some place, which may not be where you live. Many professors live in Rome and then travel south—you know the farther south you go in Italy, the less desirable it is. Ask any Italian where the south begins, and he'll give you a point about 100 kilometers south of wherever he is. [Laughter]

At any rate, there's a whole different sociology associated with doing business in Italy. The experiment got defined over time. It was a collaboration between a group of universities in Italy and a similar group of universities in the United States—Caltech, Michigan, Boston University, Drexel. I can't remember all of them right now. The way that the MACRO experiment was designed, when we finally got it all together, was into twelve equal parts, referred to as supermodules. They aren't all identical: The six supermodules that are the lower ones in the apparatus were filled with rock, and the upper ones were filled with air. And in the upper ones, we had room to install all the electronics for analyzing and storing the huge amount of data that came from the apparatus. The apparatus, in length, was around 72 meters, in width 12, and in height 10. So it's a big thing. It's like a small castle. [Laughter] And this inside a cavern that had been prepared for it that was around 18 meters wide, at least 15 meters high, and around 100 meters long. The apparatus filled most of the cavern—there was a part of it that was unfilled, which was intended for installing another apparatus if a proposal came in.

The experiment consisted basically of two kinds of detectors: One was scintillators—that's what we built here at Caltech—that were mineral oil, like what you put on a baby's bottom, very fine mineral oil, wonderful mineral oil, in which we put a little contamination that made it flash whenever a charged particle went through it. And then there were what are called

streamer tubes, which have the property that whenever a particle goes through and ionizes the gas, it makes a local electrical signal that can then be read out from both sides. There were many layers of these. And the scintillators were 25 meters long. In general, the way the apparatus was split up into parts was typical of the way we do things in particle physics. We in the United States took care of all the scintillator systems. The Italians took care of all the streamer-tube systems. These detectors had been invented by Enzo Iarocci, so he was the world expert on them. They did all of the streamer tubes and all the mechanics; they were on the site, and they took care of all that. So the general method was, we here at Caltech built the containers that held the “contaminated” mineral oil, and these were 75 centimeters wide, 12 meters long, and 24 or 25 centimeters deep.

COHEN: Now, you had graduate students working on this?

PECK: Yes, Barry and I had graduate students. As is customary, whenever we’re building, we always have to keep costs as low as possible, so we make things as simple as possible. We hired unskilled labor, essentially, from the Pasadena labor market. That’s the method we have used—not only on this but on the experiment I’m currently working on, MINOS—for building large amounts of apparatus. So you make it simple and build it as cheaply as you can, and you still tend to run over budget. It seems, from the outside, that we’re well supported, but internally it doesn’t look like we’re so well supported. [Laughter]

So at any rate, we built the scintillators. When we had enough of the boxes put together, we put them in a cargo container, carried it off to a ship to then be shipped to ports in Italy, where they would initially go to the Frascati National Laboratory, near Rome. It’s the central high-energy physics laboratory in Italy, and that’s where we had a large workshop, where we started setting up the experiment before the tunnel was prepared.

COHEN: So you then spent all your summers in Italy?

PECK: Then I spent summers in Italy, which I continued to do until a couple of summers ago. It wasn’t bad. I mean, somebody has to do it. [Laughter]

COHEN: Was the experiment successful?

PECK: Oh, yes, this was a very successful experiment. Well—excuse me, it would have been more successful if we had found things. We set a huge number of limits on things that aren't there. We made one discovery—in fact, it was a discovery that was clear in the very first data we took. I mentioned that there were twelve supermodules. In the summer of 1989, we had only one supermodule, and data was being analyzed in that by a graduate student of mine, and there was an anomaly in it. And that anomaly is now well established. At the time, it was not well established and we did not have the right apparatus to really give it a definitive interpretation, but it was clear that we had an anomaly and that the number of mu mesons we observed coming out of the earth, going straight up, were too few. There were too few by about a factor of 2. And it's now known—this is now a well-established phenomenon—that it's due to the fact that neutrinos have a little bit of mass, and they can oscillate from one kind of neutrino to another. Nature makes neutrinos on the other side of the earth by cosmic radiation hitting the atmosphere. These neutrinos then transit the earth, and somewhere underneath our apparatus they collide with a nucleus. There are both electron- and muon-type neutrinos, but those that are the muon type collide with a nucleus and produce a muon, which then goes up, and it sticks out—it's going in the “wrong” direction—because essentially everything else is going down.

COHEN: So this was just a collateral discovery?

PECK: Well, it was in our proposal. It was one of the things we realized we could do with our apparatus. In fact, everything we had in the proposal, it turned out, we measured; we either set a limit on it or we measured it. There were lots of things that are bread-and-butter sorts of things. But one of the interesting things we wanted to find out was, can you see stars in neutrino “light”? We looked at the skies a lot—this is an observatory. So we looked at the skies in neutrino “light,” and we didn't find anything. There seemed to be no stars at our level of sensitivity, which is probably 2 orders of magnitude higher than anyone else had ever had at the time we were doing this. We couldn't see any stars, unfortunately. I would have loved to have found a star. [Laughter] Seeing stars from under the ground. The famous supernova, 1987A, was in '87, two years before we had an apparatus. And at that time our apparatus would have been too small to have gotten a signal. At best, we would have had one supermodule at that point. When

we were finished with the entire thing, however, we had 600 tons of scintillator oil, and that was sufficient for us to have seen a signal from 1987A if it had occurred then.

So a very important part of our experiment was to be sensitive all the time to the possibility of a burst of neutrinos coming from a supernova. We were sensitive, certainly, to everything in our own galaxy. If a supernova had gone off anywhere—whether or not it was seen by optical astronomers—it would have been visible in neutrinos and, given the standard-size supernova, we would have detected it. So we could set limits on the average number of supernovas in the galaxy over a certain period of time. Of course, there are various limits that have been set, but we have a hard experimental number. We didn't see anything in five years with a pretty big apparatus. [Laughter] Of course, many other people had looked, too.

COHEN: So you were satisfied with this experiment?

PECK: Oh, very satisfied. It would have been a lot more fun to discover a few more things. But setting limits and saying that it ain't there is important also.

COHEN: So then what decided you to turn it off? Why did the end come?

PECK: Well, the end came because it gets tiresome running a big apparatus like that. It really takes a lot of work. You've really got to be on top of everything all the time, keep it all calibrated, and take care of all the stuff that breaks. It just deteriorates if you leave it alone. Experiments don't run themselves—they require a lot of active participation. We had said at the beginning of the experiment that we would run for about five years. When five years was up, the director said, "Out, you guys! We want to put something else in there." [Laughter]

COHEN: So they didn't want you.

PECK: They didn't want us there anymore. Furthermore, I think there was some political pressure from others to use the space. Carlo Rubbia is the grand man in Italian science—he's a Nobel laureate.

COHEN: He's at MIT, isn't he?

PECK: Well, he had been, but he's no longer there now. His picture appears in ads; he's a very popular guy. And Carlo had proposed one of the experiments that was approved; it was approved before MACRO was done. It's called Icarus, flying close to the sun. It's still being worked on, and it's still not built. [Laughter] Nonetheless, it was decided that the hole we were in should be used for Icarus. But that was OK—we'd had five years. You know, you go from five years to ten years, and all you do is double the limit. [Laughter] If you make a measurement, you go from five years to ten years and you reduce the error bars by a factor of 1.4. So when you're setting a limit, doubling the run time decreases the limit by only a factor of 2—for twice as much work. That's a lot of work. So we decided it was time to stop.

COHEN: What year would that have been?

PECK: This is in December 2000.

COHEN: And that was the end of your Italian sojourn?

PECK: Well, I think I went one summer after that.

COHEN: By this time, you must have been division chair?

PECK: I was division chair from '93 to '98. The experiment was started, as I said, around '85 or '86. We got our first data in '89. We finished the apparatus—all twelve supermodules, all put together and running in 1994, and then we ran for about five years.

COHEN: So now you're on another experiment, which I don't even know about.

PECK: That's right. On the new experiment, I've not taken any major responsibility, because I expect to retire very soon and I don't want to be retiring while I have a major responsibility. I want to be able to just take off without telling anybody. [Laughter] At any rate, I'm on a new experiment that's called MINOS. It has the rather curious name of Main Injector Neutrino Oscillation Experiment—"main injector" because at Fermilab a new accelerator was built in order to inject protons and antiprotons into the Tevatron, and it's referred to as the main injector.

But it turns out that it has a lot more beam time available than is necessary just to keep a proton-antiproton beam going in the Tevatron. So there was, from the very beginning, a proposal to have a neutrino capability for the main injector, called NEUMI—Neutrinos of the Main Injector.

[Laughter]

[Tape recorder turned off]

CHARLES W. PECK**SESSION 4****October 30, 2003****Begin Tape 4, Side 1**

COHEN: We were going to talk about your life as an administrator.

PECK: Yes, well, I had been a professor here—as I said—starting in '65. And the notion of administration and any politics, any interactions, was all distant to me. I didn't know anything about it. I had no interest—certainly no knowledge—of any of that. My first experience with administration came when Ed [Edward C.] Stone [Morrisroe Professor of Physics and chairman of the Division of Physics, Mathematics, and Astronomy, 1983-1988] was looking for a new physics executive officer, in about 1983 or '84, I would guess. Geoff Fox had been EO prior to that but decided he didn't want to do it anymore, so Ed invited me to do the job. And, of course, being Ed, he was completely organized and he gave me a statement, a beautifully prepared memo describing exactly what the job would contain. So I thought about it for about a week and decided, Well, OK, I guess I should do this if I've been asked to—Caltech has given me a lot and I should give some back.

