NOBEL LAUREATE

Willard F. Libby

Interviewed by Mary Terrall

Completed under the auspices
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Oral History Program
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CONTENTS

INTRODUCTION

Carbon-14 dating will inevitably remain the most memorable of Willard F. Libby's credits, because its uses are so dramatic and so easily understood. And although it was the basis for his 1960 Nobel Prize in chemistry, Libby himself did not consider it his best work. A man, a scientist of wide-ranging interests and activities, Libby had much to be proud of.

He was born in 1908 in Colorado but grew up in the rich farming region of Sebastopol, California. At his father's insistence, Libby enrolled in the University of California, which by the late 1920s had developed the leading chemistry department in the world. Before he received his B.S. in 1931, Libby had built the first Geiger counter in the country and had laid the groundwork for his doctorate, which he completed in 1933. He then stayed to teach at Berkeley for eight years.

This interview traces Libby's subsequent career: from his work on Uranium-235 enrichment in the Manhattan Project, the Carbon-14 research, service on the Atomic Energy Commission, to his years at UCLA. Its unique value, though, is more, perhaps, in what it has to say about the relationship of science to politics and government.

The last half century has seen a mushrooming of scientific advances, and a parallel rise in government's support of and reliance on science. Libby, quite a political man, was in his most productive years during this entire period. He has much to say about other scientists who involved themselves in politics, and was active himself in many ways. On the difficulties of administering scientific projects, nothing could be more vivid than Libby's version of his contacts with General Leslie R. Groves. Whenever the commanding general of the Manhattan Project came to inspect the progress being made on the all-important barrier for the gaseous diffusion plant, Libby burned up a chunk of concrete and set the general laughing. No native chief shown a cigarette lighter could have been put off more easily. Those interested in the question of to what extent important decisions should be trusted to experts will find much to think about in these interviews.

Willard Libby spent the last twenty years of his career at UCLA, as a senior chemistry professor who paid special attention to promising freshmen, as director of the Institute of Geophysics and Planetary Physics, and as prime mover for the graduate program in Environmental Science and Engineering. He died in Los Angeles on September 8, 1980.

INTERVIEW HISTORY

INTERVIEWER: Mary Terrall, assistant editor, Oral History Program, UCLA. B.A., Folklore and Mythology, Radcliffe College.

TIME AND SETTING OF INTERVIEW:

Place: The first, second, and fourth sessions took place in Libby's Santa Monica home. The third session was conducted in his UCLA office.

Dates: August 10 and 17, September 8 and 11, 1978.

Time of day, length of sessions, and total number of recording hours: The sessions were conducted in the mornings, each lasting about an hour and a half. A total of five and a half hours of conversation was recorded.

Persons present during interview: Libby and Terrall.

CONDUCT OF INTERVIEW:

The Oral History Program in the mid-1970s initiated a series of interviews with scientists, of which the interviews with Vern Knudsen, Mildred Mathias, and Willard Libby are the products. At the time of this interview, Libby was one of two Nobel laureates on campus.

Terrall came to the program as an assistant editor with an interest in science. Prior to working at UCLA, she had participated as an interviewer in a Massachusetts Institute of Technology oral history project concerning the recombinant DNA controversy. She began her work on the Libby oral history by reviewing the copious press clippings, by reading the Nobel laureate's acceptance speech, by consulting a bibliography of his work that had been compiled at UC Berkeley's Bancroft Library, and by studying a number of the scientific papers Libby had written.

While her plan was to deal primarily with Libby's scientific career in a chronological fashion, the interviewee frequently digressed into the field of political issues. He had been actively involved in the social concerns of the post-World War II period.

His opinions were vivid, strongly held, and frankly expressed. Terrall's view is that some of this material, as a result, is the most revealing in the oral history.

EDITING:

Editing was done by the interviewer. She checked the verbatim transcript against the original tape recordings, editing for spelling, punctuation, and paragraphing, and verifying all proper nouns. Words and phrases inserted by the editor have been bracketed.

Libby reviewed and approved the edited transcript.

Stephen Stern, senior editor, reviewed the edited transcript before it was typed in its final form. Front matter and index were prepared by Oral History Program staff.

The original tape recordings and edited transcript of the interview are in the University Archives and are available under the regulations governing the use of permanent, noncurrent records of the university. Records relating to the interview are located in the office of the Oral History Program.

At the time of his death in September 1980, Libby left 180 boxes of scientific papers, research notes, and memorabilia to the university. This material is available to scholars in the Department of Special Collections of the University Research Library. In addition, seven volumes of papers and speeches by Libby have been collected and published. They are available to readers in the Chemistry Library, 4238 Young Hall, UCLA. The series of volumes, entitled *Willard F. Libby Collected Papers*, includes *Radiocarbon & Tritium*, *Radiochemistry, Hot Atoms, & Physical Chemistry*, *Radioactivity & Particle Physics*, *Radioactive Fallout & Technology*, *Solar System Physics & Chemistry*, *Papers for the Public*, and *Talking to People*. The first volume was edited by Rainer Berger and Leona Marshall Libby. Mrs. Libby edited the remaining six volumes alone. The first volume was published privately by Geo Science Analytical of Santa Monica. The subsequent six volumes were published jointly by Geo Science Analytical and UCLA. All are dated 1981.

TAPE NUMBER: I, SIDE ONE

AUGUST 10, 1978

TERRALL: Okay, I know that you were born in Grand Valley, Colorado, in 1908. What was your father doing there?

LIBBY: My father [Ora Edward Libby] was a farmer; he raised sugar beets. And we were born there in that little log cabin in Grand Valley. It was a home birth. My mama [Eva Rivers Libby] was eighteen.

TERRALL: So you were the oldest?

LIBBY: I was the first child, yes. She had four others later, but I was the first. My daddy was a Maine lumberman, as he would go every year into the Maine lumber woods. He never had any schooling, my father, but he was a very well educated guy. So he came out to Colorado and married my mama when she was sixteen, and I was born when she was eighteen. He waited a year, he told me once. And then she had two other boys and a pair of twin girls.

TERRALL: Was your mother a native Coloradan?

LIBBY: Yes. Her parents were from Georgia, and my daddy was from Maine.

TERRALL: I see.

LIBBY: A very interesting marriage. It worked fine.

TERRALL: Was your father a good deal older than her, then?

LIBBY: Oh, quite a bit older, yes. Eleven years older than my mother.

TERRALL: I see. So why did he make the decision to move to California?

LIBBY: You know, I never knew that. He was doing quite well in Colorado, but they suddenly decided to move to California. And I remember the ride. I was five years old when we came on the train.

TERRALL: Had he decided where to move to? Had he already bought a farm?

LIBBY: Well, he came there to do what he could do. He had limited resources, so he actually became a manager for a corporation.

TERRALL: Oh, I see.

LIBBY: And he was a very good farmer. We didn't have any problems. We had a wonderful life living on the farm. We had our own cow and our own vegetable garden; made our own butter, made our own bread. You've never eaten so well. You can't buy that kind of food; it's impossible. So the children [stepdaughter and family] going off to Hawaii, I've encouraged them to see if they can't do the same thing--stay away from the grocery store.

Well, anyhow, we went to Sebastopol [California]. That's an interesting fact--Sebastopol is named Sebastopol because it's the southernmost [point] of the intrusion of the Russians. That's why it's named Sebastopol. Fort Ross was a Russian church, and that was just north of

Sebastopol. But they came down almost to San Francisco, and Sebastopol was the farthest [point]. That's where we grew up.

TERRALL: Was it a very small town in those days?

LIBBY: Still is. Yes.

TERRALL: A farming area.

LIBBY: Farming, yes, although now the real estate developers are taking it over. I was up there a couple of years ago and talked to my old friends. They had me speak to the chamber of commerce; it was the fiftieth anniversary of our [high school] class. It's a long period, fifty. And so we talked about it. And most of them are millionaires--oh, yes, very rich, just on that gorgeous real estate. My daddy never quite understood me, in the sense that he understood my two brothers and my sisters. I wanted to stay and work as his farmhand. I worked one year as his farmhand after I graduated high school.

TERRALL: Oh, you did?

LIBBY: He never understood that.

TERRALL: He thought you'd want to get out?

LIBBY: Yes. I wanted to stay. I said, "Look, if I stay I'll own this, and I'll own half the country in ten years." He said, "I know you will, but you go to Berkeley." That's how I went to the university.

TERRALL: I see. So as you were growing up, you worked on the farm in the summers and so on?

LIBBY: Yes, well, while I was in high school we did many things. I was a football player, got my letter for beef--I was large. [laughter] I didn't have any real ability, but I was big enough. But I was student body president, and had all the normal activities--valedictorian and so on.

TERRALL: Going back a little bit, can you remember when you first got interested in science?

LIBBY: Well, I was interested in everything, actually. I read everything that I could see. I still read quite rapidly, unless it's science--then I slow down. But I read many, many things, and the result was a general knowledge. When I went to high school, my English teacher told the class, "This is a Nobel laureate." I was quite embarrassed because I thought it was uncalled for. She didn't say what field. Turns out she was right.

TERRALL: But you didn't have a pet subject?

LIBBY: I was very interested in chemistry the whole time, but perhaps more interested in the stupid way people believe and behave--namely, sociology. It's incomprehensible how the human race can almost destroy itself every day, and yet still survive. It's so easy to improve our status by just doing a couple of things.

So I read cowboy books; they were always very simple.

Zane Grey books were my favorites. How these cowboys would behave, be rational. See, the western law of the six-gun and the noose, which was built up when there was no other law, was very quick and very certain. Now, when I got to Sebastopol, five years old, I had my first experience. They lynched three men in Santa Rosa, which is the town next door.

TERRALL: And you saw it?

LIBBY: No, but the next day I carried part of the rope to school. It was pretty rugged. These three men had raped some women, so the whole town came out and hanged them--contact with frontier justice. Of course, after that, things modernized quite quickly. I'm not sure that justice was any better but . . .

TERRALL: . . . things changed.

LIBBY: So then we went to school. We lived in a little place called Cunningham Station, a few miles south of Sebastopol. My father was farming, trying to make a living for us. We were three children then. And we had bad luck. The house burned down--I remember the night the house burned down. My dear mama--she was so brave and so beautiful. She got us out of the fire, and she was celebrated in the newspapers for doing this.

TERRALL: Was that your first house in Sebastopol?

LIBBY: It was Cunningham Station, which is south of

Sebastopol, about four miles south.

But then my daddy got this management job for this San Francisco corporation, and we had a substantial home and we could live there. That was part of his wages, that our home was furnished.

TERRALL: What kind of a corporation was this?

LIBBY: A banking company who owned land. That's very rich land up there.

TERRALL: So they owned land there, and he managed the land?

LIBBY: They paid him a salary and gave us a house.

TERRALL: So he didn't actually own a farm there?

LIBBY: I don't think he ever did. He did in Colorado. In fact, we still own property in Colorado.

TERRALL: That must be worth money now, too.

LIBBY: Yes, I think so. He told us all, he said, "Never sell it." But we still own forty acres of land right in the middle of the shale country, and oil companies have approached us, and we've signed over rights to them, and they've done some developing.

TERRALL: So your father was the manager of this rather large farming concern.

LIBBY: That's right.

TERRALL: And you worked on the farm at least that one year after high school.

LIBBY: Well, the essential point is my daddy was a physical person, and I was an intellectual person.

TERRALL: Did you get encouragement of that from him?

LIBBY: Sure. I used to read all the time, and he'd say, "Why don't you go after some girls?" And I said, "Well, I do that, too." He came after me once. He said, "You've been doing too much of that; I heard some of your activities. Go back to reading." [laughter]

TERRALL: What effect did World War I have on your family's life there?

LIBBY: Well, we had some neighbors who went off and got killed. That had quite an effect.

TERRALL: Did it affect the economy of the area at all?

LIBBY: No. The economy was just booming. You can't imagine. It really was something. When I was in high school, the whole place was just <u>very</u> wealthy. No poverty, none of these problems.

TERRALL: Even then.

LIBBY: That's right. That's in the twenties. Then, of course, came the thirties, which was quite another story. But in the twenties, when I was in high school--I graduated in 1926--you couldn't make a bad investment. It was all up.

TERRALL: So your father had chosen the right place to move to.

LIBBY: He was a very wise man. He wanted the family to grow up, like--well, my son-in-law Peter, with his four children, is going to the big island of Hawaii--that's a wise man. Get away from TV and let them breathe clean air and swim in good water. That's a very wise man.

TERRALL: Well, you said your father encouraged you to go to Berkeley. What was your reaction to that?

LIBBY: I went to Berkeley.

TERRALL: But you said you did think of staying there [in Sebastopol] and investing.

LIBBY: Well, I argued with him about it. I said, "Look, you let me be here, and in five years I'll own the ranch." He said, "I know you would. But you go to Berkeley."

TERRALL: Do you think he had some career area in mind for you, or he just thought that you would be better off to get an education?

LIBBY: No. See, my record in high school was such that I could have gone anyplace.

TERRALL: Did you consider going to another school?

LIBBY: No.

TERRALL: Why was that? Just because it was close?

LIBBY: I wasn't too fond of the idea of going to school. I really wanted to be a businessman and a farmer. But my daddy said, "Get your tail off to Berkeley"--it did the thing. So I went down to Berkeley, not knowing Berkeley was the finest university in the world. I'd never heard of it, frankly. [laughter] So I went down to Berkeley and I went into a boardinghouse. It happened to be a chemistry boardinghouse--most of the people there were chemists.

TERRALL: Oh, really?

LIBBY: So that's how I became a chemist.

TERRALL: Oh, I see. So when you went there, you didn't really know what you were going to go into?

LIBBY: I looked in the catalog and I took the course that had the broadest selection--namely, petroleum engineering. I think it's probably still true. If you want to pick out the course that has the broadest number of required courses, it's probably petroleum engineering. So I did that. And that meant I learned a lot of things which have subsequently been proven and useful; like I learned to make line drawings, and I learned to do field surveying. I can go out and run a line from point A to point B within a quarter of an inch, even though it's ten miles long. That I can do. It's very impressive, by the way. And then I had the fun of living in the summer camp with the engineers and learning to talk with them the way they talk. They're very nice people. So all my life I've been extremely interested in practical applications. Although my Nobel Prize is for a kind of theoretical matter, I've been very interested and very heavily involved in business and industrial activities. That's what I'm doing now in my retirement.

TERRALL: I see.

LIBBY: But we went to Berkeley and went into petroleum engineering, and I lived in Mrs. Kittridge's boardinghouse, 2606 Bancroft Way--it's long since been demolished. But we had a fine boardinghouse there, and most of the people were chemists. They were graduate students in chemistry, and here I was a freshman. So I gradually got the idea after a couple of years, and I transferred from petroleum engineering into chemistry. That's how I became a chemist.

TERRALL: So it was specifically the influence of those other students that led you to chemistry?

LIBBY: Yes, I would say so, plus the greatness of the chemistry at Berkeley. It was the leading chemistry school in the world, by far. I finally got that idea through my fat head and joined them.

TERRALL: I see. Were there particular professors that were influential?

LIBBY: Yes. Gilbert Newton Lewis, the head of the school, was the most influential, and [so was] my own professor, Wendell Latimer, who was a disciple of Lewis. I built the first Geiger counter in this country, and Wendell encouraged me to do that.

TERRALL: Was that as a graduate student?

LIBBY: I was a senior, actually, when I built it.

TERRALL: So you were working with Latimer already as an undergraduate?

LIBBY: That's right. Most universities have these honors research courses.

TERRALL: Were there other people around the country working with Geiger counters?

LIBBY: No. We built the first one. And we had an enormous lead as a result of that, because to measure radioactivity, you had to have a Geiger counter. We had an enormous lead, and when Ernest Lawrence came on with the cyclotron--the first accelerator was built at Berkeley--we were all equipped to measure it and work on it. We had a very hot activity. It came up more or less spontaneously. I thought of going into biology, but there's one quality I claim: I know a doable problem, and biology isn't doable. So I turned away from biology--it's just too hard. What you have to do is something you can do. So I went into atomic chemistry, and we built the whole field of atomic chemistry at Berkeley.

TERRALL: When you were graduating from college, was it sort of a natural thing to go on to graduate school?

LIBBY: Yes. In fact, it melded together. I was doing my PhD work when I was a senior. I got my PhD in two years, because I had really done the thesis already.

TERRALL: To go back to your father: was he pleased with the way things were turning out with your education?

LIBBY: He didn't understand a thing I was doing. He used to come down and see me, and when I got my PhD, they all came down and saw me graduate.

TERRALL: Did your brothers go to college?

LIBBY: Well, one brother went to Washington State [University] and became a veterinarian. The other brother didn't do anything; he went into real estate and business. They're both just fine. Quite rich. [laughter] I'm not exactly poor, but my brothers are quite wealthy. I think it depends on what your objectives really are. My objectives were--fundamentally, I had a curiosity to explore. In other words, I was a research person from the beginning; I didn't know it but I was. My literature teacher who said, "He'll get the Nobel Prize," didn't know what the field was. I had written her some kind of a piece; she read it to the class, and that's what she said. Her name was Bradway, a very fine teacher. We were very fortunate in our little school there in Sebastopol, called Analy [Union] High School, in having a number of very good teachers. We had only 500 in the whole high school--it was a union. [It had] a number of very good teachers, [and] a fine library, so we could read everything we wanted to read. Our school system has deteriorated terribly. That was a much better education than you can get now. At the same time, we had fun.

TERRALL: Well, to get back to the problem of radioactivity. How did it come about that you started to work on that?

LIBBY: I was just looking around to do what's new.

TERRALL: Was Latimer already working in that area?

LIBBY: No. He never worked in that area at all.

TERRALL: I see.

LIBBY: I went into him one day and said, "Wendell, I want to be your student, and I want to build a Geiger counter, and I want to discover new radioactivities in the natural elements." And he said, "You've got a deal." That's the way it happened. That's what I did do my thesis on. I discovered the natural radioactivity of a rare earth element called samarium, which is now one of the important daters for the oldest rocks in the solar system.

TERRALL: Oh, is that right?

LIBBY: Yes. Samarium. It's doubly interesting because it also is unique--it's an alpha emitter, and it's the lightest-weighted alpha emitter in the whole periodic table. That's the first thing I looked at, and that's my luck, you see. It was active. So I went on from that, and then the cyclotron started working, and it was, of course, making radioactivity all over the place. We were all very busy.

TERRALL: What was the feeling about the hazards of radioactivity at that time, when it was still a very new field?

LIBBY: We didn't give it a single thought.

TERRALL: You didn't. That's what I figured.

LIBBY: Never gave it a thought. We got some pretty bad burns, too.

TERRALL: When do you think people started really being concerned about that?

LIBBY: Oh, I don't know. It's all drummed up. We have much larger hazards. Cigarette smoking is a much worse hazard. But we have reduced the exposure, say, from chest X rays in hospitals quite substantially, but to what end?--you know, at considerable cost. We are in a state of society now where we're regulating in ignorance. This is costing us very dearly. We're not curing the smog. Do you think we're curing the smog? You live here, don't you? Tomorrow I have a conference with two corporation executives; I'm on their board. What can we do? They're a very good company. I'm going to talk to them about face masks. I'm sure they'll recoil in horror, but that's how bad it is. It's hard to assess how many people are dying early because of the smog. Very difficult.

But after the Berkeley experience, we went to Columbia University.

TERRALL: Let me just go back for a minute before you get to Columbia. After you got your PhD and continued on [at Berkeley] as an instructor, did your research continue along the same lines?

LIBBY: Oh, yes, sure. I don't know how much you want in the way of technical content. I'd be glad to give you whatever you want.

TERRALL: Well, I'd just like a brief description of the direction your work went in.

LIBBY: Well, we did several things. The first thing was to build the first Geiger counter. And that sounds very small, but it was tremendously important. It gave an instrument so you could work. See, Ernest Lawrence's cyclotron was a great thing, but he had no way of measuring the products that were at all sensitive. I did that. That worked just fine. Then we started using chemistry on the problems. Ernest was a physicist, didn't know any chemistry really, but he brought his brother, who was an MD, John Lawrence--now a regent at the university--and he came out. And John and I worked together on medical isotopes. We did the first medical isotope.

TERRALL: Oh, you did?

LIBBY: Yes. I wouldn't have said it otherwise.

TERRALL: I didn't realize that.

LIBBY: We did radioiodine, we did radiophosphorus, radio-sulphur.

TERRALL: And those isotopes were produced at the cyclotron?

LIBBY: Right. I used my Geiger counter and counted them.

They did the biology, and I did the counting. And a long series of papers came out of that. I think it probably is one of the most important in the whole history. As you know, I got the Nobel Prize for radiocarbon dating, but I think I've done things that are greater.

TERRALL: And you think the medical isotope work would be . . .

LIBBY: Well, it's pretty damned important. And it's growing all the time.

TERRALL: These were isotopes that were used as tracers?

LIBBY: We discovered the isotopes.

TERRALL: Right. They were used later as tracers?

