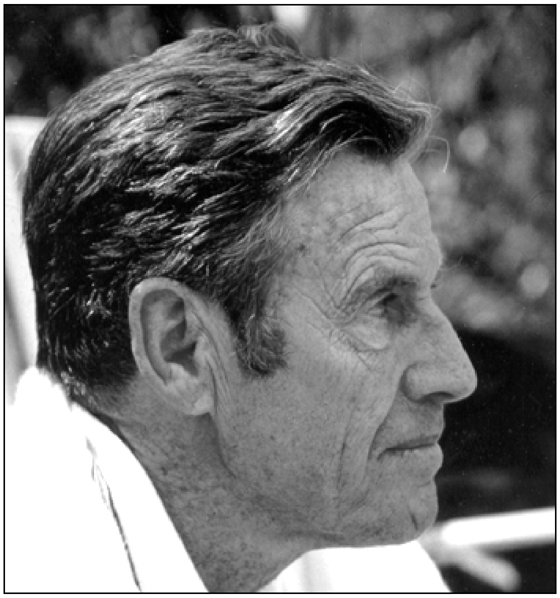

ROBERT F. CHRISTY
(1916- 2012)

INTERVIEWED BY
SARA LIPPINCOTT

June 15, 17, 21, and 22, 1994

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Subject area
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Abstract

Robert F. Christy was born in Vancouver in 1916, received his undergraduate education at the University of British Columbia, and took his Ph.D. degree with J. Robert Oppenheimer at Berkeley in 1941. He was an early participant on the Manhattan Project, working with Enrico Fermi at the Metallurgical Laboratory of the University of Chicago on the first atomic pile. In 1943 he went to Los Alamos as a member of the Theoretical Division under Hans Bethe, where he devised what came to be known as the Christy bomb, or the Christy gadget—the plutonium implosion device tested at Alamogordo on July 16, 1945.

After the war he returned briefly to the University of Chicago, where he and his wife shared living quarters for a time with Edward Teller and his wife. Caltech was then seeking to build up its theoretical physics faculty, and Oppenheimer, who was teaching there part time, recommended that the institute hire Christy. In 1946 Christy accepted Caltech's offer of an associate professorship. He worked chiefly on the application of theory to cosmic-ray experiments in particle physics, later moving into nuclear physics and astrophysics, including important work in the 1960s on the pulsations of RR

Lyrae stars, which are similar to but smaller than the Cepheid variables used as cosmic yardsticks. In 1967 this work earned Christy the Eddington Medal of the Royal Astronomical Society.

In 1970, Christy became Caltech's provost, a post he held for the next ten years. After Caltech president Harold Brown left to join the Carter Administration as Secretary of Defense in 1977, Christy was also acting president of the institute, until the advent of Marvin L. (Murph) Goldberger a year later. In the mid-1980s he became a member of the National Research Council's Committee on Dosimetry, which investigated the radiation effects of the Hiroshima and Nagasaki bombs.

In the interview Christy recalls his childhood in British Columbia; his undergraduate years at the University of British Columbia; his graduate work with J. Robert Oppenheimer at Berkeley; and his work on the Manhattan Project, first with Enrico Fermi at the Metallurgical Laboratory of the University of Chicago and then as a member of the Theoretical Division at Los Alamos. He recounts his wartime work on the critical assembly for the plutonium bomb ("the Christy bomb"); the Alamogordo test, July 16, 1945; the postwar concerns of ALAS (Association of Los Alamos Scientists); his brief return to the University of Chicago and move to Caltech; friendship with and later alienation from Edward Teller; work with Charles and Tommy Lauritsen and William A. Fowler in Caltech's Kellogg Radiation Laboratory; Freeman Dyson's Orion Project; work on the meson and RR Lyrae stars; fellowship at Cambridge University; 1950s Vista Project at Caltech; his opposition to the Strategic Defense Initiative; and his post-retirement work for the National Research Council's Committee on Dosimetry and on inertial-confinement fusion.

Administrative information

Access

The interview is unrestricted. A slightly different version of this interview was published in two parts as "A Conversation with Robert F. Christy" in *Physics in Perspective*, 8 (2006), 282-317, 408-450.

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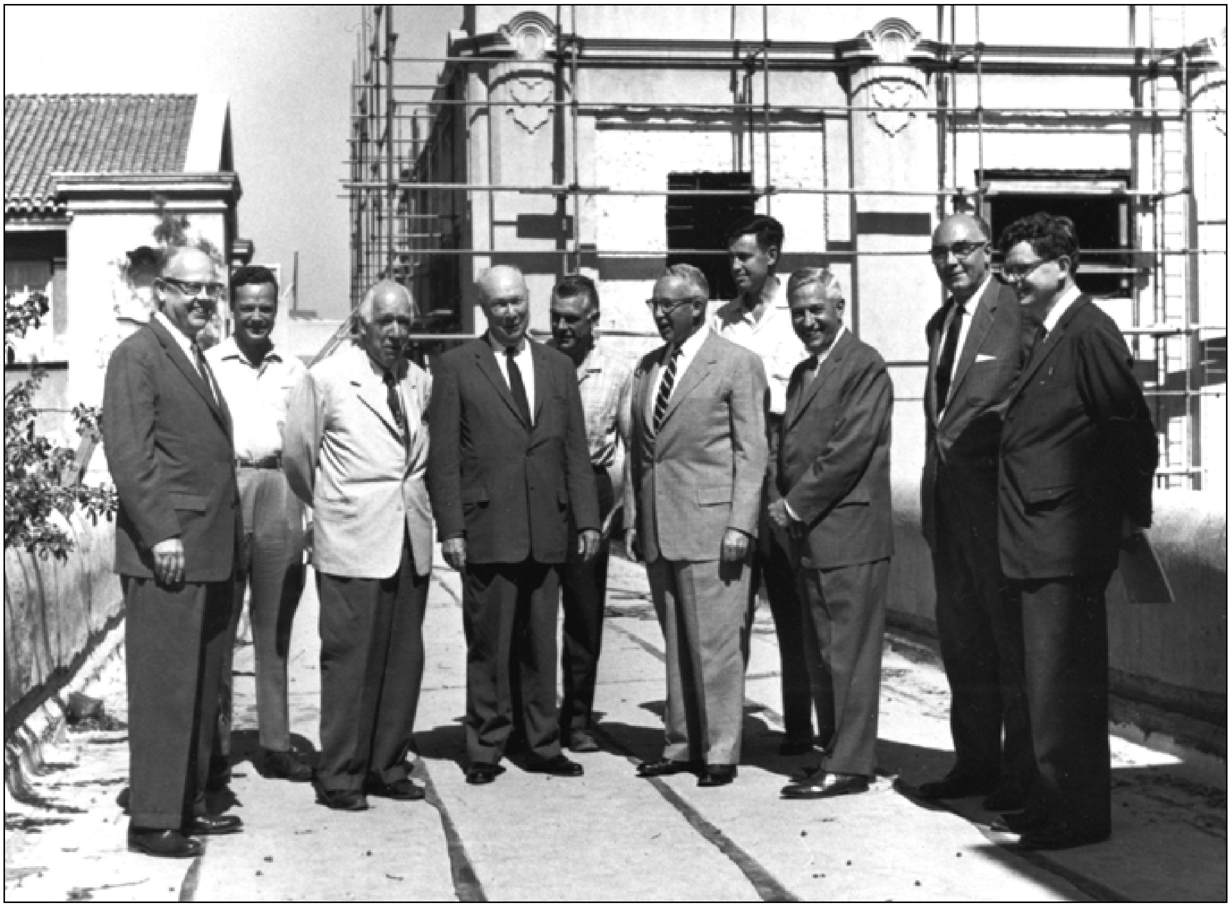

Niels Bohr's visit to Caltech, June 1959. The physicists greeting Bohr (*third from left*) are (*left to right*) William A. Fowler, Richard P. Feynman, William Houston, John Pellam, Robert F. Bacher, Robert F. Christy, Lee A. DuBridge, Carl D. Anderson and Thomas Lauritsen. Caltech Archives photo.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ROBERT F. CHRISTY

BY SARA LIPPINCOTT

PASADENA, CALIFORNIA

Caltech Archives, 1998

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Robert F. Christy **by Sara Lippincott**

Pasadena, California

Session 1	June 15, 1994
Session 2	June 17, 1994
Session 3	June 21, 1994
Session 4	June 22, 1994

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LIPPINCOTT: Dr. Christy, you were born in British Columbia, is that right, in 1916?

CHRISTY: Yes, Vancouver, British Columbia, on May 14, 1916.

LIPPINCOTT: What did your father do? What sort of family were you born into?

CHRISTY: My father was an electrical engineer. He got his degree at McGill. He was born in England and had come over to Canada. I think he was a second son, or something like that, and he had to make his own way. My mother was a schoolteacher before they were married. I don't think she taught after that.

LIPPINCOTT: Was she a native Canadian, or English too?

CHRISTY: No, she was born, I think, in Winnipeg, Manitoba. And her family had been in Canada for a couple of generations at least.

LIPPINCOTT: Did you have any siblings?

CHRISTY: A brother older than me by nearly three years.

LIPPINCOTT: Did he become a scientist as well?

CHRISTY: No. Things were tough, and he wasn't able to go to college.

LIPPINCOTT: Do you have any early scientific memories, any recollections of what turned you towards physics?

CHRISTY: I played around like the other boys, and I didn't do anything special in elementary school—except that in mathematics and spelling and so forth, I was better than most of them. And I skipped a couple of years. So by the time I got to high school, I was rather younger than most of the others. I just found mathematics very easy. And that's all I can say about it. I was not particularly fascinated by mathematics; it was just easy. I just did the work and usually got well ahead of the class—did all the problems in the books, because there wasn't anything else to do. I would say it was easy, and I just did it.

LIPPINCOTT: What about subjects like English or the arts?

CHRISTY: Oh, I was not so good. I understood the material perfectly well. But I could never write essay-type questions. Because, somehow or other, I never wrote the kinds of things they wanted me to write. So I got C's in English and history—until the end of high school.

But in high school, I liked mathematics. I was given an extra course in mathematics. They didn't teach trigonometry in my high school. And, in fact, there were only eleven grades, whereas it's now twelve. When I went through high school, there was no junior high school. It was just elementary school and then high school—eight grades of elementary school, which I did in six years, and then three grades of high school. So I had only eleven grades, and I skipped two of those.

As I was saying, my high school math teacher recognized that I had some special abilities in math. So he arranged to give me an extra course in trigonometry, which wasn't normally taught. And I did that; it wasn't hard.

LIPPINCOTT: Were there physics courses in your high school?

CHRISTY: Yes. I had a physics teacher, and he really interested me in physics. He came from England. And he had a peculiar way. If you asked him a question, he'd come back to you with a question. And he'd make you think of the answer. So somehow or other, I became interested. I took chemistry in high school and did very well in it, because it was factual material. Any factual material I did very well. I was not particularly turned on by chemistry—but by physics I was attracted.

I remember I would raise questions that sometimes were a little bit unusual. For example, I remember when we were talking about the refraction of light—which is bending light—I came up with a question that the class thought was very naive, which it was. I said, "Oh, is that the reason that if you're sitting in the bathtub and your big toe sticks up out of the water, the part that sticks up appears so much bigger than the part that's down below the surface?" And he says, "Yes." And the whole class burst into laughter, which is understandable. But I was interested in the physics of it. That turned me on.

Now, when I graduated from high school, times were pretty rough. I had no money to go to college.

LIPPINCOTT: This must have been around 1930 or '31?

CHRISTY: Somewhere around there, yes. I know I went to the university in the fall of '32—but I entered there as a sophomore.

LIPPINCOTT: There was a depression in Canada, as well.

CHRISTY: Oh, yes. But when I graduated, I didn't do anything very special. I got top marks in mathematics and science, and did fine in French. But I didn't do exceptionally well in English and history.

However, they changed the examination system. At that time, they introduced a grade twelve, which was the equivalent of first-year college—introduced it in high school. So I took the first year of the university in high school, because it was cheaper. I could live at home.

Then, in the examinations at the end of grade twelve, they gave us general exams—and this was given to the whole province of British Columbia—but the exams were all one-word answers: true/false questions or multiple-choice questions. And on that, I could do English and history. I could get top marks in anything, in that kind of exam. So I got the highest marks on these exams of anyone in the province.

LIPPINCOTT: Was this the Governor General's Gold Medal?

CHRISTY: Yes, the Governor General's Gold Medal. But it was solely because of the type of exam. It makes a big difference to a quantitative mind.

So because of that, I got a scholarship, which, I believe, paid my tuition at the University of British Columbia, in Vancouver.

LIPPINCOTT: Could you live at home when you were going there?

CHRISTY: I lived at home. The tuition didn't amount to very much—it was something like $250 a year. But that made the difference between my being able to go to college and my brother not being able.

LIPPINCOTT: And you were probably only about sixteen or so when you matriculated?

CHRISTY: I entered in the fall of '32 as a sophomore. Yes, I would have been sixteen.

So I went to the University of British Columbia, and proceeded to get very good grades there. Except in the first year, when I had to take some English and some economics. And as usual, I got C's or something. But everything else I always got top marks in. After the sophomore year, I didn't have to take any of those things anymore.

LIPPINCOTT: There was a lack of interest on your part, I guess, in English and economics.

CHRISTY: I enjoyed it, but why should I take something that the teacher doesn't think that I do well in? I enjoyed it. And in fact I took some after-class special things in English. But it was not for grades; I did it because I enjoyed it. But if I wanted to satisfy the requirements, forget it! It didn't do me any good.

LIPPINCOTT: Do you remember what some of those English courses were that you were interested in and you took?

CHRISTY: Oh, there was modern poetry. There was a group meeting in the evening. I still remember some of what I learned. As I say, I enjoyed it, but I could not give responses that the teachers were looking for.

I concentrated on mathematics and physics, because those went together very well. I enjoyed the physics and the mathematics, and it got to be more of a challenge as I went on—although I never found it very difficult in college.

LIPPINCOTT: Did you take courses in quantum physics?

CHRISTY: It hadn't filtered down to the University of British Columbia in that period. There was no one there who knew anything about it.

And I continued, and got my bachelor's degree there. I think I graduated at the top of the class.

LIPPINCOTT: And you were nineteen when you graduated. And then you went on to take your master's at the university?

CHRISTY: Yes. I went on to take a master's degree there. The preparation there was not the tops, and I had a much better chance of getting a fellowship at a good university if I had more training. So I took a master's degree there. Also, no money, no jobs, so you stay in school—stay in school as long as you can.

LIPPINCOTT: This was an effect of the depression?

CHRISTY: Yes. If the jobs had been around, I don't know what would have happened, since my family was very short of money, and my brother was working at whatever he could to help support them. You see, my father died when I was—two, I think. He had gone east to Oshawa, in Ontario. There was a power station there, and he was working at the power station. I don't know the details, but he was electrocuted. Then my mother was left without very much money and two young boys. All she had was a very small amount from workmen's compensation as a death benefit from my father, which was never enough. So her mother—my grandmother, Alberta McKay; my mother's maiden name was Hattie McKay—she moved into our house. We had a nice house in a nice part of Vancouver—a pleasant, three-bedroom house. My grandmother moved in. She was a widow; her husband had been a contractor. She moved in, and she lived with her brother and sister—they were named Wood. My great aunt worked as a secretary. And that was the only real income in the family. Her brother had tried various businesses from time to time, but mostly they failed. So he was basically uninvolved.

LIPPINCOTT: And fortunately, you had a scholarship.

CHRISTY: I had a scholarship. You see, the jobs weren't available. If jobs had been easy to come by, I probably would have had to work. But my brother was earning just a pittance in newspaper distribution. And it didn't cost me very much to bicycle off to the university. And it seemed to make more sense for me to continue in college.

When I got my bachelor's degree, again, the prospects were minimal, and it seemed reasonable to continue. Because, again, it didn't cost me anything. I got paid enough as a teaching assistant to cover any costs. And I took two years for a master's degree, which is unnecessary. But at the end of two years, I was more or less at a comparable age to other people going to graduate school.

LIPPINCOTT: You must have been fairly advanced to have gone straight to Berkeley to [J. Robert] Oppenheimer's group.

CHRISTY: Well, I was helped in two respects, I think. One was that I had the good fortune to have had some very good colleagues at the University of British Columbia. Our undergraduate classes were small—there were six or eight people, but they were very good students and they nearly all went off to graduate school in physics or math.

In the year ahead of me, there was a very good student—a Russian émigré named George Volkoff. He approached the question of going to graduate school in a very thorough, systematic way. He researched as to where the excitement was in theoretical physics: what's going on, and who's publishing the papers? And his research showed that a group at Berkeley under Oppenheimer was where things were really happening. So he applied there and somewhere else, too. But he really wanted to go to Berkeley. He brought to Berkeley the knowledge that there were some good students up in British Columbia. And he was the first of us, I think, to go to Berkeley—a year ahead of me.

So the next year, I applied there. I had a good record, and as I say, they knew Volkoff, and he was good. So they gave me, a fellowship there, which was $650 a year. It covered all my expenses for the academic year. I ran out of money at the end of the academic year, and then I had to come home.

LIPPINCOTT: Before we get to Berkeley, the two years in which you were a graduate student at UBC, I wanted to know a little bit about the theoretical physics that you did there, since you walked straight into quantum mechanics with Oppenheimer.

CHRISTY: Yes, that was pretty rough.

We did practically nothing in the way of modern physics at British Columbia, because no one knew any. We had a course in what was happening in modern physics—some of the experimental work—but nothing in the way of theory. No one there knew any nuclear physics.

But a bunch of us got together and we gave ourselves a course in quantum mechanics. It was based on a well-known text that had been produced by [Linus] Pauling here at Caltech—Pauling and [E. Bright] Wilson. So we gave ourselves a course in elementary quantum mechanics, which at least taught us something about the world, but not much.

Well, we went to Berkeley, and there we took whatever there was. And the quantum mechanics with Oppenheimer was very difficult. I would probably say that it was the first difficult course I had ever had.

The next year, another student came down from UBC—a Japanese student named

Shuichi Kusaka. We all came with, you might say, minimal preparation, but nevertheless we had a very solid background in the basics. Pretty good math. And we'd had, as I say, lots and lots of courses. I knew how to calculate monkeys climbing strings all over the place. That's the kind of mechanics courses that we had at UBC, to calculate the acceleration…weights on one end of the string, and a monkey is climbing the other.

We found things hard. We had to work when we went to Berkeley. But so did everyone else. We were not particularly far behind other students. There was just a very advanced group of the best students from all over the country there. And it was work.

LIPPINCOTT: How many were in Oppenheimer's group?

CHRISTY: Well, I never counted them, because there's all sorts of different stages of development. There were probably twenty people around—probably five or six each year. But the number actually was growing each year.

LIPPINCOTT: Was he your exclusive teacher?

CHRISTY: No, no. You took a regular set of courses. I had a course in electromagnetism from E. O. Lawrence, who was a terrible teacher. He was much more interested in what he was doing in the Radiation Laboratory. So he paid no attention to the teaching. He didn't prepare; didn't organize it. He was making his first or second cyclotron. But there were things that went on there that really opened my eyes to what physics was about.

Theoretical physics—I learned that through the Oppenheimer group, with him. But I remember there was a journal club there, and that club taught me what was going on in the world of physics. It was a seminar that met one evening a week for a couple of hours. And at the seminar, a series of different people would get up. They had been asked by the organizer—and it was Lawrence who ran the seminar—to report on this, report on that. These were recent papers that had come out in different journals—experimental work mostly. So we got very brief reports with discussion from the audience of what was going on in nuclear physics—in all of the exciting areas of physics, but mostly nuclear physics. This was a real education, because where I'd come from, no one knew anything about this world of physics, really. This was the flowering period of nuclear physics. And to be in the middle of the most exciting group in the country at the time when the field was just flourishing, it was a tremendous experience!

Julian Schwinger came as a postdoc, as far as I recall. Leonard Schiff came as a postdoc, NRC fellow, or something. They were all very remarkable. Leonard Schiff, who later wrote a well-known book, was an excellent teacher. Oppenheimer would organize these young postdocs to present a series of lectures on some subject, with Oppenheimer in the audience as a critic.

LIPPINCOTT: I've heard he was terribly hard on speakers.

CHRISTY: Yes. You quivered if you had to get up and speak in front of him, because he knew the difference between right and wrong, and he was not afraid to say so.

LIPPINCOTT: He didn't particularly think of the human feelings of whoever was . . .

CHRISTY: Don't know if he did or not. The fact is that he was focusing, I think, on the science. And if he didn't like the science or the way it was presented, he said so. He may have intentionally done some of this. That is, whether he understood the fact that people were shaking in their boots or not, I don't know. You see, the real world is rough. And he introduced us to the fact that the real world is rough. You've got to be in there and be right and on top of what you're doing. And if you're not, you're going to be trampled.

