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

Interview with Louis Wouters

May 20, 2004
Livermore, California

Interview Conducted By
Mary Palevsky with Carol Gerich

Oral history is a method of collecting historical information through recorded interviews conducted by an interviewer/researcher with an interviewee/narrator who possesses firsthand knowledge of historically significant events. The goal is to create an archive which adds relevant material to the existing historical record. Oral history recordings and transcripts are primary source material and do not represent the final, verified, or complete narrative of the events under discussion. Rather, oral history is a spoken remembrance or dialogue, reflecting the interviewee's memories, points of view and personal opinions about events in response to the interviewer's specific questions. Oral history interviews document each interviewee's personal engagement with the history in question. They are unique records, reflecting the particular meaning the interviewee draws from her/his individual life experience.

Produced by:

The Nevada Test Site Oral History Project
Departments of History and Sociology
University of Nevada, Las Vegas, 89154-5020

Director and Editor
Mary Palevsky

Principal Investigators
Robert Futrell, Dept. of Sociology
Andrew Kirk, Dept. of History

The material in the *Nevada Test Site Oral History Project* archive is based upon work supported by the U.S. Dept. of Energy under award number DEFG52-03NV99203 and the U.S. Dept. of Education under award number P116Z040093.

Interview with Louis Wouters

May 20, 2004
Conducted by Mary Palevsky with Carol Gerich

Table of Contents

Interview with Louis Wouters

May 20, 2004 in Livermore, CA
Conducted by Mary Palevsky with Carol Gerich

[00:00:00] Begin Track 2, Disk 1.

Louis Wouters: My name is Louis Wouters. I was born in Antwerp, Belgium, October 29, 1921. My father had an occupation that required him to travel a great deal, and he was fortunate enough to take us along. I was the only child so that wasn't too difficult. In 1930 he decided to settle in the United States, in California, and I went to grammar school there in Oakland where we lived, and eventually made my way to the University of California to get my bachelor's degree.

My interest in physics began in the mid-thirties. I was especially taken by the media reports about Ernest Lawrence's cyclotron. So when I went to Berkeley I spent a little time nosing around the physics department and looking into some of the things that the physicists at that time did. And finally I was brave enough to go and see Lawrence and suggest that maybe I could spend the summer working around the lab, helping around. He was able to arrange that, fortunately, and my association from the lab began in the summer of 1939.

Mary Palevsky*: Let me ask you just one question about the cyclotron. What was it, as a young boy that you were looking at this stuff that caught your attention?*

Oh, because I had developed an interest in amateur radio and electronics, and I had done quite a bit of ham work myself as a boy. Well, not a boy, but a young teenager. And when I read about the cyclotron and its radio frequency equipment and other electronics, that kind of turned me on. And also the business of radioactivity, that was very interesting to me. And so these interests converged for me.

And so when the Second World War started, I had been helping around the cyclotron and I had already developed some ideas which were beginning to be used, and so I wasn't a "gofer" anymore. [laughter] So I got transferred to the Manhattan District Project *right* after the war started, when I was still in my senior year, and I went to full-time. And the university very conveniently altered its requirements for graduation so that I didn't have to spend too much time in classes that last semester.

What do you remember about learning about Pearl Harbor? Do you remember that day? People describe it—

Oh, I was studying for finals in December, in early December, because back then the university was on a, what shall I call it, a six month—

Semesters.

Semester, two-semester basis. And I was in our living room, had what at that time passed for stereo, it wasn't really stereo but hi-fi, and I was listening to some classical music. As a matter of fact, it was a really beautiful day that day, because we lived up in the hills there. It was warm enough that we had the front door open so we'd get fresh air coming in. And all of a sudden the music was interrupted and they announced about Pearl Harbor. Well, that really, you know, turned everything around. I realized right at the moment, boy, we're going to be involved in this in some way at the lab. I just knew that right away. Well, I was already aware that they were working on fission at the lab, that they were doing basic research work on fission, and at that point, I would say within days, Lawrence converted his thirty-seven-inch cyclotron into a mass separator, a mass isotope separator. And I went to work right away on that program and stayed with it through the war, developing the Calutron, and then I was transferred to Oak Ridge in 1944 to assist in getting the manufacturing plants, the separation plants there at Y-12 [Oak

Ridge] getting them running, which was a very intense and exciting experience because for one thing I'd never been away from home for that length of time. And so, let's see—

[00:05:00] *Let me ask you about Oak Ridge for a second. I know that we were just talking to Duane Sewell about it this morning and I've spoken to Herb York. Did you know either of them there?*

Oh yes, sure.

And you had known them back here too.

Oh yes, sure, same program. Oh yes, I had worked closely with Duane Sewell up on the hill when the 184-inch cyclotron was converted into a Calutron test bed. And Herb York, I can't remember when Herb York came on, but I know that he was at Oak Ridge with us. Where he was before, I can't put my finger on.

So you did work at Oak Ridge?

Yes, and when the war ended I transferred for one year to Tennessee Eastman, who were the basic contractors that we had been working with there, mainly to make a little more money so I could get back to graduate school.

What do you remember about the end of the war?

Well, the people at Y-12, the scientists, were not completely in the know, so to speak. There was a very strong need-to-know separation there in the Manhattan District Program. As far as I know, Bob Thornton, who was the head of our group at Oak Ridge, was the only one who had a good knowledge of Los Alamos and why it was there and what they were doing. I didn't know about that until afterwards, of course. But we were aware that there was an organization like Los Alamos mainly because of the weird shipping notices that we got for equipment being moved. It

wasn't anything consistent; occasionally we would hear about it, and we concluded—that's a whole story of its own. I don't want to take too much time—

That's OK. Yes.