LIBBY: Tracers, but also sometimes therapeutically. One of the best examples is thyroid; where you have a hyperactive thyroid, it sequesters iodine. So you put in a little squirt of radioiodine solution and put a Geiger counter in, and you see it count, and you know by that measurement whether it's hyperactive. If it is, then you give it a squirt a thousand times stronger, and it burns it out--standard operating procedure. Now, in the brain surgery area, where somebody has a stroke or whatever, you give a squirt of radio-labeled protein, and then they go round and probe and see where it is, and cut in and cut it out. Simple-minded, anybody could think about it, but somebody had to <u>do</u> it, and John Lawrence and I did it.

TERRALL: So that was in the thirties.

LIBBY: That was in the early thirties. And we did other things. The chemical uses of radioactivity are very large. In the study of chemistry, we're limited mainly to the thermal energies, but in radioactivity we have all kinds of energies, as a result of recoils and various nuclear reactions. The nuclear reactions are so much more energetic than the chemical reactions. There's a whole new field called hot atom chemistry, and we developed that. I'm just now working on my hundred and first PhD. I had about twenty of them in the field of hot atom chemistry, very important people, and they've done great things. It was very exciting during the thirties.

TERRALL: Did you have graduate students working with you in the thirties?

LIBBY: Oh, yes, of course. My first graduate student was--I think it was '37. I was just four years an assistant professor, you see. Just very young. I think it was '37; it may have been '36. Some of them have retired already. I see them from time to time. Some of them died, and even one of them is in jail. They're a mixed bag. Pretty good on the average, though.

TERRALL: Did you know [J. Robert] Oppenheimer during this period?

LIBBY: Yes. I knew him well. I was a student of his, actually.

TERRALL: As a graduate student?

LIBBY: No, as an undergraduate. He was a fine teacher. I used to take his courses because he lectured so well and so clearly. And I'd go over there and take his courses just because he was a good lecturer. I never agreed with his politics to any extent whatsoever, but he was a fine physicist and a very good teacher.

TERRALL: Well, at that time were you aware of his politics?

LIBBY: Yes.

TERRALL: That was generally known?

LIBBY: He was an active Communist.

TERRALL: Would you say there was a visible Communist presence on campus?

LIBBY: No, I don't think so. The majority of us wouldn't touch them with a ten-foot pole. I'd still go to his lectures, because he was a fine lecturer and a fine scientist. I think to the day of his death he knew exactly what I thought of him. We didn't have any secrets about it. It's a strange thing that so many people think that all that was so secret at Berkeley. The Communists were very active. They were trying to recruit like mad.

TERRALL: Well, that's what I was trying to get at.

LIBBY: Yes. Not very newsworthy, but well known. I recently read a biography--autobiography, I guess you'd call it--of Jessica Mitford. She was at Berkeley at that time and an active Communist, and she tells all this. I knew all these people that she talks about. But they didn't convert me.

TERRALL: Did they try?

LIBBY: Oh, they tried like hell. Some of my weaker-minded friends fell. No, I can't see the fundamental theory of communism; it just totally eludes me. People are <u>not</u> equal, not in <u>any</u> way at all. Anybody who believes that they're equal is doomed to death and failure and misfortune. What you have to do is fit people together to where they'll do what they can do to the best of their ability, and people are not equal. Did you ever read about Einstein's feeling about public education? He said, "Close them down. Close them down." He had such a miserable experience. He was a high school dropout. Probably the greatest physicist of our time. He had to take a job as a lousy patent clerk, and while he was there he wrote three major papers which got him a full professorship at the University of Berlin, which was the top university in the world at that time. He had no use for schools. Well, he was a very unusual person. He came from a poor Jewish family. He had this extraordinary ability, and he was a very lonesome guy. Apparently, he married a fairly ordinary woman who didn't understand him--very lonesome guy. He had no use for schools. Amazing, isn't that? We ought to think about educating the unusual as well as the disadvantaged. We spend a lot of money on disadvantaged people. We ought to spend a lot of money on unusually able people. Well, Gilbert [Lewis] was very fond of Einstein and knew him, so by secondhand, so to speak, I got to know Einstein and followed him.

TERRALL: Did he visit Berkeley?

LIBBY: He would come from time to time. I was just a lowly student, you know, and you don't walk up to a great man and shake his hand. Though after I discovered alpha radioactivity of samarium, I began to get a little attention from some of the big shots. They began to notice this tall guy from Sebastopol who had something.

And then things began to happen. The only way you can produce neutrons is to irradiate beryllium with alpha particles. Now I'll have to be a little technical here. The only way you can get alpha particles is from radon. The only way you can get radon is from radium. Now, this happened to be a standard cancer treatment, radon needles. They'd take the gas from the solution, and put it into little tiny needles, and stick it into people's cancers. (In fact, my mama had that treatment; it extended her life twenty years.) Well, I used to go over there to San Francisco on the ferryboat and pick up radon from the solution. I had little beryllium-filled bulbs in which I'd put the radon. Then I'd come back on the ferryboat with a lead basket weighing about eighty pounds (because of the radiation), and then we'd work furiously for a week with all kinds of experiments. Gilbert Lewis was very active, though he was in his late sixties. He'd be there at all times, day and night, working on the experiment. And it turns out now that his idea that the neutron had wave properties was correct. It's been fully confirmed now fifty years later--fully confirmed. But he finally got discouraged, and he said, "I've got to work on something else. I can't do this anymore; it's not paying off." He was the highest-salaried professor in the history of this country. His salary was $10,000 a year in 1912, and no income tax; worth about a third of a million a year now. And so he'd talk at us, and he'd say, "What do you find interesting?" That was the thing. That was his way of measuring us. He had <u>so</u> much power. He could command professorships just like that. I was so pleased when I got my doctorate that he appointed me at Berkeley, because it was a kind of iffy thing. Here's this guy saying Geiger counters are chemistry. It was the first time <u>that</u> had ever been said. Of course, ten years later, everybody agreed.

TAPE NUMBER: I, SIDE TWO

AUGUST 10, 1978

TERRALL: I know that in 1941 you got a Guggenheim Fellowship and went off to Princeton, is that right?

LIBBY: That's true. I was married in that year to Leonor Hickey, and we were married for twenty-five years following that. (My present marriage [to Leona Woods Marshall] is my second marriage.) She was a physical education teacher and never understood what the hell I was doing from the day one. We were good friends, but it was that simple--I was pretty lonesome. But I'd come, after seven years at Berkeley, to a period where I could take a year off, a leave, a sabbatical. So I had a Gug[genheim], but then Pearl Harbor happened right in the middle of it.

TERRALL: Right. But before Pearl Harbor, you wanted to spend a year at Princeton?

LIBBY: I did some work at Princeton.

TERRALL: How did you happen to choose Princeton? Was there someone there you wanted to work with?

LIBBY: Yes. Sir Hugh Stott Taylor, a very famous catalyst chemist. I'd always been interested in catalytic chemistry and went there to work with Dr. Taylor. A former student of mine at Berkeley was there, as a graduate student, and I helped him with his thesis. And we were having a very nice time living there in that luscious place--you know, Princeton is really lush. I forget the name of the little side street we had an apartment on, but you could walk five steps and you were in the middle of everything. But that's when Pearl Harbor happened.

TERRALL: So you only got to be there for a few months really, right?

LIBBY: Right. So the next day I went to New York.

TERRALL: How did that come about?

LIBBY: I did it.

TERRALL: You weren't asked to come?

LIBBY: No, I just volunteered.

TERRALL: Did you know what was going on at Columbia then?

LIBBY: I guessed. It wasn't all that secret. So I went to Harold Urey.

TERRALL: Did you know him before?

LIBBY: I never had met him before, though he also was a Berkeley PhD. But way ahead of my time. He's now in his eighties and I'm in my seventies, you see.

TERRALL: Right.

LIBBY: When you're that young, that's a long period of time. So I said, "Harold, here I am." He said, "Okay, I have just the job for you." So I went to work on the chemistry of the [gaseous] diffusion plant. If I do say so, I think it's the best thing I ever did. Still classified.

TERRALL: Oh, all that work is still classified?

LIBBY: I gave a little talk last November, kind of hinting at how I did it, but I think it's better than the carbon dating, that work. We took an impossible situation and turned it into reality. The most active gas in the world is uranium hexafluoride. The requirement in the diffusion plant was that this stuff run through the plant, and in 2,000 years not make a visible film of corrosion. We met that. I'm quite proud of that. I hope before I die that somebody will declassify it so we can say it. It turns on a phenomenon, which is probably too technical for your record, called electron tunneling. There's a property of matter called waves; and electrons, being material particles, have this wave property. Now, in the corrosion of, say, aluminum by air, the slow process is the electron coming out of the aluminum to join the oxygen on the surface. That turns out to be the slow process, and that's tunneling. And the way they do it is they just go through, because they're waves; they're not particles. That's how I solved the chemistry of the diffusion plant.

TERRALL: So you were working on specifically making the metal that was going to be used for the pump and so on? You were working on making that metal less susceptible to corrosion?

LIBBY: No, I was talking about the problem of the parts, the machinery. This plant [Oak Ridge, Tennessee] is like a hundred U.S. Navys lined up. It's unbelievably large. And yet the requirements for losses were so minimal. It took a real chemical breakthrough, and none of the guys knew what the hell the idea was. In fact, Urey used to be kind of discouraged at times. He even told the general once, "I don't think we're going to make it." But [Major] General [Leslie R.] Groves, he bet on us. He was right.

TERRALL: How much did you know about the whole Manhattan Project effort in the Columbia lab?

LIBBY: Of course, we all had a general idea what we were working on. But we were very secretive also. Thank God we were, because [Klaus] Fuchs never got to us in our diffusion plant chemistry. He tried very hard. After we knew he was a spy, we recognized his attempts.

TERRALL: But nobody knew that until much later, right?

LIBBY: Much later. See, later I became an important official--eventually a member of the AEC [Atomic Energy Commission] and so on--but in those terribly important years, the forties, not even [Enrico] Fermi knew what the hell I was doing. We were good friends, good personal friends, but he didn't know what I was doing. How can you tell a physicist that this quantum mechanical principle of electron tunneling is going to build a billion-dollar industry? You can't tell him that. So I kept quiet. I just said, "Yes, General, we have the answer." He believed me and not Urey. Very honest man. See, Urey never did get into the nitty-gritty of what we were doing. He believed me, but he didn't have self-confidence enough to believe it himself. See, what we did was use an entirely new approach. We said we'll identify tunneling. There's a characteristic of tunneling, namely, it'll have a low temperature coefficient. So we spent all our time on the temperature coefficient of the rate at which UF_6 reacted with these various materials. Then we picked the best one.

TERRALL: You wouldn't have known what other subgroups in the lab were working on? I mean, the atmosphere of secrecy was . . .

LIBBY: Well, our lab was pretty open. I had 140 people working for me.

TERRALL: Oh, you did? In your own group?

LIBBY: Right. On the barrier, and on the corrosion problem. See, we had two problems. One was the corrosion, and the other was the construction of the barrier. We worked on both of those, and we solved them both. And, unfortunately for this interview, they're both still classified.

TERRALL: I see. Well, I've read some of the history of the Manhattan Project, and I've read that there was a certain amount of tension in the lab between Urey and [John] Dunning.

LIBBY: Yes, well, forget it.

TERRALL: You don't think that was important?

LIBBY: The less said the better. I bridged the gap between them. They were both nice guys. I don't know why some people can't get along without--it's just strange. But you see, the thing that Dunning had was the original idea of the diffusion plant where you had a porous metal matrix, or a porous matrix, through which the gas diffused. It was pretty obvious, but it was Dunning's idea. And what I did was make it work. And I think it's probably better than the radiocarbon dating. But we can't say. It's a smashing application of quantum chemistry, absolutely smashing. It's still applicable in other ways, if we could declassify it and bring it out. It's been thirty years; it's not going to last all that much more.

TERRALL: What was the atmosphere like in the lab?

LIBBY: Gung ho.

TERRALL: Were you getting a lot of pressure from outside, time pressure?

LIBBY: Well, let me tell you how we'd treat the general. There was only one boss and that was General Groves. And every time he'd come around, we'd burn him up a slab of Transite. Do you know what Transite is? Well, I can take you out there; I have one out there on my barbecue. It's solid concrete. So we'd take a slab of Transite, and we'd light it up and burn it with fluorine. And the old general would just laugh like hell; he just loved that. Here we were burning concrete; he thought we were the greatest chemists in the world. You know what? He may have been right. He just might. We had 140 people, and they're now populating or have populated the best schools and laboratories in the country. After the war, I was offered a job by a major corporation. I said, "Fine, if you'll take twenty-five of my boys with me." [laughter]

TERRALL: But when the general came to visit, was it . . .

LIBBY: Oh, he'd just pop in. He had a belly like this, and he'd just come. He wasn't polite <u>at all</u>, but he <u>was</u> the general, and he had two or three colonels and majors following behind him. So we'd light up the Transite and burn it, and that'd make him very happy. I remember one time he had a big review: Were we going to build the diffusion plant or not? So I had a box sitting on the lecture table, which was about six feet long, and so by so [gesturing], and I said, "Now, General, in this lie the barrier tubes." And the door was open so he could peek if he wanted to, but that wouldn't do him any good. And I said, "We have the answer. You go ahead." He went ahead. One of the great gambles in American history.

TERRALL: So you knew that the construction [of the diffusion plant] was going ahead at Oak Ridge.

LIBBY: It wasn't going ahead until the general said.

TERRALL: No, but it was going ahead before they acutally had the barrier in hand.

LIBBY: Oak Ridge did other things, but the main thing Oak Ridge did was uranium separation. But there were other processes: for example, Ernest Lawrence's spec[trometer], the calutron, and Phil Abelson's thermal diffusion, and so on. Also they were making some bomb parts. The Y-12 area was making bomb parts. See, the first bomb was a uranium 235 bomb, and not a plutonium.

TERRALL: Yes, I know. But was it known at Columbia that all the construction was in fact going on at Oak Ridge?

LIBBY: Well, let's put it this way. I knew Enrico and Laura Fermi at Columbia. I had a kind of general idea what he was up to on the [atomic] pile. But he hadn't a clue what I was doing. I didn't tell him anything. The general had the policy--and the government had the policy--to fragment the information. If you're at some place, you can know everything, but you can't know about the other places. And they had them all over the country, you see. In consequence, it was pretty damn good secrecy. I remember the day the Hiroshima bomb was dropped: my wife didn't know. She hadn't heard a word of what I was doing.

TERRALL: She didn't know you were working on the bomb?

LIBBY: Not a bit. She knew it was important because of all the priorities we had. If I wanted to go someplace, we'd get it. But we had adopted two small children that year, the month before. We were awful busy taking care of those babies--they were twins. She was caught totally by surprise. The other time she was surprised was the day I got the Nobel Prize. She never understood what the hell I was doing. We got a phone call from Stockholm: "He says you've won the Nobel Prize." [laughter] Three-thirty in the morning.

TERRALL: They always call in the middle of the night, I guess.

LIBBY: Well, they have to, because of the time. But intellectual mismarriages are a very important point. People should marry if they have intellectual interests in common, not only sexual. Intellectual--very important. I have a gorgeous second marriage; we're writing papers together. It's her babies who are here, her grandchildren.

TERRALL: I see. Well, I know that Kellex Corporation got involved in the barrier design also.

LIBBY: They were very much involved. In fact, what we did at Columbia was the research, and M.W. Kellogg [Corporation] and Union Carbide actually built the plant.

TERRALL: So they did the production end of it.

LIBBY: But they would follow us like fleas, you know. Every time we got a new result, they were on top of it within the hour. That's how close it was. And after they built the plant--I remember one time it stopped working--they called me up and said, "What's wrong?" I said, "Turn down the cooler," and they said, "You're an absolute genius." It worked. [laughter] The plant had quit separating. What they'd done was to convert the gas into a liquid, and therefore it wasn't separating. So I said, "Turn down the cooler."

TERRALL: Oh, I see. Did you actually work on the pilot plant for the production of the barrier itself?

LIBBY: We never had a pilot plant. It's the most remarkable industrial achievement of all time. We went directly from the lab to the full plant. Never had a pilot plant.

TERRALL: Of course, the whole barrier development took a long time, but there was a debate over whether to go ahead with the so-called Norris-Adler barrier or whether to go ahead with this newer one.

LIBBY: Now you're getting into a sensitive area. I happen to hold the patent on the barrier that was used, for which I got one dollar.

TERRALL: Oh, really? That was an improvement on the Norris-Adler?

LIBBY: No comment. Sorry.

TERRALL: Oh, really? Okay.

LIBBY: It's interesting you would ask. It's a very sensitive question.

TERRALL: Sensitive in a legal way?

LIBBY: No, secrecy. If I answered that question, they'll know how to build it. See, there's a lot written on the Norris-Adler barrier.

TERRALL: But that wasn't the one that was finally used.

LIBBY: I didn't say that. Right? You are flirting with a very sensitive area.

TERRALL: So after the barrier went into production . . .

LIBBY: Well, we had other problems. It's such a gigantic plant. Have you ever been to Oak Ridge?

TERRALL: No, I've never been there.

LIBBY: Well, take half of Los Angeles and make it into a plant; that's Oak Ridge. You can't believe the gigantic size of this thing. As I say, ten U.S. navies lined up is Oak Ridge. We used 10 percent of all the electric power in this country when that plant started. Ten percent.

TERRALL: I know they had their own power plant which was huge.

LIBBY: Well, 10 percent. One out of every ten kilowatts went into that plant. Just unbelievable. Now that very feature has now phased it out, so we're going to the centrifuge, which uses less power because of the rising cost of electricity. But it was a beauty. It was an absolute beauty. It gave us the command of the whole damn world. It did indeed. All due to electron tunneling.

TERRALL: Now, did you go back and forth to Oak Ridge after the plant was working?

LIBBY: Oh, sure. Certainly; very regularly. We retained our labs open until the plant was operating. Then we closed them down. Smack down, fired everybody. All the rest of them became national labs and that kind of crap.

TERRALL: Oh, that's right. Was that before the bomb was finished?

LIBBY: In October '45 we closed down the Columbia lab. Smack down. Urey went to Chicago, and I followed him to Chicago. And everybody else went off to another job, and just closed it down. That's the way it should be. But these other labs kept going, trying to make like they were doing important work--well, some of it is fairly important work. Our sad situation today is that we're not doing science anymore. It's very sad. And the Europeans are getting ahead of us. I'm very strong for engineering and industrial applications, but you have to have some science to feed them. I sent a three-page mailgram yesterday to Senator [S.I.] Hayakawa on the Senate's consideration of the NASA bill, because this idiot [Senator William] Proxmire had said we [should] cut out all science. Purify it. "We've been to the moon. What more is there to do?" says Proxmire. So we're fighting him very hard. We're going to win. So I'm spending more and more time in politics. It's very important that scientists do this, because the course of our society and our future is determined by science. Hell, the Russians will be sitting in the White House if you don't. They're damn close to it now. Do you realize we had the lead in '45, '46, '47--we could have called the Kremlin and said, "We're moving in." They couldn't have done a thing about it. That's the way it might be, if we don't watch our p's and q's. There are two areas where we are totally negligent. One is bacteriological and the other is radiological, but particularly bacteriological. We've chosen unilaterally to not do anything, but that doesn't mean they aren't. Can you imagine what would happen to this city if a good chemist went down to the water supply?

TERRALL: Well, you mentioned being involved in politics. Were you involved in politics back, say, before the war and during the war?

LIBBY: I was very busy doing my science; really, I didn't have much time. It was after I matured and got farther along that I got into politics. Somebody asked me the other day, "How were you ever appointed AEC commissioner?" I said, "Well, I was a damn good politician; that's how I was appointed." So later on, in the late forties and early fifties, I was working in politics. We had tremendous decisions. For some reason, Oppenheimer had decided against the hydrogen bomb, and I fought him, tooth and nail. And I won. That's why I was appointed to AEC.

TERRALL: I think I'd like to take a break here, because I'd like to talk about the later work and the AEC business in another session, if that's okay with you.

LIBBY: Okay.

TAPE NUMBER: II, SIDE ONE

AUGUST 17, 1978

TERRALL: I'd just like to go back a bit to a few questions about the Columbia gaseous diffusion lab. You said when you volunteered to go to work there that you had a good idea of what was going on there, but I wanted to get at whether you knew that they were working on gaseous diffusion.

LIBBY: No, I did not know. I went to Dr. Urey, who was at that time the most prominent scientist at Columbia, even eclipsing Enrico Fermi, who was still there, and simply volunteered--this was the day after Pearl Harbor. I had written a paper at Berkeley a couple of years before on a new idea I had for separating isotopes. I thought maybe he might be interested in that, and he was. In fact, I was impressed that he had already read it and knew about it. And then he told me that he wanted me to join him and do the chemistry for the diffusion plant.

TERRALL: So you didn't really know what you were getting yourself into when you went there. Now, how long had the diffusion work been going on there?

LIBBY: Well, it's an old idea. I think the English were the first ones to say that you could do it, and the people in the physics department at Columbia. John Dunning, who was dean, and several others had said they could envisage how if you made a porous solid, like a brick, and then passed a gas through it, it would separate. My job was to make the brick such that it wouldn't react with uranium hexafluoride. Uranium hexafluoride is probably the most reactive gas known, so I had quite a job. I'm very proud of it. I think it's better than my carbon 14 dating, frankly. Never been published; it's still classified. But I'm very proud of it. We worked on that for four years.

