Nevada Test Site Oral History Project
University of Nevada, Las Vegas

Interview with Barbara Germain Killian

April 4, 2005
Albuquerque, New Mexico

Interview Conducted By
Mary Palevsky

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Mary Palevsky: *I thought we could start by you telling me something of your background, your full name, place of birth, date of birth, and how your life path got you into the world of science.*

Barbara Germain Killian: I was born November 25, 1935 in San Diego [California]. I was named Chu-Teh, C-H-U-hyphen-capital T-E-H, Barbara Killian. Chu-Teh, it turns out, I went through school with that name. My dad had gone to a Catholic school when he was a young man, a German Catholic school in St. Paul, Minnesota. And [he] had a classmate, who became a Maryknoll missionary and wrote about Chu-Teh, who was one of the individuals on the Long March with Mao and felt he was a very fine man. My mother wanted to name me Suzay and Dad convinced her that Chu-Teh was close enough and I should be named Chu-Teh. So I went through high school with the name Chu-Teh and I would find myself being assigned to a boy's gym class or something like that on occasion because they weren't quite sure what sex Chu-Teh was.

So you were called Chu-Teh.

Yes, and then all my friends—when I went back to the fiftieth high school reunion, all my friends know me as Chu-Teh and they still call me that.

Wow!

And when I graduated from high school and was getting ready for college, that was 1953, things were rather tense with Russia at the time, so Dad told me to have my name changed, which I did,

so I dropped Chu-Teh and went by Barbara Kathryn at that point. I became Barbara Kathryn Killian until I was married to Pat Crowley.

Now let me back up a little bit. So were you raised in the Catholic faith, then? Your dad was a devout Catholic? Or…?

No, he was not devout at all. He was quite the roustabout. As a young man, he would come to visit my mother in Huron, South Dakota by parachuting out of an airplane onto her front lawn. And he always had a sports car or an airplane or something like that. And he just enjoyed doing those sorts of things. He was a welder and later became a tool-and-dye maker. I was an only child.

I was going to ask.

And I think my father had a great deal of influence upon my going into science. I have talked with other girls who had similar experiences, ladies in the technical areas, a number of them that I have known have had strong influences from their father.

And their fathers, how did that play out during, say, your high school career? How would that manifest, that kind of support?

Well, I was the most spoiled person you could possibly imagine, but he had a couple of rules, and one of them was that you never tell me a lie. If you ever tell me a lie, you're out of here. If you ever get in trouble with the police or anything like that, you're out of here. The other one was, you have to be home before I wake up in the morning, which was usually between three or four o'clock in the morning. And you try to let me know where you're going to be, and if you're ever in any trouble, you call me. But there were no restrictions on what I did or anything. But I knew that I had to do that, I knew I had to make my grades in high school, or he would've tossed me out.

Literally tossed you out?

Oh, he would have. There was no question in my mind. And he loved me very, very dearly, but he wanted me to know that there were rules and they were pretty liberal but they were there.

That's so interesting.

And I think it was a great way to grow up. I really do. I was just terrible. Anything I wanted, he would get me, and he could ill afford it, but would really go out of his way to do it. And my mom was the same way. And I loved them both very dearly.

[00:05:00] *Yes, I imagine. Now where did you grow up? You grew up in—?*

Grew up in San Diego. During the Depression, my mom and dad traveled a great deal because jobs were very scarce, and he was a welder, primarily, and body-and-fender type of person, and so he would find jobs here and there and they would travel. When they first came to San Diego, they camped out on the beach because that's what they could afford and lived off of fish and had a little tent. That was the way life was. He got very upset because the Catholic hospital wouldn't take my mother and she had to go to the county hospital, where she got excellent care, but that, I think, kind of drove him even farther away from the Church.

I see.

I was not reared as a strict Catholic, although my parents were both Catholic. They would take me to every church in town. They had the Mormons come to the house. They went to the I Am Church, which you have probably never heard of.

No.

People stand up and holler, `I am! I am!`

Oh, I Am Church. Yes, I have heard of that.

I had friends in the Lutheran Church and I'd go with them, or Presbyterian Church, whatever. And so finally Grandma was putting a little bit of pressure on me to go to the Catholic Church, and so I tried it and I was finally baptized at about the third grade or something. If I wanted to go to church, Dad would take me, or Mom would come along, or sometimes he would even go, but there was never any pressure one way or the other to do that or not to do it.

Interesting.

Yes.

Chu-Teh. So what happens when you get to the decisions about college?

Well, I knew in high school or even grammar school that I was interested basically in science and mathematics. Because they came easy. And so I knew it would be something along those lines. And no one on either side of the family had ever been to college before. And I was thinking about going away. Dad said, Why don't you spend your first year or two at San Diego State and then if you want to go away, I'll do everything I can if you make the grades to get you into someplace. And he died in my sophomore year, and so that wasn't possible.

And that was what year?

Let's see, I graduated from high school in '53. It was '55. Nineteen fifty-five. But I knew that I would be in one of those kinds of areas, and at the time I kind of thought, well, maybe teaching is the thing to do. My second semester at State, or my second year at State, I was told, Well, don't you think you ought to take an education class? And I went to one of them once, no, twice—I think I went to two of them—and decided that I'm not cut out to run a movie projector, if you remember the old-time movie projectors. And so I decided I wasn't going to be a teacher.

That's hilarious.

And I don't regret that at all. I just don't have the patience, I don't have the—I'm just not cut out to be a teacher. I have tried to teach people things, the people who've been working for me and I enjoy that, but on a rigorous, repetitive kind of scale, I couldn't do it.

Yes. So then what happens on your career path in college?

It was a toss-up if I was going to major in physics or mathematics, and I ended up with a major in physics and minor in mathematics and astronomy. At the end of, or the second semester in college, my first math course was kind of a disaster, analytic geometry. I was kind of active in school things, trying to work and a bunch of stuff, so I didn't do very well in this first math class and got a "D" and had to repeat it.

Well, I always wore a big brown alpaca coat, which my dad could ill afford but he got for me. One of the fellows in the class who was also repeating the class, Patrick, **[00:10:00]** wanted to see what was underneath this alpaca coat; took me out, and so we ended up getting married between our junior and senior year, and he was also a physics major.

There were three of us—Pat, myself, and a fellow named Bob Combs, who ended up being the best man at our wedding—who were not veterans. Everyone else was a Korean veteran. Now you come out of high school and high school wasn't all that easy for me. Some people say how easy high school was. Well, it wasn't that easy for me and I did have to work, even in high school. But here were guys, four or five years older that we. They'd been around the circuit a little bit. Many of them had children, many of them had wives working, many of them were working nights at the aircraft factories and things like that. And you come out and know that they really mean business. It was competitive, there's no question about that, but once you made friends with them and once they found out that you were willing to work and could help them and they could help you, you developed some very good relationships.

But it was quite a change from high school. I went essentially inactive in the sorority I had joined. I had to work to some extent, probably twenty hours a week. And I don't regret that at all. It made things kind of tight time-wise but I think I learned a great deal from doing that.

What kind of jobs did you have?

I worked. I had a really nice job as a cashier and gift-wrapper at a place called Dohrman's which was a china-silver shop; then I had a job at the library, the public library where I went after school in grammar school; then later on I got a job as a reader for the astronomy professor, which was very nice.

That must have been a nice job.

Yes. One summer, I worked at Convair Astronautics in San Diego. Pat worked there, too, and another girlfriend. So jobs were available in those days, and two dollars an hour didn't sound like an awful lot, but it went quite a ways in those days.

And they were hectic times. I always had a car, and my mom always sewed my clothes. I remember once I said, `No, I don't want you to sew for me anymore,` and so Daddy took me shopping and I got a skirt. I went to school and someone else had on the same skirt. And it was a girl who didn't have a very good reputation; I came home and said, `Mother, would you please sew for me again?` And she did.

That's a nice story. So you marry in fifty…?

Six. Is that right? Yes, we married between our junior and senior year. And we were both working at Convair that summer, and we wanted to get married before school started because things got pretty hectic then.

And so my mom was back in South Dakota visiting her sister, and I call her up and say, `Pat and I are going to get married.`

And I didn't realize what she had said but she said, Do you have to get married?

And I said, Yes, we have to get married.

I didn't understand what she meant. So she come charging out to San Diego and goes critical. And [I said], No, no, we have to get married before school starts because we want to be settled.

Well, she made the wedding dress and we had a nice little ceremony.

So that's when you move out of your parents' home?

Yes, we got a little apartment near school. And I was working as a reader then for the astronomy professor and Pat continued on with twenty hours a week at Convair Astronautics. So between those two jobs, we were able to pay our tuition and our books and our rent and food.

What kind of work would you be doing at Convair?

It was before computers really took place and we had these calculators called Marchants **[00:15:00]** and just a lot of hand calculations to the things that you can just do with a couple of commands on the computers today. And I remember one of the hottest things alive was when they finally brought out the square root Marchant, or the square root Friden [Friden SRW]. It was a big calculator. To be able to take a square root was just the cat's meow. I mean it probably took, even then, five seconds or so for it to crunch on through. There's just no comparison to what you can do today.

So then you finish at San Diego State with a major in physics, is that correct?

Yes. We started interviewing. People would come to campus to interview and we talked to our professors and we both wanted to go to graduate school but we knew we'd probably have to work for a little while first and so. I remember an interview with Westinghouse, and talking about there not being very many women in the field at the time, and this fellow was talking with me and—

Interviews were typically for a half-an-hour, and fifteen minutes into the interview, I'd asked him a couple of questions which he kind of skirted, and so I finally said, you know, What would I as a person fresh out of school be doing in Westinghouse?

And he said, We have never hired a woman in physics or in the technical professions, but we're very interested in your husband.

And I got up and I said, Excuse me. I shall relay that message to him.

And Pat didn't show up for his interview.

So it was just right out there.

Yes, it was. We just don't do that. But that's the way times were then.

So then what?

We had an interview with a fellow named Bill Thompson who was in the Personnel Department at Livermore at the time. And a very presentable young man at the time. He's still a very nice fellow. We ended up becoming friends. You know, none of that nonsense. He said, Well, we'll let you know in about a month or so, after the interview, and we told him we were interested. And Pat's acceptance came in the mail on Friday. Mine didn't come. I was just really downcast. And so we were creating all these plans, oh, we'll live here, oh, we'll do this or that or whatever. And so mine came on Monday.

Good. And what were the positions at that time?