We concluded, yes, we concluded there were several other sites. It was clear that there had to be, because we knew perfectly well that the product from our plant was going somewhere, that it wasn't the finished product. By the end of the war, we were pretty well aware of what it was going to be used for. I would say by the last year that I was at Oak Ridge, that the scientists, the young scientists there at least were aware, and we may have discussed it amongst ourselves but we really kept our mouths shut about—

That it was for a bomb?

Well, for some kind of a military device, probably an explosive, but we couldn't be certain. We knew that calculations of that kind had been made even before the war started. How much of the isotopic material was needed to get a runaway reaction. A question of whether that would be explosive or not had not been answered. The thing might come apart before it could detonate, really, just from the heat. But you must remember that these are very dim recollections that I'm talking about here, OK?

I know. That's all right.

Well, as I say, I'll pick it up. I spent a year in Oak Ridge. I had contacted the lab at the end of the war about coming back as an assistant, and this is a rather interesting little side story because it does bear on how these matters go in times of national emergency and how they turn off and what happens afterwards. Everyone went home, assuming that that was the end of things, just like they did in the twenties. Well, it turned out not to be the end of things. When I asked the lab in 1945 if there was any possibility of coming back as an assistant on the hill there, they were

very dim about it. They said, `Well, you might be able to come back as a graduate student helper, but as far as the kind of a job that you had even before the war`— they were contemplating that things would be quite tight, that they wouldn't have much of a budget for a while at least.

[00:10:00] So that was one of the reasons I stayed in Oak Ridge, because the TEC [Tennessee Eastman Company] people, especially the head of the department there, had gotten to know me. He wanted me to stay there in the worst way. He wanted me to come to work for Tennessee Eastman full-time and they were willing to even fund my graduate studies at the University of Tennessee. There's some background as to why this is, and I'm not sure if it's classified or not, so we won't go into that.

Then a year later I called Berkeley again and they said, `Yes, we'll be very interested in taking you on as an assistant.` Talk about a change in attitude, I'll tell you! And that was entirely because in the interim people had, Lawrence especially, had become aware of the risk that the Soviets posed. People have questioned Lawrence's views about the Cold War and about our competition with the Soviets. I had the opportunity on several occasions, especially one that I'll tell about later, to get a pretty good idea of how he felt about that, and he was a very, very intense patriot, believe me, he really was. He put the lab's work ahead of everyone else's at every stage of the game. He was that foresighted.

Of everyone else's in what sense?

In the sense that he would risk, to some degree risk, changing the lab's course and shifting people to programs that had not yet been approved, that had to do with, for instance, the thermonuclear later on. Not at the time that I went back to Berkeley but several years later. Part of this was because he had fairly close contact with the AEC's military representatives in Washington. So he was pretty much keyed in on what the latest data information was on our

military status, the nation's military status. Please, I often use the words "we" and "our" in a national sense as well as a personal and local sense, so you'll have to sort that out.

OK. Thanks.

Well, when I got back to Berkeley they were very strongly working on converting the cyclotron to higher energies. There were people interested in studying the nucleus more strongly. And Lawrence also was working with some military people on radiation detectors, and in 1947 or 1948 he asked me to look into crystal counters and scintillation counters, and so I began specializing in that.

First time I was called to Washington was, I think, in late 1948, right after the Sandstone tests [Operation Sandstone, April/May 1948]. The Navy found that they were in woeful shape when it came to nuclear radiation measurements. Ernest Lawrence heard about that and he said, `Go to Washington to this meeting and tell them about scintillation counters,` or words to that effect. So there I came to Washington, walked into this meeting which was a panel of admirals and generals and—

And you're less than thirty years old at this point.

Right. You don't forget little things like that. Actually I can't remember much of what was said or what I said, but the gist of it generally was that they needed ideas and more research and quick research on radiation instruments. And shortly after that the AEC formed this photomultiplier steering committee, and I was appointed to that. And that was my first major undertaking, so to speak, outside of the lab because the work we did on that, that I did when I was at the lab, was partly coordinated through that group.

OK. So let me see that I understand this. After Sandstone, which you weren't involved in—

No, no, I wasn't involved in it at all.

[**00:15:00]** *—there's this realization that that there's not an ability to measure radiation accurately, quickly, whatever, and this is a technical crisis that needs to be handled. So at that point you're working no longer simply as a lab person but on a national level with the AEC.*

That right. That's right. Now, all through my career I've had that happen to me in a number of cases, and it isn't until quite a bit later that I recognized just what you said, that I was working both nationally as well as at the lab. But let me continue. The question of how the lab got into the thermonuclear business in Livermore has been reviewed by many people. And one of the interesting things is that when I read those versions I see many places where the authors disagree with one another, and me, disagree with me, or I disagree with them, maybe.

Right. So you'll give us your version?

Yes, I will do that. Not here of course but—

Why not here?

Well, that would take too long.

Oh, OK. Well, can you give me a short version? Because I'll tell you what my personal interest is. Because as a researcher I'm reading a lot of those various accounts, and I think it's very valuable to have the viewpoint of someone who was here on the ground, not just from the scholars. You tell me what you want, and you don't have to tell me what you don't want.