COHEN: This was in no way a full-time job of any kind?

PECK: No. The job of executive officer in physics—it's different in different specialties, but in physics the job is to take care of all the teaching and all the student-related things and to take care of the teaching aspect of the professors' lives. Whenever they complain about the fact that there's no chalk, well, then they complain to the EO and you make sure it's there—talk to the janitor and make sure he does the things that are necessary. And you take care of the instructional labs, everything having to do with instruction of students. And I think that's about it. I'd have to read the details from Ed's memo, but those are the things I remember that were the dominant part of the job.

So, in this position, for example, a computer lab had been established by Geoff Fox, who had managed to get a donation of a bunch of XTs. They were just coming out at that time. They

were the latest things on the market—they were wonderful. And we thought somebody ought to teach Caltech students about programming, so we started doing it in the physics department. I guess Geoff had been in charge of the new lab the year before, and in '83, as EO, I took the job of teaching that course. And as EO, I was in a position to do things, so I used some money to connect the machines all together, and at the time that was a very revolutionary thing to do. We hooked them all together and put them on a common server, so students could get programs and data from a common place rather than our giving them pieces of paper and having them type stuff in. I rather enjoyed the idea that I could actually do something useful. So I, in fact, wired all of Bridge [the Norman Bridge Laboratory of Physics], allowing the possibility of interactions between different parts of the so-called Physics 77 labs; more generally, the undergraduate teaching labs were all hooked into this network. They all had access to this common server. And that I rather liked.

But the job consisted, as I say, of making physics teaching assignments, working out what we were going to teach in a given year, who's going to do it; room assignments—if the professors had any particular teaching desire, I would try to arrange it. And I think it's fair to say that the EO job in physics has stayed pretty much that way over the years. It's different from the EO jobs in either astronomy or mathematics, where the executive officer does have other responsibilities than in physics. Physics is taken care of because, historically, the chairman of the PMA Division has been a physicist, so he has taken care of other aspects that are unique to the discipline.

One of the amazing things I discovered was the informality of Caltech at that time. For example, I decided, upon reading the PhD requirements for physics in the catalog, that the accretions of the ages were showing rather strongly. It just was a mess! People had added something here, taken out something there, added something else, so it was a mess. So I just decided to rewrite it. I rewrote it entirely, sent it to the people who publish the catalog—

COHEN: These were requirements for people wanting to come here for a PhD?

PECK: That's right. So I simply rewrote it. I didn't change the fundamental substance, but I simply rewrote the description. I sent it to the people who published the catalog, and there it was, in the catalog the next year. [Laughter] I could have said anything, as far as I know.

[Laughter] It didn't occur to me to ask anyone about it; I just did it because it seemed the right thing to do. The thing that amazed me was I didn't have to get approval from any faculty committees. I think now you probably have to go through the Faculty Board, and it has to be blessed by various people, and so forth, but at that time we still had a little bit of the informal Caltech left.

COHEN: Nobody complained about it?

PECK: Nobody complained. I didn't change anything of substance; I just rewrote the thing in English, that's all. [Laughter] So people could figure out what it said.

Another thing I did was completely revamp the way we were doing the qualifying written examinations for students. We had until then given each year a completely fresh, newly created examination—two of them—to all our graduate students for admission to candidacy. It was clear to me that the problems were getting harder and harder, because the same people were making them up, and of course they had to be cleverer each year than they had been the preceding year, so after a while the problem difficulty was clearly getting out of hand.

COHEN: Were fewer people passing?

PECK: Well, I don't know if that was happening or not. We probably adjusted the passing grades to accommodate this.

COHEN: So everybody could make it?

PECK: No, no. That was determined at the time of admission. We always assumed everybody who was admitted would make it—not all of them did, of course. I never kept track of the attrition rates, actually. But that wasn't one of the considerations. What counted was what we decided was the passing grade on that exam. If nobody solved a particular problem—or only one or two people got it—then it's probably because the problem was simply too hard, so we should probably not count it in the test, for example. But it seemed to me that a better scheme would be to simply give the same exam as last year, or to have a small set of exams and simply cycle them over the years. The worry was, of course, that the exam would become public knowledge and

then the next students would have the solutions. But the fact is, there's no motivation for that. Why should this year's group of graduate students have any interest at all in giving the exam—or notes on it—to people in the subsequent year? It might have happened on occasion—if a student had a buddy coming up, for example—but it's clear that it never was a problem. We've used this scheme now for—

COHEN: You mean, somebody can't go online and bring up the last five exams.

PECK: That's right. All the exams are very carefully handled so that the students have to return the exam—they only have it during the exam time. Of course, somebody could copy out whatever they want, but there's no motivation for that. So the fact is, although many people were worried that it might be a leaky system, it apparently has not been.

At any rate, that was a three-year appointment, at the end of which I decided, why should I want to do any more administrating?

COHEN: How much of your time did it actually take?

PECK: Well, it took enough that it bothered me. I couldn't just take off and go to SLAC or someplace. I had to make sure things were tidy before I left, that there were no outstanding problems. And then, you have to go around and talk to people and convince them to teach something they don't want to teach. I remember having a terrible time trying to convince Murray [Gell-Mann] that he ought to teach something. He hadn't taught anything for I don't know how long. I had had him as a student; Murray was a *great* teacher! And I tried to appeal to his ego, but of course, that's impossible. [Laughter] I remember vividly trying to get Murray to teach. I had been told by the chairman that everybody taught, and he didn't give me any exceptions to that. So therefore I was trying to find out what it was that Murray would teach. I told him what a great teacher he was; how much I had appreciated his teaching. He said, "No, I don't want to teach. When I came here, Bacher [the division chairman] made an agreement with me, which was that if I would come, I would teach only when I wanted to, and I would teach what I wanted to."

Well, OK, so I went back to my office and tried to get in touch with Bacher, but I couldn't. So I talked to Ed Stone. Well, it turns out that, indeed, Murray didn't have to teach; Ed relieved him of teaching. [Laughter]

COHEN: What about Feynman? Did he teach?

PECK: Oh, yes, Feynman taught. Feynman really wanted to teach and he enjoyed it. He did teaching all the time—thought it was his duty. He was paid entirely by Caltech, so in a sense it was just this moral sense, which would mean that he had to teach. Murray, of course, got a significant fraction of his salary from the high-energy physics contract. But Dick never wanted that. He was not working for anybody but Caltech; he wanted all his salary from Caltech. That's my understanding—I have no independent information. I don't know how I learned that.

But at any rate, as I say, after three years I was sure I did not want to be an administrator when I grew up. I'd had quite enough of it by that time, in spite of the fact that it was, relatively speaking, a simple job. I, of course, thought it was much more than simple. I quipped that being executive officer was sort of like being a graduate student in administration—you had the responsibility for getting everything done but you had no authority to do it. I now know, having been division chairman, that that's hardly the case. But that's how it appeared to me at that time, because, of course, the things that division chairmen do are known in some sense only to division chairmen. It's all people's problems, and others don't know about it. I didn't know anything about it while I was executive officer. That was all completely hidden from me, as it is from almost everybody, except those who are in the chairman's office.

So that's when I swore off administration. And then I was on the next chairman search committee, which must have been in 1988. This was for division chair for PMA, because Ed was terminating his position there—I guess he didn't want to continue. Some people weren't happy with his administration, because they felt he spent too much time doing research—all of two hours a day. [Laughter] But at any rate, there was some lack of happiness with Ed's administration. As far as they were concerned, he wasn't spending enough time working on the problems of the division. Five years is a long term anyway. So after five years—I don't know, but I assume Ed was not invited to stay on—a search committee was formed. And god! My heart fell to my feet when I was asked to leave the room at one point. [Laughter] My god, are

they talking about putting *me* in that job? I certainly don't want to do that job! Well, happily, no, they did not ask me. I was on the committee that recommended Gerry Neugebauer. So Gerry was recommended, he accepted the job, and he then became chairman.

Then came the end of the next five years [1993], and it was clear we needed a new chairman. A committee was again put together, and I was worried because I wasn't on the committee. And then I was even *more* worried when I was asked by Paul Jennings, the provost, to have lunch with him one day; it must have been around May or so. We didn't talk about being chairman or anything. There was a little bit of a burnt sulphur smell about the whole affair that suggested to me that perhaps there was a bit of fire going on that I wasn't entirely knowledgeable of. That worried me. [Laughter] And then, of course, it became evident, on July 9th, or somewhere around there, when Paul called me into his office and asked me if I'd be chairman.

I immediately wanted to know what was going on with LIGO [Laser Interferometer Gravitational-wave Observatory]. At that time, LIGO was in a terrible situation—referred to as the Drever-LIGO problem. The campus was in an uproar. I had had a memo from Barry Barish in May, when the committee was still searching. We had both come here as assistant professors at the same time; we were very close to each other, good friends, and he said, “You know, we've never seen dissension of this level at Caltech in all of our years here.” It was unique, as far as we were concerned, and probably unique for everybody here, I would imagine. And it was really ripping things apart.