Junior physicist. Pat went into the Theoretical Department and I went into the Experimental Department. And what we did is—he was in the Theoretical Department, but the Experimental Department had a whole bunch of divisions. Art Hudgins was the head of Personnel at the time and he said, Well, now, you go out to these different divisions, and he set up some appointments for me, and talk with these people and tell me which one you want to end up with. And so I did, came back, and said my first choice is to go to work

with Bob Fox. I always sort of saw him as the technical brains behind a guy named Ted Merkle who was very personable. Merkle was kind of like [E.O.] Lawrence; Lawrence, very personable, very—he was fast on his feet, had a lot of those same traits, and people just liked him. Bob was a little more reserved, a little more quiet. But they had been through graduate school and things together and they had worked fairly closely for a long time. And Ted Merkle, at the time, was head of the nuclear ramjet project, and Bob had this little project off on his own for a thorium breeder **[00:20:00]** reactor. And anyway, I ended up going back—I liked the things that Bob was doing. I liked a couple of the other things, too, but I went back [to Art Hudgins] and said, Here's my priorities.

And he said, Oh, Bob wouldn't have you because you don't have a Ph.D.

And I said, Well, fine, you know, you asked, here's what I came back with.

And he calls me back a half an hour later and he says, Bob wants you to come to work for him.

So it worked out real well, yes.

So at this point, when you're looking at these jobs, you're still thinking that you're going to go to graduate school? This is a job you're going to do in the interim to save money, or…? Once you hit Livermore, what's your mindset there?

Well, our mindset is—well, first of all, to do a good job and to learn things on the job. By that time, you could take classes at Livermore through the University of California; which later became—and then I think it got—University of California through primarily Berkeley and then they transferred it over to Davis and it became what they called "Teller Tech." And so we were taking classes like that, and that was possible, and you could probably go through a master's degree. If you showed a lot of promise, then they would maybe help you through your doctorate.

As things later evolved, both of us felt that the work was so interesting at Livermore that it was just—you couldn't really be a graduate student and still work at Livermore. Some people did and some people did it very well, but I just always found the things I was doing at work so interesting, I never wanted to put them down.

And so we both ended up quitting the lab for a couple of years, or for a year is what it was, really, and going back to graduate school in aeronautical engineering at Cal [University of California, Berkeley]. We weren't that keenly interested in basic, basic physics; we were more interested, both of us, in the more mathematical aspects of it. And the Mathematics Department was just way too out in the fuzzies for us. I don't know. I like to build bridges rather than philosophize. That's just a difference in approach.

So that's a master's, then?

I never did get a master's I had to finish a thesis, but I ended up just liking the stuff I did at the lab, so I never did get an advanced degree, technically; but had probably about three years' worth of coursework between being at Livermore and when I quit that year. So I have had the coursework but never settled down long enough to do what I was doing at work plus write a thesis.

Right. So what happens after Cal? You…?

Come back to the lab.

You did come back to the lab.

Yes. In fact, that's where I first met Larry [Germain]. We were coming back to the lab and Pat was brought right in. It turns out that the division in which I had been had a ceiling on it and were overstaffed and so they were trying to get rid of people. So again I was told to go out and interview, which I did; turns out that Larry worked for what was called B-Division, which is

essentially primary devices. And I interviewed with him, and he was at the time the assistant division leader. And the division leader was Chuck McDonald, six-foot-two, big, strappling guy. Lots of people liked him but, loud and just—he would not interview women. And so it fell to Larry to interview women.

Oh, so it wasn't that he wouldn't hire women. He would just not interview women.

Right. And so Larry told me that Chuck came by after he had interviewed me that evening and said, `Well, what'd you think of that woman you interviewed?` And **[00:25:00]** Larry's response was, `She's a smartass and she won't come to work for us.` He's never changed his opinion of me, either. So anyway, that was quite a few years before we started going together and stuff.

I remember when we first came to the lab, the first time, I think it was, and I went over to work with Bob Fox. I was asked point-blank, by one of the group leaders, not Bob, `Are you a woman or a physicist?` And I said, `Well, I happen to be both.` And they just didn't know how to quite cope with that. Bob was just great, he really was, and there was never a problem with him. And there wasn't a problem with coworkers. And I wouldn't say there was a problem, but there was always kind of this feeling, `Are you really going to make it?` And you just do your best and do what you can and see what happens.

Right. Generally, what was it like to be at Livermore in those days, the feeling, the work atmosphere, what kind of work environment was it?

Well, where we were working was in some so-called temporary buildings. They were barracks buildings, and upstairs-downstairs in sort of an "H" shape, I think. And there was no air conditioning in them. I almost got murdered when I came back from a two-week vacation, having left some fruit in my drawer. They closed the door and were all standing the hallway

looking to see me open the door when I came back. It was terrible. And so it was kind of uncomfortable in the afternoons, but it wasn't bad. One guy would run around without a shirt. A lot of the people wore shorts. It was—women didn't. Women always wore skirts and stockings, and you wouldn't go without stockings and stuff.

In a way, it was informal, and people in general kind of pitched in. And it was easy to make friends. It was easy to make contacts. Some really lasting friends were made at that period. We were all young, at least there was a group of junior physicists, about a half-a-dozen of us at that time in that general division area.

And you got support. If you needed something, you got it. When I was starting to—wanting to do some things on the computer, I was assigned a programmer right away. Bob said, you know, `You're supposed to do physics and we'll get someone to do the programming for you.` The secretarial support was always good. Pat and I always tended to work when we needed to work, and so we would frequently go in, in the evenings. We would frequently go in on the weekends. You just worked that way. And you just never thought anything about it.

It was a small town in those days, and we rented a little apartment. Then we were finally able to buy a little house that had an apartment on it. We sold it when we went back to school—ended up renting from the people to whom we had sold when we returned to Livermore. It was an upstairs-downstairs kind of a thing. The apartment was upstairs and the main house was downstairs.

But yes, people were friendly; they were decent. There were a lot of little parties and things like that. People tended to—I joined this thing, it was American Association of University Women. They had a group in town, and so there were dinner parties like once a month; you

would go to different people's homes for them. There were a zillion clubs; a photography club, a book reading club, a number of **[00:30:00]** them probably, any kind of hobby you might have, there was probably a club for it in town. They had a very active small theatre group that we loved to attend. And it was just a—there weren't very many good restaurants in town or anything, so you'd go to San Francisco, you'd go to Berkeley. It was a good way of life. It was pleasant.

And you, in your job, you're feeling challenged, this is something you want to be doing, the job itself?

Oh, yes. You'd go home, think about it, and you'd come back and go, Oh, that's how I'll do it. There was always a challenge. We were having a difficult program, starting to see some patterns or something going on—and I had never had a course in matrices at that point—and I had essentially done a matrix type of solution. Well, Bob couldn't understand it either, so he took it to some other mathematician and they said, `Oh, what she did is to invert the matrix.` [And he said], `Oh, OK.` I had no idea what I was doing, but it all worked out.

Now Larry says, `She'll never work for us. She's a smartass,` but did he hire you or…?

No. What happened was that the thing I was working on before I left the lab and was working as a consultant while we were in school was a numerical calculation of high energy gas flow. And it turned out that this had become an area of considerable interest at the laboratory. Sometimes they had a bomb underground and a lot of pipes coming down to view or take samples or do diagnostics experiments. And there was a lot of controversy about how much energy would go into those pipes.

I guess the thing that really triggered me thinking about energy flow in pipes was in the Plowshare project, which was peaceful uses of nuclear explosions; that was the division into

which I had gone before I left the lab. They had this concept that you put the bomb underground but—OK, I'm sorry, let's start again. You dig a deep hole. Maybe you dig a 600-foot hole. Well, you place the bomb at something like 300 feet and then stem it above that. The idea was that the bomb goes off and it pushes all the bad stuff—the radioactive materials "down the hole" was the term we used—and then the ground shock would come around and close off that hole. And they wanted to use that in their cratering experiments or applications. And they did a test where that was tried and it failed. The ground shock did not close off the bottom of the hole.

Which test was that, do you know?

It was Palanquin [4/14/1965]. And what happened was that it cratered. Part of the problem was the fact that it probably wasn't stemmed very well above the device, and the other thing is that the ground—when the shock wave and the energy started going "down the hole", there was so much pressure in the hole, the ground shock couldn't close it. And so there were a lot of unknowns at that time about the trade-offs between things going down the hole or in a pipe and the ground shock closing it off. And so those were some of the things we were trying to address. And the calculations on which I was working were helpful in that arena. And so one of the fellows who took an interest in what I was doing was John Nuckolls who later became lab director. And at the time, John Foster was director of the lab. And Nuckolls went to Foster and Foster essentially hired me and put me back in the same division and said, `You just take her. I'll pay for it.`

[00:35:00] *Put you back in the…?*

Plowshare Division.

In the Plowshare Division.

Yes. And he said, `Just hire her. It's OK.`

Now let me understand correctly. You had been doing these calculations as a consultant while in school.

Yes.

And remind me, you said it but I have to be clear, the consultant to…?

It was a consultant to the Plowshare Division.

To the Plowshare Division.

Yes.

And then you were hired by the Plowshare Division, is that right?

When I came out of school. Because we had formally quit. We withdrew our retirements because we were going to live on our retirements and the sale of the house to go to graduate school.

To go to school. Got it.

So I started out working for Bob Fox. Then, I had quit for a short period of time, for just one semester. I spent a semester at Stanford in mathematics and decided I just didn't like it. Even that was too far out in the fuzzies for me, so I came back to the lab. And when I did, a number of things had happened in the division. Bob had left and his project had left also. And so they wanted me to write up some reports of this guy who just was too lazy to write up reports before he left. And I didn't want to get stuck with that, so I said, No.

What were some of the issues with the ramjet that were going on?

I was not really very involved with that, so I really can't tell you. But some of the kinds of things that they were doing were basic reactor calculations, the whole fuel cycle, how the neutrons get captured and what kinds of rods you need to put in, things like that. It was a lot of neutronics, I guess a little bit of thermodynamics. They were fairly interesting kinds of calculations. And so I got involved in using some of the codes. Usually you would have someone help you set up the

problem and then help you run it, and they were using programmers on these codes and things like that. And then I did some of my own kinds of things for Bob Fox, where we added the thorium cycle.

So this is to get this ramjet to be able to operate in space, is that right, this reactor?

Yes. And it was basically fairly successful. I think it could've been built; it probably could've operated, but I think that the biggest factor was probably the politics of radioactivity. By the time they forecast it could be done and whatnot—this was kind of the early sixties and a lot of the public sentiment was getting kind of scared about those sorts of things.

Now one of the things, and you correct me if I'm wrong on this, because there was the reactor program, the Rover program.

That was Los Alamos [National Laboratory].

Right. And then there was the ramjet that was Livermore, in my understanding.

Yes.

And I have seen those two areas out at the [Nevada] test site. But some of the—what's said about the ramjet, it was supposed to be designed to be able to operate still after there had been some kind of nuclear confrontation. Do you know anything about this?

No, I really don't.