TERRALL: When you first went to Columbia, how many people were already working there in that lab?

LIBBY: I'd say a couple of hundred.

TERRALL: Already, at the beginning?

LIBBY: Yes.

TERRALL: Then it expanded.

LIBBY: They were trying to make the barrier. See, my job was the chemistry. But every time they'd make a barrier, the UF_6 would eat it up.

TERRALL: So you were involved in the testing?

LIBBY: I was involved in inventing the process. The process wasn't invented until you could make a material which could stand the UF_6. What the physicists and the engineers would say: "If you have such a material, <u>then</u> that's fine." But I found the material and proved that it would work, and that's the contribution.

TERRALL: So you were working on two things. You were working on developing the barrier itself and you were working on the problem of corrosion.

LIBBY: The one I'm really proud of is the corrosion problem, because I had an understanding of the corrosion theory and corrosion chemistry, which was thirty years ahead of time. It's now being recognized. It was November last when I first said a public statement about it at a meeting in Philadelphia, at the University of Pennsylvania, on biophysics, of all things, because it turns out that the basic principle which makes it possible to obtain a material which can stand uranium hexafluoride is the same principle which is involved in many biophysical processes. And I was the dinner speaker. They'd known for years I'd been after this principle, and so what I told them at the dinner was that it was the way I managed to do the chemistry of [gaseous] diffusion. I say I [although] we had 140 people that made contributions, but it was my idea. I'm very proud of it. It brings up a very interesting point about the way society operates. That plant wouldn't have been built without that idea. Very interesting.

TERRALL: Well, you said last time that when you went to Columbia you had never met Urey before.

LIBBY: That's correct.

TERRALL: I wanted to ask you what kind of leader was he in the situation he was in. How did you get along with him?

LIBBY: Just fine. The problem was he didn't know what my principle was and what I was betting on. See, I kept telling him I could do it. Here was a billion-dollar commitment, and he was on the S-1 [Executive] Committee.* Harold was a very fine physicist, but I had problems in chemistry with him. This was a chemical matter. I don't think you want any technical discussion on your tape, but that is the essence of it.

TERRALL: I just wanted a little bit of your recollection about Urey.

LIBBY: Well, I knew about Harold because he was a very prominent person, a chemical physicist. He got the Nobel Prize for discovering deuterium. It was a tremendous discovery. And he'd followed my career quite a bit, too.

TERRALL: So he knew of you.

LIBBY: Yes. And after the war he took me to Chicago with him.

TERRALL: At Columbia, I'm curious about how closely he followed your work.

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*Manhattan Project advisory committee.

LIBBY: Not very closely. He was an extremely busy guy. You see, the chemistry division was one of several divisions in the laboratory, of which he was overall director. And he was on the S-1 Committee to advise General Groves on how to operate. He had a lot of responsibilities. The one thing he did, which I always admired him for doing, he picked his people and he let them do the job. He never looked over my shoulder once. Well, one time I was getting pretty--I was a young fellow, and I was getting pretty busy, and so he appointed an older man to be head of the division and I'd be deputy, which meant I'd be doing the research and he'd be running the paperwork.

TERRALL: Who was that?

LIBBY: His name was Paul Emmett, a very famous chemist. He stayed about two years. After things calmed down a little bit, I became head again because the work load was lower. But that was wise of Urey because I was overworked. I had six secretaries, and that kind of nonsense, and yet I was spending almost eighteen hours a day on the job. So he had the wisdom to bring in Paul Emmett; but Paul, as soon as he got into it, saw we were doing just fine. After the administrative load had backed off, he went off to do other things. Then we went on, and in those three years, '42, '43, '44--almost four years--'45, we did it. And it is one of the most remarkable chemical achievements ever made.

TERRALL: Well, were there scientists working independently of the Columbia lab on the barrier problem?

LIBBY: No. We were all integrated.

TERRALL: I thought there were people working at M.W. Kellogg.

LIBBY: There were various groups. Like Bell Telephone had a group.

TERRALL: And Kellogg.

LIBBY: Well, Kellogg worked with us. They were the engineers. See, we were the scientists and we worked very closely with the engineers.

TERRALL: So there weren't other people working on other ideas about the barrier?

LIBBY: Oh, yes, there were many of them. But they all had me in common. I was the only one saying about the material. Our group was the only one dealing with the matter; "Can the UF_6 eat it up?" And most things it would eat just fine. If you let UF_6 gas out into the air, it won't flow a millimeter until it has reacted with the air. It is <u>some</u> gas. And the thing we did was to bring in the principle of quantum mechanics, which solved that problem.

TERRALL: That was the tunneling, right?

LIBBY: Tunneling. We looked for a material that could be controlled in its rate of reaction by electron tunneling and we found it. That was it. And we had a good test, but in those years tunneling wasn't all that well known. Urey had written a big, fat book on chemical physics and didn't even mention the word.

TERRALL: So it was a very new thing then.

LIBBY: In chemistry it was brand new. Only one person, Nevil Mott from England, had ever mentioned it. There may be two or three others, but the point is I knew about it. And I put them together and solved the problem.

TERRALL: So the other people who were working on other barrier designs were not concerned. . . ?

LIBBY: The design doesn't matter all that much.

TERRALL: They weren't concerning themselves with the corrosion problem then?

LIBBY: That's right. The physical nature of the barrier is important, and they did very good work. How you make this stuff--the cheapest way to make it, and all that--is important, of course. But the crux of the diffusion plant was the corrosion problem, the chemistry. There are many aspects of the chemistry, but the main one is the reaction with the material. See, if you react, you not only destroy your process gas UF_6, but you plug the holes and you're out of business very quickly. So we had to prove that

that rate would be less than one wavelength of light in 2,000 years. That was our maximum. Now, you can't measure that, so you have to do it theoretically, and that's the way we did it. What we did was put our efforts into proving that our theory applied. You do that by measuring the temperature coefficient of the reaction. Tunneling has a very low temperature coefficient. We knew we had it. I remember one time General Groves visited and I had a big six-foot-long coffin-shaped box, and I said, "General, the barriers are in there." He didn't ask for me to open it. Well, we had a candidate barrier in there all right, but it turned out later that that was not the proper one. See, I can't tell you what the material is; it's still classified. But I had twenty-five leading chemists in my gang. They couldn't have stopped us, really. We were on top of it. But the thing about Urey is that he trusted us. He kept complaining every time I saw him at a party: "When are you going to have that barrier?" because he recognized that the problem was corrosion. There are many different physical designs of the barrier which would work. I don't know quite how to explain to you the viciousness [of] uranium hexafluoride. Maybe I told in the last interview, but I'll repeat, the old general would come around with his potbelly and be standing there looking stern as a bear, and so I'd take a piece of Transite--do you know what Transite is? It's concrete--we'd have a fluorine torch, and we'd light it on fire. He'd give the most godawful laugh, the general. That got him happy. [laughter] But he also had a point: any group of kids that can do that must know what the heck the score is. We did know.

TERRALL: Now, how much contact did you have with General Groves?

LIBBY: He'd come around every couple of months to stick his nose in. We'd always burn a piece of Transite for him, and that would make him happy.

TERRALL: But did you talk to him about. . . ?

LIBBY: No.

TERRALL: You didn't talk to him yourself?

LIBBY: Oh, I talked to him. I was head of the group. But. . .

TERRALL: . . . but about the problems you were having.

LIBBY: No, it's amazing; we didn't have all that many problems. I've never seen a piece of research move so perfectly. We knew we had to find a material whose growth rate was controlled by electron tunneling. That I knew. That's the way I guided the whole thing. So we tested three or four likely candidates and the fourth fit. And that's the material.

TERRALL: You had the theoretical idea very early on then?

LIBBY: Oh, yes.

TERRALL: When did the breakthrough finally come, of finding the right one?

LIBBY: Well, I went to work there essentially in January '42, and we had it within eighteen months.

TERRALL: And then what was the rest of the time?

LIBBY: Have you ever seen the diffusion plant?

TERRALL: Well, the diffusion plant was in Oak Ridge.

LIBBY: Yes, ma'am, but it was our plant.

TERRALL: But you were still working at Columbia.

LIBBY: I was working at both places. We were the guys who designed it, and the engineers at Kellogg worked very closely with us. We never built a pilot plant. We went right from our lab tests at Columbia to this thing which is as large as--oh, I don't know how to describe it to you if you've never seen it--you can't imagine. It's as large as the automobile business in Detroit. That's how big it is. In the early fifties, it was consuming 10 percent of all the electric power generated in the nation. It is the most fantastic development--going from the lab bench to something of that size--and, by God, it worked. And it's worked ever since.

Now, the details of the barrier I can't discuss.

It turns out my group did contribute substantially to the final configuration. We not only did the chemistry, but we contributed very substantially--in fact, hold the patent on the final barrier. But that's more or less accident. We had very good people in our group. Tony Turkevich and one other fellow and I have our names on the basic patent. But there could have been many other configurations. I always think about the thing as being mainly a materials problem, and I'm very damn proud about it. It's the best example I know of pure theory winning. That could never have been built in any other way. See, the normal approach to corrosion is you take a hunk of material and stick it in and weigh it before and after. No way. We had to prove it was tunneling. Then we had it. That's the way it happened. I'm very proud of that.

TERRALL: The barriers were actually manufactured in Illinois--were they not?--and then taken down to Oak Ridge.

LIBBY: The first ones were made--there were several places. Bell Telephone made some. We had Chrysler in Detroit working for us; Charlie Heinen, who is now vice-president of Chrysler, was the head of their group. They built the converters. Those are the large containers into which the bundles of barriers are put. And the plumbing had to be manufactured. Then you had to have a construction company build the whole thing. If you took all the U.S. Navy ships and put them together, they'd be about the size of that first Oak Ridge plant. All of them, put them all together side by side--that's the magnitude of the thing. You ought to go see it sometime. And here it is over thirty years; it's still working just fine. They built two other copies of it; they're working just fine. It's being phased out now, though, in that it's a power hog. It uses a great deal of power to pump the gas around. So the gas centrifuge is coming in, with my blessing. But they have the same materials problem. Not quite so tight, but they have to use the same materials we used. And, of course, it's the same answer.

TERRALL: How much did you know about what was going on at Los Alamos [Scientific Laboratory], the other end of the business?

LIBBY: You know what? I didn't know very much. We didn't communicate. That was the general's way of operating, to keep the various groups separated. They didn't know how we worked either.

TERRALL: So you didn't know what their time scale was?

LIBBY: We had a pretty fair idea, because we were under pressure to produce enriched uranium. See, there were two or three other ways of enriching uranium they were running at the same time. I'm so annoyed with the ineptitude of our present government. They can't do anything. It takes them ten years to do anything. They'd better change their attitude. But the way the Manhattan District operated, they had three ways. They all worked. Ours proved to be the cheapest.

TERRALL: And they were doing them all simultaneously.

LIBBY: All simultaneously.

TERRALL: When they exploded the first bomb--the bomb test--did that come as news to you?

LIBBY: It was news.

TERRALL: Was it earlier than you expected it? People must have been waiting for something out of Los Alamos.

LIBBY: See, Los Alamos didn't know what we were doing either, or how we were doing it. Fuchs, with his treachery, never got our barrier. How the Russians ever got it I'm not quite so certain, and I have evidence they don't have the best barrier. But it's now a commercial matter.

TERRALL: How about when the bomb was dropped on Hiroshima, was that a surprise to you? You mentioned that your wife, when she heard the news, had no idea that that's what you had been working on. But what were your reactions?

LIBBY: She was working on the bomb herself.

TERRALL: I mean your first wife.

LIBBY: Oh, that one. No, she had no idea.

TERRALL: What were your reactions to hearing that the bomb had been dropped on Hiroshima?

LIBBY: Well, I had two brothers who were in the army. And one of them was in the Pacific and the other was about to go; he was with Patton's army in Germany. Well, you put yourself in <u>that</u> position. The kamikazes were killing our fleet. It was a bloody war. I can see how Truman might have done it somewhat differently, but being the guy he was and the way Groves was, it was obvious that's the way they were going to do it.

TERRALL: Were you a Truman supporter at that time?

LIBBY: I've never been a Democrat, but I was very fond of President Truman in many ways. He was a very wise man. The reason I got into the government was that I supported Truman on his hydrogen bomb decision. One of the few scientists who did. In a quiet way, I went to Washington and talked to people. I just received a letter from Senator Hayakawa--that's the way I work--on the space program. Someone asked me the other day, "How'd you ever get appointed AEC commissioner?" That was a very powerful job in those days. And I told him, "I got the hydrogen bomb project moving." Well, in other words, I think scientific politics is not bad if it's done effectively. But you don't go up and make speeches. We had a dinner party last night and some very important people were here. We work that way, always have. Talk to them off the record.

TERRALL: Now when the bombs were dropped in August of '45, was the Columbia lab still running, or had it been dismantled?

LIBBY: It was closed down in October '45. It was essentially closed down. He had the plant running and working fine, except that one night, I may have told you, we got a frantic phone call. My then wife and I had just adopted twins. They were three weeks old. And the call was, "Come to Oak Ridge instantly." The plant wasn't separating. It had quit.

There's some parallel between this story and the xenon story. My wife [Leona Woods Libby] has a book about to be published in which she tells the history of the Fermi experience. She was his top assistant, and their reactor at Hanford [Washington] started working, ran fine for a few hours, and then stopped. They got the answer. And we got the answer. The engineers were mighty impressed that we could do that.

What had happened is fairly interesting in itself. Uranium hexafluoride is a gas all right, but it's not all that volatile. It had a little bit too much cooling so it was condensing on the surfaces and flowing through as a liquid instead of a gas. That removed the separation. So the answer was very simple: Cut down the cooling water, heat it up. It worked perfectly.

We had some other problems. We had the problem of educating these people. These people they put in to operate this plant had no more idea of how it was working than the man in the moon. We had to teach them. We had to produce teaching manuals and devices.

TERRALL: Oh, you were involved in that end of it also?

LIBBY: I was the chief chemist. The problem <u>is</u> a chemical problem. There are other problems. The critical problem is to keep the chemistry in check. And on such a vast scale, it really was a miracle. I give old Groves a lot of credit for keeping his hands off. And I give Urey a lot of credit for trusting us, and the engineers who designed the plant, the pumps, and the mechanical, moving parts. It's really something. Now a lot of that has gone into the new gas centrifuge plant. Now we have some modern developments that promise to be even cheaper, using things like lasers. It turns out it's not so easy to make a high-powered laser cheaply. You can make them all right, but they're very expensive.

TERRALL: Well, after the war, was there discussion about declassifying some of this information?

LIBBY: It's interesting you would ask, because the only connection I had with the Manhattan District after the war was a committee called the Senior Reviewers, which meant we got together and decided--recommended, we didn't decide anything--recommended what should be declassified and what shouldn't. And I was on that for years.

TERRALL: Who did those recommendations go to?

LIBBY: Groves. Groves appointed us, but then when the AEC came into existence, they kept us going. And we were the guys--in <u>all</u> fields, not just the diffusion, but bombs and everything. So this broadened my knowledge of the whole problem, and you know who was my partner on that committee? One Klaus Fuchs.

TERRALL: Oh, he was on that committee?

LIBBY: How do you like that?

TERRALL: I didn't know that.

LIBBY: No, I know you didn't know that. A lot of people don't know that. At one of our meetings there were three Communist spies in the room. Fuchs, [Guy] Burgess, and [Donald] Maclean were all in the room. So <u>that's</u> how the Russians got going. They stole it. They stole it. They still barely have enough technology to do it. I notice in the Russian literature, though, there are more and more papers about electron tunneling written by chemists.

TERRALL: So that committee had to look over everything and decide what had to remain classified?

LIBBY: Well, if you had some paper or project you wanted declassified, you had to come to this committee.

TERRALL: Was there very much of the bomb-related stuff that was declassified at that time?

LIBBY: Well, of course, the biggest declassification act of all time was the Smyth Report.* Groves was a very interesting man. He cleared that for declassification.

TERRALL: Could you describe the Smyth Report just briefly?

LIBBY: You've never seen it? It's a whole volume describing the atomic bomb project.

TERRALL: It describes the project but doesn't go into technical details?

LIBBY: You bet it does.

TERRALL: Was there a lot of controversy over whether that should be declassified or not?

LIBBY: Well, shall we say there were two opinions. President Eisenhower thought it was a mistake. I myself think it probably wasn't a mistake. I would have cut out a lot of the material that was in there. Henry Smyth, who wrote the report, was a physicist from Princeton. He never knew anything about our work, so there's practically none of our work in there. He just said, "Urey's people separated the isotope"; that's all he said.

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*Henry D. Smyth, Atomic Energy for Military Purposes (Princeton: Princeton University Press, 1945).

But he went into too much detail on plutonium and [how] the bomb works. Really all you have to know in order to be certain about your project is that the A-bomb works. If you know it works, then you keep working on it until it works. We didn't know it worked. That's the difference. The Germans never made it. It's very interesting to read the history of the German project and to try and figure out what stumped them. The Germans now say, of course, they were against Hitler, but there's more than a little question about <u>that</u>. There are certain critical decisions you have to make, and they never separated uranium isotopes. That was a bad mistake, because the Hiroshima bomb was uranium, not plutonium. They went the "plut" route, but it took them years to realize that plut was a good bomb material. They made reactors, kind of toy reactors, but they went the heavy water route instead of the graphite. We were scared though. See, those V-2 weapons which Hitler put on London--he would have won the war if he had had a few A-bombs. Really scary. That kept us working very hard. The Japs never really tried anything. They had had some people over in Lawrence's lab in Berkeley, and I had one or two in my lab at Berkeley, but it wasn't a major effort. Now, at this point in time, we have a very similar situation. Our science has been going downhill, and most of our top talent is working in irrelevant problems. We've totally stopped [working on] two of the biggest threats: bacteriological and chemical warfare. We don't even bother to do anything in that area. So I don't know. The Russians may not use hydrogen bombs; they may not have to. We spend untold billions on delivering hydrogen bombs and A-bombs, and not a dime on bacteriological warfare. We've got to come to our senses. We're rapidly losing our wealth; that may bring us around. We were in England a couple of months ago--my gosh, the dollar isn't worth very much. Terrible. It's getting worse all the time. That may lead us to bring our troops home from Europe. All right? We've got to get something straightened around; we've got to start doing things right for a change. I've always been involved in politics in a quiet way. I'm right now working for Evelle Younger; get the priest [Governor "Jerry" Brown] back to his seminary and, I'm afraid, the peanut farmer [President Jimmy Carter] back in Georgia. We're working very hard in this way. Our argument is very simple: The nation is in great peril. They may not have to fire a shot; they'll buy us out. You know the Miramar Hotel is owned by a Japanese? Half the hotels in Honolulu are owned by Japanese.

Well, back to a more methodical approach. Do you have enough on radiocarbon? Or did I give you anything?

TERRALL: No, we didn't talk about that at all. In fact, I'd like to go to your appointment at the University of Chicago, and maybe you could tell me how that came about.

LIBBY: Well, Harold Urey did it. Urey went to the University of Chicago from Columbia after the war. He took me with him, frankly.

TERRALL: So Chicago offered him a job and he decided to leave Columbia?

LIBBY: They offered a number of people. For example, Fermi was there, too. They built a group. [Robert M.] Hutchins, the president, was a very great man. Though he was a Democrat, he was very great. And though he was a lawyer and not a scientist, he understood the value of science. So he brought both Fermi and Urey and dozens of others, including me, to establish a center at Chicago.

TERRALL: How much of a chemistry department was there when you got there?

LIBBY: Well, the chemistry department was very distinguished before all this happened. See, most of atomic science is really physics, though I'm a chemist and have been working on the chemical aspects of atomic. It's the kind of thing that Fermi did, and Urey to a great extent, which is physics and engineering.

Now, we went to Chicago instead of returning to Berkeley. See, I was on leave from Berkeley all this while. It was quite a decision, because I liked Berkeley very much. But they gave me a great opportunity at Chicago, and I had the idea of doing radiocarbon dating and I wanted to get at it. I thought my means and opportunities would be better because I would need to work alone. This is a problem where you won't tell anybody what you're doing. It's too crazy. You can't tell anybody cosmic rays can write down human history. You can't tell them that. No way. So we kept it secret. We did it step, by step, by step.

TERRALL: How was Chicago different from Berkeley as a working environment?

LIBBY: Private school. Berkeley is the University of California, and it's like working for the federal government, almost.

TERRALL: Did you like living in Chicago?

LIBBY: Yes, very much. I had very good students. And carbon dating was just one of the things I was doing. I did that, personally, with one graduate student and one postdoc.

TERRALL: Can you tell me about the origin of the idea for radiocarbon dating? When had the idea come up, and did you start working on it right away?

LIBBY: Well, it was very early because--tell me, Mrs. Terrall, what is your background? Can I talk science?

TERRALL: Yes.