COHEN: This is in '94?

PECK: This is in '93. Of course, it had been going on for a long time and lots of things had happened. I had gone to a meeting, probably it was in May or so, or April, I don't remember. But at any rate, there was a committee trying to figure out what to do with all of this dissent. And some Caltech professors made very inflammatory statements. People were shouting at each other. It was not anything that I had ever seen, and I knew nothing about it. I was deliberately keeping myself uninformed. I didn't know anything about it, but I knew it was going on, and I didn't want to be a part of it.

COHEN: It was all within physics.

PECK: Well, it was physics, but there were lots of people outside physics who were highly upset. In particular, for example, the chairman of the faculty, John Hopfield [Roscoe G. Dickinson Professor of Chemistry and Biology], was deeply involved. It was a highly emotional issue. I remember being stopped by some of my friends in engineering wanting to know what was going on. And I was ignorant. I only knew what I read in the newspapers, as it were. That's probably why they decided to make me chairman [laughter]—because I had not taken either side. I was sufficiently neutral.

COHEN: So what you're saying is that LIGO was dominating the direction of the campus.

PECK: I think so. It was certainly, as Barry put it, nothing we had ever seen around here before. It was highly divisive. Of course, I'm sure all the young professors, all the young people, didn't care. They just thought it was a bunch of old guys throwing rocks at each other. But there was certainly a core set of people within the institute who were deeply upset, and it was taking enormous amounts of time—from Gerry obviously, from the administration. [Ronald W.] Drever [professor of physics] and his supporters had brought a grievance to the Academic Freedom and Tenure Committee. It was declared that the division chairman, Gerry Neugebauer, and the provost had violated the academic freedom of Ron Drever and they had to fix that in some way.

What Gerry told me subsequently—I learned a lot very quickly, starting in September 1993—was that that's when he really felt betrayed by Caltech, because they were accusing him of an academic crime he was innocent of and completely destroyed his reputation, as far as he was concerned. Gerry is not happy with Caltech, and I believe the fundamental point starts right there, when he and Paul Jennings were accused of violating academic freedom. It didn't affect Paul as seriously, because he has a much happier disposition as a human being and is able to tolerate criticism probably differently from what Gerry is able to do.

So I knew vaguely about these things. I had been to this meeting—curious; I remember the place but I don't remember where it was; it must have been the Millikan boardroom. I was sitting next to Steve [Steven E.] Koonin [professor of theoretical physics], and he was shocked at some of the statements and the shouting that was going on. Of course, he was the chairman of the Academic Freedom and Tenure Committee that had ruled on the case of Ron Drever versus

Neugebauer and Jennings. In some sense, the only person who could solve the problem was Steve Koonin, and in fact he did. There were other things that happened in the meantime, but Koonin was in some ways the only person who could do what needed to be done, because he was the person who, in some sense, created the problem initially, if you want to look at it from one side's point of view. Or from another side's point of view, he had the intelligence to do the right thing. There are obviously two sides to this story—there are *at least* two sides to the story.

Well, at any rate, Paul Jennings asked me to be the chairman. The first thing I wanted to know was what's going on with LIGO. And Paul—I think, in all honesty—told me he felt the problem was about solved. He may have told me—I don't have any notes on it, but I know from later on—that there were meetings going on between Paul and Gerry on the one side of the table and Drever and Maarten Schmidt [Moseley Professor of Astronomy] on the other side of the table, who were negotiating to try to find a way to re-ignite Ron Drever's research, because nothing was happening. And I think Paul in all honesty felt that progress was being made at the time he spoke to me, in July. I think this had come out of an arbitration committee or whatever it was; I've forgotten the details, and I wasn't involved in it. I can only reproduce it from what I learned subsequently, and that wasn't completely systematic. But there had been an arbitration committee, which was run by—the person's name I can't remember, someone from Berkeley. And I think it was that person who suggested this arrangement of negotiating a peace treaty.

On the other hand, from a note I have from much later on, it was clear—later on, at least, whenever I made this note—that the two sides were not close to any kind of agreement, although it may have appeared that they were to Paul at the time. Later on I learned that Drever was wanting something like \$5 million and a million dollars a year for some number of years and the institute was offering \$100,000, or something of that sort. I don't have the exact numbers—I do in some notes, but I just don't remember them right now. At any rate, they were very far apart.

Well, at any rate, I spoke to a collection of people [about taking on the chairmanship and about LIGO]. I certainly spoke to Barry Barish, and to Roger Blandford [Tolman Professor of Theoretical Astrophysics], who had chaired the search committee that suggested me. And I spoke, of course, immediately, with Gerry Neugebauer, Robbie [Rochus E.] Vogt [Avery Distinguished Service Professor and professor of physics], Wallace Sargent [Ira S. Bowen Professor of Astronomy], Maarten Schmidt. I didn't know anything; I was trying to find out

things. And I noticed there were big differences in the way a given fact was represented by the two sides.

I spoke to [David] Goodstein [Gilloon Distinguished Service Professor; professor of physics and applied physics], [Kip] Thorne [Feynman Professor of Theoretical Physics]. I spoke to [professor of chemistry] Fred Anson about what it means to be a chairman. He was the dean of the chairmen at that time; he was approaching his fifth year and I think he was chemistry chairman for only one year, overlapping with me; I think he had been chair for ten years. I spoke with Ed Stone. I spoke with Robbie Vogt, who had been chairman of PMA [1978-1983]. I spoke to all the existing chairs. With Neugebauer, of course. I spoke with [professor of physics] Tom Prince; I spoke with Barry Barish.

Speaking to Barish was the first time I learned something about LIGO I had not had any suspicion of before. Barry at the time was leading a major experimental development for the SSC [Superconducting Super Collider] down in Texas, which, it turned out, was canceled approximately a month or so after I became chairman. Barry was well aware of the modern standards of what large project management involves and what the government expects concerning reportage, concerning oversight, all sorts of things. That is, how do you spend \$100,000,000? Barry was aware of all of that. He had also been put, by Gerry Neugebauer, on a committee with Tom Tombrello [Kenan Professor and professor of physics], Roger Blandford and [professor of physics Tom] Phillips. They were on a committee to advise the chairman as to what to do with this LIGO thing. So Barry was quite aware of what was going on.

COHEN: You're not mentioning Kip Thorne.

PECK: This was just outside people who were knowledgeable about management, about large sums of money; all three of those men were in positions involving such things. Tombrello when he was running the research at Schlumberger, for example. Tom Phillips in running the Caltech Submillimeter Observatory. And Barry because he was the spokesman for the GEM [Gammas, Electrons, and Muons] project, as it was called, for the SSC. So Barry was the first one to tell me that LIGO had real management problems, and in fact it was Barry Barish who really understood what NSF wanted. I don't think anybody else really knew what NSF wanted for LIGO management. I'm sure I didn't.

Then I finally spoke to Michael Aschbacher, who was the EO for mathematics. The mathematicians didn't know who I was from Adam, and I didn't know who they were from Adam. I had no idea. [Laughter]

COHEN: Mathematicians have a tendency to stay to themselves.

PECK: That's right, they tend to stay to themselves. So I talked to a whole bunch of people and then I left. I went to Italy for a month and enjoyed myself in working on MACRO. I thought a lot about the chairmanship during that time. I probably had e-mails in which I discussed things with various people. I must have talked to Barry Simon [IBM Professor of Mathematics and Theoretical Physics] at some point, because it was he who gave me this advice. I asked, "What should I ask for?"

"One thing you should ask for," said Barry, "is a year's leave when you finish, with pay." [Laughter]

"Oh, that's a neat idea; I never thought of that before."

So I in fact did, and got it. [Laughter] But I asked many people as to what sorts of things should I ask for from the administration. It seemed to me that that was a natural thing to do—a quid pro quo, not what's perfect for me but for the division.

At any rate, on September 13th I met with Paul [Jennings] again and asked him for several things: having to do with LIGO, having to do with appointments, having to do with the support of the institute for physics and astronomy and mathematics, what was the upper administration's position regarding that. And then for my year off.

I was scheduled to start on November 1st. I realized that I wanted to have some time in which to, without having the responsibility, learn what the hell was going on.

COHEN: So it seemed like a big challenge?

PECK: Boy, I should say I felt challenged! [Laughter] As I say, I had been assured by Paul initially that LIGO was not in too bad shape. He never fully disabused me of that, but I discovered eventually what was going on. I think he was completely honest with me.

COHEN: Well, they were probably hopeful.

PECK: Of course. Everybody was hopeful. If you aren't hopeful, you give up. Hope is necessary to get anything done. And so I think there was a lot of hope involved with that. But as I say, I learned from Barish, when I spoke with him, that there were LIGO management problems. I had never suspected that before.

COHEN: You thought it was just a personality clash?

PECK: Yes, between Robbie and Ron Drever, and then Drever brought in other people who were his spokesmen—Peter Goldreich [Dubridge Professor of Astrophysics and Planetary Physics] and Maarten Schmidt and Wallace Sargent.

COHEN: Formidable.