Because I think it's the ramjet, and I may be showing my ignorance here, that was sometimes called the doomsday machine, that it would still operate even if the worst had come, and there's some criticism of that because of that. But in your day, you're just looking at how to get that thing to operate in space with this reactor.

Yes, mostly it was reactor physics that we were doing at the time. And of course, being off with Bob, it was even a little more out in the floatsies. It wasn't nearly as defined because we were

still trying to see if we could add the thorium cycle, which would regenerate the fuel so you **[00:40:00]** wouldn't have to tear it down again; it would sort of generate your own fuel.

That's why you said breeder reactor.

Yes, breeder. So I was not ever involved with the engineers and things like that.

At this point, had you been to the test site when you're working on this?

No. It wasn't until a few years later that I got out to the test site for the first time.

Back to John Foster. What kind of person was he as a director of the lab?

I think he was probably pretty good. Larry, of course, knew him fairly well. I remember right after Palanquin, I happened to be at the lab and they were showing a movie of it. There was a lot of laughter in the auditorium and Foster came storming down through the auditorium and hollered, `It's not *funny*, guys.` And everybody just went [makes sound indicating everyone peopled quited down]. I think he was probably pretty decent and pretty fair. At my level, I didn't have that many real interactions with him. He would come around occasionally to see what we were doing or something, but it wasn't—I think most people really kind of liked him. He had one quirk. He would stick his tongue out and touch his nose. It was kind of like a lizard. He'd be sitting in a meeting and until you got used to it and knew that was going to happen I had to tell myself, Barbara, you're not supposed to stare.

Yes. So am I understanding correctly that with what happened in Palanquin, Palanquin didn't work the way it was supposed to, then the work that you were doing became important for Plowshare because you're doing calculations on this.

Yes. And I can't remember just exactly—well, Plowshare continued, then, with the gas stimulation and things like that, oil stimulation, and so it went on to other things, but cratering kind of got squashed, and public sentiment, again, was moving in the other direction.

Right.

But the opportunities at the lab were really great in terms of being able—if you needed something, you could go talk to a chemist, a mathematician, an engineer. There were just no holes. You could talk to a theoretical person. You needed something, people were there to—just all jumped in and worked together. It was just a very—in time, that atmosphere got very eroded, in my opinion, and [people said], `Well, if I'm going to spend fifteen minutes with you, what's my charge number?` And you can't do business that way. You just can't. And I know a lot of people have tried, and I respect them for continuing to try, but that's not my style. I think life's too short to worry about every peanut.

One of the things we did—kind of getting along those line, and this is where I kind of first got to the test site—we did essentially a nuclear shock tube experiment, which I helped design. [Gives article: Crowley, Barbara K., H. David Glenn, and Robert Marks. "An Analysis of Marvel--a Nuclear Shock-Tube Experiment." *Journal of Geophysical Research* 76, no. 14 (1971).] I worked very closely for a number of years with this fellow [H.] David Glenn who is a Ph.D. physicist. He was kind of the engineer—well, definitely a physicist, but he could also figure out the engineering and help with the electronics and stuff. And he could do detail. I'm not **[00:45:00]** particularly a detail person. He was great to bounce around my wild ideas on and he'd sort of say, `Well, no, Barbara, that's not going to work because….` But anyway, he was really great at that kind of thing. We did a number of experiments leading up to this.

Shortly after I came back to the lab, there was an experiment that they wanted done that had a large—inches in diameter—pipe on it that looked at the bomb. I was approached one day, asking if I thought it would be safe, and so I spent the next weekend doing nothing but a bunch

of calculations and trying to get an answer together for, I guess, it was Monday afternoon. And I had decided that it wouldn't be, and had my calculation and everything in hand. Well, it turns out that the fellow who was head of Test Division at the time, or the test program at the time, and some of the other people were on one of these round-the-world eclipse trips.

And who was that? Who was the head of the Test Division?

Charlie Williams, I think, was the head at that time, and Harry Reynolds was also on that expedition. Anyway, both of them were gone. And Carl Houseman, I believe it was, said, OK, we'll take the pipe off. Well, they—Williams and Reynolds—came back to the lab and all hell broke loose, in a sense. They were really ticked off that the test had gone.

Without this pipe.

Yes. And so Charlie Williams had a hundred foot of pipe—I was at the time working in a trailer—had a hundred foot of pipe delivered outside my office door and he said, You go shoot some high explosive in it and prove to yourself that you're not going to get any energy at the end of his 100-foot pipe. I said, OK, Charlie, I'll do it, but we're going to do it my way. We're going to design it right. So Charlie and I would pass in the hallway and we'd both go [indicates attitude] you know, we wouldn't speak, we wouldn't do anything.

Finally, Dave and I got the thing designed and put together and everything like that. What we did, we didn't just put high explosive in the pipe, but Dave had got a big gun barrel from the Navy, a cannon, because underground, you shoot something off underground, the earth is tamping this, it's keeping the energy in a very small volume. If you go and take high explosive and just shoot it off in the air, of course it's not going to go up the pipe because it's all going out in the air. And so we said, OK, we'll do it and we'll do it right, and we put some

nice instrumentation in the pipe and all of this stuff, and we got lots of energy down at the end of this 100-foot pipe. Charlie Williams started talking to me again.

Now where did you physically do this, at Livermore?

We did it at a place called Site 300, which is a little bit east and a little bit south of the Livermore site.

That's what I thought, yes. And that's where high explosives kinds of things were done out there.

Yes, primarily. And so we did that, and then we also—let's see, did we have some—? No, I think that was about our lowest energy shock tube experiment. We did another shock tube experiment, too, where we used a compressor that was invented by a guy named Voitenko in Russia, and that would give us considerably higher energies of the gas flow.

So we did a number of experiments along those lines, which then led to this nuclear experiment. So we'd had a bit of experience before we went in to design this experiment called Marvel. It was a Plowshare program. It was a Plowshare test. What we primarily wanted to do was really monitor the test and see how much energy went down an open hole. And so what we did is, rather **[00:50:00]** than put it vertical, we laid it on its side, and so if this is the surface, we went down a shaft and then the pipe was placed in sections horizontal. And this is when I started going out to the test site.

Let me understand this a little bit. If you're going to be shoot—[referring to article] oh, this is for your line-of-sight pipe for the experiment itself.

Yes.

So it doesn't matter that it's not vertical itself; it can be horizontal.

Yes, because the energies with which you're dealing are so much higher than the effect of gravity that it doesn't matter if it's horizontal or vertical or upside down or anything.

OK. So the safety question has to do with what? When you answered that you didn't think the initial one was safe, it was because what would have happened?

The initial one.

When you were asked to first—oh, I'm sorry, maybe I'm not understanding you.

Ms. Killian added the following bracketed section to clarify technical details.

[The initial situation, which I did not think was safe, was to be a low yield test, Oakland, with a relatively large—inches—pipe on it for diagnostic purposes. The open pipe would come vertically up the emplacement hole—feet in diameter—which would be backfilled after the open pipe was placed in it. My calculations indicated that: high energy and high pressure gasses could get into this pipe and expand and not close when ground shock arrived at a later time. The open, or not sealed, pipe could result in a vent path for the gasses in the nuclear cavity.*

The Plowshare and Marvel interests were much larger "pipes"—which were not really pipes but just an extension of the emplacement hole. However, the physics and calculations used to predict the flow of high energy gasses in the pipe—or hole—and the interaction of the pipe—or hole—with the ground shock are essentially the same. Plowshare wanted to get rid of the radioactivity "down the hole" and have ground shock close the hole and keep it underground. If that did not happen, we wanted to know why. We were also interested in how much energy could be channeled down such a hole, what the shape of the resulting underground cavity was—things like that.

Two previous tests with open holes beneath the emplacement point, Dub and Palanquin, did not make any measurements of the flow of energy in the open hole. We did on Marvel. A shaft was sunk to about 600 feet, then a tunnel was excavated horizontally for 400 feet. Inside

* Some years later, the containment community would appreciate the risk of placing large diagnostic pipes on low yield tests that do not produce much ground shock.

the tunnel, sections of 1 meter diameter, about 18 inches long transite—sewer—pipe were laid and sealed together. Four alcoves were constructed perpendicular to the pipe in which instrumentation was placed to measure when the gas shock arrived at locations in the tunnel and its pressure. The volumes between the pipe and the tunnel walls was were backfilled with "density matching" grout as were all of the alcoves. The bomb was placed in a one meter diameter canister at the end of the one meter pipe closest to the shaft, and this area was well as the shaft was also backfilled.]

But this is a Plowshare program, so you're using a device in there that would theoretically be for Plowshare.

Could be. [Theoretically, but it was not a "Plowshare device."]

Interesting. What part of the test site was this on?

U10ds?

OK. We can look it up. [Confirmed U10ds1]

Quite the northern part of Yucca area.

So that's when you started going out to the site?

Yes. I remember the first time I went out. Gary Higgins who was the division leader then took me under his wing and, `Oh, Barbara, you've got to be careful of this and that and the other thing because you're going underground and the miners are funny people, they're very superstitious, you've got to be—.` He was priming me all day long, and it was never a problem.

One of the things that we did is we had layers of some chemical tracers built into some sections of the Marvel pipe. Past shot drilling some samples would tell us where the tracers ended up and how much got eroded from the pipe walls. And so I sat down with a couple of the miners one day and I explained to them what we were trying to do. And I said, `Anything you`

put in down here, I would like to have a sample of it. And they looked at me kind of strange and [said] OK. So they built me this great big treasure chest box and every time I'd go down there they'd say, Barbara, here's the things we've saved for you. And they were just so neat. It was just never a question of—I mean they thought it was a big joke at first; then they started, OK, yeah, well, yeah, we'll do that. They were just so good about it.

Well, that's interesting because one of my students reported that one of the women she spoke to who was a support person wasn't allowed in the mine because of the superstition about women in the mines. They were bad luck.

Yes, that was indeed the tradition. I had a girlfriend who was—she had a number of hang-ups. Her sister was a leading neurologist-surgeon in New York City and pulling down **[00:55:00]** hundreds of thousands of dollars a year at that time. And she was always kind of the other little puppy. And she started going out to the test site and found that there wasn't any help; there wasn't any support. She would come back with just dreadful tales. And I just never found that.

She was a scientist also.

Yes, she had a master's from Stanford, and I think she got rejected for her Ph.D.; I'm not positive. But she would come across in a way that miners or real field hands just couldn't quite stomach.

Well, I think that was going to be the second part of what I just said, is that I've also had reports during this oral history of the opposite, that there was another woman who said, No, I had no problem because it wasn't an issue for me and it wasn't an issue for them. So I think there's the myth and then there's the reality of how people operate. And the other thing that miners talked a lot about is how whatever the physicists wanted, any scientists, scientists and the engineers, they did everything they could to do it for them, and their one worry and

hang-up was safety. Oh, [scientists] coming in, in flip-flop [sandals] and shorts and no hat, and that was where it would be like, Where are these guys' heads? But as far as the relationship—and I haven't asked this question; people just report to me how interesting that was for them and how they always did everything they could for the scientists that came from the labs.