Now, a few of the people I knew, friends of mine, basically they couldn't take it, and they dropped out. The ones who could take it did very well. When you finally got through Oppenheimer's instruction—this whole business around him—you really could stand on your own feet.

LIPPINCOTT: Did you have any experiences like that with him?

CHRISTY: Not much. He treated me pretty nicely. But I remember, after I came here to Caltech, I presented an abstract at a Physics Society meeting. And you know, an abstract is a ten-minute presentation of something you have worked out. He was there in the audience as a visitor, and I had not had a chance to discuss it with him ahead of time. So he got up and said, "You're wrong!" Well, it turned out I was right. But you can't argue with Oppenheimer in public, because you'll be demolished, whether you're right or wrong. Anyone who he felt had said something wrong, he'd say so. He wasn't always right himself. All I can say is that it's rough, but you do learn to stand on your own feet. I know we ended up at Berkeley either hating him or admiring him, one or the other. And I was among the admirers. I would essentially have done, at that time and for many, many years after, I would have done anything for Oppenheimer. He was way up high on a pedestal, and I just felt that way about him.

LIPPINCOTT: It was his intelligence you principally admired?

CHRISTY: No, his whole personality, not just his intelligence. He could be so sweet and nice. He had a most amazing personality. I have a hard time describing—that's why I couldn't do well in English; I have a hard time describing things. But it wasn't just because he was intelligent. He could be very gracious—exceedingly gracious.

LIPPINCOTT: Were he and [E. O.] Lawrence good friends?

CHRISTY: Yes, they were. Lawrence wasn't an intellectual. Lawrence was a totally different type of person. He was a doer. Oppenheimer was an intellectual. We were introduced to all sorts of things in Oppenheimer's company. Every once in a while there'd be a party—often at his house. And all of the people would go. Well, I was introduced to martinis at his house. He was a martini drinker; and after that I used to enjoy having a martini.

LIPPINCOTT: Were they five-to-one, or three-to-one, do you know?

CHRISTY: I believe it was three-to-one. And most of the people I knew became martini drinkers. We weren't heavy drinkers, but nevertheless that was the drink of choice. Always gin.

He had many other friends who weren't from physics, too. And at some of the parties, we met some of the other friends. Some of those names have appeared in the Oppenheimer hearings. One was a French teacher. His name was Haakon Chevalier. We met him. Later, at the time of the hearings, it seemed as though Oppenheimer had denounced him in some way.

And I don't know what that was about. All I know is that he was a French teacher whom I had met there, and I know nothing more. And we met other people, too.

LIPPINCOTT: He had an interest in the arts.

CHRISTY: He had an interest in many, many esoteric things. And he had an interest in left-wing causes. That was the time of the Spanish Civil War. That was a cause he was interested in. He was opposed to the fascists in Spain, and he was a supporter of the communists.

LIPPINCOTT: Yes, but not one himself.

CHRISTY: I do not think he was himself. But he was a supporter. He would help organize benefits for them. Because he had a quite prominent position, he could have big parties, and people, I presume, would donate. Now, we graduate students weren't donating, because we didn't have any money. But he knew other people. So as I say, it was a broad milieu that we were introduced to—not just physicists.

LIPPINCOTT: Were your friends about that time mostly physicists?

CHRISTY: Well, I knew some girls in other parts of the college. You see, there I had sometimes different living arrangements. The first year I was there, a group of us—four of us—Volkoff, Christy, MacKenzie, we were all from UBC. Ken MacKenzie was an experimentalist who later went to UCLA. And Bob Cornog was another experimentalist, who came from I don't know where. We all "batched" together—except it was not quite batching, because Ken MacKenzie was married. His wife was a nurse.

I remember one period in which—and I forget exactly when it was—when Volkoff and I shared a double bed. We arranged the bedding so that there was my side and his side, and the two didn't join. [Laughter] Well, we batched and we did our own cooking, and so forth.

Then the following year, I branched out, and I got some kind of partial support for living at the International House at Berkeley. If you've ever been to Berkeley, it's sitting up on a hill there in back of the campus. It's really a beautiful location, and it's very convenient. I was a busboy; I learned to balance a tray. I was a busboy, and I got my meals for free, I think. Anyway, it was partially subsidized.

I met a very broad group of people there. And there my friends were mostly people from other fields, not just from physics. I knew a philosopher and enjoyed his company. Met a few girls.

LIPPINCOTT: [Robert] Serber was there when you were there.

CHRISTY: Serber was there as a kind of permanent assistant to Oppenheimer. He was there year after year. [Julian] Schwinger came and Schwinger went; he was there for only a couple of years.

LIPPINCOTT: Was Schwinger an exciting sort of person?

CHRISTY: No. [Laughter] Schwinger was a very strange person. His normal hours were to get up around 3:00 in the afternoon, maybe 4:00 or 5:00. For a while, when he was giving a series of lectures to us, I was delegated to go to his apartment at 3:00 and make sure he got up so that he could come to the lecture at 4:00, or whenever it was. But he lived that way. He was very pale. And he always seemed to be looking somewhere else. He didn't want to look you in the eye. I suppose he must have been shy or something like that. But most of us were shy. He was very young at the time—I think he was about the same age I was, and he was a postdoc. But he liked to work by himself in the late hours. He was very pale because he never saw any sunlight at that time. He didn't exercise, that I know of. There was a major transformation in Schwinger when he came and took a job out here at UCLA, because suddenly he began to see sunshine, and he began to indulge in some exercise. But at that time, he was exceedingly introverted and brilliant and withdrawn.

LIPPINCOTT: Would you say he was the most intellectual of the group, or just different?

CHRISTY: Oh, there were a lot of smart people there. You may or may not have heard of Willis Lamb. Willis Lamb was one of the young men there.

LIPPINCOTT: Where did he come from?

CHRISTY: I know he went to Columbia eventually. Don't know where he came from. But he was a very shy and bright person, too. And you may or may not have run into the name Phil Morrison. Phil was a year ahead of me. He was one of Oppenheimer's students. And he had a very sophisticated background. He was quite strongly left-wing; I don't know how much involved he was. He had a fairly deep involvement with the left wing.

LIPPINCOTT: You weren't terribly political at that point.

CHRISTY: I was not particularly political. I was a Canadian citizen. That didn't make any difference. I did things, but I mostly was not very political. I went to meetings sometimes because other people were going. But I never really got terribly involved.

LIPPINCOTT: Emilio Segrè was at the Radiation Lab about that time, too, I think, with Lawrence. Do you remember?

CHRISTY: Well, I probably met him. I didn't know him quite as well. I met Luis Alvarez, who was there. He was one of Lawrence's young men, and a very strong physicist. He impressed one more than Segrè did. Ed McMillan was there. In fact, he took over and taught my electromagnetic class after Lawrence kind of gave up. Ed was a very, very good physicist. These are people that you perhaps know of. Most of them, or many of them, are Nobel Prize winners. So I say, it was quite a remarkable group.

LIPPINCOTT: Now, Oppenheimer used to come down to Caltech for the spring term every year. Did you come along?

CHRISTY: Yes. The last two years I came along. I was there as a graduate student for four years—from '37 to '41. The first two years, I think I came home to Vancouver and spent the summer in Vancouver doing whatever I could. I remember I spent one summer with my old high school friend Oliver Lacey, who was an excellent tennis player. And I played a fair amount of tennis with him. In order to make it more even, he would use his left hand. We even joined the fanciest tennis club, because somehow or other there was something for the summer and you could join for almost—I forget what it was. This was the Vancouver Lawn and Tennis Club, where you got to play on grass courts, which is unusual. You know, in Vancouver at that time, the courts were either clay courts or grass courts—concrete didn't exist.

LIPPINCOTT: Do you play tennis now?

CHRISTY: Oh, yes, I play it quite regularly. That is my game. I do other things, too. I ski. But tennis is the one I do most regularly.

LIPPINCOTT: Tell me about coming down to Caltech. That would have been your first exposure to Caltech.

CHRISTY: That was probably the summer of '39 or '40.

LIPPINCOTT: Did many of Oppenheimer's students come down?

CHRISTY: Yes. It was customary, because Berkeley, being on the semester system, started in the latter part of August; and the first semester was over at Christmas. And then the second semester, at that time, was over early in May. And that was just about in time to come down to Caltech. They would leave, I think, in late April, and come down. Oppenheimer would teach a course down here. So a whole bunch of us would pile into cars and come down, find some place to live, and we'd attend his classes here, too.

Oppenheimer, you know, was just learning how to teach. And before I was there, his classes were very, very difficult. He had not taught many years, and he had not learned how to really communicate well. He was learning all the time, and he got to be a fantastic communicator. So his courses were very difficult. And what people did before I got there was, they would take the course and listen, but they couldn't get it all down. Because if you listened, you couldn't write; and if you wrote, you couldn't do it all, because it went so fast. Sometimes there would be a group; one person would be delegated to write and the others to listen. And then they'd get together afterward and try to make out what he'd said and get a set of notes written. This was on theoretical physics. [Tape ends]

Begin Tape 1, Side 2

CHRISTY: It was customary, before I got there, for people to take his course twice. In one year, they would kind of get the general gist of it, and the next year they'd take it again and they would really get it.

But he decided that wasn't fair. Because what happened was he would see these faces in front of him that had had the course before, so he had to raise the level of the course in order to give them something to do. So his course got more and more difficult each year, because he was always giving it to people who'd had it before. So he decided to make a change. When I got there, he said, "No more! You take it once only!" And I guess he toned the course down a bit, so that it became possible to do it. And that's the way I had it.

He gave problems that were basically small research tasks. I think this was the way he sorted people out, as to who could do and who didn't. Some of these problems would take you a week or two to do. So by the time you got through his course, you had an idea where you stood and he had an idea where you stood. It was very solid training.

LIPPINCOTT: I believe you did seven papers when you were there with him, mostly with [Shuichi] Kusaka.

CHRISTY: With Kusaka. I also wrote one with Joseph Keller.

LIPPINCOTT: I have a list of your papers, and I noticed that most of them seem to have to do with gamma rays and what was then called the mesotron.

CHRISTY: Yes, that's right. The one with Keller was basically his thesis. And it was something on atomic structure. We had to do a terrible amount of calculation. And I was essentially Keller's assistant; he was a year ahead of me. I helped him with the computation. We did all this on these hand-operated calculating machines, punching them for weeks on end.

LIPPINCOTT: Didn't they use those sorts of machines at Los Alamos, too?

CHRISTY: Yes. Then I did some papers with Shuichi Kusaka, who had come from British Columbia. We were friendly but not friends; we were colleagues. The work at that time was—we did two kinds of things, both of which were my thesis. And he was, you might say, an assistant to me on my thesis. We both made very, very difficult calculations independently. Then we'd compare. Because when you do a very lengthy difficult calculation, you don't know whether you've made mistakes. So this was a way of making sure you didn't have mistakes.

LIPPINCOTT: You were trying to pin down a meson?

CHRISTY: Yes, we were trying to learn the spin of the meson, which was called at that time the mesotron. But my thesis was vindicated when…At the time I worked on it, the meson—the mesotron, as it was called—was thought to be responsible for the nuclear forces. But as such, it had to be a very strongly interactive particle. And yet the mesotron that was observed in cosmic rays seemed to penetrate long distances and do nothing. So this was a mystery. Nevertheless, the mesotron was thought to be responsible for nuclear forces. And people figured that it had to have spin one, because of the nature of nuclear forces.

So I calculated what a spin-one particle would do interacting with the atmosphere. Then later on I compared that with experiments and found out that the experiments in cosmic rays did not agree with what you would get from a spin-one particle. So I concluded that it had to be spin-zero, because everyone knew it had to be either zero or one. But then I pointed out that as far as the cosmic-ray experiments go, it could be either spin-half or spin-zero. Later, it was found that the meson in the cosmic rays is spin-half. So that was consistent with what I had. As I say, everyone believed, when I did it, that the meson had to be either zero or one—I threw the half in because it hadn't been disproved.

I got interested in cosmic rays then, because that was the principal source of high-energy interactions. I also became interested at that time in seeing how the theory applied to the experiment. And that's something that has stuck with me through my whole life—this bridging between theory and experiment.

LIPPINCOTT: So you, in some sense, were experimental as well.

CHRISTY: Well, I was interested in experiments, and I occasionally would hang around the lab. I was interested in experiments, but I was a theorist. As I say, I was an unusual theorist in that my greatest strength was not in creating new theories, which is what [Richard] Feynman and Schwinger were very good at, but rather in seeing how theory and experiment related. This is probably what I did best.

ROBERT F. CHRISTY

SESSION 2

June 17, 1994

Continue Tape 1, Side 2

LIPPINCOTT: You said you wanted to correct a few things.

CHRISTY: Yes. I mentioned that my father died when I was two. But I indicated that my grandmother and her brother and sister had moved in with us at that time. That wasn't so. My brother and I lived with our mother, until I was ten. And then my mother died. And *then* my grandmother and her brother and sister moved in with us. So during the period from when I was two to ten, we lived with our mother. And then my grandmother and her brother and sister came to live with us. We had a fairly nice house. They had a house at the time down in central Vancouver, more convenient to the downtown area, but our house was more suitable for young children. And I suppose that's why they moved out to our house. I don't know.

You also asked if I had done anything special about science when I was young. And I've often thought it was a little bit odd, because I just had a normal childhood as far as anything outside of school was concerned. I was a normal boy and did what boys did—played and had fun. I didn't do anything special as far as studying. I didn't study outside of school particularly. I did read fairly extensively, but I didn't read science, the way a number of young people did. I just read whatever I found in the house, mostly Dickens. It was nothing to do with science, and I knew nothing, basically, about science. I didn't know that it existed. My total knowledge of it at that time was that when I finished high school, I admired my mathematics teacher. And I thought maybe I could get to be a mathematics teacher some day. I didn't learn anything about science, didn't know science really existed as something one did.

LIPPINCOTT: Did you have thoughts about nature?

CHRISTY: Yes. And I mentioned one issue that I brought up in a physics class. As I say, I was probably more observant about nature. But I didn't read about things in books. So I seem to have been rather different than other young people who went into science.

My brother and I did make ourselves a crystal radio. We did tinker, such as we could. We didn't have much instruction, you see—just my mother. But we repaired our bicycles and kept those in good shape—and such things. But we didn't go beyond that particularly.

I also thought I should mention to you some summer jobs that I had. I had four summer jobs that I got into—this was during my years in BC, mostly, and little bit after I went to Berkeley. The first one—and really a very good summer job; this was probably before I graduated from the University of British Columbia with my bachelor's degree—my aunt worked as a stenographer. And her boss apparently had some outside connections with mining operations. So she persuaded him to give me a chance as a general handyman around a placer mining operation. That's where they take these great big nozzles and shoot [the ground] and wash it all down. So I spent a summer working at that mine in central BC, as a general handyman. This was the first time basically, I'd ever been outside of Vancouver, except for the immediate suburban environment. It was in the area not far from Williams Lake.

LIPPINCOTT: What kind of ore were they mining?

CHRISTY: They were looking for gold. But it was one of these crazy ideas that someone had—that the old route of a creek lay there, and if they could just get down into the old place where the creek used to flow, they would get gold. Well, we washed away great mountains of earth and found essentially nothing. But for me, it was a useful experience. I was out, active, and I did odd jobs. I was supposed to assist. And I went to town to bring back provisions. The camp was about five miles up a lake from town. So I would take a big scow with an outboard motor and drive it down the lake and get provisions. And then I would haul those aboard and drive them back up the lake to the camp. I was sent off to cut trees. They made riffle blocks, which are big blocks of wood, down at the bottom of the sluice boxes, which have cracks in between. So when the gravel washes over this, the gold gets caught in the cracks and lodges there. So I would be sent off to find suitable trees and cut them down with a cross-cut saw and saw them up. I learned a fair amount about handling myself in the woods, with saws and axes and so forth—a healthy experience.

LIPPINCOTT: But it didn't turn you into a geologist.

CHRISTY: Not at all. It didn't turn me into anything. It just gave me my food and a tiny bit of money for the summer. That's all. I think I was given forty cents an hour.

The next summer, I got the best summer job that I ever had—and can't imagine a better one. It was for the Canadian Geological Survey, doing topographical mapping—that is, mapping the contours of the country. There were several parties out. And the main reason this was done was to try to employ some university students during the summer. My first job was to set out with another young man. It was a party of maybe ten or so people. And we traveled around by great big canoes with outboards on them, on all sorts of lakes, which are now known for being salmon fishing resorts. It was just empty country. And we would hike out each day—this companion and myself, and there were several parties—and we'd be told, "Follow this creek," or "Try to go up toward this mountain." And we had to judge the distance we had gone by counting paces. And we had to practice, so that we were fairly accurate at measuring distance as we walked along. We had a pocket altimeter to tell us the elevation we were at. And we had a little board with a compass on it, which we would set up every half mile and orient it with the compass. And then we would draw on this paper, on top of this table, a little line indicating the direction we had been going. And we'd keep track of our directions from the sun, and whatever other means we had to keep track of the direction we were walking.

LIPPINCOTT: Was this all uncharted territory?

CHRISTY: Yes, we were mapping it.

LIPPINCOTT: You could have gotten lost, couldn't you?

CHRISTY: We never got that far from camp. About six miles was the normal distance you could travel out and make a map and then come back. But when you came back, you didn't come back the same way you went, because you were following some particular trail or creek or something, wherever it led you. And when you came back, you'd come back the shortest way you could think of. And since we were camping on a lake, you headed for the lake. But you then realized one important thing—that you have to know, when you hit the lake, whether camp is this way or that way. Because if you hit the lake and you go this way, and camp is that way, then you're in real trouble. So you learn a lot about basic orientation in doing this—about keeping track of where you are. If you didn't show up, then someone would go out looking for you, and sooner or later they'd find you. But I didn't get lost. People didn't get lost. Because you are making a map, so you've got a pretty good idea of the terrain.

So I was hired, as I recall, at $90 a month. And then, when I was halfway through, I was told that I had got a raise. I was made kind of the lead man of a group. So I got a raise to $120 a month. And to be paid $120 a month, free and clear, to go and hike in the wilderness, that's fantastic! You can't imagine a better summer job. So I became very fond of hiking and walking and climbing. We climbed mountains. We covered the country. We were trying to map a fairly large piece of country.

LIPPINCOTT: Just for comparison, you said that the annual tuition at UBC was $250.

CHRISTY: Yes, I think so.

LIPPINCOTT: So that was a great chunk of money in those days.

CHRISTY: Oh, yes. I mean, you had your keep, and you came back with some money in your pocket. That, as I say, was a first-rate summer.

However, that didn't last. I guess the government ran out of money or something. So the next summer, I couldn't find anything. And I finally got hired on by a government crew that was essentially a gang of unemployed people off in the middle of BC repairing some old railroad spurs. But they were employed under the unemployment program.

LIPPINCOTT: A bit like the WPA in this country?

CHRISTY: Yes, a little bit like that. And I was hired, not because I was precisely that, but supposedly to work with them during the day, and then at night I was supposed to teach them English and such things that would be useful to them, because they weren't very well educated.

But I found, after working ten hours a day at manual labor, that I had no energy left come evening. I was paid twenty-five cents an hour for that. So I got $2.50 a day. But out of that I had to pay back $1.25 for my room and board. So I ended up with $1.25 free.

So in the evenings—since they had no energy to learn and I had no energy to teach—what they did was, they played poker. And not being a gambling person myself, and not knowing how to play poker, I didn't play with them, but I watched. So I learned a little bit about how you play poker.

LIPPINCOTT: Are you good at it now?

CHRISTY: No. As far as I'm concerned, poker is a gambling game, and if you don't play it for real money, it's not the same game. And I never had a desire to play for real money. I have told my children that I learned the perils of gambling when I was six years old. I had a bag of marbles, and as you know, you play marbles for keeps. There are certain games of skill with marbles. And if he wins, he takes your marble, and if you win, you take his marble. Well, I played marbles for keeps, and I lost all my marbles when I was six years old. And I was very unhappy. And I've never gambled since. So I learned the lesson cheaply. But I tell my children that I lost all my marbles when I was six years old.

So I came home after six weeks of that. I wired home and said, "I can't take it. I'm not getting anywhere. And I'm not going to make enough money to do anything." So they sent me money to get a bus ride home.

Then I tried—I think it may have been the same summer, or it may have been a different summer—I tried another job. I'd read about how you could make money selling encyclopedias. So I tried going door-to-door selling encyclopedias. I tried that for several weeks. I think I sold two sets of encyclopedias and made ten or twenty dollars in several weeks of work. I concluded then something that I've believed ever since. And that is, I'm not a salesman. I can tell people the truth, but I can't sell something. So I learned something—that I'm not a salesman and I'm not a manual laborer. [Laughter]

So, these were additions to my experiences.