Well, one of the things which I find a bit of a shame is that Ernest Lawrence is by far given too little credit. There is no question in my mind that he had a major hand in setting up this second lab. What the relationship was with Edward Teller's initiatives, my own personal opinion is that he gets more credit—no, that's not really what I should say. The credit is not shared enough with other people. That's probably a better way to put it. He certainly deserves a lot of credit, but there are a lot of other people that deserve credit too, like [Stanislaw] Ulam at Los Alamos. In

my judgment he probably was the key figure in making the whole business go. But that's a personal opinion. You know, other people may disagree with that.

Then there are these stories about the relationship between Los Alamos and Livermore in the beginning and how Los Alamos interfered or tried to change the business of Livermore forming, and I can't accept those.

Really. Why not?

Because the intensity and the drive with which Livermore was set up was just too great. Good God, we were going like crazy to get this thing going! And that was back in 1951 before we even moved out here. Back then, we weren't quite aware of where we would be, but that we would be doing this. Lawrence was completely committed to the idea, as far as I could tell. He had a small team of men, of engineers and others, in Berkeley examining possible sites as early as the winter of 1951-52. That you don't read about or hear about. And there's an example of what I meant by saying that he committed himself ahead of time, OK? The point being that he wanted to be *ready* for whatever decision was made. That's really what was behind that. It wasn't that he knew we were going that way, but that was one of the options. And so he wanted to be ready for that option. That's the option he preferred and he wanted to be ready for it.

And that option was that you all—?

To set up Livermore.

[00:20:00] *Oh, here you would begin designing nuclear weapons, developing, that's the option?*

That's the option. In *my* judgment, part of the reason for that was because Los Alamos had an excessively academic approach to things. And there were a number of men, military and civilian like Lawrence, who felt that that we would be dragging our feet if we approached the problem

that way. Livermore was as much to get *Los Alamos* off its butt, if you don't mind my putting it that way, as it was to do the thing ourselves.

Now I'm going to interrupt you a little here because this raises a question for me, because Los Alamos was the place where the A-bomb was successfully built during the war.

Oh yes, sure. Yes.

So was there a shift postwar that made Lawrence make this judgment?

Oh, oh absolutely. Well, as a matter of fact—

Or why would Lawrence make this judgment? Historically I'm trying to sort of understand it.

[Sigh] I know. I'm tempted to say that Los Alamos dragged its feet during the war, but that's not true.

Well, it could be.

No, there were times when it had that appearance, but I really do think it was because the technology and the science were so new that we were going to stumble around before finding the answers. In retrospect, of course it's clear, this is the way we should do it. But my gosh, how many things in life, in human experience, have been that way? In hindsight? But with the thermonuclear, there were a number of paths. And you know, recently I was reading this critique by Hans Bethe. I don't know if you've seen it.

I haven't seen it, but I know Hans Bethe very well.

In that paper, he tries to tell about the designing of the thermonuclear, and in order to do that he comes up with a set of schemes that they had at Los Alamos, and I forget the exact words, but they were Scheme A, B, C, D, E, and so forth. You've heard of that.

Is this his paper he did back in the eighties?

Yes, 1983.

Yes, I know that paper. [Hans A. Bethe, "Comments on the History of the H-Bomb," *Los Alamos Science* 3, no. 3 1982]]

And that was a repeat of a paper he had written earlier in 1956.

That was classified, right.

That was classified. OK. And then one of them was broken down into Part One and Part Two, I read that. That really got me because when you look at it in retrospect, and I hope I'm not getting too sensitive when I say this, that the real thing is that there were many people, and that what you do in the end is a mix of these ideas and these paths and all of that. And the way you mix them depends on what kind of a weapon you're interested in, what kind of an end product you're going to put together. And so this is one of the reasons why I was saying earlier, well, you know, it's a whole bunch of people who should really get credit. Teller's main contribution was that *he just didn't let go*. He kept going with it, kept on and on and on, and Lawrence was—some people may say he was riding Teller's tail, but that's not true. Lawrence was pushing just as much in his own way, on his side of, shall we say, the concrete side of it, the business of actually doing something.

Carol Gerich*: Louis, Bruce [Tarter, former LLNL director writing the lab's history] and my research backs you up a hundred percent. We think Lawrence is not given enough credit and he was the real driver. Without him there would be no lab.*

I'm sure of that. Yes. I didn't think I was going into *this* stuff. I find it interesting to be able to tell you this, believe me, and it is—

Mary Palevsky*: Yes, and I think it's interesting and I think it's important.*

And there are people who probably disagree with me very strongly, what I'm saying, you know, but this is my perspective, this is what I get out of it.

[00:25:00] OK, well, once we had chosen this place and Lawrence had said *go*, from there on it was a bunch of young guys who had never been influenced by ancient academia. Ah ha! Many of them had just come back from the war and had seen how bad things can really be. They came from backgrounds that have been *lost* today in our society, unfortunately.

Such as?

Family. Excuse me for being very conservative and very ancient about it. Religion, to a certain extent certainly. Respect for the individual and for society and for the role of civilization, of civilized living. And as I say, they had seen the worst of life and they weren't going to allow that to happen, and the way to do that was to get going and work and make their contribution. And boy, I'll tell you, it was a *great* ten years in the beginning of this lab, great ten years. By the time we got to about 1965 or so, bureaucracy began to overwhelm us.