I thought the support people there were always just top-notch. I can't remember having a single problem. And I respected that I was on their territory and that they had a way of looking at things and a way of doing things, and I didn't want to get in their way. But I wanted to get a job done, and I think they felt they wanted to get the job done, too, so let's just do it.

Right. So you go in with the specifications for this particular experiment and then they carry it out.

Yes. And then we would go back periodically and see how things were going, where they were, and if we had any suggestions or anything, how things might be changed or how, who had questions about what went here or what went there and stuff. You always see something in the field that you can't appreciate back in the office. And so both Dave and I had this feeling that we were going to get out there, and we probably did like every other week while it was under construction and things like that.

Well, the other question this raises is it must have been some kind of accomplishment to get this thing approved as a test. No? How does that come about that they are going to do your experiment?

Well, I wasn't that familiar with the whole process at that time. Basically, though, in those days a consensus would kind of evolve, or the train of thinking would go: `Gee we really do need an experiment along these kinds of lines`. And exactly what it would look like or exactly whose it would be or anything like that, or how much it would cost, wasn't really looked into in

the depth that was later the situation. Like when we went to do these high explosive experiments, well, I guess I spearheaded that. And I went into the division office and told them what I wanted to do and they said, OK, we'll think about it. And about a week later I said, Are you done thinking yet? And they said, Go ahead and do it. And that was it. I mean there was never, you get seventy-five nickels and you have to spend them in thirteen days or anything like. It was always OK, go do it, or, You want a programmer? This person will work with you. Or, You want a technician? OK, we'll get you **[01:00:00]** one. There was never the—there was an opportunity to do things. There was an opportunity to try things. The whole culture was, if you've got a silly-sounding idea, let's hear it and let's talk about it and maybe there's a chance it could work; if there was any kind of a chance or people kind of respected what you did, well, they'd give you a chance to go do it. And it wasn't you have to prove everything five ways from Sunday before you can go out there and do it. Just go. Go see what you can do and we'll give you the support to do it. We'd go see engineers, we'd go see chemists. There was never a question of, Well, I spent an hour and forty-five minutes with you. Can your budget afford it? There was none of that.

And that literally happens now, that if someone from one place goes to another department or division, it's a budget item?

I can't say that for sure now, but it was certainly that way before we left Los Alamos, and it was getting quite a bit that way before we left Livermore. I mean fifteen minutes is probably too little, but if it was anything that was going to take a day or two, you'd definitely have to have a charge account or something.

Interesting. Well, the atmosphere you're describing sounds a lot like what my sense of what, as a child, with my dad's work at Brookhaven, it's a scientific, intellectual—it's like a university lab,

in a sense. You're just out there for knowledge's sake and you go where you need for the knowledge.

Yes.

So there's a similar, what you're describing is—I couldn't say because I was a little girl growing up but this era that I think was the atmosphere that my dad experienced at Brookhaven.

Yes. The national laboratories have been very good about that, or had been very good about that kind of thing before they—I felt things had changed quite a bit.

And what direction did that change come from, would you say?

Well, it came from when ERDA [Energy Research and Development Agency] was starting up and then certainly got more and more under DOE [Department of Energy]. I think things were still pretty freewheeling all the way through the eighties. I don't think people wasted money at all. You could say had we known what we know now, it was a dumb thing to do, but a lot of things just weren't known then. And I think that it was fun working in that kind of an atmosphere. I think you have to have it. I think maybe the space program saw a little bit of that, and I think some of the people who worked in Palo Alto [California] on the computers and things like that, some of the companies there, felt some of that; where people would sleep on cots for a week or two on end while they were trying to crash through something. But it's a fairly—the government or the people who were behind it, there was a need for what you were doing, a desire for what you were doing, a market for it, which later vanished. And that makes a lot of difference.

[01:03:50] End Track 2, Disc 1.

[00:00:00] Begin Track 2, Disc 2.

Let's talk about containment, then, because that is really a subject matter that I find fascinating and interesting. So you can tell me what you think.

Very important. I was kind of in the calculational side of the Plowshare program. In fact, a lot of it happened after Baneberry. They had review panels before then, but they really got serious after Baneberry. And I had done some calculations on Baneberry and a lot of people had.

Let's back up a little bit and talk about Baneberry itself and what that was, from your perspective, like at the lab and all that happened.

I wasn't that much involved in the test program prior to Baneberry, and most of Plowshare, that was handled more in the Test Division. It turns out that Larry [Germain] had been in B-Division and then he went on over to the Test Division, and he was, I believe, scientific advisor on Baneberry. And there was information about the down hole operations that he was never informed of, with respect to the geology.

Really! And who would it have been that should have been telling him these things?

I can't really remember the organization that well, but I think it was the person who was in charge of the drilling operations at the time perhaps should have or maybe that person didn't realize he should have. I don't know. But I do remember that Larry talks about a number of things being a real surprise to him when they went back to do a lot of the investigations. And I think a lot of things tightened up as a result of Baneberry. There was a lot of concern, of course, and I think it was a six months' moratorium, more or less, during which we did not test.

Now just in your personal time line, were you married to Larry at this point or with him when Baneberry occurs?

Let's see, Pat and I were divorced in the late sixties. We weren't dating or anything like that, but we had kind of gotten to know each other through things at the lab and we had interdivisional

kinds of support. Support was starting in Plowshare for the test program, so more of the people that were doing calculations on Plowshare and the Plowshare program was crapping out, and so a lot of our effort was being devoted, then, to the test program.

Yes, it was a turning point in containment and how it was handled. There'd be monthly meetings in Las Vegas of the Containment Evaluation Panel [CEP] and we would do calculations to back up what was going to happen. There were good reviews of what they expected to happen on an upcoming test. And Larry represented Livermore on the Containment Evaluation Panel and he was the chief person on the Livermore side that was involved with the nitty-gritty of it. And I was in and then later ran the **[00:05:00]** group that did the calculations in Plowshare in support of that. During that kind of period, it was after Baneberry, that there were something like twenty-three or twenty-four people in my group, and most of them had never been to the test side. And so I had the secretaries arrange the charter flight to the test site and had a day trip for the whole group. And it was really very worthwhile. They could finally—the secretaries, the production assistants, all these people who had no idea what all these things were about that people would come in and mutter about, they could finally see what they were like. And I could never understand why more people didn't do that because it made a lot of difference when they were typing a report, what it meant when people would refer to things or give them a dimension of something; they could get a better feeling of what it was all about. And I thought it was very important they do it. Nobody gave me any problems and there was no overnights or anything like that, so it was a pretty cheap trip, but I thought it paid off.

Let's see, what did we do? Yes, so our group would essentially support them. And then Pat and I were divorced, and Larry and his wife were divorced, and we just started going together. Pat and I were married for something like sixteen years, I guess. And it was a good

marriage. It had its ups and downs, but he wanted different things out of life than I did, and so we just went our separate ways. We're still very good friends. In fact, he was here putting in the chair lifts when Larry came home from the rehab hospital…

That's remarkable, actually.

Yes. And we've stayed very good friends with his ex-wife. I'm thankful for those relationships.

One more question about Baneberry. I've read some of the [James] Carothers interviews from Livermore where he asks different people about Baneberry, and some of what I hear and people say to me is that we didn't understand the geophysics well enough to be able to—now I'm paraphrasing here—to predict that this would happen. But what you seem to be saying is also that some of that actual data that might have refined someone like Larry's understanding wasn't actually communicated.

Well, it wasn't there before the shot, was my impression of what had happened.

"Wasn't there before the shot." What do you mean?

Was not available. We never heard about it in the Plowshare Division where some calculations were being done on containment in general. But at the time, we didn't go out and calculate every shot like we did later. One of the problems was, and I would still have to go back and look at some of it, but they encountered a clay which is called montmorillonite, or we referred to it as that, and evidently it's a fairly unusual geologic formation. You don't find it in most holes you dig. And it turns out that under pressure, montmorillonite essentially loses all of its strength and becomes like a liquid. And the presence of a layer of that or layers of that, I can't really remember just what the geology of the site was or the stratigraphy of the site, but that can give you a lot of problems; your shock wave isn't going to go the way you think it's going to go and can channel energies in different kinds of directions and whatnot. Usually when you detonate an

explosive underground, it just kind of spherically goes out. Maybe there's a layer and something will bounce a little bit, it'll be more ellipsoidal in one direction *versus* the **[00:10:00]** the other, but it is basically fairly spherical. Some of the calculations I had done, including the layering of the montmorillonite at the time; I understood it from the data we did get, I would put it into the calculations as a material with no strength, and you get all kinds of funny swirls going on which can channel energy, it can open and close things. A lot of things can go on. It wasn't the old spherical kinds of situation. I think that was part of it. The other thing that I wanted to spend more time looking at is that when you get something like that turning into a liquid, it can serve as what the oil patch would call a fracting fluid.

Fracting fluid?

In other words, it causes fractures. They call it—sometimes they'll try to fracture with water; they'll put walnut shells in it and pump it underground and try to get the formation to fracture so that you can stimulate—you have more fractures so you can get more oil or gas out. And so I think that there may have been some relationship between what happened and maybe the montmorillonite serving as a fracting fluid to kind of make things worse. We were working a little bit with Halliburton at the time, and I remember just kind of getting into things and finding that the montmorillonite might have had some of the same viscosity as some of the fracting fluids that they do use. And so it's something I never really completely pursued, but I think that could have been, possibly. Who knows? I mean one of Barbara's ideas.

And so this is your post-Baneberry analysis that you're doing that.

Yes.

What's interesting about that—I'm going to say this in layperson's terms; you can tell me if I've got it right—is that in addition to the qualities of the montmorillonite causing it to essentially

become liquid, which is a complication in and of itself, is that it almost seems that you're saying it could also have had an actual, what do I want to say, proactive action that it did that further exacerbated the problem, in addition to the fact that you didn't get the spherical formation that you expected because it acted differently, its properties.

Yes.

It also had properties that would have actually made it do something in addition to that.

Possibly.

Possibly, is your possible theory. OK. Because one of the physicists said we really still don't understand Baneberry, someone said to me, just in passing, I don't have this—

That's probably basically true. I think we have some pretty good hints about what happened, some pretty good clues. But like I say, a lot of these things were just sort of left a little bit up in the air; although the thing that did happen was that there was a lot more conscientious review after Baneberry than there was before. I think that everyone will agree to. And so whether we really know precisely what caused Baneberry, there were some good spillovers from it.