LIPPINCOTT: We got up to where you're almost ready to go to Los Alamos. But before we go on, I wanted to talk a bit more about your coming down to Caltech with Oppenheimer while you were still a graduate student. I wondered if you met Richard Tolman and Charles Lauritsen.

CHRISTY: I met them at that time. Tolman was a very wise, sophisticated, and thoughtful person. And Charlie Lauritsen, he was also wise and thoughtful, but he didn't always give you that impression so much. What I remember most of Charlie is the parties they used to have every Friday after their seminar at Kellogg—they had the party usually over at Charlie's house.

LIPPINCOTT: Did both of those men work in Kellogg?

CHRISTY: No. Kellogg was Charlie Lauritsen's laboratory. Tolman was basically a theoretical physicist, or theoretical chemist—I think he was in the Chemistry Department. But Willy Fowler worked in Charlie Lauritsen's laboratory, as did Charlie Lauritsen's son, Tommy, later. Willy Fowler had just graduated. So I got to know both Charlie Lauritsen and Willy Fowler. And that was I guess important later, when I came to Caltech, because it was their invitation that brought me to Caltech.

LIPPINCOTT: What were the Friday parties like?

CHRISTY: Oh, singing songs, having fun, drinking martinis, occasional games, but mostly conversation and some drinking.

LIPPINCOTT: Would Oppenheimer and his students go to those parties?

CHRISTY: Yes.

LIPPINCOTT: And then in 1941, you received your PhD. Your dissertation was regarding the meson spin?

CHRISTY: Yes, I think I told you something about that last time. Actually, I could have gotten my degree a year earlier. Oppenheimer told me that I could have my choice: I could write my thesis and get through a year earlier, or I could write a better thesis and wait a year. But he said, “There are no jobs available if you get your degree now, so you might as well stick around.” So I took that advice, and I stayed an extra year, and was able to write more papers. My thesis actually consisted of two papers, bound together. The papers were published at that time. They were both written with Kusaka: the first one was on the interaction of mesotrons with nuclei, and the other was the experimental accounts of what had happened. I wrote them, but Kusaka worked with me, kind of checking and verifying; but it was essentially my independent work, but he was helping me with it. And those two papers were all that my thesis consisted of, really.

And then, instead of no jobs, there was one job, at Illinois Tech. And one job is better than no jobs. By the way, at that time, I was married. Just after I got my degree, I married a girl I had known in Vancouver. And we were married in Vancouver. She came to Berkeley with me for the rest of the summer. And then we drove east.

Joe Keller had a job at Washington University, in St. Louis. He was married, and I think maybe he had a child by that time. Anyway, I was commissioned to drive his car to St. Louis. So I drove with my new wife to St. Louis, in Joe’s car, and brought it to him there. And then we took the train or something from there to Chicago.

LIPPINCOTT: Is that where the Illinois Institute of Technology was?

CHRISTY: Yes. In Chicago. Do you know Chicago? Because it was a shock. Illinois Tech was on the South Side of Chicago, not far from the stockyards. The Illinois Central tracks ran right through the middle of the campus. And they were coal-burning locomotives. So everything was covered with coal dust. And I soon heard stories from my colleagues there about being held up at knifepoint at night on the el tracks—there was an el station there—and having their overcoats taken, or some such thing. It was a very poor part of Chicago, a black slum.

I had supposed, in my naiveté, that we’d find a place to live near Illinois Tech. I soon found that you don’t do that—in fact, you don’t appear around there at night at all. So, Illinois Tech being on the South Side, the nearest semi-acceptable region was farther south, in the area of the University of Chicago. So that’s where we ended up looking. And we found an apartment within about a half a mile of the University of Chicago, which was a very decent neighborhood.

LIPPINCOTT: Was Serber at the Illinois Institute of Technology?

CHRISTY: No, Serber was at the University of Illinois. He got a job at the University of Illinois in Champaign-Urbana. And another one of the [Oppenheimer] group, who was a year ahead of me, got a job at Champaign-Urbana, too; that was Sid Dancoff. He was a good friend of Phil Morrison. Now, where did Phil go?

LIPPINCOTT: So none of your colleagues were at the Illinois Institute of Technology?

CHRISTY: Oh, no. I knew no one there. I was hired as an instructor. Instructors are usually given fairly heavy teaching loads. And since this was the first time I had ever taught, I had to learn the material myself.

LIPPINCOTT: How did you like teaching?

CHRISTY: I found it perfectly all right. I don't think I ever considered myself a brilliant lecturer. But to organize material and present it—I liked to do that. So I think it was reasonably successful. It was very difficult—a lot of work. You see, I couldn't just repeat things that I had been taught, because you had to adjust it to the needs of the school and the students' level. And I was asked to teach someone there—I forget who—who had decided to take a reading course in electromagnetic theory. And this course consisted of doing problems, and I was supposed to show him how. It turned out that these problems came from a famous Caltech course—William Smythe's course—which consisted basically entirely of very tricky problems in electromagnetic theory. And this was a course well known, everywhere, as one of the hurdles to getting a degree at Caltech. So I found it difficult, because I had to learn these problems in order to instruct this young man.

Two of the students there—the Latter brothers—were very smart young men: Richard Latter and Albert Latter. Albert ended up over at the RAND Corporation, in Santa Monica. And he did exceptionally good work for them for a long time. He was one of the leaders over there. Dick I had as a student here at Caltech. So I remember those two particularly. So you go to any out-of-the-way place in this country, and you'll find absolutely outstanding young people there, trying to learn.

LIPPINCOTT: This was 1941. And you probably weren't there very long when you were asked to come to the Metallurgical Laboratory?

CHRISTY: That's correct. I joined Illinois Tech in September—I guess it was—of 1941. And I worked there through the fall quarter of '41. But I needed some intellectual stimulation, and there was nothing for me at Illinois Tech, because there was no one there who knew as much physics as I did.

So I found that it was convenient to go to seminars at the University of Chicago. And I attended seminars there in fields of my interests, including cosmic rays and such things. I had to keep in contact with physics to find out what was going on and to do science. But I didn't really know anything was going on about atomic energy. Actually, when I was in Berkeley, there had been a number of people leaving one by one to go to MIT. Lawrence and McMillan—they both disappeared and went to MIT. But I didn't know what was going on. I think, because I was a student, people were circumspect. They didn't talk about things that need not be publicly talked about. So all of this was going on, and I didn't know it. I knew they went away to work on some kind of war project, and that was radar. But I knew absolutely nothing about the atomic energy project—although there had been some work going on in Berkeley already, I think, in the summer of '42. They had a conference there.

LIPPINCOTT: Did you go to that?

CHRISTY: No, I wasn't there. I knew nothing about it. I wasn't invited. And reason enough for that, I suspect, is that I was a foreigner; I was a Canadian. And I guess I was much more naive than other people, because other people claimed that they immediately thought of bombs and chain reactions and all this stuff as soon as they heard about fission. Whereas, it didn't occur to me. I mean, fission was an interesting scientific phenomenon, but I must say that it never occurred to me that you could make chain reactions, and that the chain reaction would lead to the release of nuclear energy.

LIPPINCOTT: But when you were going to the University of Chicago for seminars, you must have [thought of it].

CHRISTY: No, I knew nothing.

LIPPINCOTT: Was [Enrico] Fermi there then?

CHRISTY: No. Basically, it had not been organized at the University of Chicago. There was work at that time going on at Columbia under Fermi, and going on at Princeton, with [Eugene] Wigner. But there wasn't any work at Chicago. Chicago was chosen as a center, but not because there was already work going on there. They started to set it up, I think, sometime in the fall of 1941. They were recruiting people, just barely getting going in the spring of '42.

LIPPINCOTT: And the spring of '42 is when you began going to seminars there.

CHRISTY: That's right. And then they thought of hiring me, because I was in the area and I had a background in nuclear physics, generally knowledgeable. So they invited me to join the project.

LIPPINCOTT: Who is the "they?"

CHRISTY: It would have been either [Samuel K.] Allison or Wigner. And I think probably Wigner, but I'm not really sure. He knew I was a student of Oppenheimer.

So they invited me down and told me something about the project, about what they were doing. What I was told was that they were trying to make a chain reaction. And the purpose of the chain reaction—well, it might be to drive submarines, or whatever. I was not told about bombs at the time. Again, I didn't really think of bombs. Because a chain reaction was a very unwieldy thing; the reacting pile was about as big as this room—not something you could use as a bomb. But, as I say, it sounded exciting, and I would be there with famous physicists to work with rather than in the sticks working by myself. So there was no question; I wanted to join them. I said, "Yes, sure, I'll join you."

So they told me essentially what was going on at Chicago but not at Los Alamos. And they didn't tell me about bombs.

Then they found out, when they tried to get more data on me, that I wasn't an American citizen. So they put through a quick clearance on me. It didn't bother anyone very much.

LIPPINCOTT: At some point you did become a citizen. If we could just digress, when was that?

CHRISTY: Well, I put in for permanent residence, I believe, when I first came in 1937, or maybe later. But it took five years of residence to become a citizen, and I wasn't eligible until around '43. And it was at Los Alamos, sometime in either '43 or '44, that I went before a judge in Santa Fe and got my citizenship… [Tape ends]

Begin Tape 2, Side 1

LIPPINCOTT: We were talking about the Met Lab. Was that a euphemism of the Manhattan Project?

CHRISTY: Yes. The Manhattan Project encompassed many different operations, and the Met Lab was the Chicago part of the project. But there was, when it got started, there was the Los Alamos Lab, there was the Oak Ridge Laboratory. The Manhattan Project also encompassed the reactor building up at Hanford, Washington. It encompassed the Columbia project, where the isotope separation was worked out. So there were many different operations under the Manhattan Project, which was kind of the general contracting organization.

LIPPINCOTT: And the Met Lab was begun specifically to build this pile?

CHRISTY: The idea was that Chicago, for whatever reasons, would be a good place to design and build a chain-reacting pile.

LIPPINCOTT: What kind of work did you do?

CHRISTY: Well, I did two kinds of work. I did quite a bit of work for Wigner—who, you may or may not know, was a very famous Hungarian theoretical physicist. Eugene Wigner. And I did work that he proposed, like working out details of the lattice. The idea was that the uranium is arranged in what is called a lattice. And uranium is surrounded by graphite. That's not the same as having it uniform. It's different when you have the uranium in lumps. So I was supposed to work out lattice constructions.

Now, I don't know why Eugene had me do that, because I had never done that. It's something that a solid-state physicist would know how to do in his sleep, but I'd never done it. And Eugene knew all about these things. Anyway, he asked me to do it, so I worked at it. And I learned a lot. When you're asked to do things you have never run into before, you learn a lot.

LIPPINCOTT: The graphite was supposed to slow the neutrons?

CHRISTY: It slows the neutrons down. And it does most of the slowing down, but occasionally they will interact with the uranium. They may be absorbed, as in a so-called resonance absorption, which does not make fission but makes uranium-239. That was not particularly useful. Uranium-239 decays to neptunium and then plutonium-239. And that didn't help the chain reaction. If you make too much plutonium and not enough neutrons go on to make the next reaction, you may have something that looks beautiful but it won't chain react. So you have to have a balance. You have to have one neutron saved to make the next fission. And then, any others you can use for useful purposes, that's fine.

I did calculations related to constructing a lattice—calculations related to engineering. I'd never learned any engineering before, but I had to learn it. I mean, heat transfer: how, when you start generating the heat in the uranium, how do you get it out?

LIPPINCOTT: Was there some apprehension about the process?

CHRISTY: Well, there were two main issues: One is, can you design something that will chain react? That was the Number One issue. The secondary one was: If you can make something that chain reacts, can you engineer it so that you can operate at high power and get the heat out, and get enough plutonium?

The first issue was to make the chain-reacting lattice. So I did calculations with respect to that. I did calculations with respect to the control of the chain reaction. How do you keep it from exploding? How do you keep it just simmering along nicely? And it was understood how to do this, but I made actual calculations, seeing what would happen if it was a little bit off.

Then, at one point, I was assigned to help Fermi. He was making measurements of these experimental piles. He'd make a small stack, and he would measure the distribution of neutrons. He would put a source of neutrons at the bottom, and the neutrons would diffuse upward in this. And by measuring the neutron diffusion in this smaller stack of uranium and graphite, he could find out whether, if you made it very large, it would be a chain-reacting pile. So he was doing a lot of experiments. Every time a new batch of graphite would come in, or a new batch of uranium, you'd test it to see whether or not it was good enough.

LIPPINCOTT: About how many people were involved in the project at that time?

CHRISTY: I don't know. I had my job to do. I found it very interesting.

LIPPINCOTT: What was Fermi like to work with?

CHRISTY: Oh, he was really a fascinating person—I think even more fascinating to have lunch with. There used to be a group that would go over with him to Hutchinson Commons to have lunch. And most of the talk would be about physics. And any problem in physics that came up Fermi would proceed to discuss, and then go on to say how you would deal with that, how you would solve it. To have lunch with Fermi was an education. But he was always joking.

LIPPINCOTT: How was his English?

CHRISTY: Good enough. I mean, all these people were foreigners, and they had accents. Fermi had an Italian accent. Wigner had a Hungarian accent. Oppenheimer was the only one who didn't have an accent. [Laughter] But, as I say, it was a fascinating experience to learn physics from the masters, you might say—by working with them.

LIPPINCOTT: Did you know John Manley?

CHRISTY: I knew him, yes. He did know English. I didn't really work with him; he was an experimentalist. I don't know why I was assigned to work with Fermi. I might say that I found Wigner very difficult to communicate with. But I wasn't unusual in that respect.

LIPPINCOTT: Do you mean on a human level or just language difficulties?

CHRISTY: No, it's because of the way he thought. I mean, I would go in to talk to him about a problem that he had assigned me to work on. And I never was sure, for twenty minutes or so, whether what I was saying was getting across to him—or that what he was saying was getting across to me. Our minds operated on different levels, and it was just very difficult for us to communicate. I'm kind of straightforward.

Now, Fermi was easy. Oh, he was so easy to understand. Except that he had these kinds of leaps. You'd hear a lecture by Fermi, and you'd think, "My, gosh, that subject is so simple!" Fermi made everything appear crystal clear. And then afterwards you'd go and try to write it down, and then you'd realize that it wasn't quite as simple as you thought. He, however, made everything appear exceedingly simple. Eugene [Wigner] was totally different: he was not a good communicator at all—but nevertheless a very great physicist. So I learned a lot, but I found it difficult working with Eugene.

LIPPINCOTT: There was kind of an old canard around Los Alamos that the Hungarians were Martians. Did you ever hear that?

CHRISTY: Oh, I didn't hear it there—I've heard mention of it.

LIPPINCOTT: Wigner being one, and [John] Von Neumann, and [Edward] Teller.

CHRISTY: Well, they were all very different. They were different from us, you might say, but they were different from each other, too. I had the good fortune at Los Alamos of learning things from Von Neumann. That's a most remarkable man—so straightforward. He could make the most difficult subject appear easy. He would lay it out for you, and it was obvious. And you can learn things so readily from a man like that.

LIPPINCOTT: Were you present when the pile started up in December?

CHRISTY: Yes. Before that, for a while, they needed all hands who were willing to help machine graphite. You see, the graphite would come in billets of approximate size. But if you're going to stack things up so that they make a very tight regular stack, it has to be precise. You can't have slight bends in it, because it won't stack together.

So the wood shop at the University of Chicago in the physics department was commandeered as a graphite machining shop. You know, graphite is black, and when you start machining it, the place is filled with black dust.

LIPPINCOTT: Oh, and bad for your lungs, too, obviously?

CHRISTY: Don't know. We probably weren't as careful about many things as we should have been. And some people, I'm sure, were injured because they inhaled things they shouldn't have. Herb Anderson was an experimentalist who worked with Fermi at Columbia. He came to Chicago and was kind of Fermi's—Fermi had two right-hand men: One was Wally Zinn, who was somewhat more senior, and the other was Herb Anderson, who had started out as a graduate student and was kind of Fermi's lieutenant. And—I'm not sure where—Herb apparently got beryllium powder in his lungs, [probably] from making radon beryllium sources. You had to get beryllium powder and then you had to get radon gas, which is bled off from radium—they pump it away from radium, and they pump it into a little container, and compress it in there, and seal it off. And for two or three days, this radon gas is very active. And the alpha particles from the radon bombard the beryllium oxide, which makes neutrons. It was a neutron source. But making it was a hazardous occupation.

In addition, we dealt with beryllium in other ways. So Herb got berylliosis, which essentially ruined his lungs. He was employed after the war by the Project, I think—basically as compensation because his health had been ruined. So he was employed there. He had a little dolly with an oxygen cylinder on it. And wherever he went, he went with his oxygen cylinder on a dolly and hooked up to his nose so that he could breathe oxygen.

LIPPINCOTT: How old was he? I presume he died.

CHRISTY: I don't remember now. He lived actually quite a number of years. He was a little older than I was, but he lived quite a number of years at Los Alamos.

I just mention him because there were a lot of hazards that we were aware of. For instance, people knew that radium was dangerous—plutonium is not nearly as dangerous as radium. Radium is a *very* radioactive substance. People actually handled plutonium, but they didn't stay near it very long. Chemists worked with the plutonium dust, though, and they probably had problems. I saw only the metal, and it's not particularly hazardous.

LIPPINCOTT: Tell me again about the startup of the pile.

CHRISTY: I was invited over as a guest. I worked in this graphite machining shop that I mentioned. And I'd been working with Fermi. So I was invited. I was one of the rather limited number of people there at the startup.

LIPPINCOTT: Do you remember who else was there?

CHRISTY: It's in all the books.

LIPPINCOTT: Do you have any particular memories of that?

CHRISTY: I remember the occasion very well. It started in the morning and went on, after a luncheon break. But you see, Fermi had everything planned and under control. Because prior to this startup day, Fermi made measurements on that pile every day as it was built up. He would measure with a neutron source. Basically, he would withdraw the neutron source, and then he would observe how the activity measured up high in the pile, how it decayed in time. And by plotting the decay rate, he could tell how far it was from critical. And as he built it up higher, it would decay more slowly. So he had a plot of exactly how the pile was going. And he knew, as a result of that, that one more layer and it wouldn't decay, because he's gotten essentially to that point.

So, as I say, he had it all worked out ahead of time. He knew exactly where he was. In fact, he built the pile up to the point where he knew it was above critical beforehand. But he had absorbing rods put in, and those absorbing rods stayed in there until Fermi said we could remove them. It was totally controlled.

I mentioned that I had done some calculations on control. And I'm sure Fermi had done them independently, because [the calculations were] easy. But the delayed neutrons are a very important factor. In every fission 99 percent of the neutrons come out instantaneously—within fractions of a microsecond.

LIPPINCOTT: They don't do any good?

CHRISTY: No, they do good but if that's all you have and you're above critical—then *whoosh*! The control is the fact that one percent of them are delayed for seconds, to almost a minute. If you're more than one percent over critical, you have what's called fast critical, as opposed to critical. Fast critical is when the material is critical without the delayed neutrons. That's bombs. Bombs—or anything that's explosive—run on fast critical. But if you get that in a reactor—that's what happened in Chernobyl, I think; they were making tests, and they got to fast critical. So you always have to run reactors where it's within one percent of critical. [Reactors] depend on the delayed neutrons in order to keep on working. And then, you see, instead of having a fraction of a microsecond as the multiplying time, you have seconds to minutes as multiplying time. It makes all the difference in the world.

But this was all known ahead of time—not as well as it is now, but it was well enough known. So we kept it very clear: nowhere near fast critical. But if you're within the operating range of that one percent, then it's an easily handle able machine. And Fermi knew that. So the scene was all a show—except the rest of us didn't know all this. Fermi had been doing these calculations—every day he'd go home and he'd calculate what went on.

LIPPINCOTT: So the rest of you didn't know whether the chain reaction would start up or not?

CHRISTY: We were pretty sure it would, because we were over there in order to see it start. So we already knew it was going to happen. Only Fermi knew exactly when it would happen. As I say, it was his show.

So he made various tests in the morning, getting closer and closer to critical. And when he saw that the next step would be above critical, Fermi said, "Now it's time for lunch." He shut it down. Everyone went off and had lunch. Then we came back after lunch, and Fermi continued the tests. And it went supercritical.

I mention this because I think it's important to realize that these things are very controllable if you know what you're doing and are careful.

LIPPINCOTT: It sounds like a very stately process.

CHRISTY: When it's handled sensibly, yes. And when the activity started to slowly rise, after pulling out the rods, Fermi announced that it was supercritical.

LIPPINCOTT: Did everyone cheer?

CHRISTY: Oh, yes. It was a very exciting moment. And Wigner broke out his famous bottle of Chianti, which he had brought along for the purpose. Everyone knew that it was going to go chain reacting, but only Fermi knew when.

LIPPINCOTT: Was there any talk then about the Germans' efforts to make an atomic pile?