LIBBY: Well, while I was at Berkeley we were trying to find useful isotopes for biology, and biochemistry, and industry. And the only carbon isotope we had, had a half-life of twenty-one minutes--carbon 11. Well, that leaves something to be desired. But there was a big hole in the isotope table in mass 14. So we made a "largish" effort at Berkeley to find carbon 14 and to measure its property in half-life. We had to kind of estimate its half-life in order to look for it, because otherwise we wouldn't know how much to make. And we guessed incorrectly; we guessed ninety days. It turns out to be 5,700 years. So we failed. We didn't make anywhere near enough to find it. But my then student, who was working on this for his doctoral thesis, went on, together with Martin Kamen, and they made a tremendous pile, and then they found it.

TERRALL: Who was your student?

LIBBY: Samuel Rubin. He and Martin discovered carbon 14.

TERRALL: When was that?

LIBBY: Forty-two. I was at Columbia working on other problems, and they found it. And then Sam was killed in war, and Martin became a biochemist, and I kept my eye on the ball. So when I got to Chicago in '45, I went to work on radiocarbon dating, because we knew the whole time that the cosmic rays were making carbon 14; what we didn't know was the half-life. And these experiences of how Sam and I had failed on the ninety day [half-life] and they had succeeded on the brute force meant the half-life was much longer than three months. See, carbon dating wouldn't have been at all useful with a three-month half-life.

TERRALL: But you had already the idea for looking at the cosmic ray phenomenon?

LIBBY: Oh, yes.

TERRALL: Even when you were at Berkeley?

LIBBY: No. There was a discovery made just at the time I left Berkeley which disclosed. . .

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TERRALL: You were talking about the discovery that had been made relative to cosmic rays.

LIBBY: Well, what we had known for years is that neutrons, when released in air, make carbon 14. That was discovered in the mid-thirties. We were one of the discoverers of that reaction. But it was largely due to [Martin] Kamen and his professor at Chicago [W.D. Haskens]--Kamen came from Chicago. It's interesting the way these schools work together. In about ten days, we are going to La Jolla and celebrate Martin's retirement party. I'm going to tell this story. A lot of it isn't known, except to Martin. But the essential observation was that a professor at NYU, Serge Korff, had sent a neutron counter up in a balloon and had seen the count rate rise as the balloon went up. So he proved that the cosmic rays were making neutrons. That's all you need. That's carbon dating.

TERRALL: Now, that was somewhere at the end of the thirties?

LIBBY: '39. Serge is a very good friend of mine, and I've often talked with him about why he didn't go ahead and do carbon dating. Well, he has a good answer: it's a hard job. At Chicago, the first thing we had to do was measure the half-life, because the Martin Kamen-Sam Rubin work was brute force; they estimated the half-life to be twenty-five thousand years. Well, when you have one estimate of three months and another one of twenty-five thousand years, the first job is to measure it. So that was the first thing. Fermi used to ask me, "Why are you spending your time measuring the half-life of carbon 14?" I said, "Well, it needs to be done." He said, "But you could work on these others." I said, "Yes, I know." He didn't know what I was after, and I didn't tell him. I didn't tell Urey either. And the reason I didn't was that I've learned one thing: if you have a really original idea, they won't support it. It has to be mediocre in order to get support, and I've had a hell of a lot to support. But if it's really original--like, suppose I have an idea to cure cancer--you couldn't possibly get support for it. There would be fifty-nine reviews against it. The peer-review system is built to kill new science. And it's killing this country. We're going right like that. [gestures] We fill the library with pretty mediocre stuff. But anyhow, we went on.

TERRALL: Now, who was working with you at this point? You said you had a graduate student.

LIBBY: I had a graduate student measuring the half-life, and a postdoc. As far as they knew, that was the point.

TERRALL: What were their names?

LIBBY: Well, one was named William D. Hamill from Notre Dame, he was the postdoc. And I had an assistant the Argonne [National Laboratory] hired for me; Engelkemeier was her name. Let's see, what was her first name? I forgot her first name, but she was a good lab assistant. It's a humdrum job, and fortunately several other people were working on the same thing, and fortunately we came out with the same answers, within reason. So we then knew the half-life was between 5,000 and 6,000 years, which was perfect, ideal. So then I published a little article pointing out that Korff's discovery meant there must be C^{14} in nature. So I had a graduate student [Ernest Anderson] whose job was to measure C^{14} in nature. He did it. Working together, we managed to discover natural radiocarbon.

TERRALL: So you measured C^{14} in different samples of living things?

LIBBY: Right. Now you appreciate Korff's comment, because in order to make these measurements, we had to develop a whole new technical approach called low-level counting, and we did that. To give you some feeling for it, the counter we use over in UCLA has a natural background of 4,000 counts a minute. The total count from living radiocarbon is 45. Now, if the date's to be any good, we have to measure it to at least 1 percent; that means a half a count a minute out of 4,000. Now, what my student and I did was eliminate that background. We used massive shields of iron, and then for the penetrating particles, we used a system called the anti-coincidence shield in which we surround the dating counter with other counters and set them in anticoincidence, so any particle that goes through the central counter will trigger one of the outside counters. We have them wired so that they don't record under those conditions. Simple idea, but it worked beautifully. So our background is now 12 counts a minute instead of 4,000.

So Korff has a point. But we also had a lot of luck. Before we developed the low-level technique, we discovered natural radiocarbon by isotopic enrichment. We took methane from a sewage plant, thereby being certain it was living material, and enriched the carbon 13 in an isotope enrichment lab. Well, I happened to have a friend [A.V. Grosse] who happened to have an isotope enrichment lab, who I'd done some favors for. Takes a lot of luck to do that.

TERRALL: So that was another way of measuring natural radiocarbon.

LIBBY: It was the only way that was possible at that time, because the natural level is so low that no ordinary counter could pick it up. We hadn't yet got the low-level technique. So, the final episode in the carbon dating was when Jim Arnold came as a postdoc to work with me on the ultimate test. Namely, we'd take materials of known antiquity and see whether they'd check. They did check.

TERRALL: You also had the problem of figuring out whether the C^{14} was distributed evenly throughout the world.

LIBBY: That was Ernie [Anderson]'s thesis. He did that as part of his thesis. He had two parts to his thesis. One was the Baltimore sewage methane, and the other one was going to museums and collecting samples from all over the world. And the second part of his thesis showed conclusively that it was beautifully mixed. Of course, that's almost obvious when you have a half-life of 5,700 years.

TERRALL: Was that including the mixing of the ocean waters?

LIBBY: It's not clear what does the mixing. The oceans probably mix. The air and the winds, of course, mix. See, what 5,700 years means is that some of the C^{14} atoms in Julius Caesar's body are in you. That's how thoroughly it mixes. We've never been able to find, except in very isolated and special circumstances, any deviation. We found it in the Nevada desert. There are certain fish and plants growing there in those salty lakes which are different, but other than that. . . .

TERRALL: Which have different levels of carbon 14? How do you explain that?

LIBBY: They're alkaline and they're full of carbonate, and the carbonate is old, and if it's old, it doesn't have C^{14}. See, oil doesn't have C^{14}. But any ordinary--anything you bring in. We even tested living material from the South Pole which we got through Admiral Byrd. You see how we do research? They're all our friends, but we don't tell them what we're up to till later. I'm still working that way on important things.

It's very difficult to get any real support for a new idea. It's a hard thing to swallow, but it's true. There's nothing democratic about science at all. The idea that everybody should have sixteen graduate students and produce five PhDs a year is nonsense. Science and the arts are very similar. There are some who can, and most people can't. And our large expenditure on science is not bringing us science; in fact, somewhat the contrary. We're barreling down certain roads and not really paying attention to the serious problems. Biology is the worst example that I've ever seen. I've been a member of the National Academy [of Sciences] for many years, and I hardly even look at the magazine anymore. There may be 2 or 3 chemistry articles and there are 300 biology articles. As far as I can see, none of them mean very much. I have a friend who has become a biostatistician, a former nuclear physicist with Luis Alvarez at Berkeley. He showed me a reprint of an article where he showed statistically that there is no evidence that you can do anything about cancer. None.

TERRALL: Well, to get back to the C^{14} business. When was it finally published?

LIBBY: About '48, we got things nailed down. It was a couple of years before we had enough examples to convince people. But it moved quickly. Here they say that you can't build an atomic power plant in less than ten years, and I say it depends on who's <u>building</u> it. You could build it in eighteen months. I built the hydrogen bomb in eighteen months, and the nuclear navy in twenty-four. No, it moved very rapidly. And then about '49, we made public disclosure.

TERRALL: I see. And is that when you started getting involved with working with archaeologists and anthropologists?

LIBBY: Well, we'd come to get involved a little bit earlier, because I had told a close circle of friends in late '48 what we were doing, and, of course my postdoc, Jim Arnold, and Ernie Anderson. It was interesting what happened. We were publishing these articles. We published the article saying there should be C^{14} in nature. Then we published, eighteen months later, an article that said we found it in Baltimore sewage. Then an article appeared by Professor Evelyn Hutchinson from Yale University. He says, "I think the doctor's up to something." So about three months later, we gave the first radiocarbon date. He put it together.

TERRALL: But you'd already been testing samples.

LIBBY: Sure. But that freed us to get some materials from the museums. It's not the easiest thing to walk into a museum and ask for a five-thousand-year-old mummy. "Here's a young chemistry professor who wants to burn it up"--uh-uh. So what we did was get some powerful friends in archaeology and geology to work for us and they got the samples.

TERRALL: So you already knew archaeologists.

LIBBY: Oh, yes. I've always known everybody. Always.

TERRALL: Chicago people?

LIBBY: One was. The committee [Committee on Carbon 14 of the American Anthropological Association] consisted of men from Peabody [Foundation], University of Pennsylvania, and Yale, and the Field Museum in Chicago--four members, very distinguished--[Frederick Johnson, Froelich Rainey, Richard Foster Flint, and Donald Collier]--they were convinced instantly, once I explained.

TERRALL: So they helped you get samples.

LIBBY: We never could have got them. Arnold and I tried, and they didn't understand what we were talking about. I think I was the youngest full professor at Chicago at that time. I didn't have all that much influence. And I still hadn't told Urey or Fermi. I remember the day when we gave a seminar in the University, and Fermi afterwards shook hands. [laughter] This has happened several times in my life. On the work on the diffusion plant, I didn't tell about the tunneling. It doesn't help to confuse people. I told them I knew how to do it and I was doing it. I'm firmly convinced that there's <u>no</u> democracy in science--just forget it. There are too many people trying to make like scientists who don't know what they're up to, with a watering-down effect which is enormous. We're spending an awful lot of money on science, and I think it's particularly bad in biology. Think of the money we spend on cancer.

TERRALL: So Fermi didn't know the details of why you had been measuring the half-life until that seminar?

LIBBY: No, that's right. In fact, he was very puzzled, because he knew something about my work on the chemistry of the diffusion plant. Of course, Urey knew it and that's how I got appointed; they wouldn't have appointed me, except they knew that. Also my Berkeley work was known to them. It wasn't exactly the worst thing that was ever done. Now, the way Fermi worked was he just kept ahead of the pack. He was so good and so fast that they never could keep up with him. So he essentially was operating under a very secretive system. Now, it's a hard regime. I'm now working on my hundred-and-first PhD. You can't treat PhDs that way--they have to know what they're up to. Except poor Ernie--but he had a good thesis without carbon dating, you see. His thesis wasn't on carbon dating; it was on natural radiocarbon. That's a good thesis.

TERRALL: So after this was published, did you have a lot of requests from archaeologists? How did that work out?

LIBBY: I did the whole discovery of radiocarbon on $2,500. When I went to Chicago as a young professor, I went to the dean and I said, "Now you've hired me, now give me some money." He said, "Okay, how much do you want?" I said, "Twenty-five hundred bucks." He said, "You've got it." I didn't tell him what I wanted to do with it.

Now, my work on the half-life was financed by the Argonne National Lab. They were building up the Argonne lab there, and all the faculty at the University in the sciences were free to come and use the facilities.

That's how the half-life was done. And the work on the Baltimore sewage was financed by my collaborator A.V. Grosse, who was running the isotope separation lab for other purposes. But he never knew the carbon dating until later on. He thought he was working on natural radiocarbon which was a perfectly respectable problem. We, incidentally, discovered tritium also, which is another story.

TERRALL: You mean in the sewage?

LIBBY: In rainfall. It's very similar to carbon 14. See, in carbon 14, after it's made in the air, it burns to CO_2 and is then mixed with ordinary CO_2 and enters the biosphere. Now, tritium, which is another cosmic ray isotope, being an isotope of hydrogen, goes into water and is rained out. It's very similar, except for the chemistry.

TERRALL: It has a much shorter half-life, right?

LIBBY: Right. It has its uses. We've done a lot of things with tritium.

TERRALL: What are the uses for that?

LIBBY: Oh, hydrology, atmospheric circulation.

TERRALL: Was that around the same time that you were working on that?

LIBBY: Couple of years later. See, we had radiocarbon dating in hand by, say, 1950. So we did tritium and did more carbon dating. We did a lot of carbon dating. We did seven hundred at Chicago, and now we have easily a hundred thousand, maybe a hundred and fifty thousand radiocarbon dates. We're going into our tenth international conference next fall, a year from this fall, in Europe. The last one was at La Jolla and had twenty countries represented.

TERRALL: So how did this work? Your lab must have expanded considerably after you started doing the dating.

LIBBY: No, other labs were built. People came to learn, and then they also improved. The technique we use now is far superior to the one Arnold and Anderson and I developed at Chicago. It was pretty primitive, but it worked. We did 700 dates. They were good enough to convince people. Then the money started coming in voluntarily; we never applied for a grant. I remember Paul Fejos from the Axel Wenner-Gren Foundation--there's a foundation in New York called the Axel Wenner-Gren Foundation. Wenner-Gren was a Swedish billionaire who established a foundation. When I got the Nobel Prize, he was my host in Stockholm.

TERRALL: And they had put money into it earlier?

LIBBY: Into the foundation. He was very proud that I got the Nobel Prize for work he had supported. Well, he had supported it after the fact. University of

Chicago is the one that really supported it. But they didn't know what they were supporting; they were supporting me. Science is a very subtle business. Now, out of the 100 PhDs I've had, there's a wide spectrum of achievement and performance. Even though you know someone is bright, you cannot bet with all certainty on success. And sometimes the ones who aren't quite so bright will perform more consistently.

But the carbon dating experience was somewhat like the diffusion plant work. It depended on a central idea which we kept very closely, which we developed step by step. I hope these biologists have some central idea, the way they're spending money. I'm tied up right now with a member of the [UCLA] medical school staff on a search for a new enzyme which controls the growth of bone. He's a great surgeon. This guy can mend your bones in the most miraculous way by using bone meal. So there obviously is something in ground bone which is making bone grow like mad, and so we're trying to separate it. Now, we <u>do</u> have an idea, that <u>is</u> an idea, and he's gotten support for years, and I'm his main chemical adviser. Science is very hard. It's not easy. We got blocked because we didn't have sensitive enough counters on the radiocarbon and we invented a new counter. On the diffusion plant, we got blocked, and I said, "Look, the controlling matter is whether it reacts with UF_6. You may want to build it this way or that way, but build it out of these materials and I'll tell you what to use."

TERRALL: After the radiocarbon dating was published, did you. . .

LIBBY: It was completely open then.

TERRALL: Were you personally involved with collaborating with archaeologists and anthropologists? How did you enjoy that?

LIBBY: Well, it was great fun. We have two people now writing that story: one, as I told you earlier, an archaeologist, a graduate student, and the other one a history-of-science graduate student. It's very interesting that the historian of science is more interested in the carbon 14 than he is in anything else I've done, and I've done quite a lot of things. It seems to grab people, radiocarbon. I never had any problem lecturing on it--people seem to intuitively understand. You know, it's pretty esoteric science when you get right down to it. It's not trivial.

This might be a good place to stop. After the Chicago experience, I went to Washington and became a politician.

TERRALL: Right. I wanted to ask you one more thing about Chicago. Were you involved in working with C^{14} as a tracer for biological studies at all?

LIBBY: Yes, we did a few little things, but the main thing we did was called a horse farm where we built a large greenhouse and we grew plants with radiocarbon. That is, we had an enclosed space about the size of this living room, and we put radioactive CO_2, intensely radioactive, and put a whole bunch of different vegetables to see which ones could take the beating and which ones couldn't. And as a result, we got a whole barnload of radioactive harvest. And it was just at the point when I was going to put animals in there that President Eisenhower appointed me to the AEC, and that stopped that.

TERRALL: What were you planning to do with that?

LIBBY: Make a radioactive horse.

TERRALL: Why?

LIBBY: Horse serum, biochemicals--for research purposes.

TERRALL: Were the radioactive plants used for anything then?

LIBBY: Yes, some of them were used, but not as much as they should have been. A very interesting thing happened. The little chemical companies were synthesizing C^{14} compounds and they didn't like competition. The AEC had the policy of not competing with industry. And then I became AEC commissioner, so I had to enforce that policy. [laughter] So we kind of closed down the horse farm, but other things were happening then.

TERRALL: I wanted to get into the whole H-bomb business.

LIBBY: That was happening all the while I was doing radiocarbon dating.

TERRALL: So in '49, when the Russians exploded the first A-bomb, can you give me your reaction to that?

LIBBY: We were aghast. I had thought that Fuchs had sold us out.

TERRALL: Were you surprised, or was it something that you were just sort of waiting for?

LIBBY: No, I was surprised. But you see, I was supposedly a friend of his. We worked together on the declassification committee. I was in England on a meeting of this committee. It's an international committee of three nations--Britain, U.S., and Canada--though not in all subjects. There were some subjects where they were excluded--like the diffusion plant; they were excluded. But Fuchs was a member. And we were on the way from Portsmouth to London when we heard that he'd been arrested for treason.

TERRALL: Now, when was this?

LIBBY: '49.

TERRALL: About the same time the [Russian] bomb was exploded.

LIBBY: About the same time, just about. And Burgess and Maclean were found out, and there were a couple of other Communist spies. It turned out we were infiltrated with Communist spies. There's absolutely no doubt that that's why Russia got ahead so quickly.

TERRALL: Well, I know that Latimer, who you obviously knew well, was. . .

LIBBY: He was my professor.

TERRALL: He was your thesis adviser, wasn't he? He was concerned about developing the H-bomb. I mean after the war.

LIBBY: Well, most of us at Berkeley were ahead of our time in the sense of atomic armament. And he knew, and I knew, of course, that there was a strong Communist contingent at Berkeley. And Oppenheimer was more or less the head of it. This was well known. Now, on the other hand, we all respected Robert for his great abilities, and after all, Russia was an ally, but this led into some nasty developments--the Oppenheimer hearings, and so on. I talked to Lewis Strauss a lot about that. He said, "Bill, I tried to avoid it. I offered him [Oppenheimer] several ways out." Lewis was the chairman of the board of the Princeton Institute for Advanced Study, where Oppenheimer was made director. Lewis was the chairman. But Oppenheimer wanted to fight it down to the line, and he lost. But we have a similar problem right now. I've worried very much for our beloved country and world, and I don't think we're going to help it any by being stupid about political matters. I'm still connected with the government--not in anything really substantial, but I do spend a fair bit of time working.

TERRALL: Well, in 1949, when Latimer went and spoke to Lawrence and Alvarez, did he discuss it with you at that time?

LIBBY: See, I wasn't there; I was at Chicago. I used to talk with Wendell; after all, he was my teacher. But I was in Chicago. I left Berkeley in 1940 and never went back. That bothered Wendell quite a bit, because I was one of his favorites.

TERRALL: Oh, he was hoping you'd come back there after the war?

LIBBY: Yes. But when I explained to him what Chicago was offering, he said, "My gosh, that's great." He was very proud that one of his students would be offered that. But he died in the early fifties. In his last years he was terribly ill, and I never told him about radiocarbon dating. I'm sorry he died before he heard that. He would have been very proud of that.

TERRALL: Did you know [Edward] Teller personally? He was a Chicago professor, although he was at Los Alamos.

LIBBY: He's a national person.

TERRALL: But I mean at that time.

LIBBY: He and I are working together for Hayakawa, for Evelle Younger. We work together all the time.

TERRALL: So you knew him then?

LIBBY: Yes. He's an internationally famous scientist. He's a very good one. Not such a good politician, but he's a mighty fine physicist, I'll tell you; they don't come much better. I'll tell you who had a lot of respect for him was Enrico Fermi. A lot of respect. Another one was George Gamow--had great respect for him. Probably Fermi, Gamow, and Einstein are the three greatest theoretical physicists of this century--probably.

TERRALL: How much had Teller told you about his research and his ideas about the superbomb? Did you ever talk to him about it?

LIBBY: Well, not until--you see, I was really busy at Columbia.

TERRALL: What about after the war?

LIBBY: After the war, we talked a lot. In fact, I encouraged him to push on his H-bomb project. And I used my contacts in New York and Washington to support him, and I know that's why I was appointed by President Eisenhower, because he knew about that. And there weren't all that many scientists who agreed with Edward. And we had to build a separate laboratory in order to really clinch the H-bomb. Ernest Lawrence helped in that.

TERRALL: Well, in getting to the decision, I know that Teller was very active in lobbying, in talking to people visibly in Washington, and I was wondering what your role was and who you talked to?