Yes, I guess one of the things that's fascinated me, coming as a layperson and thinking of the bomb, nuclear explosives, in terms of those—this is a really surface thing, but in a sense, that's that mushroom cloud. Once you go underground, a whole lot of understanding of physics and geophysics has to happen that really has nothing to do – you correct me if I'm wrong here – with what you're trying to find out with the device, but you must do in order to detonate the device, which is reactions of the earth around it.

Yes, you've got to be careful, you know, how deep you put it, what the content of the material is, or if you're above the water table, below the water table, by the water table, where you can get a big reflection. The properties of the material—so you get a little bit of difference in properties of

the air where you can break windows in Las Vegas or not break windows in Las Vegas, depending upon what that is. It's even amplified when you **[00:15:00]** go underground because there are so many different things in the geology that can affect the propagation of that stress wave, or shock wave originally, through the ground.

That's what seems interesting sort of in the evolution of the science that the weapons labs and the testers are doing is having to deal with this other environment that may not have anything to do with what down the road is seen as an application of this weapon or use of this weapon. Unless you're thinking about exploding weapons underground to bust bunkers and stuff. If you're thinking about using them in other ways on the battlefield. Maybe not you, but to the military it matters.

Yes. Usually. Although a lot of this does have its spillover into both verification and trying to figure out what yields the other guy did, and it also spills over into going after bunkers and underground facilities.

OK, so that was a little bit of a detour so I can understand it. So Baneberry causes the Containment Evaluation Panel, and then you're still in the Plowshare Division but you're doing calculations for them, is that how that works?

I was in a group in the Plowshare Division, then I became the group leader, and then went on from there. And it was about that time that Larry and I started going together and decided to be married. And there was a meeting in Los Alamos that we went out to. It was a Containment Evaluation Panel meeting. And we decided that the following weekend, we would get married. And so we did, and we wrote our own ceremony and we decided to get—the Catholics wouldn't remarry me, of course, and the Episcopal person in Los Alamos didn't want to just dash off and marry somebody without knowing them for a month or two. And we didn't want to take a month

vacation so this guy could get to know Larry, so we found a Unitarian fellow who was just really neat. And we said, We'd like to write our own ceremony. Is there anything you have to do or say or anything like that? He says, No, anything is fine. Usually at a wedding, there's two people, usually of two different sexes, and a third person, and any two of the three who agree that a marriage has taken place, it has taken place. So you don't have to say anything in particular. I had never known all these things before.

Sounds like, "whenever two or more of you are gathered in my name, there is love", right?

Yes, there is love, right.

Some version of that. So you wrote your ceremony and you said you had it in Bandelier?

Yes, in Tyuonyi, the ruin there, the circular ruin. We paraded in with a guitar player, and there were probably about fifty people who had come, and they gave me a very nice little shower a couple of nights before; we had a little reception at La Fonda [Santa Fe, NM], and then went off for about a two-week honeymoon and did Indian country, you know, the dream of every twelve-year-old.

And so we went back to Livermore and things started popping. One of the things we did was we said what is your near relative policy? And at this point, Jim Carothers had been made associate director of Human Resources. He was no longer in the test program. And—

This is 1970-what, then, when you're married, just so I get this?

Thirty years this year, so it was in 1975.

And I didn't quite get what you said, what "your near relative policy"—what does that mean?

Well, a cousin, mother, wife, sister, brother. That was kind of the term they used for it, you can't hire a relative. Like how you work with a—

Oh. How you work with a relative.

Or don't work with one.

Or don't. Got it.

When we came back from our honeymoon, that wasn't the case. Larry was still in the Test Division, and then **[00:20:00]** the position of division leader of what was still primarily the Plowshare Division—well, it was called K-Division because we had a couple of other contracts—came open and Larry was given the job. And so I went out and I said, `Well, tell me what your near relative policy is`. OK. Well, they come back. Jim Carothers would not come to see us personally we had socialized with Jim and his wife and Jack Kahn who was made Carothers' deputy. I mean most of our time was socializing with these people. The minute Larry was made division leader and I asked, `Well, what's the policy? What should I do? Because, you know, I shouldn't work directly for him`. We were told by two people Carothers had sent to see Larry and me, `Well, our lawyers are cheaper than yours`. And I just kind of go, `I don't want to sue you. Hell, I know you're going to win`. I mean, don't put me through that. I'm not going to waste four years of my life. I didn't come out and say this but it was obvious that's what I thought and what Larry thought. And so I went around the lab, trying to find another position, and every time I found one or would interview for things, `Well, we'll let you know`, or it would come back, `Well, you'll have to interact with Larry`. And so what I finally did was I got myself accepted to Stanford. They had this program where it was a year program to get a master's, an MBA [Master of Business Administration]. And so I went and I passed all my tests and I got accepted to it. And I wrote a thing to the administration at the laboratory, to the director. But I don't know if it ever really got to him. I asked, `Can you help me in some way?` I wasn't asking for full salary or anything like that, or even full tuition. I was saying, if I can't work in the technical area because I'd end up interacting

with Larry, I'll go into the administrative area and the interactions could always be cut or that kind of thing. The reply came back, No. What I found out later was that Jack Kahn, Carothers' deputy, was refused, rejected from the same program a year or two before, and if he couldn't get accepted to the Stanford program, nobody else was going.

He'd been rejected from the Stanford program.

And so I started getting telephone calls, and Charlie Williams, the guy that delivered the hundred foot of pipe to me, calls me up and says, Come on up to Idaho. And so I went up there for an interview. And Bob Brownlee said, Come out to Los Alamos. And we did, and that's when John Hopkins hired us.

I see. That's a very interesting story. Now was Williams running things at Idaho at that point?

Yes.

I thought so. I thought from that name that that was right. So did you go to Idaho or—?

I did, for an interview, but Larry wanted to hang onto his retirement from the lab, which at that point he'd had quite a few years in; I didn't have quite as many years. But I don't know that I could have really taken the winters up in Idaho; I think I certainly would've given that a try because I was a bit younger then. But it would have been quite a ways away from the things we love.

Yes. So then Brownlee said come down to Los Alamos and so you both came down.

Yes. We came for interviews, and John and Adele Hopkins, in their usual gracious way, invited us to dinner. And Barbara says, Well, we'll bring some wine. And there's this long silence on the other end of the line, and, Where you going to find it? You know, because this was Sunday. And I said, Oh, not to worry, we'll get it. They didn't sell liquor on Sundays then [laughter].

I remember that.

Yes, remember that? So we took a big box of chocolates instead, which I'm sure they needed like a hole in the head, but nonetheless. And the first thing we do is we ring the doorbell and they open the door and Larry walks in and he says, `What are you going to do when the earthquake comes?` His first words to them. Never hello, it's good to see you, or anything. They have a fabulous pottery collection and they have shelves near the ceiling of incredible pottery, and so Larry was wondering. So I thought, well, there goes that job, but we got hired.

So this is '75 still that you come here or—?

[00:25:00] No, '76. It was—

Seventy-six.

Yes, about a year after we were married we left Livermore.

And what positions did you move into, then?

I went to work for a group called—J-9, the way they interpret things, their near relative policy at Los Alamos—

That's the question.

Which essentially should be the same thing that's at Livermore because they're both under the University of California. But Jim Carothers was just too bloody whatever to look into anything or make it up or whatever, or find out what could be possible. As long as you don't work for the same boss, or work for each other. And so I went into one of the groups in J-Division, and Larry went into what was called JDOT, J-Division Office, Technical.

So you're both in J-Division. You're just in different groups.

Yes, we're both in J-Division. Yes.

And John's the head of J-Division at that time?

Yes, and so he was Larry's boss, but somebody who worked for John was my boss. And then Larry moved over to a new division named G-Division was formed for geosciences. Bob Brownlee was made the division leader of that, and he asked Larry to come over as his assistant or deputy or associate or something or another. And so then when Larry left to go to work for Bob, then I went into the division office, technical and worked for John. And then Larry went to the director's office and I went to work for Bob Brownlee over in G-Division.

Interesting. He's someone I still need to talk to.

Bob Brownlee?

Brownlee, yes.

You'll find he's a neat guy. Well, after Don Kerr came in, he wanted to do matrix management, and we grew up with that in Livermore and that was no problem. I was very familiar with it. You pit Mother against Father and you get what you want and—

Tell me more about that, you pit Mother against Father and you get what you want.

Well, I mean here's the guy with the money and here's the guy with the people. And here you are. Well, you've got to work for the guy that has the people and then you've also got to work with the guy that has the money. And so you decide what you want to do because you're going to do it anyhow. That's my attitude. Larry, too. And so you just start building a case to be able to do that, or if one disagrees, then you get the other one to jump on—I mean there's ways of playing things off against each other that you can do that. And so in lots of respects, if you know how to operate it, it's one of the nicest situations of all. But everybody got all uptight about it.

At Los Alamos.

Yes.

Because they'd been doing things a different way.

Yes.

Now Brownlee says that you're going to do things this way? Who says you're going to do matrix management?

Don Kerr.

Don Kerr. Sorry.

When he came in as director after [Harold] Agnew left. That's what he wanted to do, and that's what he proceeded to do. And Kerr brought in with him Rosemary Harris who was Payne's [Harris] daughter. And in fact, she's my best girlfriend. And she was in administration. We didn't become friends till later when I started working with her in Program Development. But she was the dragon lady. Everybody was intimidated by her. Larry just thought she was great, you've got to meet Rosemary now. Anyway, we had a good time.

I think Kerr thought that Bob Brownlee and Larry were good friends. What Don did when he brought him into the Program Management Office, he gave Larry the money side of the hot dry rock program and a number of other programs in G-Division. And Bob Brownlee felt that he was being ripped off because he no longer had both-sides-of-the-house power, and he thought that Larry was trying to stifle him and stuff like that, and things got very touchy for quite a while. They finally got—I'm not sure he ever really completely has realized that Larry **[00:30:00]** didn't care that much about any kind of power position. I mean he'd given up all that stuff a long time ago. We both have come to the realization that babysitting is not a lot of fun. And, you know, you get a group of twenty, thirty, forty, fifty, a hundred people and all you're doing is being a babysitter. And some people like to do that. That's fine. Some people think it's power, but not if you're a good one.

So you're saying you would just prefer to be doing the work itself in a smaller group.

Yes. And Larry's always felt the same way, too.

What do you think was Kerr's motivation for this change in management?

He had seen it work in other places, and basically it can be a very good form of management, particularly when things started getting fragmented as much as they were. I mean all your money no longer came from the AEC [Atomic Energy Commission] because the AEC didn't exist. You'd get this little dibble from one division in the AEC and then you'd get another little dibble from another division in the AEC, or the DOE, then you'd get something from the DoD [Department of Defense]; then you'd get something from, I don't know, some other federal agency or the scientific gurus, and so you had a lot of different budgets and a lot of different things to juggle. And it became in many divisions a very, very difficult kind of a thing to do. I can see you really do kind of need someone on both sides of the house. To me, it's just a natural kind of a thing to do, in a way. But people were just very much opposed to it. They thought it was giving up their power. Life's too short to worry about that.