OK, so let's take some time to talk about those first years, then, because I think that's historically really valuable for us to understand better. So you've got not only people who had the privilege of not having to go to war, but you've got people who had seen the war. I'm going to phrase this question in terms of what you just said about hindsight. Looking back, we now see that the Cold War went on for a really long time. And one of the ways I've been beginning to think about the contrast with nuclear history is that I think everyone expected, working on the Manhattan Project, or hoped *that that single weapon would either early on deter the Germans from using it—we won't get into that history—or end the war quickly once the German defeat came. When you get into the Cold War and the development of nuclear weapons, you've got something more open-ended—*

Let me give you a little tip here. One of the things that puzzled me and has puzzled many other people is how the Soviets behaved after Berlin. They didn't get into Japan till the very end, with

a small landing party at the tip of the island there. I think Stalin was very disappointed that we had managed to discover and make the atomic bomb and display it, because I suspect, and this is a suspicion on my part, that if we had committed this half-a-million troops or whatever to an invasion of Japan, it is a distinct possibility that he would've decided this was a fine opportunity to take over Europe. The thing that inhibited him from doing that was the atomic bomb. And there are little clues here and there which I happen to think kind of indicate that. Take it or leave it.

Yes. What's an example of one little clue?

His behavior at Potsdam. The things that he said and the way he responded to Truman when Truman told him about the atomic bomb. The fact that they did not move large troops across Siberia to the Japanese side. Yes. And, well, I can't say—those are the main ones, OK?

Correct. Thank you. That's all I'm asking for, are the main ones. So that's to my question about the Cold War threat. And you were going to say something about Berlin. Stalin and Berlin. How he behaved.

After Berlin.

After Berlin.

Yes. Oh, excuse me, that might've been a poor choice of words there. What I meant by that was after they had occupied Berlin, then that was the end of the war, of the European war.

Oh, I see what you're saying.

[00:30:00] Yes, OK, all right, I'm sorry.

After Berlin. Got it. I'm thinking Berlin during the Cold War.

No, no, no.

You meant once Berlin was occupied during World War II, then their behavior in the east, in the Pacific. I understand.

OK. So we were talking about the early days of the lab. You moved out. What year is that that you moved out to here?

1952. September 1952. Where do we go from here?

Tell me a little bit about the things you were working on early on when you come out here.

OK, I'll tell you. Let me—I think I know where we ought to start. My first expedition to Nevada Test Site was in March of 1953, and it's something that I won't forget because I took the train. Got that, Carol?

Carol Gerich*: I got it. There was an argument with Carothers in the oral history about that, if you remember.* [See LLNL Archives 1983 oral history of Louis Wouters by James Carothers, Item ID# 2582.]

Mary Palevsky*: I read it. I did.*

She was talking to me about it earlier.

I read it last night. He was saying you couldn't have taken the train or you shouldn't have taken the train.

Oh no, no, no, other people took the train with me. I wasn't the only one on that train.

I know you weren't, yes.

Carol Gerich*: Yes, that's right. No, but Jim Carothers was saying that you were alone.*

And I do remember that when we got into the car—there were several cars to pick us up and take us out to the test site at the old Las Vegas railroad station—I do remember that when I got into the car, much to my surprise and not exactly shock but kind of wariness, I sat next to Edward

Teller. He was going out to the test site on his first visit. And that's what I remember of that first trip.

Mary Palevsky*: So he was an intimidating—what kind of figure—?*

No, no, he slept most of the way. He was tired. I was tired. I think we both slept most of the way.

But your impressions of him, if he hadn't been sleeping, I mean he was—

At that time?

Yes.

Neutral.

Neutral.

Neutral, yes. That's the best way I can say. We had been exposed to him quite a bit here, and I told this story to Carol [Gerich] before, and that is that when the lab got started we needed a lecture hall, which we didn't have. There wasn't any place big enough to really handle fifty or sixty people. We ended up in the high school auditorium in Livermore, . And Teller told us all about the thermonuclear business and the designs and all that, all highly secret stuff, and all there were were two guards at the two doors of the Livermore High School auditorium. It was that casual and informal back then. No one would dream of bugging the Livermore High School auditorium if they weren't told ahead of time, you see.

That's right.

Carol Gerich*: And when I interviewed Edward Teller I asked him about that, Louis, and he said, I may not talk about it.*

Really? That's Edward all right.

Carol Gerich*: He still would not talk about it.*

No kidding. I can't find that very secret or anything. My goodness.

Mary Palevsky*: Well, one of the things you did say in the Carothers interview that fascinated me, and you may have nothing more to say about it, but you may, was your realization of how easy it was when you realized it, how it is to make, I guess, an atomic weapon or a thermonuclear weapon or both?*

To put it together. Well, certainly the A-bomb was not difficult, in hindsight. I won't go any further into detail about that, but yes, it seemed to me easy. I certainly didn't have any problems with the analysis and the arithmetic. It was not that bad. But of course my main address was in making measurements in nuclear radiation, prompt radiation reaction history, that kind of thing. That's the main reason I was transferred out here, was to assist in setting up the diagnostic programs.

Right. Now as a layperson I have a question about that whole realm, this thing you call reaction history, and also maybe because I'm the daughter of an experimental physicist. But reading some of the things that I read from that other interview and other things that I've read, tell me if I'm correct here. There seem to be two things going on. One is the experimental physics where you're really trying to figure out what happens when certain kinds of explosions take place or **[00:35:00]** *reactions take place, for the science of it. And then there also seems to be this—is it separate or do they overlap? The question about what weapons do and how weapons act and how weapons behave. So the weapons tests are themselves sort of experimental physics but* more *because you're looking for certain kinds of weapons to do certain things? That's a big, long, complicated question.*

Well, probably the best way to answer that is to point out that as we went down the road of nuclear testing, the test programs divided into military interests, which were really pointed towards what are called "nuclear effects," and the actual design work, which was the nuclear

designs, the actual weapon designs that we do. Now I have always been very concerned about that because I think this country made a mistake in dividing those.