PECK: Formidable, that's right, exactly. Curiously, it was only astronomers, now that I think about it. The physicists tended to be rather neutral on the thing. Like most people, they just wanted it to go away, but it was the astronomers who kept making it come back again—it seemed to us physicists at any rate. And, of course, Ron has a unique personality.

COHEN: It didn't hurt that the other person involved here had offended so many of these people over his years of being in administration.

PECK: That's right. Poor Gerry had managed to offend all these guys in one way or another. He was the director of Palomar [1980-1994].

COHEN: Yes. But I meant Robbie Vogt.

PECK: And so had Robbie, of course. In the case of Gerry, though, the problem was especially bad, because he was not only the director of Palomar—which incidentally, I might say, when we in the search committee had recommended Gerry as the chairman of PMA, we expected he would step down as director of Palomar, but he didn't. It was too close to his heart, much too close, and he must have arranged that whatever it was the committee recommended was voided by the administration. But at any rate, Gerry was both the director of Palomar and the division

chairman, so therefore these astronomers had no one to complain to about the director of Palomar, except to the provost, which certainly would be viewed by Gerry extremely negatively. He had a very strong sense of the line of command—you never go above the head of anybody.

Well, at any rate, I agreed tentatively, after Paul and I had thought about it a bit, and I must have said, on some date I don't even have noted, "Fine, I'll do it." As I say, I thought I would have until November 1st to organize my affairs. But then I was talking to Gerry one day, and he was busy typing away, writing a memo, resigning as of October 1st, or something.

Gerry was in a tremendous fit of anger against the president [Thomas E. Everhart, Caltech president 1987-1997] because of something that had happened—I don't know exactly what it was. But Gerry was tremendously angry, and he was writing his letter of resignation as of October 1st, and the division was then mine, and he was getting out of there.

Well, this spoiled a few ideas I had, because now suddenly I would have to learn many things. I didn't know the faculty; I didn't know who the astronomers were, or who the mathematicians were, and in many cases I didn't know half the physicists. So I had to meet all these people, and not only meet them but solve all their problems and do all the things I was supposed to be doing as chairman at the same time. So that's how it turned out. And on the first Monday in October, I think it was, I became chairman and found myself sitting in the chairman's chair, with nobody else in there, for the first time. Gerry had spent a lot of time telling me about every faculty member and giving me thumbnail sketches. He's really a perceptive and smart man; I'm endlessly amazed at the skills that Gerry Neugebauer has displayed, and not only as chairman. Like all people, he has his blind spots, but he displayed a tremendous amount of leadership when he was division chair and he was extraordinarily helpful to me in making the transition. I didn't know anybody; he recognized that and was helpful. But he walked out and there I was.

So I had an extraordinarily steep learning curve for the next three months—October and November and December. I started keeping notes of everything at that time, because I couldn't remember all the stuff that was going on. I discovered enormous fires burning all over the place that I hadn't known existed at all. Not everywhere, of course; in general, physics was no problem, except for LIGO. The problem was in astronomy, where everybody was mad at everybody else, it seemed. There were grievances being brought by people—formal grievances. Just before Gerry's term finished, there was a grievance that had been brought by a person who

worked at Big Bear Solar Observatory against the director thereof. I sat in, so I saw my first grievance negotiation. And needless to say, it didn't stop then; it kept on going. Other grievances were then filed.

COHEN: This was all at Big Bear?

PECK: There was a historic grievance that had presumably been taken care of long before I appeared on the scene, in which the person who was making the grievance was unhappy with the final result. So he was threatening to, and finally did, bring a subsequent grievance. Then there was another grievance, from another person. So there were three grievances from people in the Big Bear Solar Observatory.

COHEN: Were they all on the same subject?

PECK: No, these were different things, though generally related to the same fundamental problem. If it were not for Ann Bussone [Caltech associate general counsel], I don't know how I would have lived through that. Ann was working for Human Resources at that time, and I don't know exactly what her title was, but she's a very smart woman, a very good lawyer, who had background, and I didn't know anything about what the hell this was all about. She took me gently by the hand and led me through things, gave me information, informed me of the legal aspects of things. I owe a great debt of gratitude to Ann Bussone; she's great. I just happened to see her the other day; she now is on the lawyers' side of Caltech, working in the attorney's office, and I think she spends most of her time at JPL. She's a real asset to this institution, and I certainly hope that is noted by other people. Without her, I would have been completely at sea as to how to handle those personnel problems. Of course, in the long run I was the guy who had to make the decisions; I had to write the memos; I had to try and find ways to make people happy and not create further dissension.

And I was learning about all the problems in LIGO, too. I discovered, to my horror, that it wasn't just LIGO against the Caltech faculty. There was a little group of people—Robbie Vogt and [LIGO detector leader] Stan Whitcomb and Bill Althouse, who had been a technician working with Robbie in space physics. He's a superb engineer and problem solver. They were the chief group of people in LIGO, and they had circled the wagons and were fighting off the

Indians. And I discovered that the Indians were not only the Caltech faculty but the people at MIT and the NSF. I couldn't believe it. I mean, there was a war on every front—between LIGO and MIT, between LIGO and NSF, between LIGO and the Caltech faculty, between LIGO and Drever, et cetera. There was chaos! It was unbelievable!

The president was involved. The provost was involved. It was using up a lot of their time. It turns out that the president, of course, had entrée to various places in Washington. Robbie would complain about the fact that [Robert A.] Eisenstein talked only to Everhart—Eisenstein was in charge of the physics division at the NSF at that time. The NSF person who interacted primarily with Robbie and the management of LIGO here was Dave Berley. Dave Berley was a particle physicist, whom I knew in that context, who had gone to the NSF and had been put in charge of LIGO. He may still be there, for all I know. But during all of my administration, he was the NSF person in charge of LIGO, particularly. I then discovered that Lew Allen, a former director of JPL [1982-1991], was special assistant to Everhart for LIGO. Shortly after I came, Ned [Edwin L.] Goldwasser—another particle physicist, whom Everhart had come to know and trust quite well at [the University of] Illinois—was invited to Caltech to be a special assistant to the president for LIGO. I found there was a man named Gene Giberson, who had been a high-level engineer at JPL and was now retired, who was also one of the people who were trying to solve this problem. And each of these people knew a different aspect of it, I discovered gradually, although they all were generally aware of the multiplicity of problems. Certainly Lew Allen was.

So these were people whose existence I didn't know of until I was chairman. But it was clear that Tom Everhart and his provost, Paul Jennings, were consumed by this problem. Paul was at frequent meetings concerning LIGO. I have, in fact, in one of my books, the minutes from a Faculty Board meeting in which [professor of astronomy] Judy [Judith G.] Cohen asks the president, “Why is it that it's eighteen months since you said you'd do something about taking care of Ron Drever and nothing's been done yet?” And there was a long speech that Tom Everhart made in which he basically said, “I've been spending most of my time on this problem.” Certainly the administration was well aware of what was going on.

COHEN: Were the trustees ever involved with it?

PECK: Not that I'm aware of. I never knew a trustee who seemed to be particularly knowledgeable about it—if so, they never talked to me about it. Robbie, of course, who knew the trustees very well, always made sure that he was not on campus anytime the trustees were. He did not want to be in his office and have a discussion with a trustee. **[Tape ends]**

Begin Tape 4, Side 2

COHEN: [Laughter] What a cast of characters! It could be an opera.

PECK: [Laughter] It could be an opera. You can't believe it. I go through my notes and I'm just as startled—I'd forgotten most of this; I had to go through my notes to remember it.

So during those first three months, I was gradually learning what all the professors did. What did a mathematician do for a living? I didn't know. What was going on among the astronomers? I found they were all fighting with each other. And there were some small coalitions, but it was pretty friable—it was different groups fighting about different things. Although generally speaking, there was one common enemy, and that was Gerry Neugebauer. Gerry was not appreciated, and I think he should have been appreciated by the astronomers a hell of a lot more. He did a lot for Palomar and Keck [W. M. Keck Observatory], from what I know of it, but that was not acknowledged by the astronomical community, that I was aware of.

But the main problem for me was finding out what was going on in LIGO. As I say here in my notes, Jennings thought the LIGO arrangements were stable and that I would have plenty of time to learn the job. One of the things I wanted to be sure of at the time I took the job was that there would be no change in the arrangements regarding LIGO for at least a year, because I didn't want to be involved in that problem for a long time—I wanted to find out about other stuff first. Well, it didn't work out that way.

So I managed to get through those first three months and learned a heck of a lot of stuff. I was astonished at the number of things going on.

COHEN: You had given up your research for a period of time?

PECK: That's right. Well, when there wasn't a huge fire going on that needed to be put out immediately. I of course had meetings—I guess on Tuesdays. We would always have an

afternoon in which the graduate students and professors would get together and talk about things, the students would tell what they were doing, the postdocs, and so forth. And I maintained contact with MACRO during this period. I did miss a collaboration meeting, which came probably at the end of that October. That was impossible; I couldn't go to that. I always regretted it, because it was in southern Italy, and I would have loved to have gone down there. [Laughter] It was in some little village, down in a part of Italy you needed protection to go to. [Laughter] But certainly on any active, day-to-day basis, no research was going on in my life during this period of time. But once a week I always tried to arrange, when I could, to attend meetings with people about MACRO. We had collaboration meetings every six months. The next one was in Corpus Christi, as a matter of fact, sometime around May or June, and I managed to go to that. Of course, while I was there, I got a phone call from Ann Bussone telling me about what was going on with the grievances. Oh, goodness! [Laughter] I'm getting ahead of myself here.