Yes. Before we get into the details of some of the work that you did, did you notice a cultural change, just because it's out there so much of the different cultures of Livermore and Los Alamos, did you as a working scientist notice that kind of cultural change in the work atmosphere at the two places or?

There was a little more tendency toward conservatism at Los Alamos than there was at Livermore. And it's not a strict or strong kind of a line. You'd find people that were innovative on both sides of the fence. But there was, I think, a little more conservatism at Los Alamos. Los Alamos, particularly in the—well, let's see. People were always very friendly to us, were always very open, and it was easy to make friends there as it was at Livermore. I think that when you went out to the field to do something, and it depended upon what division you were in, that there

would be scrutinies in different ways and things like that. Los Alamos had a number of people in the field test program that had been there for a very long time, and I think they really helped influence it and helped give it a character that didn't quite exist at Livermore. Father Campbell, for instance, you've probably heard of him, Campbell either liked you or you were out. There was no middle-of-the-road. And he had really gotten it in for some of the geologists who he thought were a bunch of prima donnas and they didn't go out to the test site anymore. I remember when I was the new kid on the block, we got together one afternoon, Tom Weaver and Fred App and myself. They had done a satellite hole, which is a hole that they do before they do the main emplacement hole, to get a feeling of the geology. **[00:35:00]** And it hadn't been stemmed yet, and so we said, Hey, we want to get some ground motion measurements, so the three of us sat down and decided what we wanted. So at the end of the meeting—this is a Friday afternoon—[I say]—OK, what do we do now, guys? Oh, we've got to go see Father Campbell.

You called him Father Campbell?

Most of the people did, at least the people who hadn't been around twenty years did. And how should we do that? Weaver says, Oh, I've got a doctor's appointment on Monday. Fred App says something else, you know. [So I said,] OK, I'll go see Campbell. And they said, We've got to do it real soon because it's going to be stemmed real soon and, you know, got to do it Monday. So dumb Barbara [says], OK. So I come bebopping in at 7:30 in the morning and I go up to see Fran Rogers, who's Campbell's secretary, and of course they always come in early in the morning. And so I go up and I tell Fran what I want. And she said, Well, you can see him at three o'clock. And I said, OK. Right at that point, Campbell comes out of his office. He starts asking me what I want, what I'm doing.

And so I say, We want some instrumentation in the satellite hole, at this point, I'm in his office, for ground motion, blah, blah.

There's about twenty people or twenty-five people in there: a bunch of the division leaders, the chief engineers, and the rest of it. There's problems on the main hole.

Campbell turns around to me and he says, Go to hell, Barbara.

And I said, God damn it, Campbell, you're not going to tell me to go to hell. I've been told to go to hell all weekend by the best of them. I'm seeing you at three o'clock.

[And he said], Yes, Barbara.

We got the instrumentation. But, you know, I've told this story to other people, particularly women, and they [say] he shouldn't have done that. Well, he was treating me like anyone else. And if you can't stand up for what you believe in, he's going to write you off. And I wouldn't expect him to treat me any differently, or I would hope he wouldn't. And for somebody to think, well, you're a woman put in this awkward situation, well, that's—I never looked at it that way. If you can't stick up for what you believe in, I wouldn't mind his writing me off, you know. And he did that with everybody. He wasn't singling me out in any particular way.

I did have a chance to talk to him last year.

Oh, wonderful.

And was sorry that I didn't get to see him this time coming back, because that was someone I would've talked to again, but I did get to experience the Campbell experience. And a lot of people talk about him, but that's a really interesting story. So at your three o'clock meeting, you told him what you wanted.

Yes. [And he said], OK, Barbara. Bye. [And I said], OK. Thank you, Campbell. Bye.

Interesting.

Yes, that's the way he operated.

So then you're obviously spending more time at the test site once you get to Los Alamos, I would think.

Yes, in spurts. One of the first things I did there is—these fiber optic cables were coming in, as opposed to the big electrical cables. Or they were still using the big electrical cables, but there were a lot of applications where you could get by with the fiber optics. And they're nowhere near the problem that the larger cables are in terms of keeping the gases in underground. What we would do is when you put your bomb under the ground in the hole [drawing diagram], and then you backfill it is the terminology, and you have all these cables coming up, well, lots of times these are huge bundles of cables. And so they started doing what they called CTE plugs. I can't remember exactly what that stands for right now, but it's not a concrete but it's a material that will flow around and then harden up. And so there would be **[00:40:00]** some of these plugs poured in the emplacement hole where the cables came up. Well, when the fiber optics came in, we just had to show here's how we would do that kind of stemming and here's what it would look like and all that kind of stuff. So it was a nonplusser, but it was kind of fun to do and it took me out to the test site a number of times—like the instrumentation we had on that one hole and things like that, there were things that would get you out for one thing or another. And also worked some DoD contracts at the time where we put tracers in some of the Department of Defense tests and things like that. Chemical tracers.

So explain in layperson's terms what that involves. What are they looking for in the tests, then? Effects or—?

Well, one of the tests that was done was in a room and they had some pipes coming off of it [drawing diagram]. And what we did is implanted some chemical tracers around the bomb to see

how this room mixed. In other words, would tracers from here mix with tracers from here and go down, or would just tracers from here go down, and things like that was some of the information we were trying to get a hold of. And then the DoD ran out of money and never really funded the analysis afterwards. That would be very typical.

Interesting. I have several questions about that. That's a room—

About the size of this room.

At the bottom of a shaft or at the end of a tunnel?

The end of a tunnel, a king bedroom size.

At the end of a tunnel. I've seen pictures of some of those big rooms that they would make and fortify with concrete and wires and stuff like that. So you're talking about a room like that.

Yes.

Now can you say what the DoD was looking to find out in a test like that?

They wanted to see what kinds of flow they got in the pipes from this room.

In order to what?

Better understand their hydrodynamics, or flow of gases, high energy gases, in pipes.

So you put the tracers so then you can then monitor that flow into the pipe.

Not so much monitor with respect to time, but just kind of see how it really got into the—or—

Analyze it.

Yes, kind of where it came from in the room to go into that pipe. Was there a lot of mixing or not was what we were really trying to find. A lot of times on the DoD programs, what would happen is they'd run out of money or their thoughts would get channeled somewhere else. It was a much more iffy environment than the labs.

Interesting.

I'm not saying all the programs were that way or anything like that. A lot of them had a lot of good long-term continuity. But it just seems like they oftentimes were subjected to more on-again, off-again kinds of operations than the labs used to be, anyway.

Would that have to do with the politics of the moment, would you say, for the funding? Is that—?

Yes.

And again, they're trying to see this so that they can understand some – if it's DoD, I'm imagining – some military aspect of it.

Yes, what the DoD would do is most of their tests would be done at the end of a tunnel [drawing diagram] and say you'd enter it this way and here would be a tunnel, they would have large pipes underground, and the bomb would be sitting over here. Then they would have a large experimental chamber or place where they exposed stuff. They were interested in how gamma rays, temperature, anything coming from the device would affect stuff down here. And so the intent was to design the pipe so that when the weapon was fired, the gamma rays and temperature would get to the exposure stuff but the pipe would close before the hot cavity gasses came roaring down the pipe. **[00:45:00]** They would also have large high explosive chambers that would collapse the whole tunnel and pipe in that area. And then after the test, they'd come in back over here to the area where stuff was exposed and examine the stuff and also maybe monitor it with respect to time during the shot.

So it's a continuation of the question of the effects of nuclear weapons.

Yes.

Sort of from the burning houses out in the fifties to its effects on instruments and—

Yes, electronic equipment, warheads, all kinds of things. Yes.

Right. Because the miners, too, have talked about this reentry process, going back in after a test and when it's safe and those kinds of things. OK.

John [Hopkins] sent us both over to Geneva at one point. We had one session together, and I had one separately and Larry had one separately while we were in—In the earlier years, Larry used to go over for Livermore on United Nations Committee on Disarmament. He would be a technical representative. He got selected, and John put him up for it, I guess, to represent, or to help the Department of Defense as a scientific advisor. And [Edward] Giller was over there at the time, and his wife.

Oh, really.

Yes, and also some other friends of ours. And so that was about a six, seven-week session, I guess, and Larry went over, then, in that role to the Department of Defense to negotiate the Comprehensive Test Ban Treaty [CTBT]. It was trilateral at the time, and Giller was head of the DoD delegation, I believe. And then there was a DOE delegation that was headed, I think, by Bob Duff and I worked for Bob Duff on my stint over there for the Committee on Disarmament. And, let's see, OK, they were trilaterals at the time: Russia, the United States, and Great Britain. And so we spent, I don't know, six weeks, something like that over there. And Ed's wife, Millie, I think you'll like her. Anyway, it was a chance to get into some of the—see some of the negotiations and whatnot. And at the time, the United Nations Committee on Disarmament would talk about the Comprehensive Test Ban Treaty, but the thing I was really involved with was the negotiation of the radiological weapons treaty.

So tell me more about that.

Well, I should sit down and re-read the thing. It's been quite a few years. Because I don't really know how you monitor anything like that. It's essentially a treaty that says that you won't take

nuclear particulate and try to disperse it. And the means of, not an explosive, but they're termed radiological weapons, I guess you kind of play like Hansel and Gretel and sprinkle it around the water systems or railroad stations or something like that. And maybe you have a small explosive in it or something like that, but it's not a nuclear explosive, but it intends to disperse the radioactive material. And I don't know, it seemed to me like kind of a pointless sort of a thing because I don't know how you can ever monitor anything like that. But there is such a treaty, and it has since been signed.

So you were a technical advisor on that.

Yes. And it was kind of fun. It was a whole different kind of a world. And then I went back for another session, I don't know, six, seven weeks when Larry wasn't there, and then Larry went on one when I wasn't there.

And what era is this? This is around—?

Late seventies.

Late seventies?

Seventy-seven, seventy-eight, yeah.

[00:50:00] *You raise the issue of the negotiations side and the international side, the meaning of everything that's been happening at the test site and that's happening in the Soviet Union. So let me ask you a little bit about not politics* per se *but what is it like to go from testing to the negotiations side? Is that a natural thing for you to be doing? Some people talk about how the move from thinking of deterring the enemy to actually, you know, working on negotiating with the enemy was a shift that they had to make. What was your experience of that? Is that a good question? Maybe it wasn't an issue for you.*

I didn't feel it was an issue. No, I guess I didn't have those kinds of feelings, *per se*. I mean I knew that—well, it was just kind of the other side of the fence of the same thing, to me, rather than two entirely different things. Because you knew there always had to be politics and there always had to be two side of looking at the thing. It was just—it wasn't a good-guy, bad-guy kind of thing, or now you're my friend, now you're not, because you weren't. Maybe because I was in a much lower support role than one of the leaders of it that I didn't have those kinds of feelings. I was more in the technical support than I was actually leading the negotiations or anything like that. And so basically my role didn't change all that much.