CHRISTY: We didn't know any details. All we knew was that the Germans had started work on chain reactions. And we greatly feared that they might succeed. Now what we feared when I was at Chicago—the story that I heard, and as I say, I was naive—was that they had a big submarine program on. And it was obvious to anyone that chain reactions would be a wonderful thing for submarine propulsion, because you wouldn't have to carry a million tons of fuel. You wouldn't need oxygen, so you wouldn't need to be up near the surface, where you had to be to suck in oxygen to run your diesel engines. And electric submarines could submerge for only a few hours before their batteries went dead. So everyone knew that if you put a chain reactor in a submarine, you would have a fantastic weapon. Fortunately, they didn't. I forget when the British raided the Norwegian heavy-water plant. That was kind of a key to us that they were trying to stop the Germans from getting access to heavy water from Norway. And the reason for that was that heavy water was an even better moderator than graphite. But we figured, in this country, that graphite would be easier to handle. But the Canadian project, you know, they started off with a heavy-water reactor. They had a project going in Ottawa, and they designed a reactor that was called the CANDU reactor—Canadian Deuterium Uranium. And it was a very successful reactor. I don't know when it went critical. I presume after ours, but I don't know.

I wasn't in on the top-level decisions, when we decided to go with graphite. It may have been that people didn't think it would be that easy to get enough heavy water; because making heavy water meant basically having untold amounts of electricity. You electrolyze water; deuterium is there in one part in a hundred thousand. And you can separate the deuterium; it comes off and concentrates. So by electrolyzing water, you concentrate deuterium. That's why it was done in Norway, where there were hydroelectric plants.

Anyway, the next step at Chicago—after we achieved the chain reaction—was building the first reactor out at the Argonne. We had established an out-of-town site called the Argonne National Laboratory because we realized that it would not be wise to try to build up chain reactors on a big scale inside a city. People would get scared. So I remember helping to build the Argonne reactor.

LIPPINCOTT: When was that?

CHRISTY: Just shortly after—very early '43 or late '42. I know it was wintertime.

LIPPINCOTT: But then you went to Los Alamos fairly early.

CHRISTY: Yes, it was not very long after, very early in '43. Oppenheimer had been authorized to start Los Alamos. And he came around to Chicago on a recruiting trip. And there he recruited John Manley, and he recruited me. He knew me, and he may or may not have known that I'd do anything for him. He asked would I like to come out into the wilds of New Mexico and work—I forget when I learned about the bomb. Probably he told me that we'd be working on the bomb, I don't remember.

LIPPINCOTT: How did your wife feel about going?

CHRISTY: She admired Oppenheimer greatly, too. Although Los Alamos was out in the wilds, it sounded kind of exciting. And it was, Los Alamos was a very exciting place. Chicago is not that appealing a city.

My first job for Oppie—Oppenheimer. You know, ever since he spent time in Holland, he was known to his friends as Oppie. Oppie was spelled, strictly, Opje, because that was the Dutch. He acquired this nickname, Opje, pronounced "Oppie." And of course, most people spelled it "Oppie." But the first job I did for him was to visit Minnesota and help interpret some experiments they'd been doing there—some fast-neutron experiments. They needed a theoretical physicist to help them.

LIPPINCOTT: Where in Minnesota?

CHRISTY: That would be the University of Minnesota at Minneapolis. I think I also visited Madison, the University of Wisconsin. But I think mostly I visited the University of Minnesota. Johnny Williams was in charge of experimental work there. They were doing work there prior to going to Los Alamos. You couldn't go to Los Alamos until there was something there. So there was work going on at various sites, like Minnesota, and Madison, Wisconsin. And I was sent there, and spent a week or two trying to help interpret the work, write up some of it. And then I came back and worked on the Argonne reactor. I also started—I think even before the chain reaction—helping to design the Hanford reactors.

LIPPINCOTT: You mean the reactors that were supposed to make plutonium?

CHRISTY: That's right.

LIPPINCOTT: But you didn't even know if plutonium was going to be used for it at that point?

CHRISTY: Well, I may have known. My memory at some points is unclear. When did I first learn about the use of plutonium? I do not remember.

Anyway, I remember spending some time at Chicago, working with Wigner's group, trying to make basic engineering designs [for reactors]. That is, lumps are no good, because the heat is there, and how do you get the heat out? In order to get the heat out, you need the uranium in a rod, and then you surround that rod with flowing water, or something like that. So the uranium is in a rod and surrounded by water, and the water flows around it and gets hot—takes the heat out of the uranium—and comes out the end as high-pressure water and changes into steam.

LIPPINCOTT: This was the reactor design.

CHRISTY: That's right. As I say, we had to do that, the basic engineering design. Now that doesn't mean we did what an engineer has to do—to make sure that this fits here and all of that. But we did do the nuclear physics of what would be an engineering design: That is, would it work as cylindrical rods surrounded by water—would it react or not—because it's not quite as efficient, since the water acts as a neutron absorber. So we had to make calculations of the cylindrical geometry. And I remember doing that.

LIPPINCOTT: And Wigner was the head of that group?

CHRISTY: He was the head of the group. And Wigner essentially continued—with a semi-engineering group working under him—working with the DuPont engineers that designed Hanford.

But I was recruited at that time, and went to Los Alamos.

LIPPINCOTT: Do you remember when you got there? Was it the spring?

CHRISTY: Probably around March or something.

LIPPINCOTT: Feynman has a recollection of living in a set of cubicles with you and your wife.

CHRISTY: Yes. Before we could go up to the mesa, we were housed in a number of dude ranches down in the valley. And that was interesting, although we didn't get to do anything except commute. In the morning, a Spanish-American would come along, driving a car—a sedan—and we'd make a wild ride through the town of Pojoaque, which was a Spanish-American town not very far from the San Ildefonso Pueblo, which was down near the river—and then climb up the road to the mesa. These rides were kind of wild. These Spanish-American drivers—there'd be chickens out in the middle of the road, and they'd go charging through the chickens, and chickens would be fluttering all around. And sometimes they'd been up late the night before, and we'd worry about them falling asleep at the wheel. So the person stationed next to the driver was always ready to grab the wheel, in case. It was a little bit tense; they were not professional drivers. As far as I know, there were no major accidents.

And we went up there. And for the first few weeks, we were listening to this series of talks on what was known about the making of bombs. There's a book—*The Los Alamos Primer*. Bob Serber gave a series of talks—very clear, well presented, attended by everyone, experimentalists and theorists.

And then comes the time when we moved up to the hill. At first, as Feynman says, they had army bunk beds—lower and upper, made of oak. They were painted Army green; they were double bunk beds, the kind they used in barracks, I presume, and made by the thousands. I forget whether my wife and I had an upper or a lower. But I remember Dick was out on, I think, a porch, with a lot of bunk beds. It was simple, not really primitive. I mean, we had beds, and there was food. It wasn't like being homeless and out in the street. It was simple.

LIPPINCOTT: Was it that way for the duration?

CHRISTY: No, they were building housing as fast as they could. And the housing was very respectable. There was a little bit of housing there already—the so-called "bathtub row"—which was mostly the housing of the faculty. They were nice homes, built of fieldstone, and one-story homes of fieldstone blended in well. The site had been a boys' school, and these were faculty houses. Now, the students were housed in this big house, like a dorm. And that's where our people were housed at first. But then as they built housing, we moved out of there and we were assigned houses—if we were lucky, if you were married. If you were not married, you were assigned to a dorm. But I guess each person in the dorm had a room of his own.

My wife and I were, first of all, assigned a nice, one-story duplex—I guess we had one bedroom, a bath, living room; it was sufficient. It was just across the road from Oppenheimer's house. So it was kind of in the social area. That is, you could easily see people and, if you were invited, go over for cocktails or something. So we saw a fair amount of the Oppenheimers.

We stayed there, but then there was a baby starting to come, and we needed a larger place. So we were moved to a fourplex—two-story apartment, four apartments together. It was somewhat out in the sticks, but that's the price you pay. Instead of being in the social center of Los Alamos, we moved out to the sticks.

LIPPINCOTT: Was your baby born while you were at Los Alamos?

CHRISTY: Yes, my oldest boy was born at Los Alamos, in a post office box. Because that was our address—it was PO Box 1663, Albuquerque, New Mexico. We had a little army hospital there on the mesa. It was staffed by army doctors. And these doctors were good; they were mostly just out of St. Louis Medical School. And a lot of babies were born there.

LIPPINCOTT: I know Feynman's wife was sick at that time.

CHRISTY: She was in a sanitarium in Albuquerque.

LIPPINCOTT: So your wife could have gone down to so-called civilization if she had wanted to, I presume, to have the baby.

CHRISTY: Well, conceivably. I don't think anyone would ever think of it, because everything was fine up there. The doctors were good. They weren't specialists, but they soon became obstetric specialists, because there was lots of that. It would have been very inconvenient to go down, because we were supposed to be around a lot. Dick Feynman had special dispensation to go and visit his wife regularly. There were security concerns: it would have been very strange if there'd been a lot of wives suddenly appearing in a local hospital and people dashing back and forth. I don't think it was ever considered. Whatever happened better happen up there at Los Alamos. Now, sometimes things were lacking. The medicine practiced there wasn't the same as they practice now, but it was good—good medicine for its time.

LIPPINCOTT: You lived there, essentially, for four years, didn't you?

CHRISTY: No. We went there in the spring of '43, and left in the very early spring of '46.

LIPPINCOTT: And you were in the Theoretical Division under Bethe?

CHRISTY: Yes, under Hans Bethe. Hans Bethe was a very good man to work for, because he was a very straightforward thinker. B follows A, and C follows B—everything very straightforward. And a most prodigious worker. When Hans was working on a problem, he'd have a pad here and a stack of paper there. He would proceed to calculate, he would fill a sheet, and it would go over to this stack. Then he would proceed on. The stack, filled out with calculations, grew, and his pad shrank. He was just an absolutely amazing person. He wasn't such a brilliant thinker offhand, but nevertheless a very, very solid person. And he still is, he's a very solid person.

LIPPINCOTT: And he headed the theoretical division?

CHRISTY: He headed the theoretical division.

LIPPINCOTT: And he put you in charge of a group?

CHRISTY: Not really. A number of us kind of were allowed to work independently. Because we were too experienced, really, to work under someone else's direction but not very good about having other people work for us. So Dick Feynman started off working independently. Maybe nominally under someone's supervision, but basically he was independent. I was independent but working in a different area.

The first thing, I think, that I was asked to do was to work out the theory of the simplest chain reaction we could think of. And we concluded that the simplest thing we could think of would be a solution in water of an enriched salt of U-235—the idea being to make something react with as little uranium as soon as possible. What was the first thing we could make react? And U-235 was being created by the diffusion plant at Oak Ridge. And if we could make a chain reaction with a few hundred grams of U-235, that could be done fairly soon, because it was coming out at a steady rate, and you'd have a few hundred grams long before you'd have a few kilograms; and you'd have a few kilograms long before you'd have a hundred kilograms. So, in order for people to get some experience with chain reactions, we figured that probably the thing that could be done fastest would be what we called the water boiler. It was a solution of U-235, salt, and water. And I was asked to calculate the critical mass, because I'd worked on problems of more or less that kind before. I had to try to elaborate the techniques, because the slowing-down process is rather different in water than in other substances. So I had to make slight changes in the techniques. And then I had to get the best information I could on the properties of the materials. We were going to surround the U-235 solution with beryllium oxide pressed into cubes, because beryllium doesn't absorb neutrons, nor does oxygen. They reflect, and they slow things down; so it would be a pretty good reflector of neutrons. And I had to calculate the properties of these—the reflector and the water, and how that slows neutrons down, and the critical mass.

LIPPINCOTT: You weren't in on actual bomb design? I think the first design was a kind of gun-type bomb.

CHRISTY: Basically, the design didn't amount to much. The gun-type you couldn't do at that time, either—because you didn't have enough uranium to do it. The plants were beginning to start up, but if you wanted a kilogram, you had to wait months longer than if you wanted a few grams. And if you wanted a hundred kilograms…[Tape ends]

Begin Tape 2, Side 2

CHRISTY: People needed experience with chain reactions. And we couldn't get enough material to make a chain-reacting bomb. And furthermore, it would be exceedingly delicate to control. I mentioned the difference between fast critical and delayed critical. But fast critical for a pile—the basic time scale is the time for neutrons to wander around and come back and make another fission. They have to slow down first, and that takes milliseconds. But if you have a reaction that works on fast neutrons only, as in the bomb—a neutron comes out of this nucleus, hits another nucleus, and *bang*!—then the basic time scale is one-hundredth of a microsecond. So the basic time scale is around a hundred thousand times faster. So it's very delicate when you start playing with a critical mass of uranium-235 metal, or plutonium metal. And unfortunately, later on, there was at least one accident—and I think there were two or three—in which mistakes were made and some people were killed.

LIPPINCOTT: At Los Alamos?

CHRISTY: Yes. Louis Slotin was one of the people killed.

LIPPINCOTT: But there wouldn't have been a nuclear explosion, per se?

CHRISTY: Well, you see, you have to realize how a nuclear explosion works. If you have two critical masses, and you suddenly bring them together, then that will continue chain reacting on a very, very short time scale. So the temperature just goes shooting up to the sky. And what will stop it? The only thing that stops it is when the pressure in there gets so big that it flies apart and is no longer critical. But what's important is how rapidly the neutrons multiply, and therefore, how high the temperature gets before the [material] moves a part, because moving apart is what stops it.

I'm explaining things here that I presume must be considered declassified, because they're so elementary.

LIPPINCOTT: Well, Serber's notes are all declassified.

CHRISTY: So if you have a pile, and it gets fast critical, then the temperature will shoot up until something moves enough to stop the reaction. So the accidents at Los Alamos happened with people trying to assemble uranium-235 metal. And I believe they'd have it machined into little cubes. They would put a cube there; then they'd make measurements. Then they'd try to push it a little bit closer together and add cubes. And, as I say, this is not something that is easily controlled within a micrometer. Perhaps it could have been, but we didn't design techniques to make it that easy to do. But if you slightly exceed, then the temperature shoots up almost instantaneously, until something moves. It has to move. And a little bit of motion, of course, makes it sub-critical. And if you're just a tiny bit over critical, then it doesn't have to move very much.

LIPPINCOTT: And there's an explosion?

CHRISTY: There is an explosion of some kind: It's heat. It's just that everything gets hot. I think Slotin mentioned some kind of flash. Because all of this radiation heating your head will cause you to see things, whether or not there is actual light. I don't know exactly what took place.

LIPPINCOTT: I understand that at Oak Ridge some of the uranium samples that they were getting were being stored a little too close together. And Oppenheimer became concerned that they weren't aware that they were potentially…

CHRISTY: I guess he sent Dick Feynman to Oak Ridge to make calculations. That was complicated, and that was Feynman's story. I think what he found was, in the chemical processing, sometimes stuff lodged in a certain place in the chemical plant. And you didn't want to have too much lodged in one place, or you'd have problems. You had to know exactly what was going on in your chemical processing so that you didn't get into trouble.

LIPPINCOTT: And they were keeping some of those salts in water. And the water would have slowed down the neutrons and made it all the more effective?

CHRISTY: That could be, but I don't know all the details.

LIPPINCOTT: He says in his book that *you* were going to be sent to Oak Ridge but that you got pneumonia.

CHRISTY: Yes, I did get pneumonia. Yes, I guess I was. I had forgotten. [Laughter] I'd forgotten that I was going to be sent to Oak Ridge.

I went off on a skiing expedition. And I had a tendency at the time, if I got too exhausted, I would get sick, and on this occasion I got pneumonia. And I was laid up for several weeks.

LIPPINCOTT: Whom did you ski with?

CHRISTY: Most of my skiing was done with the European group. That would be Bethe, [Victor] Weisskopf, Fermi, and a couple of Americans. Because they liked to do alpine-type, cross-country skiing, in which you head up a mountain, and you drive as far as you can. And when you come to snow, then you put on your skis and you put skins on the bottoms of the skis—sealskin, which has a direction to the fur. And you attach it so that the fur points backwards along the skis. Then if you put your foot down, it won't slide backwards, because that's the wrong way for the fur. So you can walk along and your feet don't slide back. So you can climb right up a mountain—just slide one foot forward and then slide the next one forward, and climb a mountain with fresh unbroken snow. It's the most beautiful thing in the world: a mountain covered with snow, and no tracks anywhere; the blue sky—because there's a lot of sunshine there in the wintertime—it's the most wonderful experience! I didn't know how to ski, but nevertheless I was active, and gradually I learned.

LIPPINCOTT: Did Oppenheimer give you a horse?

CHRISTY: Yes, I was given half a horse at one time. We always argued whether my half was the front half or the rear half. I shared the horse with Johnny Williams' son, Lloyd Williams. In other words, Oppenheimer gave us two the horse to share between us. It was a horse of his. He had a number of horses that he brought over from his ranch. And in addition, there were the school horses. And I learned to ride there from a number of people there who were riders. The Serbers were riders, because they had learned at Oppenheimer's ranch. Bob Wilson had a cowboy background in Wyoming, and he was a hot rider—he could rope, he could do anything on a horse.

LIPPINCOTT: Were you good on a horse immediately?

CHRISTY: No. Well, I stayed on—that's the only thing I can say for it. I stayed on long enough to learn how to tell the horse what I wanted to do. But I never had any lessons.

LIPPINCOTT: Were they Western saddles, or Eastern?

CHRISTY: They were Western saddles, not English. A bunch of us wanted to go riding. So I said, "Well, I'd like to go along, too." So I got on one of the school horses. We went out riding in the woods. The horse knew the area, and the others knew how to ride, and I went along. I found I could stay on the horse pretty well and didn't have much problem with that, even when the horse was cantering, I learned how to go with it. But I didn't know much about steering the horse. Going straight behind other people, the horse went along, and no problem. But then we went off through paths in the woods. And they got a little ahead of me. And I thought the path turned to the right. So you lean over when you're going into a turn. But I didn't tell the horse, really, that I was planning to go to the right. I thought it would happen. And the horse thought, "Well, he'd rather go to the left." But fortunately—I always figured it was easier for me because I had very long legs, so I could hold on. And even though the horse and I didn't always agree, I held on. And the horse would go flying underneath low branches, because as long as the horse had room for its own head, it didn't particularly care. So I was down along the horse's neck, clutching it. But, as I say, I stayed on. And in fact it was years before I fell off a horse. It's just as well, because falling off isn't so much fun, and I might have stopped riding.

ROBERT F. CHRISTY

SESSION 3

June 21, 1994

Begin Tape 3, Side 1

LIPPINCOTT: We were still in Los Alamos the last time.

CHRISTY: I believe you had raised the question of the water boiler, or something like that.

LIPPINCOTT: Yes, at Oak Ridge?

CHRISTY: No, this was at Los Alamos. At Los Alamos, the authorities decided they wanted to have a chain reaction at Los Alamos as soon as they could. And we figured the least amount of enriched material it took would be a solution of uranium-235 salt in water. And this, not too much uranium-235, could be made critical and could give them experience with critical assemblies. There's a book called *Critical Assembly*. It's interesting—this is an aside—it's a technical history of Los Alamos, by Lillian Hoddeson. She went around interviewing people and wrote a very good technical history of Los Alamos. It came out just a year or two ago. For someone who is interested in the technical side of what happened at Los Alamos, it's very good. I don't think it's a layman's book.

Anyway, the decision was made. It fell to the theoretical division to calculate the critical mass, and then people in the experimental physics division were assigned the job of making it. And in the theoretical division, I was assigned the job of calculating the critical mass, which was a little bit tricky, because water is a peculiar moderator of neutrons. But I proceeded to do it, and it had a reflector around it, which I think was beryllium oxide. And I came up with a number for the critical mass. But they didn't have the uranium at that time—it was coming in from, I think, Oak Ridge. And by the time they were beginning to get enough material, there were some new cross-sections available. So I hastily revised my calculation to accommodate the new data that had become available. And I came up with a number for the critical mass—so-and-so many grams of U-235. I've forgotten the number, but nevertheless, I achieved almost instant fame at

Los Alamos, because it turned out that the number that I got was within a very few percent of the actual number that they found was needed to go critical. So I acquired tremendous fame—here is a theoretical physicist who calculated the right number! This was probably in 1943, or early ’44—something like that.

Now I will confess that anytime you hit something within a percent, it’s largely luck. But I didn’t go around telling people it was luck. So that was a triumph.

Not long after that, people saw that it would be very difficult, if not impossible, to make a gun-type assembly with plutonium. The gun-type assembly is where one piece is fired out of a gun, and hits another piece, and once these two pieces come together the material is supercritical. That works fine with U-235. And in fact that was done with the Hiroshima bomb. But it is relatively slow. And plutonium has enough spontaneous fission so that there would be a considerable rate of neutron production. And it was very likely that as the two components approached each other, the thing would detonate before it was fully assembled—what is called pre-detonation. And it would make a very weak bomb, because it would be just barely supercritical.