Really?

Yes. None of the other countries have. In other countries, the design work and the effects are merged as a continuum. And the reason is that I found myself being drawn into the problems of the military and understanding what was going on. They were so far apart and the *need-to-know* had been so constructed that they didn't know where to begin in understanding the nuclear effects business. Sure, the blast effects and the explosions can be modeled and treated as a continuum of the high explosive regime, OK? But you have to make the measurements. If you're going to measure blast and shock and all that, you're going to do it in a nuclear radiation environment, and so you've got to protect yourself. You've got to know how bad that is going to be. Well, they didn't. There was no way for them to know.

Oh. The military?

The military. Well, I mean it was very difficult for them to know, let's put it that way, because of this separation, because this *need-to-know* line had been drawn between what happens nuclear-wise and what happens effects-wise.

That's very interesting.

Oh yes. Oh yes. It was a barrier no one could overcome. The security people and others were hardnosed about that. And the sad part of it is that down the road, it ended up creating an *inverse* effect, namely that when the military labs and military side of the business began to feel confident many years down the road, they began to exclude us, OK? And that was equally bad because, I remember one case in particular, a friend of mine, Pete Haas, who was quite instrumental in the effects area and whom I liked very much. He was killed in an automobile

accident at a very poor time. He once told me, `Look, I really regret not having accepted your recommendation about making a certain kind of electromagnetic measurement in the military tests`. The reason was, he woke up too late to just what I'm saying, you know, that we were aware that there were problems that the military might have that the military could not recognize because they did not know enough about our side of the border.

What kind of problems are you referring to, then?

Problems with radiation interference with their measurements, for example. But I've got to admit that I may be overrating that problem, I don't know, but it certainly was a thorn in my side at that time.

OK, and since this is your story, your oral history

I get to say it.

You get to say it, that it was a thorn in your side, and we take that as one little piece of the puzzle and think about it.

That's right. Well, you know, like I said earlier, no other country has done this. The French merged their programs. The Soviets merged it right from the beginning. I don't know about the other nations, the smaller nations, but—

[00:40:00] *Now, again so I understand this historically, there's a divide. You think that that had its basis in military concerns, or do you think it had its basis in—?*

In security. Oh yes. I believe. I may be mistaken, but I considered that it was kind of an arbitrary decision somewhere. Now it might've been military too, but I have no evidence of that.

I ask, and here I'm on shaky ground so don't take this as gospel, but some of the things I read about, I think [Norris] Bradbury was concerned.

Oh, if you drag Los Alamos into it, yes. Los Alamos wanted to keep the military completely out of the picture. You've put your finger on it.

This is where I'm trying to go. So do you think that was an element in this calculus as well?

Oh yes. I'm sure it was. Yes, I think so. Yes. I said this to Carol some time ago and I'll tell you that in the early days when we were interacting, jostling with each other in Washington. Because you know I told you about the photomultiplier committee, the reason I'm backtracking a lot here, the photomultiplier was a very important element in the scintillation counting business. Half of the equipment was photomultipliers. The other half was the scintillators. We got to Livermore here, we worked on both ends, and I was right in the middle of that. My group did all that—where did we pick that up?

Oh, Los Alamos. If you drag Los Alamos into it—

Yes. When we were in the middle of that, which would've been about '58? Fifty-eight, 1960, that era. I remember I had developed the attitude that there was the United States, the Soviet Union, and Los Alamos. And that's pretty much a measure of attitudes in the nuclear business. They were up on their hill there; they were isolated. You know, every time I visited Los Alamos, it was like walking into a faculty club on a large campus. That was the best analogy I can make. Now, I may be being hard on Los Alamos. They did a lot of good things, no doubt about it. But they certainly projected poorly. They didn't realize that. And Bradbury was terribly defensive. He wouldn't let go of anything. He fought tooth and nail to preserve all of Los Alamos's prerogatives, and add more in terms of programs, equipment, commitments. You know, the first time that we went down to the Pacific, we recognized that we were in a learning cycle there, very much so. And most everyone really simply took it on the chin there, kind of. The best analogy I can give is that when the supply ships came down to the South Pacific islands there, they first stopped at Enewetak, which was Los Alamos's atoll, and there, their harbormasters would get on the ship and pick out all the things that had been ordered for Los Alamos, and then go through

the goodies that belonged to everybody. The ships then went to Bikini, which was our island, and we were left with the dregs. Well, that hurt.

What was it like to be out in the Pacific? When did you first go?

Fifty-six. Redwing [Operation Redwing, 1956].

OK, Redwing. But that was a joint [Los Alamos, Livermore]—

I went down there twice on Redwing, yes, I remember that.

What was that like? What was it—?

Oh, very exciting. Very exciting. I mean I'd never been in that part of the world and that **[00:45:00]** was just interesting on its own. But, well, we had to get our job done, and we had some rather unique methods for doing our measurements which were different from Los Alamos's, no doubt about it.

Yes. Can you say a little bit about how they were different, or is that—?