Well, that's interesting. So it's your expertise in this area that serves the test, and then serves the negotiations about verification or treaty-making.

Yes, that's how I generally kind of looked at it. I kind of felt a lot of these things are going to happen anyway. It was very interesting, though, to kind of see the machinations that go on, how people react to these things, and the hurry-up-and-wait atmosphere. It was a broadening experience that I thoroughly enjoyed.

Interesting. Yes, that's good. You've answered my question.

But you can't do that kind of thing very long. You'd go cuckoo.

Why?

Oh, it's hurry-up-and-wait. It's always being driven by somebody or something else. You have no control of your time, your emotion, your energy. I couldn't do it for very long. Some people I know have done it for quite a while, and I thought they got kind of cuckoo after a while. It's a whole different—I think, to me, one of the really interesting things was the contrast between being out in the field at the Nevada Test Site and then sitting in the *palais* of the United Nations.

I mean the contrast between those two kinds of things that you were still getting paid for both of them was kind of fun. And to feel the difference in the two.

Yeah, that's interesting. That's another kind of difference than I asked you for, but that's an interesting difference because you've got this broad thing we call the arms race and the Cold War, and you've got the on-the-ground work there at the test site, and then you've got how this is filtered through to these very high level negotiations. That's interesting.

Yes. You're seeing the spectrum of it. You could just feel what it's like to be out at the test site. You can still feel what it's like to be in the negotiations and things. And it's quite different.

So that's in the seventies. What are your thoughts about the Comprehensive Test Ban now, I mean the fact that we have not ratified it and that whole business? I'm getting into a political question but I'm curious what you think about that.

[00:55:00] Well, I don't think we should ratify it. I think we're absolutely stupid not to test. I don't think we know what we have in that stockpile. But that's my own personal opinion.

Yes, I know that.

I mean you cannot get from A to B on calculations alone, although that's where I made my money. That's where my career was. I don't feel you can do that. You've got to have the tests. You've got to have contact with reality. You can't let things sit around in a stockpile for twenty years and expect them to be all the same that they were the day you put them in. And you can't do new things. And there were always surprises or new things showing up. I sometimes wonder what some of these guys are smoking. To be able to think that you can calculate your way out of the box, I don't believe that at all.

Did you see the New York Times *yesterday?*

No, I did not.

There's a big front-page article by William Broad about the problem with the—I meant to bring it to you. What's the weapon, W-76? It's supposedly the weapon that is most in the stockpile. I'm getting it wrong. But it's this whole question of the deterioration of this weapon and the physics of the weapon and whether it needs to be retooled, redone, et cetera. *You'll be interested in seeing that article.* [William J. Broad, Aging Warheads Ignite Debate among Scientists, *The New York Times,* April 3, 2005]

Oh, OK, I'll go down and get it because we can get it at Borders.

You can get it on the Internet, I'm sure, off the New York Times *website. And I can send you a copy. But it's to this very question. I guess it's the weapon. I must be getting the name wrong of it, but that it is the weapon that is most in the stockpile.*

Well, it's true of all of them.

Of any of them, and there's something about the physics—there's something about what it's coated with, in some sense, a very thin layer of uranium, I think, or something. Now you're not talking to a scientist. But this is not reliable over time and so—

Yes. Well, a lot of things and—you put material A next to material B and sometimes they start talking to each other, you know, or—

It also mentioned rust, as well. So that's interesting, that you as a person who spent her life doing calculations doesn't think the calculation option is a valid one for the treaty.

I think a lot of people in the business feel—a lot of calculators in the business feel that way.

But there must've been input from your part of the world into the treaty.

When politicians get their gander up and want a treaty, they're going to have it. And for whatever the reason, might or might not be. That's always been my attitude. My dad always used to say, `If somebody wants to find out something about you or wants to spear you`

the wrong way, they'll do it. Don't bother fighting it. Go someplace else. And I kind of feel that way, in a way, about some of these treaties. People want them if the time is right, if the politics is right, the rest of it. Who cares what the science says? It doesn't matter that much. You know, the merry-go-round comes around, and I think it's coming around again.

I'm going to stop—

[00:58:42] End Track 2, Disc 2.

[00:00:00] Begin Track 2, Disc 3.

And you were saying about your—

Went into Program Development.

Program development work. OK.

Yes, and that's where Rosemary and I started working together. She and Don Kerr had a falling-out and so he set her up as associate director or assistant director for Program Development. So we spent, I'd say, about two, two-and-a-half years bebopping around the country, trying to get Defense Department contracts. I was particularly trying to sell the capabilities of G-Division, which was Geosciences Division, to the Department of Defense. And so we were trying to sell things like some of our capabilities in what we call site assessment or site evaluation, the geology, the geophysics, things like that. Had one thing called Thumper where we were going to thump on the ground to send signals in the case of electrical communications being not possible. And just had a lot of fun in these different things. And brought in a fair amount of money.

And you were saying they sent you for your master's where?

OK, the laboratory has—I don't know if they still have it or not, but they would select five people every two years, and you would be enrolled at UNM [University of New Mexico]; you'd have to take your graduate school entrance tests and all that kind of stuff, but they took care of

everything else. And so they would pay your tuition. It was the [Robert O.] Anderson School of Management, and it was a two-year program, and you took six graduate units a semester, three semesters a year. So you got to put in your [time]—and what you did is you went down every other weekend and attended school Friday afternoon for one of the classes, and then Saturday morning for the other class. And they'd put us up at the Sheraton down here in town [Albuquerque]. And it was a very good program. I learned an awful lot out of the program. It was a very good experience. And so you got to work your nominal sixty, eighty hours a week plus your extra twenty, thirty hours, forty hours a week on your graduate work. And Larry was a prince through the whole thing. He really was. I kind of went out of my skull a little bit. The lab never really used my experience in that that much, but it sure has helped me in investments. And so I got something out of it. If they didn't want to take advantage of it, that's their problem.

Things started getting—I don't know, like I said, it just wasn't the fun that it was in the past. Things got so petty about where'd you spend this day, where'd you spend that day, and what's your time card, and so much effort was put into meetings. I went into the associate director's office once with a group of guys and we're trying to get a proposal out. And these were young guys, Ph.D.s, but hadn't really developed a reputation yet. And before the associate director would let the proposal out of the laboratory, he had to stuff it with his cronies. And I don't know, maybe it helped him get the contract, but things like that just rubbed me the wrong way. And there were things like that that we just decided life was too short to put up with it. Larry could retire from the lab at that point, and I wanted to see something else besides just the laboratory, so we left and went to work for a little company called R&D Associates. They had started up an office here in Albuquerque. Their main office is out in California. I'm sorry, California Research and Technology.

California Research and Technology.

Yes. Their offices were out in California. And we stayed with them for about a year, and we were doing calculations and also program development, trying to get some contracts and things. There's nice guys in the group and we enjoyed our work there, basically, but it was just kind of run like a Boy Scout troop.

And what does that mean exactly when you say that?

[00:05:00] Come on, guys, we're going to go do this now. Well, that's OK but there wasn't the room for flexibility or being yourself or being able to do the things that you thought were important. They didn't supervise you to death because neither Larry and I can take that, everybody knows it, but I don't know, it just didn't click. And there's situations like that. And we're not group-think people.

So then we went to work with R&D Associates. John Lewis hired us into there, essentially. Oh, and what goes around, comes around. Chuck McDonald was there, too, the guy who had Larry go out and interview the women, OK? [Laughing]

Right. Now where is this place located?

It was—do you know where the marina is in Los Angeles?

Marina del Rey, yeah.

Marina del Rey. They had their offices there. It was really kind of a dilly to go out there for a couple of days. They were a prime contractor to—or it was called the base contract, and it was a five-year contract, usually—what you did is you supported the people at the Defense Nuclear Agency [DNA]. And they were opening up a new office that was only about a half-a-block from the Defense Nuclear Agency, and they wanted some people back there to man it. And so Larry and I said we would spend time back there, so we had an office here and one in Virginia.

And worked on some pretty interesting things. One of the things we did that I was involved with was a large test called Misty Echo [12/10/1988].

My primary job for a number of years was to support Major Pelkey. Major Pelkey out of West Point, was going to be a general, which I'm sure he's going to make, and he was one of these no-nonsense guys that, you know, this is the military. They come into this technical community of all these contractors who would go flaky at times. It was kind of fun to support it; because I'd tell them, now this is how this guy is going to behave, and here's how this guy is going to behave, and OK, Bud Pyatt's going to give a talk. Now Bud is allotted a half-an-hour. I said, Bud has never talked for half-an-hour, so beware of that, Pelkey. He says, OK. Old Bud Pyatt gets up and talks about his calculations and all of this stuff. Half-an-hour is up. Pelkey says, Thank you, Dr. Pyatt. Next! Bud is just sitting there with his teeth in his mouth, what are we going to do now, you know? He was really a fun guy to work with. I really enjoyed working with him.

And it was a great big hemispherical underground cavity.

Misty Echo. Was this at the test site?

Yes, it was at the test site. It was ten kilotons, and it was at the end of a tunnel, we had a big hemisphere. In fact, the whole thing was a 30 foot diameter sphere originally. And then they put all kinds of instrumentation down below the ground and filled it in with grout. And so half of it was air and half of it was ground-filled instrumentation, so you could measure the ground shock as it came along. And that was a lot of fun. We worked on that for a couple of years, on the calculations and the instrumentation.

And then we got involved in determining the yield of foreign tests, and our own tests, too, and what kinds of instrumentation you use. And they had these joint verification exercises

where the U.S. would go there and they would come here. And I was never involved in one of those exercises *per se*, but we got a lot of the data from it and helped design some of the stuff that would be used on it, and then tried to deduce the yield from the data that was developed or obtained. And so we were kind of cozy with some of the people from Los Alamos in addition to the DoD people, trying to determine the yield and how can you **[00:10:00]** do that in a couple of hours after you get the data, and tried to automate all that and things like that.

That's great. Now I'm going back a little bit in history. Determining the yield of foreign tests must've been something that had been thought about for a long time previously, no?

Oh, it probably got started about the time Plowshare got started or before, because seismic was, of course, one of the main ways of doing it, and still is.

Right. But this work you're doing now is specifically for those joint verification experiments [JVE] where you're saying what can we agree is the best way to determine yield?

Yes, because we would put instrumentation on their shots, and on our own, to see how well we could get that kind of information. And so it was what kind of instrumentation should you put on the shot, so this was all close-in stuff. It was not relying on the seismic.