Now, the reason for this was technically very interesting, and that is that in the Hanford reactors, which made plutonium, there was a big neutron bath. And the plutonium itself, after being made, would sometimes absorb a neutron and become plutonium 240 instead of plutonium 239. And it was this plutonium 240 that was the culprit—that made all of this spontaneous fission that caused the trouble. And you couldn’t separate the two isotopes out. You could run the reactor at a very low energy level, so that the flux of neutrons was low, and you could extract the plutonium then, instead of letting the reactor go until a lot of the U-238 had been converted to plutonium. If you took out the uranium after only a little bit of fission had taken place, then you might separate the plutonium before it had accumulated a big load of this 240 isotope. But then you’re going into a fantastic extra amount of chemical separations—when you have to process the uranium after just a little bit. So it was viewed as essentially a given that you were going to have too much plutonium 240, and therefore you would not be able to use the gun assembly. This required inventing something new.

Serber talks at Los Alamos, at the beginning, about assembly. Seth Neddermeyer—who had gotten his degree with Carl Anderson here at Caltech—suggested taking a shell and putting an explosive layer around it and setting off the explosive, and the shell goes *whoof*! and ends up as a lump, and there you are. It was called implosion. The concept was that you would hit the material just hard enough so that it would collapse into a ball. But they had to do a lot of tests with this concept. So they took cylinders and they wrapped explosive around the cylinders and tried to implode them. And, basically, it seemed that there were great difficulties. It didn't stay together, of course. When you'd hit it, it would go *whoop, whoop,* and all you end up with are fragments. So you don't easily know whether you accomplished something readily or not. It's just hard to say.

LIPPINCOTT: They didn't try to do this with a spherical shape?

CHRISTY: Well, with the cylinders, you could look into the end and see what was going on. With a sphere, you can't look inside. A sphere would be desired, eventually. But to test the concept, they used the cylinders to see if it was collapsing.

So it was very difficult to know what you were accomplishing—even hard to know how fast it was assembling. It was very difficult to know what went on. So Seth pursued that. Not much progress was made. But the concept of implosion had been planted.

Then, at some point, someone—and I suspect it was Von Neumann—had an idea that if you put a lot of explosive around a little shell of material, and hit it hard and drive it together symmetrically, not only will it assemble into a sphere but it will compress. You'll compress the metal into something denser than ordinary uranium. And it was known, of course, by everyone in the business, that if you had uranium at twice normal density, the critical mass would be a quarter as great, or something like that. So this meant that with a smaller amount of material you might be able to get a big explosion, by using this concept of compressing the metal. Anyway, it was an idea that everyone was taken with. But in order to know what went on, you had to know things about the equation of state of the metal: how much compression takes place under what pressures and temperatures. And you had to do very, very tricky calculations of the hydrodynamics of a sphere of metal imploding in a spherical shell.

So I was assigned another task at this point—namely, to find out something about the equation of state under very high pressures. Pressures like those at the center of the Earth. What density increase does it make? Well, again, this was something I was totally unprepared to do. So if you are asked to do something you don't know anything about, you learn a lot, because you then get into things you didn't know about before.

So I studied work that was done by geophysicists on the center of the Earth: about the pressures in the center and the density of iron in the center of Earth, how much denser was it? Their theories could be applied to the theory of atomic structure under very abnormally high pressure circumstances. I looked into that. I then worked up something on the equation of state. I remember doing iron; I'm not sure if I did uranium, too, or whether someone else did.

LIPPINCOTT: Were you working alone on this problem?

CHRISTY: I was working alone, yes. I did most of the things fairly much alone.

LIPPINCOTT: Why do you suppose Bethe chose you for this?

CHRISTY: I guess he felt it was something he knew how to do, but it would take a fair amount of time, so he could tell someone else to do it. I mean, you have to be able to assign tasks to people, and hopefully ones that they can make some progress with.

I made progress with it, and I wrote some reports on the subject. And that was helpful. And by that time, the work on implosion was going along pretty vigorously. And of course, Feynman probably told you about—or you read about—the work he did on organizing computations on implosion.

LIPPINCOTT: Yes, and he says you came to him at one point and wanted something in three weeks.

CHRISTY: Well, at one point I'm sure I did. That was closer to the end. I forget what their normal schedule was. Anyway, they worked twenty-four hours a day, with three shifts of calculators, and I think they turned it out.

LIPPINCOTT: Were these mechanical calculators that they were using?

CHRISTY: Well, at that time, they started off using the desk calculator and such things. And then, Stanley Frankel—and maybe Eldred Nelson; I'm not sure about that—and others got the idea of using IBM tabulating machines—these great big machines about four feet high and six feet long, where you feed a deck of cards into one end, and it will process the numbers punched into them—we could get it to do multiplication, actually—and then it spews out a deck of cards at the other end with this information on it. The advantage of this machine was that, although it's doing only one operation—multiply A by B and write down the answer—it does that for one item after another. So you could do that one operation on a whole sequence of things. And then you'd take the deck of cards over to another machine and it would go through and do another operation. It was the strangest sort of computation ever devised.

LIPPINCOTT: And Feynman was in charge of that?

CHRISTY: He helped organize it. I think the one who devised it first was probably Stanley Frankel—but I'm not sure. But Feynman helped to organize the operation, to make things go.

So they devised this system, with a bunch of machines and people carrying decks of cards from one machine to the other, and everything humming and clacking away. And that was the computation laboratory, a most amazing thing.

LIPPINCOTT: How did that relate to what you were doing?

CHRISTY: Well, this was to calculate implosion. Implosion is a very complicated hydrodynamic calculation, in which you ask, "What happens to all this material?" a tenth of a microsecond by a tenth of a microsecond. As the pressures build up and it gets closer and closer together, you have to follow that by calculations of what's going on throughout all the material. You have to follow every little bit of a layer. And that's what they did.

LIPPINCOTT: Was this in regard to uranium or plutonium?

CHRISTY: They were doing it for plutonium. But they could also do it for other substances. See, they had all sorts of experimental ways of testing things. And some of these tests would use iron. They didn't use uranium in all the tests they did; they used more readily available materials in testing systems.

But I got into this not as a deviser of the concepts of implosion at all, nor as one of those who were turning out the calculations. I was assigned to help the groups who were making experimental tests on implosion—to help them interpret their results by comparing what they found out with the results of the calculations that Feynman and others were doing. So I was a kind of go-between, between the experimentalists and the calculators. And that's when I would come to Dick and say, "We really need to get this thing calculated."

Now, in the course of this go-between work, I knew what was coming out of the theoretical calculations, I knew what the experiments were doing, and I knew what the main problem that everyone was worried about was. And that was: Maybe there are asymmetries in there that will ruin an implosive assembly. If it didn't have asymmetries—if you could take a really thin spherical shell of plutonium and implode it—you could achieve densities of many times normal metal density—and it would be a fantastic bomb! But no one felt confident that things were going to work that way. And the experimental tests did not really show whether it would work as perfectly as planned. So all the people who were responsible—that is, Oppenheimer, Bethe, all the big ones—were worried, because, you might say, their reputation was on the line. If they recommended going ahead with a certain type of bomb assembly, and it didn't work—their reputation was on the line. So this was the pressure that was on: you could make a beautiful assembly by means of implosion, if it went as planned and if it was symmetrical. But no one felt very confident that it would be sufficiently symmetrical.

And that's where I came up with the idea of how, essentially, to make a bomb that people would be confident of. I devised an idea that essentially made the bomb much simpler—so simple that everyone believed that it would stay symmetrical.

LIPPINCOTT: And is that when they began to call it the Christy bomb?

CHRISTY: Yes. They named it after me. And it was not nearly as good an assembly as the ideal one they wanted to make, but you could test it. And there were experimental tests going on, such as X-ray flash photography, where you could send a flash of X-rays through and see just how this stuff was coming together. And there were other tests—very interesting ones; the so-called Ra-La [radio-lanthanum], in which a source of radioactive lanthanum is put at the center of the assembly. And then as the implosion takes place, the gamma rays that come out of the radioactive lanthanum are suddenly absorbed more by the surrounding, thickening shell. And in microseconds, you trace and see the reduction in the gamma rays. They devised the most fantastically clever ways of testing what was going on in implosion. I just mentioned a couple of them. But these are the things I was involved in, trying to be a go-between between the experimental work and the theoretical work. As I say, the design I suggested gave people confidence. It wasn't very good, but it was good enough; and it gave people confidence, in that you could make it work.

LIPPINCOTT: One thing has always puzzled me. I know the Hiroshima bomb was the gun-type bomb. And I had always thought that it was the one that was tested. But it wasn't.

CHRISTY: No. It was such a straightforward assembly that no one was worried about it. Now as a matter of fact, with respect to something that I have been involved in over the years—trying to interpret what happened at Hiroshima and Nagasaki—there are questions arising as to just what did happen with that gun assembly. But nevertheless, at that time, everyone felt that they could predict exactly what would happen. And in fact, what happened at Hiroshima was, within reason, what had been predicted.

LIPPINCOTT: So they did these things side by side?

CHRISTY: The gun-type was done by a separate group, because we knew that U-235 was being manufactured.

LIPPINCOTT: And there was never a question that there would be enough U-235?

CHRISTY: Well, sooner or later you may wonder why the bomb wasn't dropped six months before. And the answer is that there wasn't enough U-235. The Hiroshima bomb was dropped not terribly long after the required amount of U-235 became available. Los Alamos did all of its work on time, so the schedule of when things could be used depended entirely on the production plants. As soon as the material became available and was converted to metal, it was made into bombs, and the first bomb was dropped on Hiroshima. And the first plutonium that became available—that is, converted into metal, and you realize there were fantastic efforts involving chemists, metallurgists, physicists, to do all of this, to make plutonium metal when no one had ever seen it before. But the timing of the first plutonium implosion bomb, which was called the "Christy gadget"—the one detonated in New Mexico, sometime around July 16th—was entirely determined by when the material dribbled in from Hanford. As soon as it became available, the bomb was tested. And it worked reasonably as expected. No one's expectations were very precise. The only question was: Would it work at all? And, of course, in order to avoid being disappointed, people will often officially guess low, so that if it doesn't work well, at least you guessed it, and if it does work well, hey, you were wrong, it was better than you said. So a lot of people guessed less than exact. But nevertheless, the yield that was actually measured at the New Mexico test was in the same range as people expected if it worked the way it was supposed to. And apparently it did work as it was supposed to.

LIPPINCOTT: I would like to talk about the test a bit, because it was stunning.

CHRISTY: Oh, it was a dramatic thing! I was not involved in the actual preparations for it. My job was calculating and so forth. But those of us who had had a significant involvement with the planning were bused down there as observers. As I recall, there were three buses that left on the afternoon before the bomb was to be tested. And I think Fermi was one of those along. So we were bused down there, to a point, I believe, about twenty-five miles away from where the bomb was to go off. And that's, I guess, something like the distance from here to UCLA—that's a fair distance.

LIPPINCOTT: How did you feel?

CHRISTY: I had always been fairly confident that the theory would work out. I felt rather confident that it would. It doesn't mean that you don't have some worries, but it wasn't as though I was shaken with great doubts about it. I felt it would work. But I had no idea what it would be like.

We were assigned the kind of glass that is used in a welder's mask. Basically, you can't see through it, except for brilliant light like a welder's arc. So we were told to hold this up in front of our eyes. And we did. And even with that, you see in your peripheral vision the whole world light up like the sun is out. But through the glass you can see the actual explosion going off. It was awe-inspiring. It just grew bigger and bigger, and it turned purple. The purple was an interesting thing, which I certainly hadn't anticipated. But it was in this ball. The debris was intensely radioactive, and it was sending out beta particles and gamma rays in all directions, and those ionized the air. So the air around this ball emitted a bluish glow, which comes from ionized air. It was most fantastic, to see this thing going up and swirling around and eventually cooling off to the point where it was no longer visible.

LIPPINCOTT: It must have made you feel quite triumphant?

CHRISTY: Well, it was certainly an exciting climax. And everyone was, I think, very much pleased—that after all this effort, it had actually worked the way people planned. So everyone felt very good about it; it was successful; it worked. But I don't remember details about who said what.

LIPPINCOTT: Then, a month later, the uranium gun-type bomb was dropped on Hiroshima.

CHRISTY: That, of course, was a very sobering event.

LIPPINCOTT: Did you hear about it there, at Los Alamos?

CHRISTY: Oh, we heard about it all right. And that it was successful. But then word began to trickle in about all the destruction. It was very sobering. I feel that we were accustomed at that time—we were in a war, and there had been many battles in which thousands of soldiers had died. So it was not as though, suddenly out of the blue, you have people killed. There had been bombs dropped on cities. There had been fire storms, and so forth. I believe people nowadays don't realize that in war your objective is to beat the enemy. And unfortunately, mostly that involves killing a lot of the enemy to do that. So war is a very bloody thing. I didn't see the blood, but at least I was aware of it. And therefore you have to view it in that context. I felt then that although this was a terrible event, it probably saved many, many more Japanese lives. They probably would have lost millions if they had had to defend themselves against an invasion. And we would have lost hundreds of thousands. So in that context, it probably saved lives. But nevertheless, it's a very sobering thing. And we saw pictures of the destruction of Hiroshima. It was practically leveled.

I have visited Hiroshima since then. I'll come to that.

And then, after that, a few days later, the Japanese still hadn't surrendered, and so the second bomb was dropped on Nagasaki. And that one did about the same things, although it was not really so successful in that Nagasaki was not a very good target. Nagasaki was a city built along several valleys. So there was not the same concentration of people in Nagasaki as in Hiroshima; so it did not kill as many people or probably do as much destruction. But the yield was comparable, perhaps a little bit higher, than the Hiroshima bomb—not greatly different.

LIPPINCOTT: Then the Japanese surrender came. And then this group was formed—the Association of Los Alamos Scientists [ALAS].

CHRISTY: As a sociological thing, I found it interesting to observe what happened with that group of scientists who had been so busy for three years, concentrating on getting something done. When it finally was done, suddenly everyone stopped working. No one could push papers around anymore or do anything. They sat around and talked; they discussed things in meetings. Basically, work stopped. I believe it was a mass reaction. There may have been groups there that had more or less routine things to do, but in terms of creating anything new, work just stopped. No one had the mental energy to push forward with anything for quite some time.

There was considerable concern about how to manage nuclear energy—or atomic energy, as it was then called. Both within the country and worldwide, discussions of this went on—meetings and so forth.

LIPPINCOTT: And didn't you have a part in drawing up some kind of a statement about how the public should be educated about nuclear power?

CHRISTY: I may have. I told you before that I have a lot of things I do not remember particularly. So if you know something I did, present it to me.

LIPPINCOTT: It was in one of the books about the bomb. That when ALAS was formed—that was in the late summer of ’45, after Hiroshima and Nagasaki—its statement was drafted by about five or six scientists at Los Alamos—Roger Sutton, William Woodward, Morris Perlman, and William Higginbotham, and yourself.

CHRISTY: No doubt it’s true, but I have forgotten it, so I can’t help you. I was fairly involved, fairly active in these things, yes.

LIPPINCOTT: And Oppenheimer wanted scientists to have some kind of a say.

CHRISTY: Before the bombs were dropped, there were some very high-level committees advising the government. Oppenheimer was a member of some of those on how to use the bombs. As I say, there’s been considerable discussion about the role of those people. I think Fermi and Oppenheimer and Lawrence were among them. I heard sometimes a little bit about it from Oppenheimer, but usually not in much detail, because when you advise the government, you’re not free to tell people what advice you gave, because the government may choose to do its own thing. So I didn’t hear too much in detail about that. And I was not involved at that level. But on other levels, as you mentioned, I clearly was involved—at the level of the ordinary scientist at Los Alamos.

LIPPINCOTT: I guess at about this time people began to look for peacetime jobs.

CHRISTY: Some people had their eye on the ball better than others. So that it was not long after the bombs were dropped when some people announced their plans to take a job somewhere else. They had made their plans early. On the other hand, I had made no plans whatsoever. I was merely doing my job. So I was left there. No one was being fired, just because you weren’t doing anything. But clearly the thing to do was to go and get a job. And I forget, but I’m sure what happened was what always happens—they call it the “old-boy network,” but really it’s the way things happen—is that someone at University A wants to hire a physicist. And they’re all at

Los Alamos. So you get in touch with someone at Los Alamos who's high up, in the know. It might have been Bethe; it might have been Oppenheimer. And you say, "We'd like to get someone; can you recommend someone?" That's the way it goes. It's the way the world always goes. So I'm sure that I was recommended. Chicago wanted to get someone.

LIPPINCOTT: Did Arthur Compton offer you a job?

CHRISTY: I was offered a job at Chicago. And since I had been in Chicago during the early part of the war and Chicago was a very fine university, I felt this was an excellent opportunity to join a first-class university. And I did that. I said, "Yes indeed!"

LIPPINCOTT: What level did you come in at? Were you an assistant professor?

CHRISTY: I don't remember. I have a feeling it probably was an assistant professorship.

LIPPINCOTT: And [Edward] Teller went there, too?

CHRISTY: Yes. Our housing arrangements there—I had a wife and one small boy, born in 1944; and this was early in '46 and another child was on the way, later born in May 1946. So my family wasn't too mobile. But fortunately, at that time, we didn't have much in the way of belongings.

LIPPINCOTT: And you shared a house with Teller?

CHRISTY: Yes. It was very hard right after the war to find any place to stay. We looked for an apartment that would be suitable for a small family. And we failed to find anything. So the Tellers and Christys thought, Well, maybe if we can't find a small apartment, we can find a big house and share it. So we did. We found something that would be called a semi-mansion or something like that, around 47th Street and Woodlawn Avenue. [Tape ends]

Begin Tape 3, Side 2

CHRISTY: The house had a very large kitchen—so large that servants could operate in it; it was a house made for servants.

LIPPINCOTT: Did you buy this house with the Tellers?

CHRISTY: We rented it, and we shared the rent. As I say, it had a very large kitchen, and it had a large butler's pantry, which had sinks and things like that. So the Tellers took the kitchen as their kitchen, and we took the butler's pantry as our kitchen. We had been accustomed to using electric hot plates and an electric oven at Los Alamos, the reason being that the wood stoves were not considered very convenient cooking devices. So we could just plug in our cooking equipment, no problem.

Then there was a large dining room adjoining the butler's pantry. And we took that as our living room. So we had the butler's pantry and dining room as our downstairs setup. And the Tellers had the living room and the main kitchen. And they could get from the kitchen to their living room without going through our butler's pantry and dining room. So without putting up any walls, we basically had separation—we could operate two independent kitchens and living rooms.

Then on the next floor up were the family bedrooms. I think the Tellers had two little children—a girl and a boy—a little bit older than ours, I believe. And they took the two front bedrooms; and we took one, and maybe another, rear bedroom. And we could access ours from the servants' stairway, and they could use the main stairway, going from the living room upstairs to the front bedrooms. So we basically had separate establishments.

LIPPINCOTT: It sounds as though they had the grander rooms.

CHRISTY: They were grand, but if you have no furniture, what does it matter whether you have a very large living room or a modest size? This was a very large dining room I'll have you know—probably about sixteen by eighteen feet, something like that. Now that's not tiny. So that was comfortable.

Then upstairs, on the third floor, there was a large billiard room—or ballroom, I forget which it was. And there were large numbers of servants' bedrooms, which were a reasonable size—like ten by twelve, something like that—but not opulent. There were lots of other people from Los Alamos coming to Chicago, and they needed temporary housing while they got themselves set up. So the servants' bedrooms were temporary housing for all sorts of people who came through and needed a place to stay.

LIPPINCOTT: Do you remember any?

CHRISTY: I'm pretty sure Stanley Frankel and his wife were there. I remember there was a chemist—I meet him from time to time at the National Academy; he's a very distinguished chemist. He tended to be somewhat short and a little squat—he still is. So that when my under shorts were up on the line drying, and his were up on the line drying, there was a very considerable disparity, since I am kind of long and lean, and he was short and squat. [Laughter] So I remember he was there, because I remember the comments about our different appearances. I remember the clothesline. And I enjoy his company very much, and I meet him from time to time at the Academy.

LIPPINCOTT: You were only six months in Chicago. What happened?

CHRISTY: Yes. Well, we proceeded, while we were there, to prepare to move out of this mansion and into our own place. We purchased half of a duplex—a house that had one common wall with another house. It was rather narrow, because mostly it consisted of a front room and a back room on each level—again, a little bit borderline in terms of the neighborhood. We didn't move in, because the occupants had a certain amount of time to make their own arrangements, and it had not yet become available for us to move in. But it would have soon.

I was busy teaching there when we got a telephone call from my good friend at Caltech, William Fowler. And I learned later that Willy was doing essentially Charlie Lauritsen's bidding. Charlie said, "Call up Bob Christy and get him to come to Caltech."