Anyway, one morning I was walking down the corridor in Bridge and I went by Robbie Vogt's office. He usually kept the door open. There was a little table in there, and that's where all the powwows took place among LIGO management—that is, the three chief people: Robbie, Stan Whitcomb, and Bill Althouse. And I saw them sitting around the table, and they looked like they were at a séance. They looked as though something terrible had happened, or as though they were anticipating something terrible. It was clear from the way they were sitting there. There was not a word being said; they were all just sitting there staring at a piece of paper in the middle of the table. So I walked in, somewhat innocently, and said, "What's going on?" And Robbie said, "Have you seen this?"

"No."

So I look at it, and I see that it is a letter from NSF saying, "The NSF does not approve the expenditure of funds to begin building the LIGO site in [Hanford,] Washington." That was to be the first site to be started, and it had to be started before some date—maybe the beginning of April or something—because a certain kind of bird lays its eggs in April and you can't disturb the birds for environmental reasons. At any rate, NSF was not going to provide the funds for doing this until NSF was happy with the LIGO management. "And would Caltech please be so kind as to send the management back to Washington [DC] on January 17th"—I think it was about a week—"for a meeting at the NSF offices."

COHEN: So it had nothing to do with the birds?

PECK: No, no. It had to do with the fact that NSF had been trying for a long time to get something called a management plan. I had read a letter from Berley sometime earlier, and it was complete gobbledygook to me. And part of it was something having to do with a management-breakdown list. I can't remember the exact name, but it's well known to anyone who's involved in modern project management. You make a management-breakdown structure. I can't remember the exact term, but it's a list of all the various activities that are involved and how they break down into various categories. You know: "Build a tunnel." Well, what do you need to build a tunnel? You take care of this, you take care of that, each task has its own set of subtasks. So you end up with 7.3.2.7.8—as an item—and then a word that describes what that item is and how much it costs and what its time schedules are. That was what we were being asked for, but I hadn't the faintest idea what was being asked for, from reading the language of the letter. Robbie didn't either, because Robbie was running this whole project very much like the skunk works, the famous Lockheed group that built the U-2. And that's how he put it himself; he was running it like the skunk works. That was his view of how to do things—cheap, fast on your feet, don't worry about all the complicated schedules and details, just get the job done right and do it cheap and right. He clearly was much enamored of that notion, and that's how he felt LIGO should be run. That is *not* the way the National Science Board or the NSF felt it should be run. And there was the contretemps.

The problem with MIT, I discovered, was that historically MIT had not wanted to be a part of LIGO in any way that it would be part of the management. There was the famous Deutch zero. Whatever [John] Deutch was—at that time, a professor of chemistry; subsequently he ran the CIA [Central Intelligence Agency] and various other important things. And many years ago, before I knew anything about any of this, when negotiations were going on between Caltech and MIT concerning the management of LIGO, the question was exclusively asked of Deutch, "What is it that MIT will contribute to this project?" And there was a big piece of paper on the table, and Deutch took a pencil and he made a zero, and he said, "This!"

COHEN: What a cast of characters!

PECK: [Laughter] So you didn't know about all this, did you?

COHEN: Well, I didn't know about Deutch's zero.

PECK: The famous Deutch zero. Now, I wasn't there; I didn't see this; I'm only repeating things that others have told me. But the idea certainly was, initially, that MIT did not want any part of any responsibilities. They would be subcontracted by Caltech, and that's legally how it is today, I think. But later MIT decided maybe they did want to get involved. Later on, it was beginning to look like maybe LIGO wasn't such a big risk. Caltech had absorbed the risk by then and there was a big payoff, so the MIT people were beginning to get the feeling that maybe they wanted to be involved. Certainly [MIT physics professor] Rai [Rainer] Weiss, who was one of the people, or perhaps *the* person, who invented laser interferometers.

COHEN: He felt very passionately about LIGO.

PECK: He still does. I assure you, Rai Weiss is a wonderful man, a fabulous physicist. But he's not a manager; he doesn't know how to run things very well. But Rai was an important part—and still is an important part—of LIGO. The two administrators at MIT were Ernie [Ernest J.] Moniz and Claude Canizares. They were two of the people who wanted to be a much more integral part of LIGO, but they were constantly being stiff-armed, held at arm's length, by Robbie. Robbie did not want anything from them except some help in getting LIGO built, and such political help as Charles Vest, the president of MIT, could provide. There's a real advantage in having a university on the East Coast—you're a lot closer to Washington, and it's a lot easier then to make the right contacts. So that's one of the important things that Charles Vest did for LIGO. Vest was very much in favor of it. He made a visit to Caltech, for the first time, actually, I think, sometime in November or December, and Robbie showed him around and took his measure and decided he was an OK guy. He wasn't one of the enemies at that moment; he maybe became one later, I don't know. **[Tape recorder turned off]**

CHARLES W. PECK**SESSION 5****November 12, 2003****Begin Tape 5, Side 1**

COHEN: We were immersed in the discussion of LIGO when we stopped last time. So why don't I let you take over.

PECK: Let me start today on something I may have already said. On January 10, 1994, I was walking down the hall in Bridge.

COHEN: And you saw Robbie sitting—

PECK: With Bill Althouse and Stan Whitcomb. And they looked like they were at a funeral. So I walked in and asked what was happening. What it was, was a letter from NSF saying they would not allow funding for the beginning of construction on LIGO in Hanford. What was supposed to be done was something called the rough-grading contract. It was to be let quite soon; it had to be let soon, or else the whole project would have to be delayed for a year, as I mentioned, because of some birds that lay their eggs underneath the bushes in the desert. This was a crisis. If we didn't get it started by a certain date, there would be a whole year's delay, and who knows what mischief would occur.

The letter said that not only was the NSF not allowing the expenditure for construction but they insisted that the LIGO team—that is, Robbie, Stan, and Bill Althouse, and perhaps others were mentioned, I don't know—come to Washington, D.C. for a meeting with the NSF on January 17th, one week later. Robbie said he had already had a meeting scheduled with NSF in Washington, one week beyond that, but they refused to delay. They insisted on having a meeting instantly, and they may have called for other people to come.

Well, it was clear to me that this was really a serious matter—with NSF stopping construction and calling people to Washington for a meeting. It seemed to me that what we had to do was all of us get together and have a series of meetings in the coming week, to make sure that those of us who were going were all in agreement as to what our positions were. I could get

no interest in this! I was amazed! I couldn't believe it—these guys would just sit there. I couldn't get any excitement, but I did get some meetings. I canceled all of my own meetings, I cleared a room out, I got all these guys together. And the people who eventually went to Washington from Caltech were Robbie Vogt, of course; Stan Whitcomb; Bill Althouse; Lew Allen; Tom Tombrello, who was on an advisory committee to PMA, I guess it was, which consisted of Barish, Phillips, and Tombrello, and I think Tombrello was also on what's called the Ex Com, the Executive Committee, which had been invented about a year before to try to solve some of these political problems that were going on. And then myself. And Ned Goldwasser, who had arrived relatively recently at that point; he was a good friend of Tom Everhart's from Illinois and special assistant to Everhart for LIGO. His job was to hold Robbie's hand, basically, and provide good advice to Everhart—and to the PMA division later on, but at this point the division was not directly involved. The line of reporting was unusual for Caltech. It went from LIGO to the provost and then to the president.

COHEN: So supposedly it wasn't your problem.

PECK: In principle it wasn't my problem, but I was in the middle of everything. [Laughter] Because all of the resources for LIGO came out of PMA, but LIGO didn't report to PMA. It was an anomaly, obviously. I was not involved in how that had developed. In fact, I noticed in my notes that as I was talking to all the members of the PMA faculty—as I wished to do—I went all over the PMA part of the campus, speaking to professors in their offices. And one of the questions I asked them all was, “What's your opinion of where LIGO should be? Should it be part of PMA or not?” In general, people felt it should be a part of PMA. That was the consensus view.

So I ended up going to the Washington meeting too. Before that, I tried to get everybody together to try to agree to a uniform view. I wanted to find out things; I didn't know what the heck this was about. I just remembered being shown a letter that had come from NSF, from Dave Berley, directing LIGO to do something and I couldn't make head or tail of what was being asked. They wanted something called a WBS, a work-breakdown structure. What's a WBS? I had no idea. They needed a management plan. What's a management plan? I'd never heard of any of these things. There was a list of about ten items, which I couldn't make head or tail of.

And that was also true of anybody in LIGO here. Robbie did not know what was being asked for. This was all formal, big-project management. Robbie and all of us had done, in a sense, small things, and Robbie, as I mentioned earlier, very much saw the skunk works as the way this ought to go. Of course, when Barry Barish saw [Berley's letter], he knew exactly what it was. He knew about big projects.