We were more adventurous in how we made our measurements, the kind of equipment we set up, how we observed the radiation from the devices, things of that kind. Back in '55, we did a couple of experiments in Nevada, and I must say that I had some very nice contacts with people in our "mirror" division at Los Alamos, J-Division. J-Division did the diagnostics there, or designed the diagnostics. Now they had a different way of doing their work there. J-Division would get EG&G [Edgerton, Germeshausen, and Grier] to actually do all the hard field work and they would send just one or two guys down to check on them once in a while. Partly because of York and Lawrence, we decided to do much of our own work. The first time we did any work down there in fifty-three, I was delegated to work with EG&G to make some modest measurements on the two shots we fired there, two little shots. In '55, we had a more elegant layout and by then we had developed a field team of our own that set up the apparatus, with help from contractors, of

course. And that's when I had the most contacts with people like Bill Ogle and Al Graves. Al Graves was a wonderful man. I really liked him. I think he understood the problems that the world had with Los Alamos, as well as the problems that Los Alamos had with the world. [laughter] Bill Ogle was a bit different. He had a lot of perception, because I took him up to one of these tests that we were going to conduct, and without saying too much or getting too classified or sensitive: Los Alamos used to run its cables out from the towers in such a way that the signal would not be interfered with by the radiation, the signals going down the cables. We decided to do it a little differently. We ran our cables down the towers, down underground, deep underground, and out in deep trenches so that the earth was a shield over the cables. And when Ogle first saw that, and we had set up what we thought was a very elegant measurement scheme on the tower. I showed him the detectors and shields and everything that we put up there. When we came back down the tower, he suddenly turned around, looked at that tower: `I see you're doing a cable experiment.` That always stuck in my mind. He saw the point, you know, because that was, in the long run, turned out to be as important, because it was a lot cheaper than the way they were doing it, and easier.

Yes. So, for people who look back at this and read this in the future, what was your experience of the tests themselves, observing those atmospheric tests themselves? I know that the first ones were famously smaller than you all wanted them to be.

I would say that 90 percent of the guys, if you don't mind the expression, standing around in the control room, watching the countdown up on the timer—you know where the control center is out there in the Nevada Test Site—they weren't thinking much about that. They were wondering if they'd got the data they were after. That's what they were holding their breath about, no doubt about that.

Would the instrumentation work?

Yes. Would something go wrong at the last minute? Would there be an abort—which we had a couple of times, when equipment, the interlocks, would trip off and we weren't able to complete the countdown. That did happen on one or two occasions. You have to go and reset everything and check it all out once more, maybe wait a couple of days for the weather. But nevertheless, I **[00:50:00]** mean—of course it was interesting because you knew that you were going to see a bright [flash]—after the second or third shot, you know what to expect and you hoped that everything would go right. If you didn't see a big, bright flash and a delayed boom, why, you knew that something wasn't working right.

Right. Right. Do you acclimate after you've seen—? Some people describe the impact of a large-scale, in this case, fission bomb, seeing those atmospheric tests. Do you get used to it, ever? Does it become familiar, in a sense?

Well, I can't see much difference in this context from high explosive tests. OK? As a matter of fact, the military in the early days when it was first tested, when the A-bomb first worked, their attitude was this was just another big bomb that didn't take as many bombers to deliver. That was a standard military attitude. They weren't aware, and here again, you know, we're touching on the problem that the military had even later, they weren't aware of the risks of radiation and fallout and all that. They minimized that. They saw that as just another side effect with so many other side effects they had to deal with in militarism and in wartime. So I'm answering your question in a kind of sideways kind of way, because I must admit I never have thought much about an acceptance of explosives in this context. If I have any concerns about acceptance, it is an acceptance of why they are used rather than the fact of usage.

OK. Tell me a little bit more about what you mean there.

Simple. We're still two thousand years away from real civilization, as far as I'm concerned.

Two thousand years?

Yes. Because mankind has got to get over its barbarisms and its leaning-toward violence as a way to solve problems. They just got to get past that. And as long as you have cultures that almost *revere* violence, why, you're not going to make much progress. And that's, to me, far more important in the world social structure than just exactly how you kill people. That's one of the reasons why I have not—my wife, for instance, and many other friends are sometimes shocked by my attitude about that. But I don't see nuclear weapons as being *horribly* different from any other kind of warfare. I don't. What the heck difference does it make how you kill somebody else? It's the very fact of killing that annoys me. You've got to get over that. One of the reasons why I felt very strongly about working on the thermonuclear was because I really do believe that it was, it is a deterrent. It deters people from going too far. It's a step in the right direction.

Yes. Well, you raise a lot of really important and interesting, you know, social, human questions—

Let's shut this off for a moment.

Sure. I'll shut it off right—

[00:53:58] End Track 2, Disk 1.

[00:00:00] Begin Track 3, Disk 1.

I think what's interesting and important for me to ask you at this point, because of the view often of weapons scientists, nuclear weapons scientists, as sort of purveyors of destruction. To hear a weapons scientist say we have to become less destructive, less sort of, I forget how you put it, in love with killing or whatever, that's my interpretation of it. But for you there's no dissonance

here. You're working on terribly destructive weapons but not for the purpose of their destructiveness, essentially. Do you follow my question, if it was a decently put question?