LIBBY: See, Edward makes speeches and I don't. If I do say so, sometimes it's better not to make speeches. He's a very fine scientist. He was the guy who invented the hydrogen bomb.

TERRALL: I'm wondering how involved you were in the lobbying effort to convince people that it was a good idea to go ahead.

LIBBY: I was very much involved.

TERRALL: What kind of activities were you. . .

LIBBY: Very quiet.

TERRALL: Who were you talking to?

LIBBY: Well, people that I'd hesitate to name even today. Very influential people.

TERRALL: Senators?

LIBBY: No. I never touched a congressman or a senator. But people outside the government, tremendous influence. I could go on now to a thing called the Committee on Present Dangers; I doubt you've heard of it. There are 101 of us, worrying about the terrible state of our defenses and the derivatives thereof. It looks terrible what our future has for us. With the dollar going to pot, we're going to have to withdraw our troops. How are we going to keep our Navy going and the Air Force going? These are very interesting questions. And nobody's thinking about them. The president just yesterday said he could have a council with consideration of how to support the dollar. Well, the first thing we have to do is to get people in the government who know how to run it. I'm not excluding Democrats, because the Committee on Present Dangers is over half Democrats. It's not in any way partisan. And I'm a member of a number of other committees which are concerned about the trend of events, but I don't make speeches. I speak at news conferences and answer questions. But we're dead serious and we're extremely effective. That's how the H-bomb got going.

TERRALL: Was that your first political activity?

LIBBY: No.

TERRALL: Can you give me the history of your political involvement?

LIBBY: Even at Berkeley, when I was a young assistant professor and even graduate student, Ernest Lawrence was trying to get his cyclotron going. We were all for him, and I helped him raise money. His first magnet was given to him by a utility. But all my life, going with my teaching, are political activities in a quiet way. I'm an ultraconservative. I believe in invention and in great new discoveries; I don't believe in mediocrity. And democracy is pretty mediocre, usually. Now, people should be allowed to live and eat, and that sort of thing, but you tell them if they cut off their breasts they won't die, and that's a goddamn lie. That I don't believe in.

TERRALL: Have your political ideas changed over the years?

LIBBY: No.

TERRALL: Can you say what would have been early influences on your political ideas?

LIBBY: I guess my old daddy was. He was a remarkable man. Tough as nails.

TERRALL: Did he express his ideas about politics?

LIBBY: Oh, sure. He had no use for hoboes. On the ranch there in Sebastopol, after I left high school, I worked for him as a farmhand for one year. And I think he was the origin of my belief in conservative policies--and great inventions and great enterprise. This business that you can make everybody rich--that isn't true; it's just not true. Now, I'm not for oppressing those who are less fortunate--in fact, I'd give them something useful to do. But the idea that they get paid the same amount as everybody else is nonsense. I became a good friend of Nelson Rockefeller in my time in Washington, but maybe we ought to leave that until the next time.

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TERRALL: Okay, I know in 1950 you were appointed to the General Advisory Committee of the Atomic Energy Commission. Can you tell me how your appointment came about?

LIBBY: Well, I represented the classification apparatus of the commission. See, we had a great problem in determining what should be released and what shouldn't be released. And I sat on the committee which decided these matters, from '46 or '47 onward. The General Advisory Committee is a general committee to advise on all matters, and I always felt that my appointment to the General Advisory Committee was due to the fact that I sat on this international committee to determine what should be released and what shouldn't be released.

TERRALL: So in 1950, how familiar would you say were you with the Washington scene, the political scene there?

LIBBY: Quite familiar, due to my activities on the determination of the classification of an enormous number of documents. And everybody wanted to know everything.

TERRALL: Now, did you stay on the declassification committee after you were on the GAC?

LIBBY: No, I think I resigned at that point.

TERRALL: Did you have to move to Washington then?

LIBBY: No, no.

TERRALL: You were still in Chicago.

LIBBY: Right.

TERRALL: How much going back and forth did you have to do?

LIBBY: What do you mean how much?

TERRALL: I mean, were you traveling back and forth every month or. . . ?

LIBBY: For thirty-five years I've made a trip across the continent once a month at least--for thirty-five years.

TERRALL: Were you still working on your radioisotope work in Chicago?

LIBBY: Indeed I was. I had about twenty graduate students, in addition.

TERRALL: So you kept that going at the same time.

LIBBY: Yes, ma'am.

TERRALL: Did you enjoy the work on the General Advisory Committee?

LIBBY: Well, you see, the law had written in the General Advisory Committee, so they had to have one. But I didn't think the commission paid much attention to what the GAC said. On the other hand, it was fun working with them; they were an interesting group of people, and so I enjoyed it, yes.

TERRALL: I wanted to get at the relationship between the GAC and the [Atomic Energy] Commission.

LIBBY: Well, as I say, it was my impression--and, of course, later I was appointed to the commission itself--I think the GAC did <u>some</u> good in kind of taking the measure of various issues and shaking them down. This matter of classification of documents and ideas is a very important one. I never could get the GAC interested, though I was a top expert in this and had worked for years on it, but. . .

TERRALL: They weren't interested in that problem?

LIBBY: Didn't seem to be. One or two, but there are nine or ten members of that committee. Now, as my influence grew, I went more myself away from the matter of classification into the matter of operation, ending up with my own appointment to the AEC. But the GAC was not interested in the matter of classification.

TERRALL: What were they interested in?

LIBBY: All kinds of other subjects. We never lacked for things to discuss. But <u>there</u> would have been a place where they could have done something useful. The commissioners were inclined generally to be highly secretive and not to release information. And I am that way myself, in certain areas. I don't think we should give bomb designs--we never have. But you have the general issue as to what the policy should be. And they wouldn't even debate that. They simply weren't interested in the whole matter. It's very strange.

TERRALL: We talked a little bit last time about the Oppenheimer-Teller debate. How did Truman's decision to go ahead with the H-bomb affect Oppenheimer's position as chairman of the GAC? In a way, he was sort of being undermined by that decision.

LIBBY: He didn't last much longer, you know.

TERRALL: Well, a couple of more years.

LIBBY: Yes.

TERRALL: I was just wondering what effects it had on the workings of the GAC to have. . . ?

LIBBY: Have you read Harry Truman's book?

TERRALL: No.

LIBBY: He makes some comments about Oppenheimer you'd find interesting.

TERRALL: Well, I was hoping you would make some comments about your experience in that situation.

LIBBY: Well, Oppenheimer was a man--very complicated, extremely complicated, very brilliant, very persuasive. Now, I was working behind the scenes for the hydrogen bomb and that's why I was appointed to the commission. We won, and that was over Oppenheimer. He was a very extraordinary man. He was close to being a Communist, his brother was, and he was surrounded with people of a similar persuasion. I don't think that he was delaying the H-bomb to further the Russian cause, but on the other hand, his actions did exactly that, because the Russians had a deliverable H-bomb before we did. And I'm sure they got the designs from these Communist spies. It's very interesting that one of their protesters now is the designer of the Russian H-bomb. I'm just waiting for the day he defects, to get the inside of that story.

TERRALL: One of the Russian dissidents you mean?

LIBBY: A very prominent one [Andrei Sakharov] was the Russian Teller, and if he would defect, we could get the inside of that story as to how they got it. I'm sure they got it from Fuchs. So my relations with Oppenheimer were, shall we say, arm's length. I appreciate his brilliance, but so do I appreciate the Russian leaders' brilliance.

TERRALL: Well, how did it work having him as chairman of this committee?

LIBBY: He tried to run everything. And he usually succeeded. He was very persuasive, very objective. While I was on the senior reviewers--that's the committee that was doing the classification review--at one meeting we had three Communist spies in the room. Fuchs was one, Burgess was another, and Maclean was the third. They were all sitting there. These are all people convicted later and confessed Communist agents. So I had my reservations about Oppenheimer, either a willing or an unwilling tool. Do you know about Communist party discipline? Very stiff.

I don't think he was a member of the party, but he was damned close to it.

TERRALL: Did you have a sense of what his relationship was with Truman?

LIBBY: I know what it was. You ought to read Truman's book. But I'm sure the president didn't make the decision out of any enmity; he made it for the good of the country, and he made the right decision, and he barely made it in time. Not enough people know what a squeaker that was. See, one hydrogen bomb can take out New York City. They had a deliverable one before we did. That's close. It wasn't very long before we had one, but it was a squeaker.

TERRALL: Now, after Truman's decision was made to go ahead with the H-bomb, was the GAC involved in implementing that at all?

LIBBY: No. They had lost their influence.

TERRALL: So it was the Atomic Energy Commission that was. . . ?

LIBBY: And DOD [Department of Defense]. There's a very important committee in the whole picture called the Military Liaison Committee, which works between, or did work between, DOD and the AEC. And their influence skyrocketed. No, the GAC never regained its stature after that. It continued to exist and to function. Other committees came into being--the reactor safeguard committee, with Teller as its first chairman, setting up the conditions under which power reactors could be built. That committee is still very important. And then in the biological area, we had some advisory committees. Out of some of their deliberations grew the School of Nuclear Medicine here at UCLA. The GAC did not ever come back to have the influence it had before that terrible mistake it made.

TERRALL: I'd like to talk a little bit about Project Sunshine.

LIBBY: Okay.

TERRALL: I understand its goals were to measure fallout and evaluate the effects of fallout on human beings.

LIBBY: That's right. Those are two goals, really. The fallout measurement is quite separate from the effects on human beings.

TERRALL: But the project had to do with both of them, didn't it?

LIBBY: Right, it did. Specifically what we aimed at was to measure the amount in human beings, rather than the effects. The effect measurement is a very different study. But we measured the amount, and then we also measured the total fallout and compared, and got the ratio of the pick-up. And that work is still going on. I still get the reports.

TERRALL: When did that start and how did it start?

LIBBY: I started it. It was in--'53, I think, was the first measurement we made. There may have been a few measurements made at NYU in the New York area before. We were kind of neck-and-neck there, the Chicago group and the New York group, in getting it going. In the summer of '53, we had a summer-long conference at the Rand [Corporation] which I set up--theoretical discussion, three months, of what the effects would be. The amount of the fallout, would it be worldwide? We predicted worldwide. It wasn't then discovered, the worldwide nature. But we did pretty well at guessing what the dissemination would be.

TERRALL: So that was the beginning of the project, the Rand summer seminar?

LIBBY: The Rand study, yes, and that silly name [Project Sunshine] got tagged onto it.

TERRALL: How did that happen?

LIBBY: I can't remember. See, it was classified.

TERRALL: Oh, the whole project was classified?

LIBBY: Yes. So we would have some problems, so we put a code name, a cover name. But we had things pretty well in shape when the Castle series [of bomb tests] in '54, March of '54, occurred in the Pacific. And this taught us quickly how worldwide it was and how widely disseminated. I've been happy that we got that, almost a year's jump on it, and we were ready to go before we had to face this worldwide fallout problem.

TERRALL: So it was that series of tests that was the first big source of fallout?

LIBBY: Yes, that was the first large source.

TERRALL: Were there other labs around the country working on this?

LIBBY: Oh, sure. We were all working together.

TERRALL: Were you involved in the coordination, the administration of it?

LIBBY: Sure. Yes, ma'am. See, before I became a commissioner, I still had considerable influence.

TERRALL: How were the different places coordinated?

LIBBY: Through the AEC.

TERRALL: It was all funded through the AEC.

LIBBY: Yes.

TERRALL: It was under the auspices of Project Sunshine that you did the strontium 90 work as well?

LIBBY: We did two things in Sunshine. One was strontium 90; the other was cesium 137. And that was a big job, and it's still going on. We have worldwide reports on both strontium 90 and cesium to the present day.

TERRALL: Was one of the things you had to do to set hazardous levels of how much. . . . .?

LIBBY: No. We didn't have much to do with health effects.

What we were doing was saying how much there is.

TERRALL. Who was doing the other part of it?

LIBBY: Well, the biology and medicine division was working on it, but the problem has never been solved. We have no idea what the effects of these small dosages are. No idea at all. And yet they legislate certain levels for drinking water and air, but it's pure guesswork. They roughly do this: they say we'll let you take, total, 10 percent of what you get anyhow. And that's about how deep it is.

TERRALL: You mean 10 percent of what there is in the natural environment anyway?

LIBBY: Yes. And most of the radiation standards are set that way. They don't know what they're doing.

TERRALL: What else can they do if they don't know?

LIBBY: Keep their hands off.

TERRALL: You don't think there should be any standards at all?

LIBBY: I think it would be better if there weren't. But what we should do is some decent research to find out what the effects are. Kind of like the cigarette business, have you noticed this recent ploy? They're sanitizing a cigarette. They've got a guy to say if the tar is low enough; then it's okay. I think regulation in ignorance is bad, universally. And we really don't know about low-level doses.

TERRALL: Well, there certainly was research going on into that though, wasn't there?

LIBBY: Only on acute doses, not on low-level doses. Well, a lot of research, but it doesn't tell you anything. Just taking a million mice and rats and measuring them doesn't tell you anything. We've never spent more money on science than we now are spending, and yet we're getting less for it. We've lost our number one position, thanks to the idiocy with which we manage--mismanage, I should say. Well, the AEC was saved from this kind of thing, because of a few strong-minded people. And I was one of them.

TERRALL: So Project Sunshine was certainly successful at what it set out to do.

LIBBY: Sure. It gave us some basis for the amount of fallout and exposure. Now, of course, if you get up into lethal doses, there's no argument. One knows that at 500 rad, you're close to death. There's no argument in acute doses. We have a lot of information. But it's these silly things, where if you're living one mile from a power plant, it exposes you in a million years, if you should live to be a million years old--that kind of nonsense. You see the same thing in OSHA [Occupational Safety and Health Act] regulations on chemicals. They're trying to ban benzene--you can't run this country without benzene--and all kinds of things. I'm just reading a lengthy article on the Storm King Reservoir on the Hudson River, probably the best proposal ever made to get cheap electricity into New York City. It was beaten down by a bunch of ignoramuses serving self-interests. We're paying dearly for mismanagement. We're damn near going broke because of mismanagement. A very large part of the oil imports is due to the smog restrictions we put on cars. I help put them there, so I know what I'm talking about. I was four years on the [California] Air Resources Board. We thought we were doing a good thing. As far as I can see, it hasn't done any good at all. What do you think about the smog, is it any better?

TERRALL: I don't know. I haven't been here long enough to say.

LIBBY: Well, it was pretty bad this year--terrible. And there are a lot of people saying, "Where's that improvement you promised us?" And they damn well should say it. So I think to regulate in ignorance is a very dangerous thing, really. And that's what we're doing, right across the board. We even get the Congress and the legislatures to put numbers in their laws. It's impossible to live this way and not pay a terrible price. Let me give you some examples. We no longer do bacteriological research for warfare. We decided--I mean the Congress and the president decided--it wasn't a nice thing to do. Therefore, we canceled it, leaving ourselves open to a very, very important method of warfare. Same thing on chemical warfare. They somehow think that if you have enough hydrogen bombs you don't need other things. I've been reading about the Legionnaires' disease. You see the possibilities? They have an outbreak--they have thirty-four cases in New York City right now. Last night they were washing down the streets in the garment district. Did you ever read about what they did during the Black Death in the Middle Ages? Witchcraft. We're reduced to that, essentially. Now, ignorance is our enemy, and what we ought to do is research. Find out what this thing is, and how come we can't control it. Well, after several years, they've got some handle on it. I understand it is a bacterium. But, my God, if you put a section of the DOD into inventing one, look out! So we decided not to defend outselves. Similar stories in chemical warfare. People who assume that they know things and then act on that basis are dangerous, very dangerous. My whole life I've been trying to fight that tendency, and to support the defense of this country, and our position, and we sure better do it.

TERRALL: To go back to the fifties, Eisenhower was elected in that time period that you were . . .

LIBBY: He was elected in '52.

TERRALL: . . . '52, right. Were you an Eisenhower supporter?

LIBBY: Right.

TERRALL: What did you think about his Atoms for Peace idea, which was in '53?

LIBBY: Great. That turned out to be my main job when I was put on the commission, the Atoms for Peace program.

TERRALL: What did you do with respect to that?

LIBBY: How many hours do you want? I worked five years on it.

TERRALL: Can you tell me a little bit about it?

LIBBY: I can tell you a lot about it.

TERRALL: Good.

LIBBY: But I don't know how germane it is. I was put on the commission because of my helping on the hydrogen bomb decision. There weren't all that many scientists who were willing to stand up and talk. I didn't talk publicly; I talked privately, very effective places. The point is I knew where to talk, which is not easy.

TERRALL: How did you know these people?

LIBBY: Well, I'm not exactly stupid.

TERRALL: But you hadn't been around Washington.

LIBBY: I'd been around one hell of a lot.

TERRALL: Around Washington?

LIBBY: Around everyplace. And I knew where the buttons were. That was one of Oppenheimer's weaknesses. He didn't really know where the buttons were. Power shifts from time to time, and you have to keep up with it. But I was appointed for that reason. And then the whole purpose and thrust of the AEC was to develop two lines, the military and the peaceful, side by side. I had a lot to do with the military. The hydrogen bomb and the nuclear navy were the two main ones, though we had other projects, some of which failed. We were building the atomic airplane; that never worked. It was a bad idea from the first. We missed one elementary point, which should have been obvious. You couldn't fly one of the things, because you wouldn't know where to land it. When we faced up to the question of where we would build an airfield--see, in the case of a crash, all that radioactivity released, you've got a real problem.

TERRALL: When you were first appointed to the commission, what was your response?

LIBBY: I accepted it.

TERRALL: Did you have reservations about it taking you away from your research a lot more than your previous commitments had?

LIBBY: Well, I'd just finished my work on radiocarbon dating, so I was ready for something new.

TERRALL: So the timing was good.

LIBBY: Pretty good. I had several graduate students I had to finish off after I went to Washington, but it turned out not to be too much of a problem.

TERRALL: So what happened to your research work?

LIBBY: It stopped.

TERRALL: So you didn't continue with . . . ?

LIBBY: Well, I had a little lab at the Carnegie Institution, on Upton Street, there in Washington, and I would go there and do an hour or two a day. My chauffeur would drive me over there, and I'd eat a brown bag lunch and work for an hour or two, and then we'd go back to work.

TERRALL: What were you working on there?

LIBBY: I was working on several things. Do you know what a beta ray is? Well, I'm an expert at measuring it. This I invented. [points at counter] That's a thin-walled Geiger counter. Beta rays are very varied in their penetrating power, and I developed, while at the Carnegie, an empirical method of going from an observed count rate to an absolute assay in the solid sample. Not very dramatic, but it's mighty important practically. That's one thing I did.

TERRALL: Did you continue on the strontium 90 work?

LIBBY: We did that, too, though most of it was being done by contractors to the AEC. The project by then had gotten very large. I did do a couple of things I couldn't get anybody to do. One was the repression of pickup of [radioactive] cesium and strontium by adding stable [strontium and cesium]. In fallout, the amount of strontium and cesium is very minimal, so we had the notion that by adding stable strontium and cesium we could prevent the pickup of radioactivity by the vegetables. And I published a simple little note on that.

TERRALL: Did that work?

LIBBY: It worked.

TERRALL: You could prevent the pickup?

LIBBY: Reduce it. But the biologists criticized me: "What are you trying to do, be a biologist?" I said, "The reason"--and this was written--"is the commissioner couldn't get you to do it. So he did it himself. Now you do it better." The botanists didn't like the way I did these experiments. People are unbelievably self-centered. They wanted the AEC to support them, but would not do anything useful.

TERRALL: The AEC in those days was actively looking for people to support on work like this, weren't they?

LIBBY: Only if they did useful things.

TERRALL: But they were looking for people to do the kind of research that . . .

LIBBY: One of my jobs on the AEC was to allot funds for research. Ninety percent of the support for physical science in this country went through the AEC. A good part of the time I was on the commission, I was the only scientist on it.

TERRALL: So you were reviewing proposals all the time.

LIBBY: The big ones. Of course, there were zillions of them coming through, and most of the little ones were not, but I was a bit annoyed by the criticism of that paper, because they had shown total disregard of the national interest. I was doing it purely to protect against fallout onslaught. That's one of the reasons nuclear medicine was created over here [at UCLA]. That was my motion, to create nuclear medicine, because we needed some place which would work on these problems and not try to circumvent our purposes. We are very poor at managing scientific research. We just don't know how to do it. I think our record is as good as anyone's has been--in five years were spent about, say, $5 billion on science and research, and I think we got more out of it than most anybody has. We did not use a committee method. Our principle was to give the money to the best people and let them do what they wanted.

TERRALL: And how did you determine who the best people were?

LIBBY: Well, I talked to them and I'd define it. That's the way it worked, all right? And I've been vindicated in my decisions by subsequent performance. It's not hard to tell a bright one. I'll tell you what is hard is to tell whether they have common sense. That's a hard quality to detect. But it's easy to tell a bright one. For years here I taught honors freshmen, and I picked the honors freshmen. My average over ten years or more was over half of the class got doctor's degrees. It was a freshmen class. We picked them--it's not hard. What you have to look out for is common sense and character weaknesses and that kind of thing which are not so easy to detect, but you can tell intelligence.