Well, at any rate, we ended up in Washington, DC, on January 17th in 1994 at the National Science Foundation headquarters, in a rather small room with a large number of people, including all of those I just mentioned. From the NSF—the people who spoke, at any rate, and I think there were other people of various kinds—were Bill Harris, who was the associate director for physics and mathematical sciences; Carl Burb, who was an advisor to the director of NSF, which was Neal Lane at this point. Lane had just taken over. Bob Eisenstein, who was the head of physics at NSF. Dave Berley, who was the NSF officer in charge of LIGO. Bob Eisenstein was a physicist; he'd been in condensed matter physics at Illinois—he was the brother, in fact, of the Eisenstein here [professor of physics James Eisenstein]. Dave Berley was a particle physicist. I had known Dave for many years in his days as an experimentalist in particle physics, and he had now moved to the NSF. He had been put in charge of LIGO and had been in that job for about a year. Then there was a man named Aaron Asrael, in charge of grants and contracts—he was one of the big shots at NSF. Then another person from NSF was Richard Isaacson, who had been involved in gravitational physics as project manager for various things, in particular for theoretical gravity, for many years. He was probably the person who had been associated with gravitational physics at NSF for the longest.

And the people from MIT were Ernie Moniz, who at the time was chairman of the MIT Physics Department. He was certainly the MIT representative on the Ex Com, the Executive Committee. Then Claude Canizares, the director of the MIT Space Lab; he was also on the LIGO Ex Com. And then there was Rai Weiss, who was one of the giants in the development of optical interferometry for gravitational application. And then there was me.

This meeting was a real watershed point for LIGO. Indeed, before we got there, Robbie said he had three possible courses of action: (1) We could go to Washington, and he would probably lose his temper, make rash statements, and end up being fired. Or (2), he could just ignore the letter and not even say anything about it. Or (3), he could call NSF and say that we weren't going to come. He felt the latter two courses of action would be viewed in various

negative ways by various people, and so it was inevitable that he was going to be fired. That was the view he had.

COHEN: Because he would lose his temper when he got there.

PECK: That's right, that's what he was predicting, and indeed that's exactly what happened. He always called me Dr. Pangloss, because I always tried to cheer him up and say things that were positive. And of course he was referring to Voltaire's Pangloss, in *Candide*—all's for the best in the best of all possible worlds. That was the view I took with Robbie as much as I could.

Well, the meeting started in the morning. There was a lot of discussion that morning. I have a number of summary points in which I decided that there were various things that were action items. I won't go through all of them, but there are about a dozen or so action items that I wrote down as important things coming out of this meeting. And they involved things such as we have to do this to the management plan. It turned out a management plan *had* been produced by LIGO, but it was not acceptable to the NSF—that was one of the problems I discovered at this time. The details about communication: Dave Berley had to somehow or another get better communication with LIGO and come out to Caltech. He and Eisenstein decided they'd have to come out and visit the place and see what was going on and understand it better than they in fact did. And people acknowledged that there had been problems.

The meeting was not genteel, shall we say. There were loud voices. People's tempers rose. There was almost a fight between Bob Eisenstein and Robbie Vogt, in my opinion. I had the pleasant job of chairing this thing—or, as Robbie put it, I was the timekeeper. I think it was Robbie who characterized it that way; he didn't want me to have the glorious title of chairman, so I was the timekeeper. [Laughter] I may be wrong about that; it might have been Berley. I happened to overhear somebody say to somebody else, "I've never seen a Caltech person like that before," referring to me. [Laughter] I was quite rational and quiet and trying to make the meeting work.

But at any rate, it was a very contentious meeting. It went on all morning. And then in the afternoon there was an executive meeting, from which Robbie, I guess, and probably Stan Whitcomb and Bill Althouse, were excluded. And, man, then everything blew up! That's when

everybody started talking, saying the things that they wouldn't say if Robbie were there. And that was fierce.

COHEN: So this was a real watershed.

PECK: This was a real watershed event. The next morning, the meeting started again, and Robbie made the opening statement. On the first day, Bill Harris had made the opening statement, and in fact it was an important opening statement. I have a careful summary of it here, which I wrote after the fact, because it was really memorable—certainly for me, at the time. But the next day Robbie made an opening statement: “There are no serious problems. There's nothing of substance in all these complaints you guys are making. It's only a problem in communications. I ask for meetings, and they're canceled. I'm excluded from Ex Com, even. I had a chance meeting with Harris in November, and no problems were brought up at that time. There's excessive attention to politics, not to the science and the technology.” And Robbie saw no purpose in continuing the meeting unless equal attention was given to the technical and scientific difficulties. Stan Whitcomb then said, “The real problems are not being addressed. This is all peripheral.” Needless to say, Bob Eisenstein's view was not in agreement with this. He says, “Look fellows, the big problem is in the external community. And it's not technical. It's the way you guys don't interact with the rest of the world.”

So the meeting continued that day, mostly sort of a rebuttal of what had been said the day before. And as you might expect, Robbie did not keep his temper very well. Right after the meeting, I wrote down what I had observed, particularly concerning Robbie's interaction with other people. And what I wrote was that he was jealous of his directorial rights; he was quick to take offense. He kept asserting that this was a kangaroo court. He interpreted remarks as insults, and that led to conflict. And so it went; I needn't go through it all. He would make polarizing comments. Probably other people did similar things, but of course my concern was with the guy who was the director of LIGO and without whom LIGO wouldn't work, so those are the comments I wrote down.

Well, we finally managed to get out of Washington that night. A cold front had come in, and it was freezing. Our plane iced itself to the ground. We had to change planes. Very, very

late. And so we finally got back sometime early in the morning. I probably just went to bed and stayed in bed the whole next day is my guess.

Soon afterward—I don't know exactly when—the president, Tom Everhart, called each of us individually. I had been worrying a lot about what I was going to report to him, because I knew I was a key person in giving the report to him. I met with him on January 24th, so this is maybe almost a week after the meeting. January 24th, Monday, 7:30 in the morning.

Probably the night after the first meeting with the NSF, Tom Tombrello and I had spent a long time on the telephone, discussing with each other what in the world to do about this, and it seemed like the only possible solution was to get Barry Barish involved. He was involved, but he wasn't at the meeting. He was involved by virtue of being on the advisory committee.

So I made the following recommendations to the president: Appoint Whitcomb as acting director of LIGO. Discuss all actions to be taken with Robbie Vogt; be sure everything is discussed with him beforehand. The real goal was to give Stan Whitcomb clear authority and get Robbie to take a vacation for a little while, while things cooled off, as it were. Convene a meeting of all LIGO employees that afternoon in 114 Bridge. The president would explain the situation—it turns out I did all of that, eventually. Send an explanatory memo to the faculty and let them know what was going on. And form a LIGO management committee to run things while Robbie was on the cooling-off period.

Well, Everhart didn't accept the recommendations, but what he did was create an advisory committee, with me as chairman. Lew Allen, Kip Thorne, Barry Barish, and me. Perhaps on the 22nd—I don't have the details here—I had gone to see Barry. He had just moved to Santa Monica, so I drove out to Santa Monica to talk to him, to ask him if he'd be willing—maybe this was after meeting with the president; I'm a little puzzled about the timing now. But at any rate, I went over to Santa Monica.

COHEN: You asked if he would take over the LIGO project?

PECK: Well, no, not exactly that, at the time. To be on the advisory committee was what I went to ask. So an advisory committee was set up; there were no actions taken yet. I had made my recommendations; other people had presumably done the same. But Everhart had wanted there to be a committee, consisting of me as chair, Lew Allen, Kip Thorne, and Barry. So I had driven

out to Santa Monica, and Barry and I spoke for a long time, walking up and down the beach, discussing what might happen, exploring the possibility that he would be the director. He didn't want to be director of LIGO; he wanted to continue doing particle physics—he's a particle physicist; that's what he wanted to do.

COHEN: And his big project died.

PECK: Yes. The SSC had been turned off—it must have been around the end of October. And what Barry was doing mainly with his time was trying to finish up some work that had been started with regard to GEM, but mainly for the careers of all the young physicists who had been involved in GEM, doing as much damage control as he could, finding jobs for people as best he could. One other possibility that we must have talked about on this walk was—well, maybe it was already happening; I don't know—was that a lot of those people could be used in LIGO. LIGO should be looking at them and finding out who wanted to get involved in a different kind of research.

So I told my secretary just to clear out the whole next week. We were going to meet all day, every day. The reason was—I forgot to mention this up until now—that the meeting at the NSF that I mentioned was on January 17th, and then there was a meeting of the National Science Board scheduled for February 10th, at which LIGO would be an agenda item and at which Bill Harris had to give a report. And at that point, LIGO could have been canceled if the NSB had said, “Look, we can't take this nonsense anymore. That's the end of it. Close it down. Do something else”—it could have happened. So NSF was very worried. They didn't want their two prime institutions—Caltech and MIT—beaten up in public. They could imagine a congressional hearing.

So we four met essentially continuously—consistent with having dinner and other obligations; there were various other things going on for me at this time. But at any rate, we had continual meetings and we finally ended up with a recommendation, which was basically that Barry Barish would take the role that Lew Allen had been playing but with more authority than Lew Allen had. I don't quite remember how exactly we phrased it. And then finally, on a Saturday afternoon, I had a meeting with Tom Everhart and gave him the recommendation of our committee. It must have been about a week—on the Saturday after he had convened us, which

was on a Tuesday. We had been working pretty hard during that period. Tom did not accept our recommendation. It was clear that I didn't convince the president.

COHEN: Did he have an alternative plan?