Right. Exactly.

And so we had, oh, we had stress gauges, we had time-of-arrival cables, we had—those are probably the two main things. But there were a couple of varieties of each kind of thing. And so we would do the calculations to see how it came out and then we would do the calculations to put all this stuff together and see if we can determine yield. I think it was very credible.

So you're working for R&D Associates, has the contract with DNA, and you work a good deal of time back in Virginia with is Major Pelkey, and that's what this experiment was about.

Oh, the Misty Echo was with Pelkey, but the JVE was after Pelkey left.

OK, so the JVE was after Pelkey left, but it's still R&D Associates, is that right?

Yes. And still through the Test Division of DNA.

Right. So does that require you to be back East, too, or is that mostly here that you're doing that?

Oh, we spent probably two-thirds of our time back in Virginia, I would say—well, between half and two-thirds, and then we were on the road at least a week a month, and then we would be here less than a week a month.

Interesting. How interesting.

And we were fortunate to be able to be together a goodly part of that time. We would have either a trip to a similar place or we'd meet up someplace or another for a weekend or something.

But he's working for R&D, as well, Larry is.

Yes.

Just doing different kinds of things.

Yes. We shared this office with four other fellows, three retired colonels who had been involved in DNA stuff and one lieutenant colonel, but he only made lieutenant colonel because he got a Ph.D. Now Giller is a case in point. He's a two-star [general] and has a Ph.D., which is quite unusual.

Yes. He told me on the phone that he had his Ph.D., but he got it early, I think, didn't he?

Oh, he got it back—

Right after the war, as I understood.

Yes, when AFSWP [Armed Forces Special Weapons Project] first started.

Right. I'm looking forward to talking to him. OK, so then what happens?

Well, then Larry says, I'm getting too old. I'm going to retire. So he retired in November of '93 and I retired in December of '93. And had some consulting contracts afterwards, and I'm now on a consulting contract through a company called ITT. I don't get paid at the present time, but it enables me to maintain my clearance and go to the base and use the archives and things like that.

How interesting. Can you say what kind of things you're consulting on or—?

The book. It's a mechanism for me to do the book. [Book on Nuclear Testing in Nevada being written by John Hopkins and Barbara Killian].

It's a mechanism for you to do the book. How interesting!

Yes. I'm not sure who pulled what strings, but somebody did.

So you maintain your clearance; you can do your work. So that's a good segue. Let's talk about the book. How did that come about? I haven't really talked to John Hopkins about this. I just met him when he was introduced to me as someone who was working on a book about the test site.

Well, I can't really give you how he first got the idea but—

I can ask him, but you.

Well, what happened is that Larry had his stroke in 2000, August of 2000, and they were down **[00:15:00]** here, they had come down, I think she was having her early cancer thing.

So the Hopkins came down.

The Hopkins came down. And we had traveled with them after we retired, after we both retired, we'd done a number of trips with them. We did the Silk Road. We did the Dodecanese and renting a boat in Athens and Greece and stuff. We just really had some marvelous vacations and marvelous times with them. So we've been quite close friends. And so they had stopped on

one of their trips down here and John said, Well, we're thinking about doing a book. And at the time, I thought Adele was going to be involved more than what she is. I think she probably would've been if she hadn't been struck with the cancer. So I said, Well, let me think about it, because things were pretty hectic for me when Larry first had the stroke. When he came out of the hospital, he was essentially a vegetable. He could feed himself and that was about it. He recognized a lot of things but he still can't really talk; but he's getting back a lot of his mobility and everything. So I was really debating. I said [to myself], Barbara, you're stupid if you don't. So I told him, Yes, I'll do it.

So we just started doing it. And I knew I would learn a lot by doing it because I didn't start at Livermore until, what was it, February the 1st, 1958. And so all of the atmospheric stuff, or most of it—I mean it was just [Operation] Hardtack II at the test site—had been done before I arrived on the scene. And then even when I did arrive on the scene, I wasn't involved in the test program for a number of years after that. So I knew I would have a lot of learning to do, but I thought it would be fun to do.

And I just have always had this special spot in my heart for the test site. It's just such a unique environment. I don't know, I can't quite describe it. It's kind of like going to the ocean. It's a whole culture, at least it was in those days, and the whole situation was just so unique. It attracted me a great deal, and so I wanted to learn more about it.

So you wanted to learn more about it. I mean that's a good reason to do anything, right, to learn more about it. So what kinds of things are you learning at this point that are standing out in your mind?

Well, the tremendous effort, and John is kind of realizing this, too, that both the Department of Defense and the Civil Defense, FCDA, Federal Civil Defense Agency, put into the site, and the

kinds of things that they did in those years. And then those Desert Rock exercises. The whole thing was just—it was a massive kind of thing, and what the labs did was exceedingly important, but it was—there weren't that many people involved, maybe a few hundred. It was the driving force and, of course, they had the responsibility of detonating the weapons and things like that. And the kinds of experiments that the labs did were very interesting and the kinds of data they got and everything. But it was a much, much larger operation than just the laboratories, by far. And the kinds of things that were done, and the amount of things that were done, repeatedly they would expose tanks and airplanes and the foodstuffs and clothing and animals—animal, vegetable, mineral—everything. It was a whole new area, a whole new field that we knew nothing about. And it just required a lot of us. And it shaped a lot of our culture—those old movies of the houses exploding and the mannequins and all that kind of stuff. It was just an era that I didn't **[00:20:00]** really appreciate; what was behind it all; how it was all set up; who was in charge of it; how much was involved; how rapidly they could do things. President [Harry S.] Truman got fed up, I guess, with all this hanky-pank about where the test site was going to be and why and the rest. And he said, `Just go out there and do it.` And that was on December 18th [1950].

We fired our first nuclear test, what was it, January 27th [1951]on [Operation] Ranger. Over the holidays, too. You're talking about a month. And the kinds of things we would do and how the site was developed between Ranger and [Operation] Buster-Jangle in the fall of '51. From February of '51 till the fall of '51, I mean they had all kinds of stuff out there in terms of facilities, places. There were places to sleep, mess halls, installing all the equipment for the experiments, and the whole nine yards. They did that in like six, eight months. And try to do that today, my God! You know, people knew it was needed, people knew it was wanted, the money

was there, [they said], let's go do this job, and everybody just did it. It wasn't, Is my division going to get this next seventy-five thousand dollars or are they? And you go to these meetings today: [People say], No, I can't do too many of those tests because it costs seventy-five thousand dollars. The big blast simulator in the South. For the big Humvees that they tote troops around in, in Iraq. Well, my God, you lose one of those, you lose a lot more than seventy-five thousand dollars, but they're sitting there squabbling about seventy-five thousand dollars to do a test. It wasn't that way then.

That's interesting to me that you as a person involved in the testing at the labs, even though it was later, are learning a lot about the same kinds of things that I'm learning about as I do this project and people talk about those early days and what was accomplished and what the culture was like.

Yes, it was—wish you could find it more often.

Today?

Yes. Of course, I guess every generation feels that way.

I guess. My view is that it not only reflects something at the test site, I think it was something about that era in this country in general, is my theory. The test site is unique, but I also think it's something that was happening many places postwar.

Yes. The World War II veterans are a really unique generation, and it's all part of that, I think.

I agree with you on that. So you're moving through sort of era by era and you're looking at different personalities, and your goal is to have this book, as we said before we went on the machine, something that is available to the general public?

Yes. Well, to some extent, but I wouldn't say we're getting too much into the personalities. [Norris] Bradbury did so much, so Bradbury's personality probably comes across a bit. That's a facet we're not really, I think, emphasizing too much, although we try to make comments about

people and things like that. But we're kind of trying to go from looking just very briefly at Trinity on up to the acquisition of the test site, that's the first part, and then the second part, going through the operations on up through the moratorium.

Right. And so you have access, then, to archival material that's still classified, and then you can take that and rewrite it so that people can know about it, is that how that works?

I do, on the base [Kirtland AFB]. I do not have that capability at the DOE labs.

Oh, you don't. It's just at the base.

Because you have to have a Q-clearance. Now John might try to get me a clearance through Los Alamos, or maybe through the archivist up there or something, we can work something out, a **[00:25:00]** year or two. I just found out that I really couldn't get access. I can get some stuff through the open net and whatnot, and I've been milking that to the point I can.

Right. But there is an archive I know. So you're talking about the archive here at Nellis—not Nellis, at—I'm in Nevada still.

Kirtland [Air Force Base].

At Kirtland. That's right. I'm in Albuquerque now.

Yes. In fact, primarily—well, there was AFSWP and then it was DASA [Defense Atomic Support Agency] and then it was DNA. It's those archives, yeah, which was the main military contractor.

Well, I'm sure—this has been a long time and I don't want your brain to get completely fried, so before we stop, was there anything you had thought of, talking to me about today, that we haven't gotten to or anything that I should know for the future?

Not really, except that I've been very fortunate in my life. I think that any woman who thinks twice about going into science, just go do it. And that whole arena has changed. You see more

women in the classes, you see a lot of that kind of thing today that you never saw before. And the big thing is, just do what you can do and do it the best you can do it. And to play games and say, well, I'm a woman *versus* a man, or to play games and say, I should be treated this way rather than that way, I don't know, it gets too complicated. I can't play poker, you know [laughing].

Well, that's interesting, and especially in the light, you know, you must've had some thoughts about this whole thing that happened with Larry Summers at Harvard when he made the comment about women not maybe having the same whatever to be able to do science, and then—

Yes, he just shot off the hip. I mean people do that.

But then the whole—there's been a lot of stuff in the paper since that from various women in science and those kinds of things. So you're saying you didn't find the—there's obviously the question of sexes and politics in a "male field." But you're someone who negotiated that somehow, so you didn't find it to be a block.

I didn't dwell on it. I felt, OK, I'm a woman and I'm a physicist. That's just the way it is. Just go do the best you can. And there was discrimination in a way, but I don't know, I was just too busy, too involved in what I was doing, liked what I was doing so much that I didn't want to sit around thinking about that kind of nonsense. I'd rather be out solving another problem or running another calculation or thinking up some new silly idea than to sit around and say, `Yeah, well, he talked to me wrong, or he got ten dollars more than I did this month, or 3 percent more raise, or whatever`. The thing you have to do is you have to make sure that they know you're interested in money. And I made that very clear before too long. And once they think, Oh, you're just a woman and you're not that interested in money because you got a husband working; well, I tried to straighten them out on that point real quick. And you have to do that or you're going to get passed over, or nine times out of ten you will, or

at least that was my experience. But if you do the best you can, if it doesn't work out, go someplace else. I mean, I don't know. Life's too short to get yourself in a box.

That's great.

I can't live any other way.

[00:29:19] End Track 2, Disc 3.

[End of interview]