Well, in a way I'm glad I got out of it. Just on that account. Just because civilization just hasn't grown up to these issues, hasn't faced it, really, you know. The United Nations still argues in terms of militarism and conflict and all that. Until you get past that, until you get past the ease with which people can acquire and use conventional weapons. Since the Second World War you've killed *many* times over the people that were killed in Nagasaki and Hiroshima, *many* times over. And I don't know. I just don't know how to deal with that question, because I've never—I'm basically a religious person, believe me, I really am. I have done quite a bit of thinking about the relationship between science and religion.

Have you?

Oh yes. I could give you a lecture on that.

I would love to have a lecture on that sometime, yes.

But [pause] I don't know, I guess I'm just mystified by the question of morality of nuclear weapons. That mystifies me. I cannot see the difference between that and the morality of conventional warfare. Let me put it that way. That's the cleanest way I have of expressing it.

That's clearly put—and so the notion of—

I just don't see any difference. People are killed in the most horrible ways with high explosives, and everybody accepts that. It doesn't make any sense to me. Well, go on.

Well yes, I think—I mean your point is important and I think what I've heard in my wanderings is a point that you raised, which is the problem of killing becoming easier with nuclear weapons, that it just needs that one plane instead of those thousands of planes.

Well yes, when I look what's going on in other parts of the world today, I can't see that it's getting any tougher either. It's getting easier in regular, conventional ways too

Yes, with technology and everything. So what is your religious background?

Well actually I was brought up in the Catholic Church. And I converted to Protestant back about, well, I would say thirty years ago, even before I got married actually. My wife is a very strongly religious person. She's very committed, a Protestant religion. And my main reason why I converted was, I don't mind saying that, is because the Catholic religion is not progressive enough. It just doesn't keep up with the needs of civilization.

And social needs, I imagine you're saying?

Oh my yes, socially. But that's just personal. That's personal.

Sure. The reason I'm asking in terms of your work is that I wonder, because of your interest in science and religion, and things just don't happen overnight usually, if you're thinking during the years that you're doing the testing and you're developing things and you're seeing **[00:05:00]** *these weapons go off. Again as a lay person who is not a scientist and not a weapons—involved in that world, one wonders how people are thinking about what to an outsider appears to be tremendous destructive force that they're dealing with. Now you've answered that to a certain extent because you're saying, Well, that destructive force and high explosives are equivalent to me to a certain degree. I guess the only difference would be the radiation effects, but you've made your other point about that. So in the time that you're working on all this testing, are you thinking in the back of your mind about these larger questions?*

I have on a few occasions, yes. Well, I have another good way of explaining it. You know, when—see if I can remember the words correctly now at this point. When Trinity was fired, [J. Robert] Oppenheimer made a rather celebrated statement.

Now I am become death. The Hindu thing. The destroyer of worlds?

No, it wasn't quite that.

Oh, I'm paraphrasing.

I hope I can remember this one now. What is it he said? Something about scientists have tasted death, or something like that.

Oh right, "The scientists have known sin."

"Have known sin." And I think he said the wrong thing. I think he should have said, The scientists have *seen* sin, would've been far closer to the truth.

Why?

Because things like that and the way they are used is sinful, in my personal judgment. You're really touching me now when I say that. They really are, and it's in the *way* they are used, not necessarily in their existence or what they do, and the same is true of high explosives. It's true of spears. It's the *way* they are used. Revolvers. There are a lot of things like that in our world. But, well, it's getting right back to the questions of civilization, civilized living.

OK. Well, thank you for explaining that.

But I can see that it is a question that's a bit troubling to you, that you can't accept perhaps, and then maybe you are reflecting very much how most lay people view it.

That's why I'm asking these questions, because I think one of my charges as a non-technical interviewer and a researcher—there's the role of the technical oral histories where you're really

trying to get a lot of that information down—but I think questions people ask historically, they certainly ask this in—

I'm glad you're asking me this because it's making me think about some other things that I hadn't fully worked out yet in my mind, and one of them is the fact that, you know, Edward Teller and several other scientists pushed Plowshare very hard at one time. And part of it may well have been that he wanted to bring some constructive value to the nuclear business. In the same way, and here's where it gets to what I am thinking, in the same way as say, for instance, automobiles and trucks and vehicles of that kind are seen, because automobiles are very destructive too. They kill people. They kill as many people each year as died at Hiroshima, here in the United States. And yet no one views automobiles in the same context. Why? Because they have a positive social value of transporting things, transporting people, and so forth and so on. So, you know, that's part of the problem with the nuclear business. We don't have that. In fact, when you go into it the only good thing you can say about the whole nuclear business, that's **[00:10:00]** aside from weapons or anything that deals with nuclear, is its use in medicine. Well, there's very little that you can criticize there, that you can lay down and say it shouldn't be done or whatever. You have nuclear wastes but they are low level. So that's about the only one. Nuclear reactors are very, very controversial. They are actually. I think it's because of the industry. It was the industry's fault. I really believe that.

They didn't handle it properly?

Oh, terrible, absolutely awful. Shortcuts all the way through, and they got carried away where we're getting carried away now in a similar way, in my judgment. Getting off the subject a little bit, but we're getting carried away with hydrogen fuel.

I've heard that critique as well.

Ah, terrible! Awful, *awful* choice!

Yes, I heard that. I heard a story about that.

Where are you going to get the energy? You still have to make the hydrogen. What are you going to use to make the hydrogen? Nuclear reactors? Go on!

Yes, I was ignorant, and then I heard this little—more detail of what you're saying, so I know what you're talking about. It opened my eyes. So yes.