TERRALL: You mentioned, in passing, your work on the Atoms for Peace proposal, but you didn't really get into it at all.

LIBBY: It's a long and fascinating subject. It began with President Eisenhower's speech before the UN in December of 1953. I was appointed to the commission a year later, October '54, and they were already under way. But there was one broad area where there was nobody on the commission who knew anything, and that was the use of isotopes. So I took that over and pushed that very hard. Now, Admiral [Lewis] Strauss pushed the atomic power, and other members of the commission pushed the atomic power very hard, and the industrialists did, too. We had a very strong effort. Atomic power is an amazing development; it's largely an American development. Though we're trying our best to ruin it, it's still so. And our atomic navy is unsurpassed. We went after the atomic power program very strongly and went after the use of radioisotopes in biology and industry and research very strongly.

TERRALL: I was going to ask you about that. What were your rules for distributing radioisotopes to researchers? Was it just whoever asked for them?

LIBBY: There were plenty of them, so . . .

TERRALL: Scarcity wasn't a problem?

LIBBY: No. After the war, you see, with the chain reactor we could make isotopes of any sort--running out of your ears practically. That isn't entirely true, but roughly, it's true. Like radiocarbon was a scarce, scarce thing before the reactor came in, but suddenly radiocarbon is present in great abundance--and similarly for most of them--so shortage of isotopes wasn't a problem.

TERRALL: So if someone wanted isotopes for . . .

LIBBY: Well, we had to set up so people wouldn't hurt themselves. I was inveighing against the silly rules on low levels, but there's no doubt you need controls on high levels. My goodness, when I was a young scientist and graduate student, I did some of the most hair-raising things. A couple of weeks ago, we were in La Jolla for Martin Kamen's sixty-fifth anniversary. (Martin was a nuclear physicist at the Lawrence Radiation Lab at Berkeley, one of the first cyclotrons, and he was in charge of the cyclotron.) And the speakers there, [Glenn]

Seaborg, [Edwin] McMillan, Robert Wilson, and myself were all there at that time. Bob Wilson told how he would turn the machine off, then crawl in between the magnet poles and extricate a sample he had placed in.

TERRALL: Immediately after turning it off you mean?

LIBBY: Yes. Well, Seaborg said, after Bob finished--and Martin Kamen was doing the same kind of thing all the time--"It's a wonder they weren't all killed." I used to expose myself to many rad per day, and I'm just turning seventy, all right? I think we shouldn't make regulations in ignorance. I think it's a very dangerous procedure. We should always ask, "What's your evidence?" If there's no evidence, don't regulate. Otherwise, it's witchcraft, positively witchcraft. You don't know what you're doing. I think cigarettes are bad, but I've been told a lot of other lies. I've been told they could do something about breast cancer and now they can't. There isn't <u>anything</u> they can do about it. And they go through all these damn lies.

TERRALL: On the radioisotope business, then, as far as the AEC was concerned, you set up the procedure for . . .

LIBBY: Well, the staff did it. We had five thousand people on the staff. I had eight full-time assistants. A monolithic organization. But it was a big job, too. Another little thing I had was finding uranium. We had a system of bounties and prizes.

TERRALL: You mean finding sources of uranium?

LIBBY: New uranium mines. And I'm trying to get people to go back to that, because they essentially haven't found any uranium since. We turned our method off in '57. We have lots of uranium left in this country, and the little old lady with a Geiger counter like this is the most effective method known. Our richest find, was in Grants, New Mexico, and that was found by an eighteen-year-old Indian who said, "What a pretty rock!" It's the biggest mine in this country. That was my work; that was what I did. I built the first Geiger counter in this country.

TERRALL: So this involved making Geiger counters available.

LIBBY: We had Sears Roebuck selling them for twenty-five bucks. Yes, ma'am. About the time you were born. Now that thing [indicates Geiger counter] cost six hundred dollars. It's purely a matter of volume. If you built them the way they build radio or TV sets, I don't know, it would certainly be less than fifty dollars. But it would be of that order, probably. A Geiger counter is the most sensitive instrument there is. Nothing else approaches it, as far as radiation is concerned. Well, that was an activity; and we found enough uranium to support our atomic power program for a hundred years. These people who say we're out of uranium don't know what they are talking about.

TERRALL: So these uranium mines are still being mined?

LIBBY: Oh, yes. See, the way it works is you have an outcrop, and that's what the little old lady finds. But once you've got an outcrop, then you follow it with coring, and they drill several million feet of cores every year in this country, exploring known finds. After I got off the commission, as consultant to Exxon, I helped persuade them to go into the atomic power business. And they are now very large in it. But I don't think anybody can beat the little old lady with the Geiger counter. And that's what we did. We did a little supplemental. We would fly helicopters around with very sensitive counters, but they're still several hundred feet above the ground, and the air shields it. See, the radiation is not very penetrating. That much soil, for example, would cut it down substantially. So it has to be a definite outcrop. But we had this Stumbling Horse Mine. This was a prospector out--he was so ignorant he didn't know that radiation wouldn't go through six feet of snow, and he was looking, and the horse fell down in the snow, and they found the Stumbling Horse Mine.

TERRALL: Where was this?

LIBBY: In Colorado.

TAPE NUMBER: III, SIDE TWO

SEPTEMBER 8, 1978

TERRALL: I wanted to ask you about Admiral Strauss. Can you comment on the way he chaired the Atomic Energy Commission?

LIBBY: Well, I was a very good friend of Lewis Strauss, but he got into some awful fights, terrible fights, and I could never quite figure how it happened. There was a member of the commission called Thomas Murray, and he and Lewis used to fight all the time, and I'd sit between them. I was larger than either of them. [laughter]

TERRALL: What did they fight about, procedural things?

LIBBY: It seemed everything. They couldn't agree on anything. And then another fight he had was with Clinton Anderson, the senator from New Mexico. And again I would stand between them. See, I was a friend of Anderson and I was a friend of Murray, as well as Lewis's friend.

TERRALL: What was his fight with Senator Anderson?

LIBBY: I think that was more justified. He had some questions of executive privilege, and he was protecting the president's authority.

TERRALL: Strauss was?

LIBBY: Yes. See, the AEC was part of the president's entourage, one of the agencies.

TERRALL: So what was the problem?

LIBBY: Congress was trying to usurp the authority of the AEC. That was the fundamental cause of the fight between Anderson and Strauss. But there was a little personality involved as well.

TERRALL: What was the outcome of that?

LIBBY: Well, I think you'd say Anderson won. It's a long story.

TERRALL: What is the story?

LIBBY: There are several books on it. We see a similar thing now between [James R.] Schlesinger and members of the Senate. Very similar. There's a natural opposition of interests.

TERRALL: You say Senator Anderson won. Did the power change after that?

LIBBY: It went over into the hands of the Joint Committee [on Atomic Energy].

TERRALL: So things really realigned. How did that affect the work of the AEC then?

LIBBY: Killed it.

TERRALL: This was in what, '55? No, later.

LIBBY: Later. I was on the commission until '59. The commission has done very little since then. The whole hassle about atomic power is due to the inaction of the AEC--or rather, I should say, because they didn't educate the public properly.

TERRALL: Strauss stayed on as commission chairman though, didn't he?

LIBBY: Till '57. He had been on the first commission, and I think it was '57 he resigned. I served two more years. By that time, I was acting chairman, then they got a full-time--John McCone was the chairman. But he barely learned the job when he went to the CIA. Then Seaborg came in for ten years and all he did was high energy physics. But the fundamental explanation is the power moved from the Executive to the Congress. And they are in no position to operate anything, absolutely. So I think, though Anderson won, the fruits of the victory were not all that great. The committee structure of the Congress has half the members [from the] Senate, half [from] the House. The chairmanship rotates between Senate and House and also rotates between parties.

TERRALL: Anderson was a Democrat?

LIBBY: Democrat, yes, senator. Now, his counterpart, a Democrat, Melvin Price, a very good man--Anderson was a very good man. Though I'm a Reagan-Goldwater Republican, I can recognize ability in some Democrats. Another one was Scoop [Henry] Jackson, a very good man. On the Republican side, we had similar--Bill Knowland was on the committee for a while when he was majority leader of the Senate, and [Bourke] Hickenlooper from Iowa, and a number of very strong Republicans, both from the Senate and the House. It was fifty-fifty. It was a large committee; I think there were some twenty members. Now, how can a committee of twenty members do anything? So we sat on our tailbones from 1960 till the present time.

TERRALL: Back in the fifties, was this a constant struggle between the joint committee and the AEC?

LIBBY: Well, the way we operated in the fifties is we did it, then we told them.

TERRALL: And is that what they were objecting to?

LIBBY: Eventually, yes. They praised us a lot though, because we did some very important things. We beat the Russians on the hydrogen bomb race; we armed this country with both hydrogen and A-bombs. I don't know whether you know it, but we were out of A-bombs when the AEC came into business. We armed the country, and that was quite a job. Then we built the nuclear navy, and that was quite a job. Then we did the Atoms for Peace--at least launching it. It's a tragedy that we didn't keep going. I don't blame Seaborg in particular, or McCone. It's just that that's the way it happened. Now, I was offered to be permanent--or at least to have a long term as chairman of the AEC. And if I had stayed, I think we would have kept the power.

TERRALL: What made you decide not to stay?

LIBBY: I wanted to go back to being a scientist, because I was fifty then, and if I didn't do it, I never would.

TERRALL: So you saw the Atomic Energy Commission as sort of an interlude in your scientific career? You were always planning to go back?

LIBBY: I came to think about that. When I got the Nobel Prize, President Eisenhower sent me a wire and said, "Congrats, Doc! I didn't know you were a scientist." [laughter] He thought I was a politician, which I took as a compliment. I was pretty close to him, and we worked together exceedingly well. I liked Lyndon Johnson, and Lyndon liked me.

We don't run this country very well, and I'm really worried. I'm scared stiff, to be absolutely blunt. I think it's doubtful that Carter will finish his term. That's how bad it is. Well, look at the polls--lower than Nixon, when Nixon resigned. To fill that job, you've got to have special qualities of leadership. Hell, he's never led anything. And we've had problems that are pretty serious. Our balance of payments is only part oil, you know. I was shocked to learn last month that it was largely cars which we are importing. Why do we need to import cars? That's nonsense. But we've got to batten down the hatches and protect our country. I would expect that we're in for a very, very serious depression. I don't think those guys are smart enough to avoid it. And this one is going to be a real one, it's going to be world-wide. Maybe I'm just Cassandra; I just don't see it. Our leadership has fallen and failed. Nixon was a pretty good president, if he hadn't gotten into Watergate. But maybe it was those qualities that made him as good as he was. He was a tough hombre. This guy is pretty soft.

Well, back to what I call the change in the role of the Atomic Energy Commission. It became like all the other agencies. The agency, when first set up, had extraordinary powers. It could do things no other agencies could do, including the State Department. We could do things the State Department couldn't do. Schlesinger has a similar charter, if he could ever get that bill going. I think it's a crazy bill, but I wish they'd do something about it. And Carter--the articles of incorporation of the Department of Energy give him tremendous power. He's much like the AEC was, which is what you need to make this thing go. Anyhow, we had an absolute ball during the fifties, and we could do things. But the fight between Lewis and Clint Anderson kind of epitomized the struggle between the two branches of government, and that's why I say Clint won, because the AEC never amounted to much after that. It never really led. I don't blame Seaborg particularly. He's not a very strong leader, but

I do think it was the victory of the joint committee which caused the shift of power. And they're in no position--they don't have the staff; they don't have the authority, to operate things. Sort of like what EPA [Environmental Protection Agency] is trying to do now on the environment. They can't do anything except be obstructive. And NSF [National Science Foundation] gets loaded with all kinds of miscellany; it doesn't have the staff or the money to do it. We're wasting enormous quantities of money and missing enormous opportunities. People are working on irrelevant things when we are desperately in need of real knowledge. And they've not even been told what's important to work on, not even knowing what we need to know. Like in the field of biology, I think biostatistics can hardly be exaggerated in importance. It happens to be a cheap way to make information. I have a friend who has moved from nuclear physics into biostatistics. He's much happier. We have a very good department here; Lester Breslow is the head of it--very good. And the things they are turning up would curl your hair.

TERRALL: I wanted to talk about the debate over the hazards of fallout; in particular, the debate centered around the question of genetic hazards.

LIBBY: Yes, well, it's a good example of public chicanery on the part of famous scientists who leave their field and say, "I know." Linus Pauling and Bertrand Russell led the argument that we shouldn't fire any bombs in the air because it will kill five babies in the whole world. Well, they knew better. They were lying. I think people who lie in public for a political purpose should be brought to task. That's what the debate was about. Nobody knows. They were saying, "It will." Nobody knows.

TERRALL: There were geneticists who were talking about genetic dangers. They weren't necessarily saying, "We know," but they were worried about it.

LIBBY: Well, I would say so. However, they were exceedingly well organized. See, I was leading the AEC side of this, and I know their tactics, how well organized they were.

TERRALL: There were certainly respectable geneticists who were worried about the genetic problems.

LIBBY: But they didn't know what to worry about. All they'd ever done was high doses. They hadn't done low doses.

TERRALL: What do you think was the basis for the disagreement?

LIBBY: Ignorance.

TERRALL: They wouldn't have questioned your data on the levels of fallout?

LIBBY: No.

TERRALL: It was just the conclusions that they differed on.

LIBBY: They said that any amount of radioactivity, no matter how small, is intolerable. But do you know that the dose rate in Denver is three times what it is in Los Angeles? The same people wouldn't point that out. That's why they're lying. They wouldn't point that out.

TERRALL: So why do you think they were lying?

LIBBY: Because they didn't realize the inconsistency. If [there is a] 1 percent increase in the natural dose in Los Angeles, then a 200 percent increase [as in Denver] ought to be damn obvious. But it isn't. Public health records show no effect. We had populations some places in the world where the increase was 10,000 percent; they showed no effect.

TERRALL: So how do you account for that?

LIBBY: They're just plain liars.

TERRALL: Liars. Why were they lying though? I mean, what was the point?

LIBBY: For a purpose. To stop the armament of this country.

TERRALL: So you think it was entirely political on their part?

LIBBY: Yes. At least [Hermann] Muller was, and Pauling was, and Russell was. All three well known. All right?

TERRALL: Muller was the only geneticist out of those three.

LIBBY: Right. But the track record is clear in all cases. All top scientists, very famous, taking advantage of their position for political purposes. So it was a bitter fight.

TERRALL: Well, there were other geneticists who weren't as outspoken, but who were worried.

LIBBY: Well, a lot of people are afraid of anything new, and that's a natural reaction. I don't blame them. But they weren't standing up and making public speeches. I just think it's terribly reprehensible for a famous scientist to speak out of his field and use his fame. It would be as though I talked about cancer cures. I don't know anything about biology. I happen to have some common sense though, and I just don't think that we ought to pay attention to these people. I've told Pauling this to his face. He knows exactly what I think. I nominated him for his goddamn Nobel Prize in chemistry--that's what I think of him. He's a good chemist. He ought to keep being a chemist. But he's a very harmful person, and he's led a lot of other people down that road. It's very interesting.

TERRALL: What was his response when you told him that to his face?

LIBBY: He just laughed. He's a very egotistical type. He had enormous influence, but we won. We've got to keep winning, and we are going to keep winning--we'd better. He's against atomic power. What does he know about atomic power? He never worked on it a day in his life. Nobody on that whole committee against atomic power has ever worked on it.

TERRALL: That is definitely a common phenomenon, but the business of the geneticists getting involved with the whole debate . . .

LIBBY: Well, let's follow it now. Muller did Drosophila with X rays; God knows that's high dose. He never did low levels. The whole debate is low level. Now, if the people in Denver don't have six legs, why should adding 1 percent in Los Angeles be a hazard?

TERRALL: I see what you are saying. It's just . . .

LIBBY: Well, damn right. It's just common sense; just common sense. You don't have to be a great scientist to follow that line of reasoning. We won because of the strength of that argument. The average person knew we were right. There was a lot of bad feelings about Pauling getting the Norwegian prize for that work of his. Didn't do the Nobel Peace Prize any good at all. They gave him the Peace Prize for this campaign of his. Didn't do them any good. See, the whole balance of power in the world now is atomic. I guess the Norwegian Parliament would just as soon go back to bows and arrows. Anyhow, it's a rankling debate and we're still continuing it; we're going to win it, watch. Watch November, just watch. The reason is, people have sense. I'd just as soon put a thing like this on the ballot and let the average taxi driver vote on it, frankly. Common sense is very rare among certain scientists, and Pauling doesn't have much. You know his latest thing is he's trying to cram vitamin C down your throat to cure everything from cancer to colds.

TERRALL: I guess there was a lot of public worrying about strontium 90 in milk, for example.

LIBBY: Well, we probably were partly responsible, because we early on declassified it, put it right out in the open. Maybe that was a mistake, because people were frightened. People don't know strontium 90 from an apple, and there's no way of telling them. So it might have been an error in judgment on the AEC's part that we published that thing. There are still parts of that which are not published, but most of it is in the open. You see, people are afraid of anything new--I don't care what it is. You'll find people against progress--there are people who are traditionally against any progress whatsoever. They'll vote against it every chance. Fortunately, the majority of Americans, at least, are for progress. That's saved our neck time and time again.

TERRALL: In 1955, after the test over the Bikini Islands in the Pacific . . .

LIBBY: Well, we would run them every two years, and we had a '54 series [the Castle series].

TERRALL: Yes, it would be the '54 series. There was a joint committee hearing where the AEC was accused of withholding information about radiation hazards from the public. Do you remember that?

LIBBY: Yes, sure, I was testifying.

TERRALL: What was your feeling about educating the public in this area?

LIBBY: Well, as I say, I was for release. But looking back, I'm not sure. I probably would vote the same way today, but it's not an open-and-shut case. Most people seem to accept you don't tell people how to make a hydrogen bomb. Where do you draw the line?

TERRALL: What about the people who were in the test area? Presumably there was some effort made to clear the test area, but there were still people who . . .

LIBBY: We've never known of any chronic radiation damage.

TERRALL: But there were the people who had acute radiation sickness--the Japanese fishermen who were in the wrong place at the wrong time.

LIBBY: They had been warned. That's as far as they go. That's international protocol. For example, when the Russians are going to shoot their missiles in the Pacific, they just give a general area, latitude and longitude. If our ships go in there, it's our fault, not theirs.

TERRALL: But those people wouldn't have known anything about the possible dangers or how to deal with radiation sickness.

LIBBY: That's right. Well, the Castle tests in '54 were very early in the fallout business. We'd had our Rand study the preceding summer, and we learned a hell of a lot in the '54 series. We really did.

TERRALL: So a lot of that wasn't known before that?

LIBBY: That's right. Now, of course, having declassified it and put all this stuff in the newspapers caused a lot of furor. There are other effects of atomic tests which have never been played up the way fallout has. It may have been an error in judgment, but the joint committee forced it in the sense that they raised the question. Of course, the AEC decided it. They decided to publish it.

TERRALL: Did you think the joint committee was justified in raising the question?

LIBBY: Oh, sure.

TERRALL: And you testified at those hearings, didn't you?

LIBBY: Right.

TERRALL: What was it like to testify in that situation?

LIBBY: Not pleasant. But you get tough. You're not there unless you have power; that's why you're there.

Now, the joint committee would bring us up about twice a month for castigation on some subject or other, but on the other hand, they'd protect us from all the other committees of Congress. We had to deal with them, period. We kept them informed on a current basis, not only unclassified but classified material. And they were subject to all the penalties of revealing it, which were very severe. So we developed a working relationship, but there was underlying it the struggle between the legislative and executive branch, which kind of came to head in the argument between Clinton Anderson and Lewis Strauss. I don't know, for the '54 hearings on the fallout thing, it wasn't so much that we were keeping it, as we just didn't know.

TERRALL: I see.

LIBBY: And we learned pretty quickly though, thereafter. We never had another fallout accident.

TERRALL: So the whole civil defense public education program started after that.

LIBBY: Yes, and I was a leader in that. By the way, there's a reviving of interest in civil defense. A friend of mine called me from Phoenix the other day and wanted some of my old materials, because down there they're quite interested. See, the Russians and the Chinese have built up a fairly impressive civil defense shelter system. Now, you can say about shelters that in a direct hit they're no use--that's true. But they are very useful if you're not a direct hit. I've always been much in favor of shelters. When I first came here to UCLA, I conducted a public campaign in Los Angeles, and we got several thousand shelters built in private homes. Interesting development there: the owners didn't want even their neighbors to know they had them. So I never could get publication of who had what. As you know, most public buildings have some primitive kind of shelter facilities in the basement, and that's very good. What we need to do is step it up and keep the food up to date, and the water and all that, the meters, blankets, and stuff like that. But I'll tell you, we've got what I call <u>pax atomica</u>. Let's just hope. I think we've got it. War is so unimaginably horrible now.

TERRALL: The AEC was always in the position of regulating the potential hazards and also of promoting weapons development. Do you see any conflict between those two roles?

LIBBY: Yes, it was a conflict, and I was very much in favor of separating those, but then they put such people on the Nuclear Regulatory Commission. You know, a government body is only as good as the people on it, and some of the appointments were pretty questionable. But I still think it's a good idea to have the two arms separated.