PECK: Not at that moment. One of the things that Tom Everhart was under a lot of pressure about was that Bob Eisenstein and Dave Berley were scheduled to come out here approximately in the middle of the next week—around February 3rd, or something like that, and he was really worried about that meeting. He felt he had to have something done. We had gotten a letter from the provost at MIT, Dr. [Mark S.] Wrighton, in which he made various complaints and said that none had been answered yet. People who had seen Bill Harris at meetings or something had come back with reports that it looked like Caltech was not doing anything. They were told that there were serious things going on but it looked as though we hadn't done anything. At any rate, Tom was under lots of pressure.

So eventually Tom asked Barry to be principal investigator of LIGO, with Robbie continuing as director. We had decided that there was no way that LIGO could be successful if Robbie were to just walk off the job. Tom had spoken to Barry; he'd gotten Barry's agreement to do this as long as Barry had authority. Barry was perfectly willing to accept responsibility for LIGO if he had enough authority to make a possibility of success. Tom then wrote a letter to Robbie, dated February 2nd, and unfortunately that letter was delivered to Robbie in very unhappy circumstances. It was given to him by the driver of a Caltech car, who was taking Robbie from Caltech to Burbank airport to go to a meeting of the advisory committee to the president of the University of California concerning the weapons labs—an important activity Robbie did and I imagine still continues to do. But Robbie was furious at this manner of getting the communication to him.

COHEN: It is a little strange.

PECK: Yes, it is. I think the fundamental problem was that Everhart had not decided what to do. It had to be decided by him eventually. He had probably not decided what to do and kept looking for ideas from I don't know where and he had this deadline, which was that the guys from NSF were showing up. So he had to get the job done. Robbie had refused, in spite of the

fact that the NSF guys were coming, to give up going to the meeting at UC; he insisted that he had to go. But at any rate, he got the letter. I had seen the letter before Tom gave it to him; I remember standing and speaking with Tom briefly about it. I didn't have any input into it, except insofar as what I had reported we recommended Tom ought to do. But I got a call that night from Robbie Vogt. He called me at home, and he was absolutely furious. He yelled at me and howled at me. I held the phone three inches from my ear and I didn't miss a word. [Laughter] He was furious. It was all my fault. He called me all manner of—god, it was terrible!

Well, the upshot was that the people from NSF finally came; an action had been taken. Robbie refused to continue as director. Stan Whitcomb was formally made the acting director of LIGO. Barry became the principal investigator for LIGO. LIGO was moved to report to the chairman of PMA, which was me. The rationalization relative to Caltech's general structure was made at the same moment. Robbie was probably formally on vacation or something, I don't remember. I went to a faculty meeting where I reported all this to the faculty. It was advised strongly that *I* make the presentation to the LIGO people rather than Everhart. Apparently Tom had not made a very good impression on the LIGO people at some earlier point. But at any rate, it was strongly recommended that I do that. They didn't know who Barry Barish was, so I told them who Barry Barish was, what he had done, and so forth, and what the arrangements would be. Robbie was standing in the doorway, listening to part of this speech—I don't know if he was there the whole time, but he was there for part of the time. I glanced over at him standing in the doorway—looking very black.

What then happened over a period of months, was—it seemed to me months; maybe it was only a month or two, I don't know—there were weekly meetings between Robbie Vogt, Barry Barish, and me in which we tried to figure out how this would work. I mean, Robbie wasn't going to be on vacation forever. He needed to be the director of LIGO, or something. Anyway, we had meetings between Barry, me, and Robbie—weekly, as I say, in my office. So these guys would give their views and their various kinds of diagrams of organization, all sorts of things going on, at the same time that work was going on on making the management plan that was needed. Barry was now involved; he knew what a management plan was supposed to look like.

I should go back just a little and say that shortly after the two people from NSF came to visit Caltech—Dave Berley and Bob Eisenstein—Barry made a trip back East. Remember, there were several fires burning; there was the big fire with MIT, and that was not settled. In fact, MIT was very annoyed that these Caltech guys were again doing the same thing. We were having meetings, deciding what was going to happen to LIGO, without any of them. Of course, the practicality is, how would we have in practice included them, because they weren't here.

At any rate, Barry made a trip to the East Coast. He met with the MIT people for, I believe, two days. He managed to put out all the fires there by explaining how he knew things were going to work. They had confidence in Barry, he was known to these guys. So Barry's the magic man—he's fabulous. He managed to raise the confidence level not only of Moniz and Canizares but I suspect he also spoke with Wrighton and perhaps with Vest—I don't know. But at any rate, he put out the fires there. He then went to NSF and told them about things in more detail. He appeared before the National Science Board and made a pitch for LIGO and for the new arrangements that were being set up, and the fires there damped down. And now he's back at Caltech and we're having these frequent meetings, working with Robbie to figure out how it will all work.

Somehow or another, it did all work and LIGO still exists. But I would say it came within a shadow of being canceled at that time.

Well, at any rate, there's lots more details, obviously, but that's enough of that. But this was what I considered one of the most important things that happened during my tenure.

Then, I should say that during that spring, we finally managed to solve the problem with regard to Ron Drever. I say "we," but I didn't have much to do with it. I was in some sense always just the glue, or the grease, trying to keep things going and make sure that people did things that were acceptable. I met with Ron Drever enormous numbers of hours.

What eventually happened there—I haven't reviewed my notes on it so I'll just speak from memory—was that the person who had been in charge of the Academic Freedom and Tenure Committee, Steve Koonin, was now provost. He had been appointed provost in the early spring or so; I can't remember exactly when, so he was in a position where he was in some ways the only person who could solve the Drever problem. He was the person who had been at the center of it initially. So what finally happened was that a deadline was created for Ron to accept one of several possibilities: One was to come back to LIGO—and Barry was immediately

involved in that, and made it very clear to Drever: “If you come back into LIGO, you’ve got to do what you’re told. It’s not like you’re a free individual using large sums of money to do anything. This money is all explicitly designated for specific purposes. And so you will be *told* what you have to do.”

COHEN: Yes, but he probably said it in a nice way.

PECK: Yes. Well, it wasn’t put as roughly as I just did. But nevertheless that was the fundamental message: “Alternatively you can accept this million dollars”—or whatever it was; I forget the exact sum—“from Caltech and set up your own group. We’ll help you as much as possible in setting things up.”

I might say that many of these ideas had arisen from what Gerry was trying to do when he was still chairman of PMA. But it now could work because the actors were different. The provost was Steve Koonin, who was trusted by all the various parties involved—the anti-LIGO party, I’ll call them, although that’s not exactly a correct name. But the people who were Ron’s promoters; in particular Maarten Schmidt, and most important, actually, Peter Goldreich. It was finally Peter, I think, who forced Ron to make a decision—and that’s happened several times. It’s very hard to get Ron Drever to make a decision, but Peter is able to do it, and Peter made Ron finally make a decision. So there was a specific day and time by which Ron had to make a decision, or else by default something else would happen. And I was sitting in my office as that hour approached, and I hadn’t heard a word from anybody, and I didn’t know what was going to happen.

COHEN: Golly, this could be an opera!

PECK: [Laughter] Absolutely, it could be an opera. And about ten minutes before the deadline—which was perhaps five o’clock on a Friday afternoon or something—*knock, knock, knock!* I think all my secretaries were gone already. And in comes Ron. He hands me a piece of paper. I don’t know what the paper says, but I assure you—we felt that Ron, from everything I’d heard, was a divisive force inside of LIGO. We did not want Ron in LIGO. That would just create more trouble, we felt. And we thought that what we were offering as the alternative was sufficiently—well, you know, it was coming from the provost; it had his stamp on it; et cetera, et

cetera. And that's what Ron took. That's what Peter had finally convinced him was the thing he should do, and that's what he finally did. There was the little piece of paper, "I take option B," or something like that. So that was a rather—well, the opera could end at that high point.

[Laughter]

So then it became my task to try to implement all the things that had been promised. He got money; he got space—I had to find space. He was given some space on one side of the synchrotron wall. He insisted on the ability to make an L-shaped laboratory, and so I managed to convince the provost to put some more money in so that Ron could drive a tunnel underneath the passageway there. So he then had the possibility of a pair of interferometer arms that were about 40 meters each. The synchrotron floor space was slightly smaller, but I had carefully checked with all users to see if that would affect their use of the synchrotron floor. It never bothered anyone.

So finally that fire got damped down a bit—although Ron was probably never happy, but he was sufficiently satisfied that the conflagration on the campus concerning his case finally settled down. And I think it's been successful since then. He, of course, has not created a big group, which is what he would like to do I think. He never succeeded in getting the \$10 million, or the large sum of money, he wanted from the NSF—but he got grants from the NSF regularly, to allow him to have a postdoc and a person who works with him. So I think he's satisfied. I have good relations with him.

COHEN: So that was the biggest problem you had.

PECK: That was the biggest problem.

COHEN: But then you had Big Bear.

PECK: And I had Big Bear. Now, contemporaneous with what I have just described concerning the problems with LIGO, and particularly the NSF and MIT and all of those things, I had had a grievance hearing with a person against the director of Big Bear Solar Observatory. There had been a previous grievance that concerned all this, and the person was not happy with the way that the director was handling it. And also another grievance—I don't remember if it got to the point of a grievance or not, but it was nearly so, by another person who was involved there.