Now, what had happened there was that Oppenheimer had agreed to go to Caltech after the war. And he went out to Caltech and started teaching. But then he found that he had so many things—committees, et cetera; he was always going to Washington—he was so involved in national affairs at that time that it was not reasonable for him to live on the West Coast. He left Caltech; he could not keep up that life. And furthermore he perhaps felt that he had kind of grown out of the life of a professor. And it could well be, after running Los Alamos.

LIPPINCOTT: He became more of a public man.

CHRISTY: Yes. In any case, before he left, he apparently met with Lauritsen and others—I don't know who were the powers that were Caltech at the time—and they said, "We want to get a theoretical physicist." And he recommended me.

This, of course, was a fantastic compliment because there's no way that I could replace Oppenheimer. I don't know what all went on—all I know is that he told them to get me. And Willy Fowler was a young man, who had recently got doctor's degree.

LIPPINCOTT: You knew him before this?

CHRISTY: Not really. I knew him, but not well. And he called me up, and he said, "We want you to come to Caltech." Well, Caltech had a very special reputation then, as it does now. And furthermore, I had always felt that I liked the West. I don't like the humid climate of the East. So, although Chicago was a first-rate place, I was tempted. But I was really happy in Chicago, too. It was an excellent job.

So I took the news home to my wife. I said, "Here, we have this offer. What shall we do?" And my wife knew what we would do. I didn't know, because I felt both were very good opportunities; she knew, because she didn't want to live in Chicago—very simple. And I understand that. I basically didn't like living in Chicago either.

LIPPINCOTT: She was Canadian, too?

CHRISTY: Well, in a sense. She came from Canada, but her family had been refugees from Russia. But they had lived in Vancouver for a long time.

So the decision was made. My wife decided we were going to go to Caltech. So we did.

I remember taking an airplane trip from Chicago across to Los Angeles in order to look the situation over, and try to find some housing. And I remember that airplane, because I found out that I didn't like flying in the airplanes of those days. I think they were DC-10s, propeller planes; they flew fairly low. They would go up to about 10,000 feet or so, fly to the next city a couple hundred miles away, and go down, exchange passengers and then go up to 10,000 feet and fly again a few hundred miles.

LIPPINCOTT: How did they get over the Rockies?

CHRISTY: They had to go a little bit higher then. Anyway, I found that in the summertime, across this country, there is all sorts of convection and turbulence in the air down near the ground. And it tops out at around 8,000 or 10,000 feet. So they would fly just high enough to get over this so it would be relatively smooth. Every time they came down, I got sick. So I got sick, hour after hour after hour. It was just terrible.

I found us a place to live, which was not a very wonderful place. I believe the address was 175 South Greenwood Avenue. It's between Colorado and Del Mar. That's an area that was built up quite a long time ago, with wooden homes of medium size. And that's all I could get. Everything was tight—housing was tight—and I had to pay, I believe it was $12,000 for that house. That was a fantastic sum of money, considering that I had nothing. And I had to go to the authorities at Caltech.

LIPPINCOTT: Did they give you some help?

CHRISTY: Yes, they did. I told them, "Fine, I'd like to come. But I can't afford to buy a house." I talked to Earnest Watson; he was Millikan's right-hand man. He was kindly, and he found that it was possible for Caltech to loan me some money. And they did, and I was able to make the down payment on the house.

Then I had to go back and get my family. We loaded all our belongings onto the train, and we went West on the train. We didn't have a car. At Los Alamos, I forgot to say, I had owned a car, for about a year. I bought a secondhand Ford there, and I used it to take people skiing. It was used for recreational purposes. And when I left, the car was sold to someone else.

So when I was in Chicago, I had no car. And the train was an excellent way to travel. I would go from Chicago to Los Alamos by train. Train was the easy way to go. You see, this was official travel; our way would be paid by one agency or the other. So overnight, we would pay to have a berth. And it was very, very comfortable. I used to enjoy eating in the dining car, in some elegance. The dining car was a very elegant restaurant. You were served good food, and you saw the scenery go by. It was just a very nice way to travel.

At that time, when we came out to Los Angeles, we had a small baby in a basket and a small boy. And the baby in the basket was up on an upper berth or something like that. Anyway, we got out here. We got ourselves settled, gradually acquiring enough furniture to live in the house. We were never too happy with the house, because it was really not a very attractive kind of house.

LIPPINCOTT: How long were you in that house?

CHRISTY: That was ’46. I would guess on the order of five years, but I really don’t know.

LIPPINCOTT: And you went right to work with Fowler and Tommy Lauritsen?

CHRISTY: I went to work teaching. In the fall of ’46, I was teaching classes. And there were graduate students around who needed thesis supervision.

LIPPINCOTT: You had a story about Feynman coming and not wanting to take any students.

CHRISTY: Well, yes, that was a little later. He came probably a couple years later—maybe it was 1950.

I hadn’t done much teaching before. It’s a busy time when you’re starting off to do things like that. But there were good people at Caltech. So it was stimulating company and lots of fun.

LIPPINCOTT: Who do you remember, besides Fowler and Lauritsen?

CHRISTY: Well, there was Lauritsen's son, Tommy Lauritsen, who was about my age, maybe a shade younger than I was. I remember we had a deal once. He had to find a house. And I remember helping him. Maybe it was when I first came out here. Anyway, I helped find him a house. And he used to say that I got to take the second house we found and he got the first one. He had a young wife. It was a very friendly group.

LIPPINCOTT: What about research? Did you have time for that?

CHRISTY: Oh yes, because I was looking after graduate students. Most of my research ideas I fed to graduate students, because I did not have time to proceed on my own independent research, I was so busy trying to keep graduate students busy.

LIPPINCOTT: How many did you have?

CHRISTY: I would have to look it up; I don't know. I know it was enough to keep me busy. Some people managed large groups of graduate students. As far as I'm concerned, one or two was lots. As I say, I was never a big manager of a large group.

I know the next significant thing was Lee DuBridge coming. Lee DuBridge was hired around the same time I was. And in the fall of '46, he came to Caltech. He had gone from the Rad Lab [the Radiation Laboratory at MIT] back to Rochester, where he had been before the war. He came here as president. And certainly—I think everyone agrees—he was a tremendous success as president.

LIPPINCOTT: Did you have much personal contact with him?

CHRISTY: Not too much, a little, because as president, he ran the whole institute. I think I got a promotion to associate professor when I came here. I believe that was my first job here.

LIPPINCOTT: Yes. And then you became a full professor in 1950.

CHRISTY: Bob Bacher came here sometime around 1950. He had been a member of the Atomic Energy Commission. And I guess when his term was up, he agreed to come here as chairman of the physics division. And that was really a great thing for Caltech, because Bacher got along well with DuBridge. They had been associates. And so he was able to get help from the president and really put a lot into physics at Caltech.

LIPPINCOTT: So this was like a blooming of physics?

CHRISTY: I think so. It really, really developed. Another reason for its development was that the Office of Naval Research was supporting physics heavily in research. And because of the associations developed largely by [Charles] Lauritsen with the navy during the war, Caltech was very heavily supported by the Office of Naval Research. This wasn't to do navy work; this was to do pure physics. And the result was that physicists had so much money from research grants, that they were able to charge basically half of all faculty salaries in physics to research grants. It was legitimate, because we spent our time doing the research. But nevertheless, it meant a really unprecedented release of institute funds, since half of all salaries in physics were being paid by outside research funds. The institute therefore had a lot more money available. So in an indirect way, in those days, the physics department was supporting the rest of the institute; because ONR was pouring its money into physics, which released money that went into other divisions, where research support had not yet developed.

LIPPINCOTT: Were math and astronomy connected to the physics division at that time, too?

CHRISTY: Yes, they were part of the division. But physics was the overwhelming part. And that's where the research money came—primarily to physics, although some to math. Since then, of course, the other divisions have developed very good research support—chemistry and biology. But it wasn't so then.

LIPPINCOTT: And you worked in the Kellogg Radiation Laboratory?

CHRISTY: Yes. Willy Fowler and the Lauritsens worked in the Kellogg Radiation Lab. And since they had brought me to Caltech, I felt obliged to help them out. And I did theoretical and nuclear physics in that context for quite a number of years. But I also did theoretical particle physics. It wasn't as though I was restricted to doing the things they were interested in. I did things they were interested in, but I also had been interested in particle physics since my doctorate work. So I tried to keep up with theoretical particle physics, too.

LIPPINCOTT: Did you ever go to one of those Rochester conferences?

CHRISTY: I went to a number of the Rochester conferences in that period. I was quite well acquainted in theoretical physics at that time. I'd go up to the meetings; I knew what was going on. I was not a leader in the particle theory, like Feynman and Gell-Mann and so forth were. But I was doing leading research in the applications of the theory to experiment. And in those days, some of the principal experiments were cosmic-ray experiments and the discoveries about the meson. I was involved in the work on that—applying what we knew about particle theory to what was seen in the cosmic rays. My dissertation was on that, and I went on with work of that kind, because there were a lot of new results coming in from cosmic rays, on strange new particles that were being found—and the interpretation of those experiments was quite interesting. A very exciting time.

LIPPINCOTT: Before we get beyond that, tell me the story about Feynman's arrival.

CHRISTY: Well, one of the first things Bob Bacher did was—he had known Feynman at Los Alamos, and he felt—and I'm sure DuBridge and other senior people in the division agreed—that the thing to do would be to get Feynman here. Now, right after Los Alamos, Feynman went to Cornell, because Bethe knew what a gem Feynman was. And Bethe long ago was the first one to say, "Feynman, you've got to come to Cornell with me." So Feynman went to Cornell right after the war. And he was very happy there. That's where his work on quantum electrodynamics, the Feynman theory, was done. But somehow or other Bob was able to persuade him to come to Caltech. I don't know all the details. I know that Bob had to promise him all sorts of strange things, like a year off. Feynman had a sabbatical coming, and he wanted to go to Brazil. So Bacher said, "Fine, you can go and have your sabbatical. Go to Brazil, and then come back to Caltech." It's the kind of deal that normally a department head or division chairman would never make—letting you take a sabbatical before you come. But Feynman was very special. And Bacher made a lot of concessions to get Feynman to come.

Another one of the concessions was that he wouldn't have graduate students. Feynman said, "Well, I don't really want to have to supervise graduate student theses." At Cornell, that had worked out beautifully, because Bethe was kind of a theoretical machine. He had all sorts of ideas, and students doing calculations on this and that. Bethe could handle large numbers of students; he had lots of interesting things for them to do. So Feynman could pursue his own ends, and he didn't have to worry about the theoretical students. He liked teaching, but he didn't like to supervise. The way he told it to me once was, that if he could formulate a problem sufficiently straightforwardly for a graduate student to do for a thesis, he could do it himself in one evening. If he could get a problem that clearly set, he couldn't refrain from doing it.

So Bacher came to me and said, "Well, what are we going to do?" And I said, "Well, I guess I could agree to supervise the theoretical students' thesis work." Because I knew Feynman, and I knew that he would be a fantastic addition to Caltech, to the physics division and for me personally, as a colleague. So I was quite willing to take on an added burden by agreeing to supervise graduate students' theses.

LIPPINCOTT: Before we get out of the fifties, just to return to the politics again: In 1954 came the security-clearance hearings for Oppenheimer. And Teller was a problem, testifying against him. What were your feelings about Teller?

CHRISTY: Well, my feelings were very strong. I told you earlier that in some sense I viewed Oppenheimer as a god. He was on a pedestal, and I looked up to him. And I was sure that he was not a treasonable person. I knew he had leftist contacts; that was well known to everyone. But I felt that it was just the wrong thing to do, for an honorable physicist to testify against Oppenheimer. It just wasn't right. And I was very upset by it. I still am. I felt, therefore, that it was really improper, it was wrong.

LIPPINCOTT: Did you try to contact Teller about this?

CHRISTY: No. I ran into him not long afterward. We were both at Los Alamos—this was in the summertime. I remember that the Fuller Lodge was where they had an eating establishment. It was a fine, beautiful old log building. And there I was, eating. And I happened to see Edward Teller. I believe he approached me with his hand out to shake my hand. And I very deliberately refused to shake his hand.

LIPPINCOTT: Do you remember if he said anything?

CHRISTY: No, I don't remember. But it was a very deliberate action on my part—impulse, of course, because I didn't have time to plan this. And it was recognized by everyone else for what it was—that I refused to have a direct association with him. I think he was somewhat hurt.

LIPPINCOTT: Have you seen him?

CHRISTY: I've seen him from time to time. Our relationship has remained cool. Since that time, I have disagreed with him in a number of areas. For example, the Strategic Defense Initiative—I have disagreed with him, but I have not argued with him publicly, because Teller operates at a much different level than I do. He's a confidant of presidents; I'm not. As I say, I merely disagree privately, and that's the way it is.

LIPPINCOTT: Also in the fifties, there was some talk about having disarmament negotiations with the Soviet Union. And you were in favor of this.

CHRISTY: I don't remember. It sounds plausible that I would have been.

LIPPINCOTT: And Tommy Lauritsen was also in favor of it.

CHRISTY: Well, I know I was in favor of discontinuing our atomic testing—our atmospheric testing, primarily. I did get into a kind of altercation with Lee DuBridge on that subject, before the Nuclear Test Ban Treaty. I believe this was when [Adlai] Stevenson was running for the second time against [President Dwight] Eisenhower. Stevenson advocated stopping atmospheric tests; that had become an issue of some debate. I know that [Linus] Pauling took a very strong stand.

At that time, either DuBridge or Bacher—or both of them—were on the President's Science Advisory Committee. And they felt, I think, associated with Eisenhower and with the policies he was supporting. We—Tommy and I—organized a group at Caltech, on the order of a dozen people, to make a public statement in the *LA Times*, or something like that, saying that we scientists supported Stevenson's stand. But we felt our words would carry a little more weight if we identified ourselves as scientists from Caltech, which we did. Now that's considered not quite proper, because then you're attaching the institution's name to your views. But we didn't say that these were Caltech's views, we just said they were *our* views. I can see some reason to argue about the way we did it, but we wanted what we said to be noticed and have an impact.

DuBridge was angry. The government's view was that there had to be tests. And I suspect that some of the trustees got upset, too.

LIPPINCOTT: So what did they do? Did they call you on the carpet?

CHRISTY: Well, no. Because no matter what Lee may have thought privately, he was always one to defend people's right to their own views—even though he felt we had done wrong, by using the name of the institution. Nevertheless, we felt that our point of view had merit—namely, that atmospheric testing should be stopped. And we wanted to support the candidate who was advocating that—namely, Stevenson. Anyway, we did get into quite an altercation with Lee DuBridge on this subject. But as I say, it never stood in the way. Lee always was a gentleman. I remained friendly with Lee for a long time after that.

LIPPINCOTT: Do you remember that one of the effects of the Nuclear Test Ban Treaty was that they had to stop testing the Orion Project. Did you ever hear about that?

CHRISTY: Oh, yes, I worked on Freeman Dyson's Orion Project. I went down to La Jolla, at least for one period of a couple of weeks or more one summer, and worked with Dyson and [Theodore B.] Taylor, and others, on the Orion Project. It was a crazy idea, which conceivably might have worked, but it was crazy. The idea was to propel a spaceship that might be as large as a destroyer through space by setting off in the back of it a succession of atomic explosions. No one could figure out how to test it. You had to have a whole mess of atomic explosions going off. But the project was too far out; it wasn't for this world, or this time.

I always used to enjoy Dyson; he was an imaginative person, with a very wide and thorough knowledge of physics. So it was an enjoyable thing to work on for a few weeks.

LIPPINCOTT: So you knew him before you went to the Institute for Advanced Studies for your sabbatical?

CHRISTY: Oh, yes, because I used to be involved in particle physics. Although I wasn't a leader in creating theories, I was involved, and did some useful work. So I'd meet him at conferences and so forth. I don't know whether I should tell you about any of my efforts in particle physics or not. There was one discovery I made—this had to do with the two-meson hypothesis. This was to realize that there were really two different mesons involved. One was the mu meson of cosmic rays, and the other one was the pi meson of nuclear physics—now called the pion. [Tape ends]

Begin Tape 4, Side 1

CHRISTY: Experiments in Britain by C. F. Powell, using balloon-borne photographic emulsions came up with some very puzzling results about the way mesons behaved. So it became really a key puzzle in particle physics, to figure out what was going on. My friend Bob Marshak, whom I had known at Los Alamos, was at the University of Rochester, and he worked this out. But before I knew that he had worked it out—before he published it, before I knew anything about that—I got the same idea and was developing it at Caltech. So we had independently perceived this idea. He published it promptly, and it is known as his achievement. At the same time, I had given the job to a student as a research project—this was Richard Latter. And it took a while, a year or so. And eventually his thesis came out [1949], and our work became known. But it was clearly no competition. As I say, this was one of those occasions where having extra students to look after kind of slows you down a bit.

It's surprising how often a new idea will pop up in different places at roughly the same time. I think, as they say, circumstances make the man, or something like that. I believe the circumstances surrounding a discovery feed into it—feed into someone in Japan, or someone in England, or Russia, or the US, so all sorts of people are thinking about the same thing. And it's not too surprising that several people may come up with the same idea. You're surrounded by facts that force you into thinking in a certain direction.

LIPPINCOTT: You must have been somewhat discouraged.

CHRISTY: I was somewhat disappointed, yes, when I saw Marshak's publication, and I was just getting a thesis going on it. But that's the way it goes.

LIPPINCOTT: Have you talked to him about it?

CHRISTY: I once wrote him a note. He died just recently. But I was active, as I say, in this period. I was following the latest advances.

LIPPINCOTT: When was this?

CHRISTY: It was in the late forties, early fifties. [Latter's thesis was published in 1949] Around that time, I began to devote more of my attention to nuclear physics and less to particle physics—probably because the particle physicists were getting a little bit beyond me, and the theories were getting more and more involved. As I say, I was an applier of theories, primarily, rather than a creator of theories.

I did do some interesting work on strange particles. It was not terribly significant. But strange particles were beginning to pop up. And much of the early work came out of Carl Anderson's laboratory at Caltech. I was interested in that; I followed it. And I occasionally had a semi-useful idea.

LIPPINCOTT: Didn't you begin to get interested in astrophysics?

CHRISTY: I got interested in astrophysics; I also pursued nuclear physics more, about this time. I did theoretical calculations in nuclear reactions.

But I had a student during this period. We used to go to seminars at the observatory office on Santa Barbara Street. So we kept, more or less, in touch with some of the things going on in astronomy. We also attended some lectures on stellar structure. So I was somewhat aware of stars because of my associations with people in astronomy. I had my student do a thesis on the source of energy in the sun. At that time, it was generally believed that the carbon cycle was the primary source of energy in the sun—this had been put forward by Bethe, and he got a Nobel Prize for it. So I had a student work on this. And as a result of this work, it became clear that the proton-proton reaction was contributing more to the sun than the carbon cycle was, although both were involved.

The student's name was Father O'Reilly—James Donald O'Reilly. He was a Catholic priest. He did a thesis on that [1950]. And it was a contribution. And it kept my attention on stars, too.

Sometime around this time, there was also Project Vista—roughly at the time of the Korean War. It was a study project organized at Caltech to study the defense of Western Europe. The concern was whether Western Europe could stand up to an attack from the Soviet Union. It was customary for scientists to participate in study projects on things they basically knew nothing about—as we knew nothing about the conduct of warfare. So we had a study project lasting about a year. We visited army bases; we learned about how the army and the air force operate.

LIPPINCOTT: How many Caltech scientists were involved?

CHRISTY: I don't remember. I know Willy Fowler was the director of the thing. Charlie Lauritsen was actively involved.

LIPPINCOTT: Were the participants principally physicists?

CHRISTY: There were some from all walks. Jesse Greenstein was actively involved. It was a pretty good-sized group—I'd estimate fifty, sixty people or so.

The project occupied an old hotel that was no longer in use. It was the Vista del Arroyo

Hotel, on the banks of the Arroyo. We took that over. We had meetings; we traveled around. One of the things we tried to contribute was the possible use of tactical atomic weapons in defending Western Europe—because tactical atomic weapons had not been invented at that time. There were just strategic weapons, for bombing cities. So we investigated whether or not atomic weapons could be used to our advantage on the battlefield, in order to help defend Western Europe. And we did know something about atomic weapons, so we made some estimates as to whether they could be made small enough to be carried by fighter airplanes. You see, the original ones had been very big. So that was one place where our expertise had some bearing. And I think we put out a chapter on that, and on other things. And I have a feeling that that chapter never saw the light of day. I think it was so antithetical to the beliefs of the powers that be in the armed services that they wouldn't ever let the chapter out. I may be wrong on this. My recollection is that the Strategic Air Command believed that atomic weapons were its private baby. And the idea that other units could have atomic weapons was, I think, practically treasonous to them, because it would interfere with their monopoly. If you want to know any of the details about this, Willy Fowler probably has a better memory than I do.

We spent a year at that. We were allowed to take a year off. And we put out a report of which, as I say, a large portion was hidden by the military. That was Project Vista.