TERRALL: In practice, did that create problems during the time you were on the AEC?

LIBBY: Not really. I think we were pretty well behaved about fallout and radiation hazards. What other major industry has our record? None. We developed atomic power, and I don't think there are half a dozen casualties in the whole business. That includes construction. I'm a director at Research-Cottrell. We were building that Willow Island cooling tower [in West Virginia] when fifty-one men were killed in April [1978]. That's ten times the total life loss that we had in atomic power, and that's one accident. We were just plain lucky. When you're doing big jobs, you can have accidents like that. Of course, it's somebody's fault, but we were lucky in the AEC; we were very careful. You know, when you go to the South Pacific and shoot off a hydrogen bomb, you're asking for it. No problems. I was reading the other day that they're thinking of bringing the natives back and taking them off of some of the islands. Well, here's an example of the kind of problem we run into by making foolish regulations. Those natives live off of coconuts and fish. Fish get their food from the ocean.

TERRALL: That's where all the radioactivity is going?

LIBBY: No, the water doesn't have it. It's long since gone away, due to the ocean currents. So the fish are okay. Strontium 90 is not picked up by the coconuts. Now, you do have blowing dust, but you know very well the amount of dust you're going to get is minimal. But I read that someone in the federal government is saying we ought to take them off again; it's marginal. Well, it's marginal when you take limits like 10 percent of the L.A. natural dose rate as the limit. On what basis? We've proven you can double it. I don't think the Coloradans look particularly sick.

The other day, I was in a meeting with a very famous MD who is an expert on these kinds of effects, and we had a seminar on L.A. smog, which, of course, we all hate. At the end of it, I asked him, "Which is worse, the L.A. smog or cigarettes?" And he said, "Cigarettes, infinitely." Now, he doesn't really know, because the measures are very, very iffy. But that was his opinion. Smog is not a serious health hazard. I know from my experience on the [California] Air Resources Board, we never could get any reputable MD to testify that it was, and one of the members of the board was an MD. Damn right it's annoying. So I think the natives could be left there, probably. It's a question of the spreading of a very tiny amount of matter.

TERRALL: I read something about the Geneva conference in August 1955 [the International Conference on Peaceful

Uses of Atomic Energy].

LIBBY: Yes. It was part of our job to set up an international organization. And this we did, in concert with about sixty countries, originally. It's now well over a hundred in the International Atomic Energy Agency in Vienna.

TERRALL: And this conference was the beginning of that?

LIBBY: Yes.

TERRALL: The conference was done through the UN, though, wasn't it?

LIBBY: No. Through the International Atomic Energy Agency. The IAEA has a separate budget, a separate governing board; it's separate from the UN. It cooperates with them and the other agencies, but it is not a UN agency. It was set up mainly to protect against diversion of plutonium. In other words, there's nothing new about this problem. And I think they've done a fine job. In addition, they've done isotopes on a scale which makes me feel very happy. And the educational job they've done in all these different countries . . .

TERRALL: Have you stayed in contact with that organization?

LIBBY: Yes. I had an invitation in May to go give a lecture in Vienna to them. And I may go later this year. But the U.S. kind of took the leading role in establishing that agency, pursuant to the president's speech. See, environmental matters like worldwide fallout are not for just one country; it's everybody.

TERRALL: Did you think the conference on peaceful uses of atomic energy was useful?

LIBBY: Oh, yes, very. The most important thing we accomplished at that meeting in '55 was the declassification of atomic power. Of course, establishing the agency itself was not minor, but the specific thing which was most important was the disclosure of the detailed design of atomic power reactors.

TERRALL: And that was really something which the United States . . .

LIBBY: It was presented there in Vienna.

TERRALL: By the U.S.?

LIBBY: By all the countries.

TERRALL: Other countries had started working on it already?

LIBBY: Yes. England and Russia and France.

TERRALL: What was the atmosphere at the meeting? Was it open?

LIBBY: Very cordial; very cordial.

TERRALL: Was that equally on the part of all the different countries?

LIBBY: Yes, I would say so. It's always been that way. The agency has been very fortunate; it doesn't tend to breed squabbles or anything. They can do things which no other entity can do. Like in our isotope studies, we could get into the central part of most countries, because agency personnel are welcome, whereas we might not be. I have great hopes for the agency to pull us through. What's discouraging me now is the way we're losing foreign trade to other countries. We were in England in May, and a month afterwards, the Japs gave a billion-dollar order to the English which we should have had.

TERRALL: For airplanes?

LIBBY: For reprocessing of atomic fuel. Japan is essentially 100 percent atomic. It's got to be. England also has got to be, though it may not be as necessary because of their North Sea oil. In other words, coal is really not viable; it's dangerous and dirty. But there's no coal even in Japan, and no gas or oil. So their choice is to import oil or use atomic power, and they do atomic power. Well, because of our president's position, we lost a billion dollars in foreign exchange. Now, that isn't good, and we're losing other things. We had the top lead in atomic technology in the whole world, and we're losing it. The breeder reactor, for example--France is way ahead. I think the Russians are, too. They haven't been as bragging about it as the French, but I think they are. The French want business, and so they do some sales work on their breeder. The British have a much better breeder program than we have. Now it's this falling back from the period of '59 and '60, when we were leading, to where we are now that burns me up. And a large part of it has been the diffusion of power. Carter can't do the energy, and the Congress can't do the energy--the result is nobody does it. I don't know what it's going to take. But we sure have a vacuum of leadership. It's not only in atomic matters, but right across the board.

TAPE NUMBER: IV, SIDE ONE

SEPTEMBER 11, 1978

TERRALL: Last time, when we were finishing up, we were talking about the Geneva conference on peaceful uses of atomic energy in August 1955. I saw in my research several news stories about Hermann Muller's problems with delivering his paper, and I was wondering what you recalled of that whole incident.

LIBBY: I don't recall anything about that. We were concerned with declassifying atomic energy and getting the International Agency [for Atomic Energy] launched. This was the first activity it had ever had. There were a lot of publicity seekers on both sides.

TERRALL: You don't remember the decision to keep Muller from giving his paper?

LIBBY: Doesn't matter at all.

TERRALL: Well, at the time it made a big splash.

LIBBY: You could trust the papers to play up the minor point. That was a great story, the declassification of atomic energy. Some of them did put it out. We got pretty good press.

TERRALL: Well, there was a cover story in <u>Time</u>.

LIBBY: Yes, that's right.

TERRALL: That was quite detailed.

LIBBY: Quite good coverage in the press. But I don't remember Dr. Muller's incident at all. I don't remember it at all.

TERRALL: Well, he maintained that he was kept from giving a paper that he was invited to prepare.

LIBBY: Well, he was a cranky old bastard.

TERRALL: Was he?

LIBBY: He was about half as good as he thought--which made him pretty good. [laughter]

TERRALL: In his own work you mean?

LIBBY: Drosophila flies with X rays. But he applied that to people the way Pauling did, with the greatest abandon, without scientific justification--none whatsoever--and caused us all kinds of trouble. He said, "Better stop atomic energy than go forward." The woods are still full of these people. We've got to beat them down. We're going to win, but it's a tough fight. People like that are less than helpful. It's somewhat similar to the religious wars, where, without basis, people would say this and that and the other thing about certain religious beliefs. But we try to be scientists, and we try to be factual, and we don't mind telling people occasionally that that's the situation.

TERRALL: Then in '57 the Joint Committee [on Atomic Energy] had a long series of hearings on the fallout hazards, and I was wondering if you could just recall the atmosphere of those hearings, and the way they were conducted, and so on.

LIBBY: We had no problems there. It was obvious that we were the only people who were doing anything. The bellyachers weren't doing anything at all. They had no data.

TERRALL: What about the committee members who were doing the questioning?

LIBBY: They were largely friendly to us. But they felt obliged to--the press coverage was enormous. Every time Pauling would belch, he would get a front page. And we were out to show how false this whole thing was. I think we succeeded on that particular point. Now, Pauling, however, won in the sense of stopping testing, but to the scientists of the world, I think we won. People are incredibly gullible and will believe anything which is told them with proper emphasis. Hitler showed that beautifully. So you have constantly to fight the battle with the liars, and there are some very good liars who are very expert at it--all the way from confidence people, right through people who claim they know things which they don't know.

TERRALL: In the course of the hearings, did a lot of this antagonism come out?

LIBBY: No. No. What they did was hear the AEC, and then they'd hear everybody else, and it would all go on the record. So then you could read the record and decide who was right. But we had the only data. That program of measurement had been set up in '53 at the Rand Corporation, as I described to you.

TERRALL: Project Sunshine.

LIBBY: We followed right straight through on that. As a matter of fact, it's still being pursued. It's the most colossal piece of radiation research ever done, and instead of banging us, they ought to thank us. It's fantastic. And it's international. We used the agency to get other countries. England did a heck of a lot of work, and we ran all kinds of tests on people, as well as foods, and spent millions at it. So we could not be accused of neglecting our duty. But as we saw the results, it was worth about raising your altitude six inches.

TERRALL: That's what you figured out?

LIBBY: That was the equivalent. That's what Pauling was bellyaching about, and Muller, and the whole rest of the tribe. That's why I call them liars. They're just plain liars. They know better than that. If somebody who wasn't a scientist said that, I could forgive him, but not Pauling and not Muller. They knew better. So it was a fairly bitter matter. I think I kept my tongue.

It was a hard thing to do sometimes. I think we had the joint committee a hundred percent. They're reasonable people; they're not scientists, but they're reasonable people.

TERRALL: Did the hearings lead to any visible effect on AEC policy?

LIBBY: No. Maybe we worked a bit harder on our research on fallout.

TERRALL: So what was the point of having that long series of hearings, then?

LIBBY: Publicity.

TERRALL: It didn't change the relationship between the joint committee and the AEC, or anything like that?

LIBBY: No. Now, when Kennedy became president and after I left the AEC, things were a bit different, because Kennedy never knew anything about atomic energy. He never was at all interested, and he didn't know anything. So he was very gullible. He was like Brown, our governor--extremely gullible, totally ignorant. He'll believe a damn lawyer who calls himself an expert. So that's where the test ban came from--Kennedy's gullibility. Actually, it turns out the underground testing is pretty nearly as good as the atmospheric, so it wasn't all that much of a hindrance. But I think people who are against new things are the majority of people; practically nobody is for new things. That's our problem. I don't care what your field is, or what your philosophy is, or what your argument is.

TERRALL: There was one more thing around that period that got a lot of publicity, which was when Dr. [Albert] Schweitzer made a statement.

LIBBY: He was another gullible one. Here he was using his great fame to pronounce on something he knew nothing about. I wrote him a letter--it was a fairly famous letter--putting down the arguments. I doubt he was able to understand one of them.

TERRALL: Did you get any response from him?

LIBBY: No. Of course, it was a public letter. We were using it as a device, and I didn't expect him to respond.

TERRALL: But you never heard anything more from him? Did he remain vocal about the issue?

LIBBY: No. Not much. So, you see, everybody who has an established position--he had an established position--is bound to be against progress. It's my firm conclusion that's true.

TERRALL: What do you think moved him to speak out on this?

LIBBY: Just natural contrariness. A good chance for a headline.

TERRALL: Did your response to his statement get a lot of publicity?

LIBBY: We got a fairly good play. It was the only defense we had. In the forthcoming gubernatorial campaign, we're going to have the same old story over and over again. We're going to maintain that Brown has been a very serious deterrent to atomic power, so we're going to keep hitting on that, also to new industries coming in here, because of his rigid insistence on legalistic and environmental requirements that really make little sense. These two things we're going to hit hard. In other areas, like education, I don't think there's much controversy of any real issue. What do you think? I can't see much difference between Brown and Reagan, frankly, on the attitude towards universities. Now, on the busing issue, that's another matter, but that isn't so much education as sociology.

TERRALL: To go back to '59, when you got your appointment at UCLA, how did that come about?

LIBBY: Well, I was fifty years old in December '58, and I decided to leave the AEC about then and return to teaching and research. I had five offers from which to choose.

TERRALL: Including going back to Chicago?

LIBBY: Including going back to Chicago. I had the Ross Chair at Purdue, which is one of their most distinguished; the chairmanship of chemistry at Stanford, La Jolla, and

UCLA. My then wife wanted to live in Los Angeles, so that's why I moved here.

TERRALL: That was the deciding factor?

LIBBY: There were other matters, but actually, as far as doing my thing, I would have been better off to go right back to Chicago, where I knew all the ropes.

TERRALL: And you were already set up there. How did UCLA differ professionally from Chicago?

LIBBY: Oh, my gosh--four orders of magnitude. I was the first senior professor appointed from outside in chemistry in the history of the institution. All the rest of them had been put in as junior faculty and grew up here. I was the first one to be brought in as a full professor. And that gives you some idea of what the primitive nature of things was. But that was kind of challenging. See, at Stanford, I would have had millions to go ahead and build a great department. Of course, I'd have had to help raise millions, too. And I darn near took that because it was very challenging. I liked the president of Stanford [J.E. Wallace Sterling]. Very personable cuss, and I liked the way he did business. And at Purdue we had good reasons, though Lafayette [Indiana] is kind of isolated. And there were some others--there was the presidency of Rice University in Houston [Texas], which wasn't formally offered, but I was approached about it.

And then a number of business offers which I didn't even consider, because I wanted to be--and the chairmanship of the AEC, to stay on. President Eisenhower reappointed me for another term. But I went to see him about it, and I had to explain to him. As I walked in--it was my fiftieth birthday--the president stood up and shook my hand and he said, "Happy birthday, Doc." We had a fifteen-minute appointment and we talked for forty-five. He had a whole line of people waiting outside. [laughter] He told me what he was doing on his fiftieth birthday. He was a major. He was the only full general I've ever met--well, there are two I've met, Bradley and Eisenhower. And he'd gone in fifteen years from a major to this fantastic position. Well, we worked together for five years, and I was very fond of him, and he knew that. I remember on one occasion I had voted against a certain move--and he followed the votes of the commission. Since I was a minority of one on this vote, he called me down to the White House and said, "Now, Doctor, you have a special commission. You are going to carry out this action which the commission has approved, and I approve simply because you voted against it."

TERRALL: What was the issue?

LIBBY: Giving the British the hydrogen bomb. I was against it. I guess the Russians had it already, and it didn't really matter all that much, but . . .

TERRALL: So Eisenhower was telling you that. . . ?

LIBBY: He wanted them to have it. And the majority of the commission wanted them to have it. So I was put in charge of doing it. I had a big entourage, and we all went to London; it went on for months and years--very quixotic, very interesting. [laughter]

But I went into see him and told him that I'd better get back to teaching or else I'd never get back to it. He said yes, he understood.

TERRALL: So you really chose UCLA on the basis of geography more than anything else?

LIBBY: My wife chose it.

TERRALL: In light of the fact that there was very little chemistry going on here, how did you like it?

LIBBY: There were some chemists here who were pretty good, and the chemistry department was the best department in the university. But it was nothing like what it is now.

TERRALL: How did that affect setting up your lab?

LIBBY: I took a challenge there, and it was rather fun to help build it. There were some good people here who weren't all that famous--that's the point. They hadn't had time to develop a position.

TERRALL: But did you then bring new people into the department?

LIBBY: I certainly did, and I brought lots of money. We didn't lack for either money or space.

TERRALL: So it didn't turn out to be a hindrance in any way.

LIBBY: It made me a manager more than I would have wanted to be. See, I was head of the space program, and I was director of the Institute of Geophysics and Planetary Physics for ten or twelve years, which is a fairly big hunk of management. But I'd had five years being a top manager, so it didn't really bother me all that much. The way to manage is to get somebody else to do it for you--just be sure to pick the somebody else who can do it. But it does mean you have to keep your eye on the ball.

And we had some other people coming here. Lynn White came here about the same time in history, and [Gustave] von Grunebaum in Middle Eastern studies, and things began to happen. Then [Franklin] Murphy came. Murphy was a very intelligent chancellor. He was indeed. Though we didn't always agree, we worked together very well. I had special opportunities, because the regents had made my chair a special chair. For example, I had direct access to the regents without going through the president of the university. I think that's the only time that's ever been given a professor. I didn't ask for it, but they gave it to me. On the other hand, I didn't ask for the university-wide appointment, because it seemed to me that meant nothing. But this regental thing was important. So as a result, I talked with a lot of the regents during our development work here, and they were very, very helpful. I became a close friend of Ed Pauley. I'll tell you one example. While I was on the AEC, I was working with the regents, because the Regents of the University of California are one of the biggest contractors the AEC has, if not the largest. And being a Berkeley graduate and so on, I knew all these characters, most of them. So I'd get involved in the negotiations when the general manager got stuck on something. The way the AEC worked was to let the general manager and the contractors do everything they could without getting into trouble, and our job was to watch them. We never had any serious problems with the regents, and that's still true. That's what, '42 to '78--thirty-six years. But there was lots of negotiating about fees and matters of clearances and ways of operating. As long as Ernest Lawrence was alive, we didn't have much of a problem, but he died in '57 before I came back. After Ernest died, we got Ed McMillan as director, and Ed, bless his heart, is not quite the man of the world that Ernest Lawrence was. Ernest would come in and talk with us, and we would make a deal and say, "That's it." Then the clerks would go out and work for six months drawing up the contract. But the deal was already made. That's the way we worked. Now, to do that here, with a new hat on--a UCLA hat--I became head of what is known as the space program at UCLA, in that my institute [Institute of Geophysics and Planetary Physics] was the main shelter for the whole space program.

TERRALL: Was that a new institute when you became director of it?

LIBBY: No, it had been going since '47, but the former director was retired, Louis Slichter, and the faculty picked me to succeed Louis. Louis went on--he died just last year--doing research into his eighties, but the university retires administrators in their mid-sixties, or did.

TERRALL: So you were administering that, as well as . . .

LIBBY: Right. But this leads up to the incident. The space program had started with Keith Glennan as director, who was a former AEC commissioner and whom I had known very well. Keith Glennan was the first administrator of NASA. And I talked with him, and we used to discuss various problems while I was still on the AEC, and we'd talk about what was going on. And then when he retired, Jim Webb became NASA administrator. Now, Jim was a very good friend of mine. We had worked together on trying to develop education in Oklahoma, and I'd helped him one hell of a lot. And he was very grateful.

TERRALL: How did you happen to be working in Oklahoma?

LIBBY: Well, I was commissioner of the AEC, and that covers the whole country. We were very interested in developing graduate education. The only way you could do our job was to get more students educated at high levels. So everything was set, when I came here, to do something for the Los Angeles area in the space program. So I set out to where what now is the Houston Manned Space Flight Center would come here. That was my project. I went directly to the regents for this. Murphy knew about it and wasn't in any way roadblocking it. The regents approved, and they picked Ed Pauley to be the head delegate from the regents. So Ed and I went to Washington to see President Kennedy, and we did see him, and he was very favorable. We were going to plunk it down where that Federal Building is right now, on Wilshire.

TERRALL: In Westwood.

LIBBY: Right.

TERRALL: This would be moving it entirely?

LIBBY: No, it wasn't built, you see. The Johnson Space Center was not yet built. They were going to put it here instead of there. So Ed and I went to Washington and called on the president, and the president understood and he said, "Just fine." But then politics got involved. There was a little member of the [city] council, a pregnant woman named Rosalind Wyman, who decided that it would be better if the federal government traded that property on Wilshire for some other property, so she could have a park someplace down in hinterland. That, plus the fact that Lyndon Johnson went to work for Texas, led to the defeat of it. Now, I think the Houston siting of the space center wasn't all that bad, frankly. But they don't have the intellectual resources there that we have in Southern California, or the engineering. Do you realize that the aerospace industries are going right through the roof in airplane orders? There's a shortage of engineers.

So we set out that the space program would be our central thrust in the work here. We got a building from NASA, Slichter Hall, and we got millions in allocations of funds. We didn't lack for space or money. And I produced, during my twenty years at UCLA--I haven't counted them, but something like twenty PhDs, maybe twenty-five, maybe more. I had a large gang with me.

TERRALL: So during that period when you were in the Institute of Geophysics, you were really working a lot on space research?

LIBBY: Yes, but mainly in chemistry, as chemistry professor. My teaching was in chemistry; my administration was over there at the institute.

TERRALL: What were some of the projects that the NASA contract covered?

LIBBY: Well, anything we want to do was covered.

TERRALL: What kind of things did you do?

LIBBY: That's a long, long story. Let's see if I can pick out some of the particular things. The first thing was to hire people. About three-quarters of the staff of the present institute, I hired. And I think they're pretty good. We've got a couple of members of the National Academy [of Sciences] in the last couple of years, and I think we'll get a couple more in the next couple of years.

But the main thing we did, I would say, was to bring some chemistry into the space program, because nobody else was trying that. The Berkeley chemists never got into the space program, never even looked at it. Harvard the same--they didn't look at it. Those are the two best chemistry departments in the country. But we did. Now, we weren't the best chemistry department, but we had space problems in mind. And I take some pleasure, in looking back on our reports, that this was true, and we have every reason to suppose that this will pay off with the space shuttle now. The work we were doing was specifically aimed at something like the space shuttle or a moon lab.