COHEN: So there were three different grievances?

PECK: There were three different grievances. It might have been that spring—I can't remember—that these people went to the Civil Rights Commission, or whatever it's called. They took it to the federal government, arguing that this was a violation of their civil rights. I'm not exactly sure I have the name right; maybe it was some agency in the Department of Education, but it was sufficiently serious that if Caltech had been ruled against, it could have led to the stoppage of all federal monies coming to Caltech. And boy, Tom Everhart knew it was serious! During the summer of 1994, I think it was, came the resolution of the Civil Rights Commission's investigation. They sent people out here, or somebody came out here, and they decided these people didn't have any real grievance.

COHEN: This was at the same time that LIGO was going on?

PECK: This was at the same time that LIGO was going on, and it extended probably a little beyond that. I know from looking at my notes. I kept independent notebooks. All the LIGO stuff was in separate notebooks, because I didn't want it to be mixed up with all the other things. And all the other books are full, too—they're dense! I mean, I was writing a book like this, every two weeks. A hundred and forty pages—there was just a huge amount of stuff going on! Man, that was baptism by fire! [Laughter]

Well, at any rate, what had been established by Gerry Neugebauer during the previous administration was a committee to make a recommendation concerning solar physics at Caltech. And this was probably chaired by [professor of physics] Ken Libbrecht. The committee's final recommendation to me—since I was the person in charge now—was that we should make a search for a new director. Hal Zirin was approaching retirement age, and he was *the* Caltech person. And what do you do whenever he retires? He's getting to that time of life and you've got to worry.

So they recommended that we do a search. We should make a thorough search for a person who would either come in as director or be suitable—you know, a young person who eventually, within a few years, would be in a position to take over the directorship.

COHEN: Ken Libbrecht was not interested.

PECK: Ken Libbrecht was not interested in being director. **[Tape ends]**

Begin Tape 5, Side 2

PECK: [Back to the resolution of the grievances. Although the case had not been ruled in favor of the complainants, the commission required Caltech to remove Zirin from his position as director of the observatory for about three months and a few other things. I don't remember the rationale behind this, but the investigation must have found some merit in the grievance and the action was punishment. I don't recall ever getting a formal report about the matter, although I must have gotten one. But I did manage to get Zirin to agree to step down for three months.]

I'm great friends with Hal—always had been. We used to talk about all sorts of things while many of these things were going on. But at any rate, he agreed to step down for three months and Ken [Libbrecht] became the acting director during that period. There were also questions of time-allocation committees—these were astronomers, so there were time-allocation committees and formal issues concerning use of the instruments. I wrote up all manner of things having to do with exactly how it ought to be done. I chaired one meeting of all interested parties in order to make sure everything was exactly kosher and nobody could bring complaints, as had happened previously.

What happened finally was that Big Bear was now, once again, under the directorship of Hal. Ken did not want to be director, but he was willing to run a search committee. So a committee was put together of people, all of whom were deeply interested in solar physics—Peter Goldreich, Roger Blandford, Ken Libbrecht, and others whom I don't remember. These were people who felt that solar physics was an important part of Caltech. I mean, it started us; that's what [George Ellery] Hale did. The committee came up with a recommendation for a young person who had a lot of interesting ideas. It was brought to the faculty, and the faculty voted it down. It was a tense meeting. Mostly astronomers were at the meeting; there were a few physicists there. I had no idea how it was going to turn out. But it turned out that it was impossible for me to recommend his appointment to the administration; there was inadequate support in the division for appointing this young person.

COHEN: And that was because they wanted to devote their efforts to the Keck Telescope?

PECK: Yes. The main thing was we have so much astronomy here. We have a huge problem in running the Keck Telescope—I'm sure different people had different views. Some of this must have been stated in the faculty meetings, but I don't remember the details. But the fundamental decision was that nighttime astronomy should be emphasized at Caltech. We should use our few positions in astronomy to strengthen nighttime observing—that is, in particular, we have two Keck telescopes to take care of. I don't know exactly how it came out, whether it came out in this meeting or not—but we should divest ourselves of Big Bear. Well, it was obvious from the very beginning that if we didn't have a director to replace Hal when the time came, that we should have previously divested ourselves and not have a crisis.

And so, with the help of people like Roger Blandford and others, I composed—or they composed, probably, and I signed—a letter that was sent to everybody in the United States who would potentially be interested in taking over the management of Big Bear Solar Observatory. Of all the letters that were sent, there were no more than two, maybe three, expressions of interest; and the one that was the most realistic—of course, connections had already been made—was with New Jersey Institute of Technology. There was a Korean postdoc here named Haimin Wang, and he had accepted a professorship at New Jersey Institute of Technology. Also, another person who was in solar radio physics here—the names are escaping me; I haven't reviewed the names recently—he got a job there also. The various people who had been bringing complaints eventually got satisfied one way or another. So eventually [1997], the observatory was turned over to New Jersey, after much negotiation, the legal parts of it being handled by Sandy—can't think of the name right now, but he really loves legalities. You know, you get one of these pieces of boilerplate that has a million words in it, all in three-point print? He would not only read it; he would understand it all. [Laughter] He was part of Caltech's legal team, and he was fabulous. He put together all the legalities to make it all work. I had a huge amount of help from Hal. Of course, the whole world community was—"Huh? What's Caltech doing?" I got lots of phone calls. There were phone calls from various scientific magazines that wanted to know what was going on. Well, what was going on was basically that Caltech decided it wanted to put its eggs into nighttime astronomy. Hal, eventually, I think, was quite unhappy about this. Initially, his first reactions—perhaps only shock—were quite modest and mellow, but I think later on he began to get angry.

COHEN: Oh, I think he was very hurt.

PECK: I can believe it. It was his life. It would have been disastrous for anybody. But my feeling was that in the long run it was the best way to support solar physics in the United States as a scientific enterprise. Caltech wasn't going to do it anymore. We didn't have support from the faculty, and they're the people who run the place, really.

COHEN: Well, it's a small faculty and it's not going to get bigger.

PECK: That's right. And so that's what happened.

COHEN: So, those were the high points.

PECK: Well, as best as I can remember right now. There were probably other things I'm forgetting about at the moment. Those are certainly the two most pressing things. LIGO was the fundamental problem, and with a huge amount of help from many people I was involved in the solution. Same is true of Big Bear. Once again, it was a large problem—not as critical as the LIGO problem, from the point of view of science overall and of Caltech in particular, but nonetheless something that seriously affected one of our senior and much admired members of our faculty, and it was extremely difficult but necessary.

COHEN: So then, things continued with the usual stuff?

PECK: Yes, the usual things went on. There was the usual business of appointments and arguing who should be this and who should do that, and what to do about space, which was always a problem.

COHEN: Now, was there the changing of the guard before you left? Was [Caltech president David] Baltimore coming to replace Everhart?

PECK: Yes, Baltimore came [1997]. I guess I overlapped with David about a year, maybe a year and a half.

COHEN: So you saw physics on the descendance, a little bit.

PECK: That's right. [Laughter] But the physics department made a strong effort to hire Ed Witten, and this was supported in the strongest possible way by David Baltimore. It was clear that attracting Ed Witten to come here would require not just the faculty and the chairman of PMA but it would require the active involvement of the president. David recognized that and he acted on it. I think the actual offer to Witten was made by David as he and Ed were walking around the grounds of the Huntington Library, as a matter of fact. Ed was on his way to a meeting in Santa Barbara and stopped by, at our invitation. And I'm pretty sure that David got to know him a little bit here and that started the process. Of course, there were lots of things that were involved, with regard to USC [the University of Southern California] and Caltech and the physics department, and so on and so forth. But David Baltimore was completely supportive in all of this; he was wonderful.

COHEN: So, you came to the end of your tenure.

PECK: Yes, so finally, after five years of agony, [laughter] my time on the cross was finished, and Tom Tombrello took the job and he loved it. [Laughter]

COHEN: He's just taken it on for another five years.

PECK: He's taken on another five years, right.

COHEN: And you went back to your research.

PECK: And I went back to research, and have been happily doing that. Although I have been deliberately removing myself and not taking on any responsibilities or students, or anything, because I expect to retire—well, I will retire very soon.

COHEN: But you continue your teaching, which you enjoy very much?

PECK: Well, I will teach formally one course this fall, and to some extent this spring. Just as I retire, I will then start teaching. But the fundamental reason is that—oh, it's complicated. At any rate, this will probably be the last course I teach, I would imagine. But I enjoy teaching, and so I look forward to that.

COHEN: So, you've been here all these many years and it's been OK.

PECK: Quite a number, and it's been OK. Yes, I've enjoyed it. I think Caltech's a great place. What a privilege to have seen and known all the people I have managed to interact with at Caltech over the years. It would not have been possible anyplace else: To have lunch with Dick Feynman quietly. To be in Murray Gell-Mann's classes when he taught quantum mechanics to us, when I was a beginning student in graduate school. I remember once Murray's voice kept getting lower and lower as the hour approached the end. And finally at the very end he said, "Everybody, walk out quietly." And so we all left the room quietly, except for one person, who was fast asleep at the rear. [Laughter] I think he actually woke up as he noticed the rustling about him, right close to the end. But it was a wonderful experience.

[Tape recorder turned off]