Then it was along about this time that I moved my office out of Kellogg and around the corner into the Sloan building. Because I felt I wanted a little bit more independence. I was surrounded by people I admired, but I wanted to be more independent—to be able to perceive my own ideas without feeling that I owed everything to them. So I moved into an adjoining building.

LIPPINCOTT: I know that just before you went to Princeton, you became interested in those RR Lyrae.

CHRISTY: I think that would be afterwards. My recollection of the order of events is that I went to Princeton on sabbatical in 1960 and appreciated very much the opportunity to get away from everything for a year and to look around fresh. I am a firm believer in sabbaticals. But when you're busy and you have a family, it isn't easy. I took them to Princeton with me. I think my older son was then sixteen, and he practically rebelled, because it was his last year in high school. It was terrible, I know. But the fact is I felt I had to go. I had an opportunity to go. I'd never taken a sabbatical before, and the Institute for Advanced Study is a terrific place to go. So although it was very hard on him—and as I say, he was almost in open rebellion—we went.

While there, I did two things. I polished off some work that I had done but had not quite written up, on nuclear physics. And having written that up, I then, for some reason or other, thought I would try and learn something about stars. I had this connection, off and on, with stellar problems. So I started reading intensively in astronomy journals and so forth to find out what was going on and what was interesting. That's the way I learn things, by reading what people are doing and finding out what the problems are and where I can make a contribution.

In the course of doing this, I developed the idea that perhaps I could make some contributions to understanding these Cepheid variables. I wasn't sure; I thought perhaps I could. So I studied this subject fairly intensively in the journals while I was at Princeton. I left there with a pretty good idea as to what the problems were, what was known about Cepheid variables, with an idea that I might be able to work up a theory. So I came back and intensively started to work on that theory.

LIPPINCOTT: The Cepheid variables had been used as yardsticks for quite some time, but there were things that were unknown about their brightness?

CHRISTY: Yes. It was unknown why they varied, what made them vary. It was known that they were apparently spherical pulsators. That is, they expanded and contracted—a regular expansion and contraction in spherical symmetry. That was known observationally, but why was it so? I knew something about the static structure of a star, because I'd studied that a little bit. And I thought: Well, this is very much like the spherical hydrodynamics in implosion. It's basically the same equations we had used—of course, with different substances—but the mathematical approach was very similar to what had been working on at Los Alamos. I had never done those implosion calculations myself at Los Alamos—as I told you, I was a kind of go-between—but I knew about them. So I started to apply that theory to stars, and to apply the numerical techniques that had been developed at Los Alamos, to this problem on RR Lyrae stars which are similar to Cepheid variables but smaller. The approach I used was, I set up a mathematical model of the star and calculated its motion. I started the star moving by imposing initial conditions that had it collapsing a little bit—just a little bit away from equilibrium. And then, of course, it had to rebound. I started it vibrating, and the model kept on vibrating back and forth.

The key question was: Could I make a sufficiently efficient program so that I could do this in a reasonable time on the computers then available, and what would happen when I started the star vibrating? Would the amplitude of the vibration slowly diminish and stop—which is how any normal thing would behave; you start it vibrating and then the vibrations die away and stop. But in the case of these stars, I found that if I picked the right stars and started them vibrating, the amplitude of the vibrations would increase. And they increased until they reached approximately the amplitude that is seen in the sky for these stars. And there the amplitude leveled off and the model kept on vibrating forever: it was a self-excited oscillation. That is, even if I hadn't started the model vibrating, it would have found some little quiver and gradually would have built up an oscillation. I started it to save time, but then the amplitude increased by itself. Of course the source of energy was simple enough. A star is a fantastic source of energy; the energy comes from the center of the star and flows out. So it has all sorts of energy available. The question is, how does it use this energy to make vibrations?

So I studied these calculations and was able to understand why such stars vibrated. I was able to show that only stars in a particular region of the color-magnitude diagram would vibrate in the way that was seen. And I was able to get relations involving their mass and so forth, that had not been gotten before.

So this was very successful. And I thought it was interesting, in a way, that the theory used to make atomic implosion bombs was the same theory I could apply to certain kinds of variable stars. It's interesting to see how things relate to each other. I enjoyed working that out, and I got some distinction, because although others had tried—there was a group at Los Alamos that had tried to do this using the Los Alamos computer codes—it turned out that it was more effective for me to start from zero and create my own codes. Because the bomb codes were so complicated and so hard to use, that it was more effective to develop a new code from scratch. And I think there's a lesson there. If you have an all-purpose machine, it may not be the best way to do a problem.

Anyway, I was able to get this done and get results, and I got some awards for that.

LIPPINCOTT: I believe you published this work in 1966. And then the next year, you got the

Eddington Medal of the Royal Astronomical Society.

CHRISTY: Yes, that was a very significant award, and I was very pleased with that.

LIPPINCOTT: Did you have to go over to London?

CHRISTY: I went over there and received it from the Royal Astronomical Society. I gave a speech about certain aspects of the work which had not yet been thoroughly verified, and the intriguing things that went on inside the stars, which told us interesting things and had not been completely verified at the time.

LIPPINCOTT: Was your audience, then, mainly astronomers?

CHRISTY: Mostly astronomers.

LIPPINCOTT: Had many physicists gotten that award before?

CHRISTY: I'm sure. Eddington himself I always considered a physicist—he's a great astrophysicist.

LIPPINCOTT: And did you begin to then consider yourself an astrophysicist?

CHRISTY: I worked in that field, because I had done some successful work in it. I've worked in a variety of fields of theoretical physics—particle physics, nuclear theoretical physics, astrophysics. And I enjoy trying to find areas where I can apply my basic understanding of physics to understanding nature.

ROBERT F. CHRISTY

SESSION 4

June 22, 1994

Begin Tape 5, Side 1

CHRISTY: I wanted to go back to something about my experience at Los Alamos, and to a lesser extent at the Met Lab in Chicago, because I think it's relevant. To me, as a young physicist just out of graduate school it was the most fantastic educational opportunity that one could imagine: to work with the leading theoretical and experimental physicists in the world—to work with them, to kind of get instruction from them. It was the most fantastic education that could happen to anyone! It was a very, very remarkable experience. Of course, it was a remarkable experience in human terms, too, but that's quite separate.

I neglected to mention that it was during the sixties at some time, I think, that I was elected chairman of the faculty here at Caltech.

LIPPINCOTT: I believe you were executive officer for physics in '68. That's according to a profile of you in *Engineering and Science*. And as chairman of the faculty, what I've got is '69.

CHRISTY: I was made provost in...?

LIPPINCOTT: In '70.

CHRISTY: In '70, was it? Well, maybe I was chairman of the faculty then, I don't remember. But in the mid-sixties, my wife and I divorced. And it came at a time when the boys had gone off to college. My older boy went to Haverford, and the younger one went to Harvard.

LIPPINCOTT: Were they scientists?

CHRISTY: Yes. The older one, Ted, tried about everything. He started off in chemistry, because he didn't want to clash with me on physics. But when he was there, he went into physics and tried that, and then into biology and tried biology. And I think he enjoyed biology more. He left Haverford and went to Berkeley as a graduate student in biology in the late sixties. He dropped out—which was more or less typical for the period—for a couple of years, until I managed to persuade him to get back and go to medical school. And he went to medical school at USC then, and finally became a family-practice physician, which I think he's enjoyed very much, and has been a very useful member of society.

My other son, who went to Harvard, had been interested in computers even in junior high school, although that was before computers existed. But I had helped him put together the essence of a computer with some relays that I got. And these relays would operate, and they would do the "yes/no" commands. He got into computers very early. Then when I started doing my calculations on the Caltech computer, he and a friend of his decided to program the computer to come out with instructions for a card stunt for a football game. You have all these people in the stands, you know, to do the card stunts. Each one has to have instructions as to what he does under certain circumstances. So my son and his friend set up a program. If you defined the card stunt you wanted, this program would write out instructions for everyone in the stands, according to where they sat.

He then went to Harvard as an undergraduate and took a combination of electrical engineering and mathematics, or computer science. And then he went to Berkeley as a graduate student in electrical engineering.

LIPPINCOTT: Did he miss out on the ferment of the sixties?

CHRISTY: Not precisely. He got into the later stages—when it was not quite as bad. But most computer people in those days essentially went into business rather than taking advanced degrees. So after a couple of years in Berkeley, he left and went to Digital Equipment. And he's moved around from one company to another. He was part of his own company for a while. He's now a manager of advanced products for Apple. But, as I say, they've both been, in their own ways, really quite successful. And I'm proud of them. Very technically inclined.

LIPPINCOTT: On the subject of student unrest in the sixties, there was a bit of that at Caltech.

CHRISTY: A little bit. That was when I was provost, I remember. I know that I assisted in standing up in front of a group of students wanting to have the administration do something which we didn't think should be done. And I stood up in front of them and persuaded them not to.

LIPPINCOTT: And how did they take it?

CHRISTY: It worked. They wanted to lower the flag for some reason that was not part of Caltech's general ritual for lowering the flag. And I defended the flag. But Caltech students were never very far out; they were a very good bunch of students, but they had a few wild times. I think most of the unrest then was on the part of the humanities students rather than the science types, because the science types were too busy working. That's probably why Caltech came through with very little—simply because people worked hard with their noses to the grindstone. They had very definite programs in mind. They didn't have time to let their thoughts wander all over as to what's going on in the world.

LIPPINCOTT: Six months at Cambridge University in 1967.

CHRISTY: Oh, that was fun! I was what was called a Churchill Fellow. This was a fellowship awarded by the fellows of Churchill College, Cambridge, which was a college founded in honor of Churchill, but it had definite connections with the US. So they brought over visitors, often from the US, to spend time there rubbing shoulders with the students and faculty. In general, it was mutually profitable. I wanted to go to Cambridge, because I had a sabbatical. I didn't teach there; I did research. It was a wonderful experience. I thought then that Cambridge and Oxford were really the epitome of college experience. I never went so much for Oxford, but Cambridge, I still think, is really tops in terms of what a college should be: the surroundings, the way things are done; everything about it. I just think it's wonderful. We don't do things that way in this country. We don't have the tradition.

Now, Harvard has a lot of tradition, but it isn't put together the same way.

Anyway, I spent some four or five months as a Churchill Fellow in the spring. I lived there at the college in a small apartment, and I went to seminars and other things having to do with physics and astronomy. It was a most enjoyable experience.

LIPPINCOTT: Whom did you see there? Do you remember anyone?

CHRISTY: Well, Cambridge had a bunch of good nuclear physicists at the time. And they had radio astronomers. And, of course, in college you meet people outside your own field—the other dons who are living there, or eating there. Dining in college is a very, very attractive experience. I'm all in favor of it. You might say it's very civilized.

LIPPINCOTT: You had been elected to the National Academy of Sciences, in 1965.

CHRISTY: Yes. I'm sure that that was due to the efforts of my friends at Caltech. Like many organizations and honors, you have to have done something to get them, but mostly it's the efforts of your friends, who nominate you and say nice things about you. And that's how you get such honors. And my friends at Caltech—notably Willy Fowler, I'm sure, and others—I think were very much responsible for that. I'm greatly indebted to them.

LIPPINCOTT: That was just about the time you were working on the RR Lyrae.

CHRISTY: That's right. I had done work that was noticed here and elsewhere. But you don't get honors just because you do something. As I say, your friends have to know it and put in a good word for you. I get forms all the time, to nominate people for certain honors. Now you wouldn't nominate someone who didn't deserve it, but there are so many deserving people who don't get these honors. It depends on your friends. It's the way the world runs—I'm convinced. So I was very pleased. I had had quite a number of contacts with the National Research Council, because I was on their fellowship-evaluation committee. I went there regularly evaluating applications for NRC fellowships both pre-doctoral and postdoctoral—mostly pre-doctoral. But that doesn't get you a membership in the Academy. That means I had paid some of my dues by being helpful.

LIPPINCOTT: Then, in '69, you became chairman of the faculty for two years. And you were fifty-three years old at that time.

CHRISTY: That sounds plausible, yes.

LIPPINCOTT: Did this kind of administrative work impinge on your research?

CHRISTY: Yes. To a very considerable extent, I allowed myself to be distracted from research into more administrative activities—in large part because I had come to the end of one piece of research and I did not see yet where the next thing was going to start. My work on variable stars I had pursued as far as I could see how to pursue it, and I had not yet started something new. So it was under those circumstances that I got involved in administration.

LIPPINCOTT: And then you were provost in '70. What did you have to do as provost?

CHRISTY: That's the academic vice president. Chairman of the faculty is very much a part-time job, in which you are looking after some of the interests of the faculty, but it's not really an administrative job. But the provost is the second man in the university, next to the president. And that was Harold Brown. He picked me to be on a committee to look for a provost, and I think I was chairman of that committee. So we selected a number of very promising names, and I traveled around the country to talk to these people and see if I could persuade them to come and take the job of provost.

LIPPINCOTT: These were all from outside Caltech?

CHRISTY: Yes. I think some of them later became presidents—and very distinguished presidents—of universities. So our committee did well; we picked good people. I went around and tried to persuade them, and I failed. There was Harvey Brooks and John Toll—a very distinguished group of people. But each time I failed, and I would explain to Harold what I'd been doing and what the interaction was.

After this had gone on for six months or so, Harold asked me if I would take the job myself. So I did.

LIPPINCOTT: Did you have some misgivings?

CHRISTY: Well, I had a very high respect for Harold, because he was a very intelligent person—very well organized in terms of running things, much more so than anyone else I ever knew. And I thought maybe I could help do something for Caltech that way.

LIPPINCOTT: What were your concerns with Caltech at that time?

CHRISTY: Just to try to keep it as good as it was. Under Millikan and DuBridge, it had developed into a fantastic institution, and mostly I wanted to keep it at that level. Harold wanted to broaden it a little. He was the one who acquiesced in the matter of bringing the social sciences here. We already had a humanities division, and it was always a little bit restless, because its members were, you might say, second-class citizens in a certain way. They were always highly respected—intelligent people who were excellent teachers—but it was generally recognized that they had not been chosen primarily as scholars but because they were good teachers. And Caltech was very lucky to have them. They gave the students an excellent education. But they were not viewed in the same way as the scientists, and they had aspirations to be treated more equally, in terms of what they were permitted to do. They had aspirations to broaden the division to include the social sciences. So that did take place, and Harold supported it—I might say, with rather more enthusiasm than I did.

LIPPINCOTT: What was the feeling among the faculty for letting social sciences in?

CHRISTY: I would guess mixed. Some probably felt that it was good for Caltech, and others mistrusted it.

LIPPINCOTT: You're talking now about economics—and psychology, maybe?

CHRISTY: Economics. There wasn't much psychology; there was a little. We did have a kind of psychology that tied into biology—psychobiology. That was started here in a rather respectable way by Roger Sperry, who did split-brain research. But it never tied into the humanities: it was the biological part of psychobiology that worked out. But the hope at that time was to find some kind of psychology that would bridge the humanities and biology. I don't think that ever worked.

Economics was emphasized. At least there was a kind of precise competence involved in economics. I never opposed it, because my boss was helping that development, and I worked with my boss. But I never had the same enthusiasm for it.

LIPPINCOTT: So the social sciences did come in about that time?

CHRISTY: Yes. Oh, I forgot. The other day I mentioned a historian here, who participated in our Vista Project, whose name I didn't remember. That was David Elliot, a very urbane, educated man, who is still occasionally around the campus, but not very much. He's retired.

LIPPINCOTT: You were provost on up through the seventies.

CHRISTY: With Harold Brown as president, yes.

LIPPINCOTT: Then you married again—Juliana.

CHRISTY: Yes, that's right. I ran into her at astronomy meetings, because I was going to astronomy meetings at the time—IAU [International Astronomical Union] meetings.

LIPPINCOTT: And you had a new family with her—two daughters.

CHRISTY: That's right. They were born in '74 and '76. That was an interesting new experience. A new family—and daughters instead of boys. The second one has just now been admitted to college. That's Alexa. She's going to Stanford. They will both be at Stanford. They enjoy it very much there.

LIPPINCOTT: Are they scientifically inclined?

CHRISTY: Yes indeed. The older one—called Juliana, although her nickname is Ilia—likes biological science. She thought at one time of being a veterinarian. She then thought of medicine. But that's as far as we can say. She doesn't know precisely which of the avenues to take. She has just finished her sophomore year.

The younger one has very strong capabilities in all of science and mathematics, but she doesn't know what she wants to do. She also writes well. She does everything well. She's a good athlete. They're both good. They're both very capable in everything they do; and they don't know what they want to do. They're young; they'll find out. I mean, when I went to college, I didn't know what I wanted to do. I hope they have an opportunity to be exposed to things that they will find rewarding, because that's what counts, to find what you want. Anyway, they're well started.

LIPPINCOTT: To go back to your work in the seventies, could we talk a little bit about the Classified Research Committee at JPL? You were chairman of that.

CHRISTY: There was, I guess, a problem in that Caltech… The essence of a scientific institution is to find new knowledge and communicate it. That principle is antithetical to classification and keeping things secret. So, in general, universities have long tried to stay clear of classified work, because it's not the way universities should run.

I remember when I first came here in 1946, I wandered around looking to see what was going on. And I came into a laboratory in the basement of West Bridge. And most scientists will invite you in and tell you what they're doing; they love to communicate. This person didn't want to talk; he shut the door. And that's not the way universities should run. And this is just an example of the fact that universities and secrets don't work together.

Well, JPL was connected to Caltech. And we did not want JPL to be constrained by secrecy to the point where people there could not talk to people on the campus. And yet some of the jobs JPL undertook had to have some secrecy. So there was a real problem.

LIPPINCOTT: How much of their work was military work?

CHRISTY: Most of it was not. There was a time when NASA's budget was getting very tight. And JPL, being a large organization, had a large number of people that they had to keep employed. And for a while they found it necessary to take in some classified work for the Department of Defense. But we did not want this to change the character of JPL. We wanted to keep the military work as a restricted thing, but it was not going to be the general pattern of JPL.

LIPPINCOTT: Did they ever do any work up at JPL on the Strategic Defense Initiative at all?

CHRISTY: I do not know. That came in after I was involved. And I do not know whether they did anything.

LIPPINCOTT: You were opposed to that, though?

CHRISTY: I was always opposed to it, because I felt that it was wrong. It would not work, and you were misleading people into spending a lot of money on a project that was technically unfeasible. And furthermore, it went in the wrong direction. At a time when the country should have been seeking ways of reducing armaments, it was a program to increase armaments.

LIPPINCOTT: This was Teller's notion, wasn't it?

CHRISTY: He was promoting it, yes. And I felt, for so many reasons, that it was the wrong way to go. I certainly opposed it. I was not usually one to take public stands and make public speeches, but I had definite views and definitely opposed it.

LIPPINCOTT: And then in '77 Brown left Caltech.

CHRISTY: Yes—fairly suddenly, I guess—to become Secretary of Defense. So Caltech was rather suddenly left without a president. And I, being nominally the second in command, was acting president while a new president was sought.

LIPPINCOTT: Was that for one or two years?

CHRISTY: I think it was a year and a half.

LIPPINCOTT: Did you feel the burdens of office?

CHRISTY: Yes. And I concluded that it was probably not proper—or wise for Caltech—for me to stay as president. I believe that if you're accustomed to being the Number Two man that means that you are following the leadership of someone else. If you're the Number Two man, you can't be the leader; you have to be the follower, because that's the nature of things. If both try to be leaders, someone's going to go.

LIPPINCOTT: And Caltech has often gone outside for their presidents.

CHRISTY: We normally have, yes. But, as I said, in my mind I didn't think I was suitable to be president, because I had become accustomed to doing the Number Two job. And, as I say, I think that's incompatible.

Now, we have had provosts here—and I should not discuss who—who were, you might say, vying with the president for the leadership of Caltech. And it didn't work. But I never felt that I was trying to second-guess the president. I was hired as provost to be his second-in-command, and he was the one who was the boss. And I felt that I was able to help him a lot and help the institute.

LIPPINCOTT: What did you think of Harold Brown's style as president?

CHRISTY: I think it was good for the institute at the time. It was very different from DuBridge. But the times were a little tough financially, and it needed tightening up. And Harold was a fantastically good administrator. He knew how to run things, and he did run them, very well. So it was right for Caltech at the time. It would not always be right. But at that time, it was the right thing. He was a strong president, and he made Caltech stronger. But he didn't, probably, make Caltech greater; he made it stronger financially.

LIPPINCOTT: Did you have to do a lot of fund-raising?

CHRISTY: No, I did provide some minor assistance in that. But the fund-raising is normally the province of the president and the vice president for Institute Relations. The provost is the academic vice president. He is involved in the internal affairs of the institute.

LIPPINCOTT: Did you have a provost while you were acting president?