TERRALL: So what kind of problems were you looking at?

LIBBY: Well, you're familiar, perhaps, with the great achievements of the space program in communications, and in navigation, and the surveillance of earth resources. All of these are great achievements of the space program. Now, in the chemistry area, what we hope and think is we can build a lab on the moon, if we do the chemistry right. Now, a lab on the moon would be the greatest step forward mankind has ever seen in astronomy and in space. So we aimed everything towards that. You have to figure how to make your supplies, how to recycle. One thing you can't really do is to make water up there. You've got to have it, and it's too expensive to haul it. This is still continuing, and one of my old associates and friends, Jim Arnold at La Jolla, is head of a massive effort to look for ice caps on the moon, at the poles. In spite of all the flights we've made to the moon, we never went to the poles. We think, from chemical reasoning, that there are ice caps on the moon.

TERRALL: And that would be the source of water?

LIBBY: Yes. You'd build your base right next to them and melt the stuff and recycle it. You wouldn't let it escape. Then you could grow vegetables, and you're off. This will all probably happen, and I think that's probably the most important thing we did. Many other things were done. [Paul] Coleman and his people, essentially Coleman and [Robert] Holzer, mapped out the magnetic field for the whole solar system, including the earth--fantastic piece of work. I haven't seen much come out of it, frankly. But very interesting. And it has some implications for plasma physics which is very important to the CTR [Controlled Thermonuclear Reactor] program.

TERRALL: Did you look at problems of the environments on other planets at all?

LIBBY: We studied a lot on Venus. That was more or less my personal work with my own students.

TERRALL: What kind of data did you have?

LIBBY: Russian data. The Russians have done an excellent job on Venus. One of our problems with the Russians is we never give them credit when they've really earned it. They showed that Venus has a very hot atmosphere consisting of carbon dioxide, in the main, and our group worked very hard to see about what could have happened to the water. And we think that there's a good chance of warm polar seas on Venus. Again, we want a polar orbiter to see whether the seas are there. There would be abundant life in those polar regions which are now even hotter than our equatorial region. It's largely speculation, except we did some experiments on the way plants grow in a hot CO_2 atmosphere, and it's pretty interesting. They grow very well, indeed. I had two postdocs on this program [Drs. Irene Aegerter and Hans Sechbach]; one of them [Sechbach] was Israeli, and he went back to Israel and is doing hydroponics and desert culture. He's made quite a bit of progress by raising the CO_2 content of his greenhouse. Other people had suggested this, but they hadn't really done much about it. So we think Venus--there's more to it than has been heard yet. But the people in NASA are not very chemically minded; they mainly want to do problems which are in the electrical engineering area.

Now, in addition to my work as head of the institute and head of the space program, I, of course, carried on my radiocarbon dating work all the time.

TERRALL: You were analyzing samples?

LIBBY: Making dates.

TERRALL: Did you have a lot of people working on that in your lab?

LIBBY: Yes. I had a separate lab for that, still do. It's not mixed up in the chemistry. We have to be very careful about contamination and keeping the place clean.

TERRALL: Is that something that is also done elsewhere?

LIBBY: Yes, there are about a hundred laboratories scattered over the world. We have made hundreds of thousands of dates now. The dates fill twenty volumes; very cryptically described, they still fill twenty volumes. So going with that is the pursuit of the geophysical applications of radiocarbon. See, radiocarbon is not just restricted to dating archaeology. For example, we date when the climates changed. We can tell when they changed, and that's very important geophysically. We did a lot of that work. And we carried on our tritium work. Tritium is a radioisotope of hydrogen and allows you to measure water. It has a twelve-year half-life, and it, like carbon 14, is made by the cosmic rays, so the rain has it in it.

TERRALL: So what do you use that for?

LIBBY: Dating water.

TERRALL: How long did you say it was, twelve years, the half-life? So you can't do much with the oceans.

LIBBY: It's very good on wine.

TERRALL: If you have a wine you don't know the date of?

LIBBY: Or if you suspect that there's a shipment of liquor which is fraudulent, you have a way of checking. But it's useful in groundwater work.

TERRALL: In drainage problems?

LIBBY: No. See, the water in the ground, which you pump into wells, is delivered there by some kind of underground flow pattern. And this is very useful in telling what the pattern is. For example, the great wells in the San Joaquin Valley are coming from the Sierra Nevada snowpack, 150 miles away. In the Middle East, we did a lot of work, particularly through the international agency. After I left the commission, we developed a lot of applications of the isotopes to field geophysics, which were done by the International Atomic Energy Agency. And tritium was one of the leaders; carbon 14 was very important, too. The world's water problems are horrendous, and we're essentially ignorant of the underground flow and what the reserves are. A lot of the cities in the Midwest are living off of melted glacier water ten thousand years old. When it's gone, it's gone. Tell them a few unpleasant facts like that--or conclusions; they may not be facts, but that's what the data indicates.

TERRALL: Because the source isn't being replenished, you mean?

LIBBY: That's right. Of course, with all the runoff water, they probably wouldn't have too much problem in replacing it with rain water. But the city of Urbana [Illinois], for example--Champaign-Urbana--uses fossil water. Most of the stuff in Nebraska and those regions is fossil water. Well, we learned all this from my tritium work.

TERRALL: But you can't date anything that old with something that has such a short half-life, can you?

LIBBY: No, but the point is [by] knowing there's no tritium in the water, then we know it's not rain water.

What else could it be? It's got to be something. See, tritium will only go back to forty or fifty years. And we did a tremendous amount of work with the L.A. Water and Power Department on their problems here in the L.A. basin, where they store Colorado River water by pumping it into the ground. I can't say that any great breakthroughs came of that, though it gave them a solid base for operating. I've always believed in having the university serve the community whenever it's appropriate. Our present program on environmental science and engineering was for that specific purpose. We now have sixty-five graduate students and thirty graduates in the field working, and this, if I do say so, is one of the more successful graduate programs at UCLA. I'm working now to get other universities to adopt it and put it in.

TERRALL: Is that part of a department?

LIBBY: No, it's an interdepartmental degree run by a committee between six or eight departments. But that came out of this desire of mine to be sure the university is useful. So we went through the experience with Mr. Pauley of trying to get the Johnson Space Center here, and failed, but we did succeed in a couple of things. We succeeded in putting chemistry into the space effort. That's going to pay off. And I think with our environmental science and engineering program, we've got a real contribution there. But perhaps the best thing I did was to bring in the faculty members of which there were about twenty brought in.

TERRALL: Into the chemistry department?

LIBBY: Into the whole university. See, the institute has a number of departments with which it's associated. Like Bill Schopf the geologist, was one of them, and he's our brightest light in the earth sciences, by orders of magnitude. You ought to read an article he wrote in this month's Scientific American. The whole issue is devoted to evolution.

TERRALL: I haven't seen it.

LIBBY: You'd better read it. I think it's better than any book you can buy on evolution. I asked Bill the other day, and he said, "I agree with you. It's more up to date, more understandable." We have a long way to go to develop a major university, but we have made enormous strides in twenty years. I'm proud to have played a part in developing it. I see some evidences now that we need some new life injected. Nothing really new has come out of UCLA for five years. La Jolla, on the other hand, is just buzzing.

TAPE NUMBER: IV, SIDE TWO

SEPTEMBER 11, 1978

TERRALL: I wanted to go back to 1960 when you got the Nobel Prize. I think we should talk about that a little bit, although I know you probably have answered questions about that a million times. Was it a surprise to you when you got it?

LIBBY: Yes, I would say so. I had a complicated--see, I knew these reporters, because I'd been in Washington for five years and worked with them very closely. I noticed early in 1960--I'd been out of the commission since the middle of 1959, but I'd still go back to Washington every two or three weeks on some excuse or other--and they started asking questions which I didn't see fitted.

TERRALL: Science reporters?

LIBBY: Not all science reporters. And particularly from the Swedish newspapers.

TERRALL: So they knew something was up?

LIBBY: Yes. But nobody's ever certain about that until the last minute. So we got a call about 3:30 in the morning from Stockholm. That was the first we really knew; the telephone call was from Stockholm. See, it was the difference in time--anyhow it was about quarter to four in the morning. It was quite an experience. I had no real idea of the magnitude of the carbon dating discovery at that time. I knew it had great promise. But their citation and their research on it brought it out. The most lucid and cryptic and pertinent description of the method is this Nobel citation from my award. They really worked on it. It's better than anything I ever wrote on it--how it works, and why, and so on. They got into the nitty-gritty. They were quite aware of my position in politics because I'd had a lot to do with Sweden in connection with the international agency. And one of my friends who's still the director-general of the international agency, [Arne] Eklund, a Swede--I knew him then. But the Nobel people are quite separate from the political people in Sweden, and they really make the decision. Nobody else makes it. So I had a kind of hint, an indication. There was a man named Hans Patterson. He told me, he said, "I'm working for you." He's a very distinguished oceanographer in Sweden. He said, "I think we have a chance." But he didn't know any more than that. As I said the other day, I've nominated a number of people, but you never know what's going to happen.

TERRALL: Because they must get many more nominations than they can do anything with.

LIBBY: Oh, sure. They're quite rigid about their procedures. It doesn't matter whether you've nominated someone before, you must renominate them every year. It makes it very laborious. [tape recorder turned off]

Franklin Murphy and I came more or less at the same time, and we worked together very well. He brought in people like Lynn White and von Grunebaum, and very distinguished people in a wide variety of fields. So we worked together. And UCLA went just like that. [gestures upwards] What's bothering me a little bit now is that they need another thrust. Von Grunebaum is dead, Lynn White is retired, and I'm retired, and Murphy's gone. But every university has that problem. You continually must inject new life into it.

TERRALL: After you got the Nobel Prize, how did that affect your professional life? In terms of demands on your time . . .

LIBBY: It made things a lot easier in many respects. But, you see, before I got the Nobel Prize, I'd been given this distinguished professorship and offered four others. It made certain things easier. But it didn't have the impact that I imagine it has on most people. I was pretty well known by the time that happened.

TERRALL: But what about in terms of being asked to do speaking engagements, interviews . . .

LIBBY: Ma'am, I had so many invitations before, I couldn't do it. I was looking at my speech file the other day while I was AEC commissioner; I think it's twenty file cabinets. I was looking at the written articles, and that's a whole file cabinet, pretty closely packed stuff. Of course, much of it is dated now, because the issues in atomic energy have moved. But one of the reasons I left Washington was this pressure for speeches was absolutely enormous. And there's no relief from the administrative responsibilities. I had eight full-time assistants to just handle the day-to-day work. I remember one time I gave a speech on patent law before the American Bar [Association]. If there's something I know little about, it's patent law. It was written for me by one of my assistants, but for political reasons I had to deliver it. Well, that's not very nice. Here I am a chemist, doing what I was cussing Pauling for doing. But there was no other commissioner available to do it, and we felt the AEC should because we'd been talking with the American Bar Association about a number of legal problems in connection with our work and they'd been very accomodating. So you get into binds like that. Then you have all these cocktail parties and social occasions in Washington, and somebody has to go. Now, there were five of us most of the time, but somebody has to go. Every night of the week, it seems, there was something. A lot of business is done at those meetings, particularly in the foreign area. See, they may not want to have it known to the press they talked to an AEC commissioner at all. They can do it this way. If you both get invited to, say, an Iranian Embassy reception, no reporter will know you talked. A lot of that stuff.

I was very glad to get back to being a professor. It took me a long time to catch up with the chemistry literature, though. It took me about three years of real hard work. But if I'd gone to Stanford, I would have been similarly burdened with administration, because I would have had to build a department and a building and raise money. I think either Chicago or UCLA. Lafayette [Indiana] is a little bit out of the circulation. But I'm happy I came here. I think we've done a few things. Now, my friends at La Jolla have gone to an exceeding level of excellence in science, both physical and biological, but they've done a miserable job in humanities. I wouldn't even call it a university, frankly--maybe at an undergraduate level, but not the graduate level. If I were [David] Saxon, I would think about just saying to La Jolla, "Now, look, you be the Caltech of the UC system and quit trying to be a university." It's funny that it hasn't worked. It seems odd. Maybe I'm wrong, but I sat for twenty years on the committee for selection of

Guggenheim fellows. It's a pretty good way to rate a university, because it's in all fields. Berkeley was always number one in the country. Harvard was number two. And we got UCLA up to about fourth or fifth, but La Jolla would rank extremely high in the sciences and it would be way down on the overall, because of the weakness in the humanities and social sciences.

TERRALL: You had mentioned the other day that you taught an undergraduate--was it a freshman honors course?

LIBBY: Freshman honors, yes.

TERRALL: Was that something you had done at Chicago also?

LIBBY: No, it was something I started when I came here. Well, others had started it, but I took it over. The faculty told me, "Essentially anything you want to do," and I said, "I want to teach that course." And so I did that, developed that. Others helped, and now others are carrying it on since I've retired.

TERRALL: But you taught that course over a period of years.

LIBBY: Out of choice. I taught it for ten years--longer, I guess, actually. The students were exceedingly able and performed very well indeed.

TERRALL: How big is the class?

LIBBY: We would aim for about fifty. Sometimes it would get out of hand; last time I taught it, it was seventy-five, and that was really too much, because it's almost necessary in that class that you know every student. It's straining when you get to fifty. But that was very productive, as far as I can see. The standard graduate course is, in my opinion, pretty nonproductive, and I'd rather have the students doing research with me and really get at it. At Berkeley, where I grew up, we had practically no graduate courses except research--that was the graduate course. I think that's the way it should be. Now, if you are going to have a big master's [program], you've got to do something for them. But Berkeley never had a big master's, and we've never had a big master's in chemistry here. But a master's can't be expected to do very much research, so they almost have to have courses to take.

TERRALL: You also mentioned fund raising that you've done for UCLA. What were the specific projects that you worked on?

LIBBY: Well, I got two million a year out of NASA, unrestricted, which we would pass around. We had a committee of which I was chairman.

TERRALL: And that was all done through the Institute for Geophysics?

LIBBY: We used the institute, but the committee was really the authority. We would meet in the institute, and I was chairman and director of the institute. Chauncey Starr, the dean of engineering, was on that thing.

TERRALL: So you had to hand out the money to the different researchers.

LIBBY: We had the privilege of doing that, right. It was absolutely unrestricted. I remember one time, talking to a friend of mine in the music department, John Vincent, who was a fine composer. He said, "You know, you've got me. I don't see exactly how money could help us." I said, "Well, now, use your head. Think about ways in which modern electronics can help you guys." Well, of course, it wasn't five years later until electronic music was in. But it didn't take with him.

We would hold conferences. I remember I helped pay the way of a certain gentleman who went to France to get some meteorites. Meteorites are very important raw materials for space research, so I paid part of his way. Now, we did collect funds from other sources. The Department of Defense was a heavy supporter of all kinds of research. My personal research was supported by the Air Force all that time. I never took money from the Atomic Energy Commission, because I thought it was inappropriate.

TERRALL: You mentioned setting up the Laboratory of Nuclear Medicine [and Radiation Biology]; that was funded by the AEC, wasn't it?

LIBBY: Right.

TERRALL: That was after you had already been here?

LIBBY: No. I gave it to them before I came.

TERRALL: Oh, okay. How did it happen to end up at UCLA?

LIBBY: Because I put it there.

TERRALL: That was before you had decided to come here, though?

LIBBY: Right.

TERRALL: So what was that based on?

LIBBY: Well, there were quite a few people who recommended it. We had a very large health physics [program] at both the Los Alamos [Scientific Laboratory] and the Argonne [National Laboratory], and something at Berkeley, and [Lawrence] Livermore [Laboratory] had a little bit, but I couldn't see that the heart of the matter was being properly pursued. It seemed to me we had to have a laboratory which was devoted to that. That's how it came. See, the Los Alamos effort is subsidiary to the weapons program, and so on.

TERRALL: Were there other such labs set up in other universities?

LIBBY: No. This is the only one. There are certain funded projects in other universities which you might say do work similar to what nuclear medicine does, but nuclear medicine is the largest in the country--I think in the world. And I think it's been successful. I must say that I haven't seen the brilliance of performance that I hoped, but it may be like much of biological research--it's essentially impossible in the first place. So maybe we were hoping for too much.

TERRALL: You mean the problems are so difficult?

LIBBY: So difficult, yes. They've been doing good things. They've got a new cyclotron down there which does short-lived isotopes. I read with great interest the work of Bill Oldendorf, in the medical school, on three-dimensional gamma ray work, and others, where by being very fancy about measuring the intensities at various angles, they can locate cancers of the brain. That doesn't mean they can cure them, but they can find out where they are.

See, UCLA never got into the atomic energy business. It was just too young and immature to play any role; neither did La Jolla. Caltech didn't either. In fact, none of the universities in the Southland did. It was all Berkeley. Due to the vision of Lawrence and some others, including a few graduate students like myself, we made Berkeley the center in the whole world.

TERRALL: Way back in the thirties.

LIBBY: Before World War II.

TERRALL: Well, you've also been involved in a number of environmental research projects, haven't you?

LIBBY: Yes, I was on the Air Resources Board--it was formerly called the Motor Vehicle Control Board--for four years when we set up the present smog regulations.

TERRALL: When was that?

LIBBY: From 1968 to '72--I think those are the right years.

TERRALL: How did you happen to get onto that?

LIBBY: [Ronald] Reagan appointed me.

TERRALL: So it was through your political contacts?

LIBBY: I assume so. I helped him get elected governor.

TERRALL: What kind of work did that entail?

LIBBY: Well, setting down regulations. The laws you obey now on smog, we made. I'm not so sure we were right.

TERRALL: And you've had second thoughts about those regulations?

LIBBY: Oh, yes. I think last summer was kind of discouraging.

TERRALL: Do you still work with the Air Resources Board?

LIBBY: No. No, Brown has got it filled up with a bunch of lawyers who don't know what the hell the score is. I can't understand this man. You would think he would know--he's been well educated--that you'd better put somebody in there who knows the subject. Of the three guys, only one of them has ever had any science experience at all. Tom Quinn couldn't be more ignorant in science. He says you don't need scientists; well, I don't agree. And they're just wasting money and opportunities. So the whole business of regulation in ignorance is something we have to face. It's just breaking this country. And I'm afraid what we should have done, with the fact that we didn't know enough, is to refrain from some of those regulations. We knew some things. We knew a lot more than anybody else knew, but we didn't know enough.

TERRALL: Is there work going on at UCLA on stuff like that?

LIBBY: Of course there's work going on; you can always find somebody to take a contract, but that doesn't mean anything is going to happen. There are only relatively few people who are able enough to attack--that's a tough problem. The nub of the problem is where the damn haze comes from. And nobody knows. I think what I'd do if I were now on the ARB, if I had a proper governor behind me, is start a major, major research. No more rules. We're going to stay with the present ones now, but no more rules until we find out what the score is, because we're not doing much good and it's costing us an absolute miserable fortune. The other day I had some friends in--I'm a director of a little company that manufactures various assorted items, and I had the president and the executive vice-president--and we talked here in this bar for about two hours about the practicality of rescinding some of these smog regulations. You see, when you've got a hundred million automobiles under control, you'd better be careful about changing anything. The costs and the effects can be absolutely enormous for the silliest little change. I remember we put in exhaust gas recycling. I don't know how much you know about automobiles, but you take the exhaust gas and put it back into the engine. That's called exhaust gas recycling. You'd think that would be pretty easy to do--horrendous. Then we had spark retard--horrendous. Every change we made was absolutely fantastic. So what I would do now, and suggest very strongly, is that we really get at it and understand the smog and quit trying to regulate in ignorance. I don't think it pays. I don't think cancer has paid. Nixon is a friend of mine--he still is--but that was a goof. He put all that money in and they didn't know what to do with it. Now, you may think that pouring effort and money down in a desirable direction is going to bring something--that isn't true. You've got to know what you're doing. This nation is falling behind technologically because of the attempts of massive attack on sociologically important problems without the necessary knowledge behind it. We've got to reorient our whole thinking about the support of science. To put it another way: we ain't doing science. We used to do science. Not now. We run around doing a lot of attempts at applied projects and getting no place. It's kind of discouraging. But, you see, being a professor, I can talk. If I had continued being a politician, I'd have my difficulties. In the first place, I wouldn't know what I was talking about--now I know what the hell I'm talking about, because I've been working, keeping up with it. See, one of the things that happens there in Washington is that you get isolated from the literature. I saw Scoop Jackson on "Issues and Answers" on Sunday--he's an old friend of mine. We got Scoop pretty well educated on atomic energy. It's an impossible life they lead. They can't keep up on things and do that. I think we might have a system where senators serve just one term, and the president serves just one term--it might be a six-year term. People get out of touch with reality in Washington. They really think everything runs in Washington, and it doesn't, of course, at all. Of course they know that, but they get to thinking that. They have their friends on the Hill and their friends in the administration; they think that's just fine. We're going to blast them out of there. You know, I had to do with the Jarvis amendment. They better be careful. I'll tell you, there are more and more people for a flat ceiling on federal expenditures, come what may--more and more. The thing that's blocking us right now is a national referendum mechanism--we don't have one. We have several states that have it, but nothing federal. You can bet your life those congressmen aren't going to introduce it.

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