CHRISTY: No. Things were on hold, because we were expecting to get a president at almost any time. I had a vice provost—Neil Pings, who later went to USC as something like the Number Two man for quite a number of years. He was very successful.

LIPPINCOTT: Then, in the spring of ’78, that’s when [Marvin] Goldberger arrived as president, and you retired?

CHRISTY: Well, not immediately. I stayed on as provost for a while, but the understanding with Goldberger when he came was that he was going to find his own provost. And I said, “That’s fine with me.”

LIPPINCOTT: Had you known him?

CHRISTY: As a physicist, yes. He was mostly at Princeton. I knew him and respected him as a physicist. He was a good physicist with kind of an unusual flair.

LIPPINCOTT: He was a particle physicist, wasn’t he?

CHRISTY: Yes.

LIPPINCOTT: Then in the summer of ’78, I noted that you took a trip to China.

CHRISTY: Yes. That was organized by the new president—for what reason, I don’t precisely know. A number of Caltech administrators took a trip to China, organized by the president. And whether it was just a boondoggle—whether he felt that he would like to have a trip to China—or what, I don't know. He went along. It was fun. To me, it was a very educational experience. We did establish contact with many educational institutions in China. China was just emerging from the Gang of Four business, and we were trying to establish the basis for teacher exchanges. But that would have happened anyway, I'm sure.

LIPPINCOTT: Where did you go in China?

CHRISTY: We landed in Hong Kong. We went to Canton. Then traveled to Beijing, traveled somewhat southwest to Xian.

LIPPINCOTT: Did you meet with Mao Tse-tung when you were there?

CHRISTY: No. We met with the heads of universities but not with heads of state. It was not a state visit; this was an educational visit. And then from Xian I think we went to Shanghai.

In human terms, as to what goes on in the rest of the world, I found this most educational. To see people whose occupation was pulling heavy loads along the roads, with their own human power—to think that that was the way things still happened in a very large portion of the world! I found it absolutely amazing—the amount of human energy that is needed in order to survive in a place like that. Now things are changing there, but I found it most educational, very interesting.

LIPPINCOTT: What about the universities?

CHRISTY: They were struggling, because they had had the cultural revolution.

LIPPINCOTT: The intellectuals were all sent out to…

CHRISTY: They were all sent out to pasture. And the universities were struggling just to get started again. So they were having a very hard time. But they clearly had fantastic potential.

They had a large number of very able students there. And, as we see, they come here and they study and they're very good.

LIPPINCOTT: Did anything really substantial come out of it?

CHRISTY: I think there probably were some connections made that facilitated some of the exchange of students coming here to study. If you want my personal view, I think it was a very nice boondoggle. A good trip.

LIPPINCOTT: And how long did you remain provost under Goldberger?

CHRISTY: I think it was on the order of six months, until he lined up someone.

LIPPINCOTT: Did you go back to teaching?

CHRISTY: I went back to teaching for a while. But I didn't find it so easy again, because after being out of it for eight or ten years, you begin to lose touch. So I wasn't as satisfied with the teaching as I should have been. I don't think I did it as well. And I was getting close to retirement, too. So I gradually retired.

LIPPINCOTT: And then you became emeritus.

CHRISTY: Yes. Now, during that period, I did try to learn a new field. Because I have always enjoyed exploring a new field to see whether I could find something where training in physics could make a contribution. And a field that I found fascinating then—and I'm still fascinated by it—is the question of what caused the oscillations in climate on the Earth that led to the Ice Age. And as you know, 25,000 years ago, there was an awful lot of ice in the Northern US and Canada. And this was not understood.

I did a lot of reading in this field—climate and so forth. I did not find any real key that I felt would unlock it. Although there were theories being propounded, I felt the ones I saw were really not fully adequate. And yet I didn't find anything better.

LIPPINCOTT: What were your ideas?

CHRISTY: My approach is usually to so immerse myself in the facts that you can then see what the constraints are. I read from time to time about the work that people have done on the circulation in the oceans, which has very profound influences on climate and has barely been worked out yet. Very complex. I don't know yet what the answer is, but I still find it an interesting field.

Climate is kind of a persistent weather pattern—persistent for hundreds of years; whereas weather is just what goes on from day to day, or month to month. People have learned a lot about weather, and they have found that it's technically unpredictable. You can predict for a few days, but you cannot basically predict for much more than a week. Because no matter how big a computer, you won't be able to make the predictions go much longer.

But climate is different. There are certain patterns of weather that are persistent for years that have nothing to do with your ability to predict from day to day. It's a superimposed pattern. [Tape ends]

Begin Tape 5, Side 2

CHRISTY: Anyway, that effort I regard as interesting, but ultimately unsuccessful. You can't win them all.

LIPPINCOTT: Tell me about the committee on Hiroshima and Nagasaki.

CHRISTY: It was around ten years ago that I was invited to join a working group that was trying to understand what happened in the explosion of the bombs at Hiroshima and at Nagasaki. The main question we were trying to understand was: What were the actual intensities of gamma rays and neutrons to which the populations of these cities had been exposed? And, of course, the relevant factor was not the exposure of those who died almost instantly or within a few weeks. The relevant factor was the exposure of those who survived and have continued to live there.

LIPPINCOTT: Would there be a difference? In other words, if it was mainly radiation from neutrons, the symptoms would be different from gamma ray radiation?

CHRISTY: That is a subject on which there is still not enough known. And that's not the area in which my own expertise took me. That's kind of a purely biophysical question, as to what gamma rays or neutrons do to you. The question I was involved in was: Ever since the bombs were dropped—or shortly afterward; I think it started in 1950—a group called the Atomic Bomb Casualty Commission, formed first by the United States, started to gather data on the Japanese populations of these two cities, as to where people were at the time the bombs went off, and what were their symptoms. Did they get sick? And it tried to follow their health. Roughly, once every year or two, these people come in for a check-up. And the commission has followed the health of about 100,000 Japanese ever since 1950. Some of these people had fairly high exposures to ionizing radiation, and some of them had physical problems as a result. Some have died of cancer, and others will probably die of cancer. Some had, perhaps, other physical problems. There were some women who were pregnant. One of the questions was: If a pregnant woman was exposed to this radiation, what would the effect be on her unborn child? So there are many things that medicine wanted to know about the effects of ionizing radiation on human beings. The number of people exposed to sizable amounts of radiation there gave doctors an opportunity to learn things that there was no other way to find out. When you can study 100,000 people who have been given some reasonably well-defined exposure to radiation, and follow all of their medical histories, this is very important information—not only for us and the Japanese but for the world.

But the one piece of information that was not very well known was: What was the actual radiation that these people had been exposed to? They had not been carrying meters with them at the time, because they hadn't been expecting the attack. So the purpose of this committee was to study the physical dosimetry. Once the committee knew where a person was and what the circumstances were—if the person was in a house, where was the house located?—it could calculate what that person's exposure to radiation was. I was part of the group of physicists—most of us associated with Livermore, Los Alamos, Oak Ridge, and some associated with Science Applications International—who were working on this, trying to find out what the exposures were.

LIPPINCOTT: When was this committee formed?

CHRISTY: It started in the early eighties, but I came into it around 1983.

LIPPINCOTT: Do you remember who else was on it?

CHRISTY: Yes, mostly people who are not really famous. There was Bill Loewy at Livermore, George Kerr at Oak Ridge, Paul Whalen at Los Alamos—not famous, but busy workers in the trenches, who were very good.

LIPPINCOTT: Were they not your generation—younger?

CHRISTY: Mostly younger, yes.

LIPPINCOTT: And you went over there?

CHRISTY: We visited Hiroshima and Nagasaki. Mostly we had meetings, and they did work for which they got paid by the Department of Energy.

We did discover certain physical artifacts that could be obtained from Japan and studied. The point was to find things that would not have been very much disturbed. For example, it was found that techniques that had been developed since the war for studying archaeology—for determining the age of pottery—these techniques were developed. And it was found that radiation on certain materials, like quartz crystals, causes excitations. And if the stuff stays cold, these electrons stay excited. Then, a thousand years later, you can warm up these little quartz crystals and look at them with a photometer to measure the light, and you find flashes of light coming out. It's called thermo luminescent dosimetry. As I say, it was a technique pursued to study ancient pottery.

LIPPINCOTT: How was this applicable to objects found at Hiroshima?

CHRISTY: The roof tiles. Roof tiles are made of pottery. And some were collected from old buildings that had not been totally destroyed, and taken to laboratories in Japan, England and this country, and they would grind up the tiles, get out the quartz crystals, heat them up, and look for the light and find out what the radiation was—the total number of gamma rays that had hit that roof tile. It was fascinating! We were able to encourage the use of these techniques and help assign people to do this work—to get the materials at all sorts of distances. Not too close in to ground zero, but at relevant distances from, oh, about a half a mile or a little less, on up.

LIPPINCOTT: Were any of your findings surprising, or unexpected?

CHRISTY: Not really. We had some idea from previous work. But previous work had looked at radioactivity left in cobalt—which was looking for neutrons. Gamma-ray radiation had mostly been done by calculation, because [gamma rays] didn't stay around. But work did quite accurately verify the very detailed calculations we had made. So there is a very good correspondence between the measurements and the calculations. The calculations could not really be trusted without having some measurements to compare them with, so the measurements were very important.

The other part of the story—the neutrons—we had more trouble with. And in fact, the neutrons are still giving us trouble.

LIPPINCOTT: What do you mean?

CHRISTY: Well, we have not yet found an adequate agreement between the calculated neutron intensity and the observed neutron intensity. Whether this is the fault of our calculations or observations, we don't know. But it's probably in the calculations. But the observations are good for telling you if you're on the right track. They don't tell you everything you want to know, but if you can pin down some things by measurement, then you can use your calculated model to tell you other things.

LIPPINCOTT: Are there reliable ways to measure?

CHRISTY: Well, there were a few ways. One was radioactivity—things that were irradiated at the time and still had a little bit of radioactivity left—not a meaningful amount but an amount that with the greatest of difficulty could be measured. And there was some radioactive cobalt left in iron that had been exposed. There was some radioactive europium left in some rocks and granite tombstones and things that people kept track of. These measurements, by the way, agreed pretty well with calculations on the Nagasaki bomb. They did not seem to agree very well at Hiroshima.

Then we discovered a new method—accelerator mass spectrometer. And that is, essentially, to look for unusual isotopes that are left over. The neutron is absorbed by a nucleus. If it's an unstable nucleus, it's radioactive. If it's nearly stable, it sits there. It doesn't give off any radiation; but mass-wise it is one greater. So you can then study how many of these nuclei there are around—and thereby measure the number of neutrons. And that method also did not seem to agree with the Hiroshima calculations. It did agree in Nagasaki. We're still trying to pursue the reason for that disagreement at Hiroshima.

LIPPINCOTT: Well, were there fewer neutrons than expected at Hiroshima?

CHRISTY: No, more.

LIPPINCOTT: Does that mean the yield might have been greater than what it was?

CHRISTY: Probably not. It probably means that we didn't do something right with our understanding of how the bomb exploded. That was the gun-type bomb, which has caused us no end of trouble.

Anyway, that work is still going on. It would have gone faster, except that each time a fair amount of progress was made, and a few million dollars were spent, the government said, "Well, gosh, we can't really afford to continue this," so the work stopped, we ran out of money. And then some new ideas were developed, and there was new enthusiasm on the part of the DOE, or someone, to put a few more million dollars into it, and it was pursued. And now we're running out of money again. This is roughly the third time we've run out of money. We may be on the verge of understanding it, but we don't really have it properly worked out. It's a continuing problem. But nevertheless, we have already improved our understanding of what people were exposed to there. Of those 100,000 people, quite a number have died of cancer, but normally if 100,000 people die, roughly a third or a quarter of them will die of cancer—that's just the normal course of events. Now, perhaps only 40,000 of the 100,000 have died already. And a fair number of those have died of cancer. By comparing the number of cancer deaths among people who were very distant from the bomb to the number of cancer deaths among those who were close in, you can begin to find how many so-called excess cancer deaths are associated with the radiation. And it looks as though there will have been something like 5,000 extra people dying because of the effects of the radiation. That's a large number. But here you have a bombing in which roughly 100,000 people died as an immediate consequence—either in the explosion or from fire or from burns—within the first few weeks. So that 100,000 people died more or less immediately, and if 5,000 died in the next fifty years or so, that's a fairly small number. So the residual effects have been fairly small compared to the immediate effects. What that tells me is, if there's a nuclear bomb and you survive the first weeks and months, then you've essentially made it. But the work of the committee is one of the principal sources of information for medical experts around the world on the effects of radiation on people. So it's been very important to try to get these numbers right.

LIPPINCOTT: How did you feel, walking around Nagasaki? How did they receive you there?

CHRISTY: Very respectfully. The Japanese basically are very friendly, the ones I have had contact with. We worked with the Japanese counterpart to this group. This is a collaborative effort with the Japanese. The Japanese people, as a whole, have a very deep concern about atomic weapons, because of the fact that they experienced them, so it's an important factor in Japanese politics. But in terms of individuals, people are always very friendly. It's not that they view you as some monster. I don't believe they harbor any resentment against individuals. I've seen no signs. But I've found it a very interesting field to get into—as to what happened.

LIPPINCOTT: Can we talk about your ranch now?

CHRISTY: Well, it was intended as a getaway. We purchased the property—it was some 240 acres—about eleven years ago. It was intended as a getaway, but it has been more work than we anticipated.

LIPPINCOTT: Well, that's an enormous amount of acreage.

CHRISTY: We haven't done much with it. But just within the last few years, we started to try to build a house. And that was work, trying to get something built that appealed to us.

LIPPINCOTT: Where is this ranch?

CHRISTY: On Route 5, if you go north, you go through a fairly old place called Gorman. And then just over the top of Tejon Pass, you come to an exit called Frazier Park. And if you get off at that exit and go west into the mountains for about fifteen miles, then, if you follow the right route, you will come to our place. You go through our gate, and our gate takes you into a nice little separate valley, which is beautiful and secluded.

LIPPINCOTT: How did you find such a property?

CHRISTY: Found it from the air. We had been looking. We looked down south, in the San Diego/Julian area, but it was too hard to get there. So we started looking up north, and we found a valley up there, called Lockwood Valley, that I'd never heard of before. It was rural, and it's only ninety miles from here. So it looked interesting. But we didn't see anything that immediately attracted us.

But I was taking a trip to Livermore in connection with this dosimetry work. And flying back, I looked down as I like to, to see what the ground looks like—particularly in the area where we'd done a little exploration. This is at the tail end of the Tehachapis. I looked down, and I saw Lockwood Valley. And then I saw next to it a little valley, very small, that I hadn't even known existed. So when I got home, I said that we should go up and look for this. We went up and we found it. It had water, which was most unusual—springs and a little stream. We then inquired who owned it, and pursued that and purchased it. It was just an attempt to get away, but it has been much more of a burden than we thought.

LIPPINCOTT: And do you ride horse back up there?

CHRISTY: Yes. I learned to ride at Los Alamos. We have some horses there—we have four horses. My second wife likes to ride, so we had thought of the possibility of having a ranch with horses. So we got this place.

LIPPINCOTT: How often do you go up there?

CHRISTY: Oh, a couple of times a week. The house is not quite fully occupiable, because we get distracted. It's being built now. The building is going on in a very casual way, with one carpenter. And we've been building a barn, too, in order to shelter some of our stuff—hay and things like that.

LIPPINCOTT: What committees are you on these days?

CHRISTY: Well, I mentioned this dosimetry committee; that still continues. And for quite a number of years now, I have been on one committee after another that has been pursuing a review of what's called inertial-confinement fusion. You usually hear about magnetic fusion. Fusion is where you somehow or other bring hydrogen atoms together to make helium. In magnetic fusion, you use magnetic fields, very intense magnetic fields, in very difficult configurations called tokomaks, to confine the hydrogen isotopes.

What I've been looking at is inertial-confinement fusion, where you try to make a very tiny hydrogen bomb—a hydrogen bomb less than a centimeter across. You don't use magnetic fields to hold the stuff together—it's held together only momentarily. It's driven together by an outside force—usually lasers. Very powerful lasers shine on this little pellet of material and drive it together very hard. And in the instant when it's most compressed it may—if you've done it right—fuse and detonate.

LIPPINCOTT: How powerful would that explosion be?

CHRISTY: It's hard to make them very powerful. And, of course, it's hard to make them very weak—if you make them at all.

LIPPINCOTT: Is this thought of as a practical…?

CHRISTY: Not as a weapon. It's thought of as a reactor-type device. The energy would then be contained in something probably ten or fifteen feet across. Then cooling material would take the energy out, and you would have an energy source. But it's very difficult to do. So there have been committees appointed, supported by the Department of Energy, appointed through the National Academy or the Department of Energy, reviewing our progress. Are we pursuing this in a sensible way? Should we continue? This work is being done at Los Alamos, Livermore, some work at Sandia, some work at Rochester, some work at the Naval Research Laboratory in Washington. And we've been going to these various places and trying to advise the Department of Energy as to where to put its effort in this direction, and what the prospects of success are. It is an alternate approach to magnetic fusion. Neither one is easy. Magnetic fusion is exceedingly difficult—both to make things fuse, and then to get the energy out—because of this enormous strange magnet they have to use. And the whole problem with inertial confinement is to make more power when you implode the pellet than you use up with your lasers. I think you could do it fairly easily, if you were willing to put a few tens of millions of joules into laser energy and not worry about whether you got it back.

LIPPINCOTT: What about safety considerations? As you're describing it, it sounds a little bit dicey.

CHRISTY: If you start with a very small amount of material—deuterium—in the way of milligrams or something, you can easily calculate that if you do succeed in making it fuse, it doesn't make that much energy.

LIPPINCOTT: And there's no problem with radioactivity, or is there?

CHRISTY: Well, supposedly fusion energy is clean energy. That's what they say. But it's a relative question. Fusion energy, for the same amount of energy, does make less radioactivity than fission does. But on the other hand, it still makes tens of thousands of times more radioactivity than we can stand. So the radioactivity made is one of the major problems with fusion energy as well as with fission energy.

LIPPINCOTT: And is there any difference between the laser type and the tokomak type?

CHRISTY: Not too much difference. A difference in the radioactivity is that fission makes radioactivity of short life, intermediate life, and very long life. With fusion, it makes short-life radioactivities and some intermediate ones, but not as much in the way of exceedingly long-life radioactivity. The same problems will exist but not as severely. But since people find [the radioactive waste problem] almost insoluble with fission reactors, that doesn't mean that a little bit less severity is going to solve things. It's going to be very difficult to deal with radioactivity. And it's also very difficult to get the energy out. It's very difficult to engineer either type of fusion to make it work.

LIPPINCOTT: So we're really not on the threshold of anything here?

CHRISTY: No way! Every time we meet and discuss the possibilities of success, it's always at least twenty years down the road. And it's been going on that way for the last forty years—fusion has always been twenty years down the road. And it still is at least twenty years down the road, if you can make it work at all. But technically, it's most challenging. The lasers are fantastic. And all the technology is fantastic. The calculations that people do are fantastic. It's very interesting to find out what people are doing.

LIPPINCOTT: It's a little like a peacetime Manhattan Project, almost.

CHRISTY: In a sense, yes. Although I worry that we don't know how to do things in this country anymore. During the war, the Manhattan Project was done in three years. We couldn't do that in thirty years now, because there are so many reviews and committees and rules and regulations. And we don't know how to do anything anymore in this country. We couldn't do it that way. I mean, General [Leslie] Groves would not be allowed to say, "Do this, do this, and just build it, and don't worry about the consequences." He wouldn't be permitted to, because he'd have to report to so many people that he'd spend all his time writing reports. We're strangling ourselves with regulations. Some of them, I'm sure, are useful, but it sure strangles our ability to get anything done.

LIPPINCOTT: Do you think that's what the problem has been with fusion, and not purely scientifically, or naturally?

CHRISTY: The technical problems are major. It's far more difficult than fission. Fission was like falling off a log; you could hardly *avoid* making a fission reactor. Fusion is very unnatural. In the center of the sun, it works naturally. But it does not work naturally on Earth. So it is exceedingly difficult, technically, to make it work. Whereas, it's very difficult to avoid having fission work. So there's a very great difference in the technical difficulty: very high temperatures that cannot be contained permanently, except by strange methods such as magnetic fields and so forth. It's not something that is at all straightforward.

LIPPINCOTT: So, is there anything that we haven't talked about that you think you might like to cover?

CHRISTY: Well, at the moment, I find that I've been fortunate enough to work on a great variety of problems. And I have enjoyed working in many different fields—a lot of physics of all different kinds. And since I was trained as a physicist, I've enjoyed very much being able to work in all these fields and make contributions. I learned some engineering during the war, because many of the problems of designing bombs and reactors were much more engineering problems than they were problems in theoretical physics. So I learned to be an engineer—not necessarily a good engineer. But I learned that you had to do practical things with your knowledge of physics. And I found it fascinating to be able to work in so many different things.