No, the reason why I can spiel very effectively about energy is because in 1972 the lab had gotten to a point where it didn't know what to do with their old gray heads, like me. So they formed a small energy group and they picked five or six of the oldest, most scurrilous characters they had and put them all in one pot.

"Most scurrilous," you said?

Yes, right. And you know, we got along fine. And we studied energy and learned all about this, all about where it was coming from, where it was going, what the bad parts of it were. So I'm quite prepared to take on any issue that deals with energy and point out just where the traps are, because an awful lot of it is pretty shoddy.

OK. Let me think for a second.

We're really going around, aren't we?

[00:12:46] End Track 3, Disk 1.

[00:00:00] Begin Track 1, Disk 2.

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

When I was reading the Carothers interview, you alluded to problems of Hardtack II [Operation Hardtack II, 1958]. My understanding is that Hardtack II was the series of tests that happened right before the moratorium.

The moratorium. I kind of thought that would come up.

I wondered what that was about.

I thought about it last night.

OK. Oh, great.

In what context are you—?

I was just curious what you meant by that, because maybe Carothers knew but I don't know what you were referring to.

Well, I would say that it was a very difficult time all around, any aspect you want to raise or look at. Because it was preceded by certain test limitation negotiations that reached a kind of a crisis point in late summer. And when it became obvious that there would be a moratorium, that the Soviets had managed to maneuver our negotiators into a hole, it was decided to start the series in September right at—let me think about that a minute. [pause] I think that there was a real recognition that we would have one more opportunity to test as soon as the tests in the Pacific were finished, other than the high altitude tests. The high altitude tests dragged on a little bit. But we got our teams back from Hardtack I, phase I [Operation Hardtack I, 1958] in the summer. I don't remember exactly what the last date of a shot down there was. It might've been even in September.

You're real close. I'm looking at the book. It was August 18. Fig.

Yes. And immediately after that we moved into Nevada, and I think the first tests in Nevada were in early September there.

Correct.

And as the series progressed, the testing became more and more frenetic. We reached a point toward the end, the last two weeks, three weeks, where the tests were—let's see what the right

word for that is—very experimental, I guess is the best way of putting it. The weapon people really didn't have the time to properly evaluate the results of the first tests before they were forced to try a variation of their designs for the next, before October 30 or 31. And it was a hit-and-miss proposition. What they did, they decided to run several tests of their various designs that bracketed the possibilities and then hopefully they would be able to analytically decide really what the best configuration, or best choice of whatever design, might be. From an operational standpoint, we didn't have enough time to recover from the Pacific tests in the sense of having enough equipment. And we ran out of cable pretty quickly because you laid the cable out there and, you know, you could use it for one or two tests, then somebody would run a truck over it and that was the end of that piece of cable. [laughter] Kind of. That's putting it crudely but, you know, that's part of it.

I understand your point.

[00:05:00] There was a very great concentration of people working frantically all over the place. It was difficult to keep track of them. There were pressures from a variety of directions for not exactly special favors but special *allowances*, shall we say, to allow participation: Can I try this on this next event, or can this military group squeeze in on this next event, on another event? The military were there too, doing their effects work. I do remember that towards the end it got so bad that there were people who I would *never* expect actually crying, the stress was that great. They were actually crying and it was just—they didn't get enough sleep for *days* on end; they could hardly take care of themselves hygienically. It was a bad show all the way around. The end of the series didn't help either because there was not a sense of relief. We were so distressed by the way it had been going on that all we could do was just walk away from it and shrug our shoulders, kind of. One of the great disappointments, in hindsight, was that we had one very

critical test that was supposed to be fired on a balloon, and it was ready to be fired about three days before what we had chosen as the end of the series there, October 31. And the weather never turned right, and the weather people kept saying, yes, it looks like it's going to break, and it didn't break. I mean it's the way the wind patterns behave there in Nevada. This particular wind pattern had to shift, had to drift eastward, which was the usual thing and then we would wait for the right moment where the fallout would be spread over a large area rather than being concentrated. And it turned out, because they continued following it, that either November 1 or 2 would've been ideal. And our test people and our politicians were so *committed* to stopping at midnight, October 31, that they said, Absolutely not. The Soviets went on and fired a couple more shots in the first week of November. That was really, you know, a mean hindsight. But it's difficult to put your finger on any one thing about Hardtack, phase II. It was all-the-way-around tough for everybody.

Right. Right. Under normal circumstances, you had these tests planned out.

Oh yes.

And there would've been a longer period of time?

Preparation time.

So you do one, you look at it, you think about it, you make your adjustments, and then you—?

Test another one. That's right. Yes. And you also have it set up so maybe you have two or three or four tests that address different issues in the design work. And so then you get—for some of these design questions—you get answers just from one test, and that answers the question, so then you are finished with that and you can go on to another design question. Some design questions require three or four tests or maybe more. Some of them may have to do with

bracketing, like I said earlier, bracketing a certain issue so you get a good parametric view of what's going on.

So certain things were put into that series that may have waited to—I'm asking—waited to a later time because of the cutoff, the October 31 cutoff?

Oh yes, definitely. They were put in too soon, that's right. Oh yes, I would say so. Yes.

OK. Yes. And was it one of those situations where things sort of take on a life of their own? Did people still have in view the reasoning behind why they needed to do this certain test, or was it just—?

[00:10:00] I don't think that was lost, no. I don't think that got lost. I think there was a general sense of annoyance at having to operate in this fashion, you know, I'm sure of that. But no, I don't think that the objectives were lost.

OK. So it wasn't one of those—

Feeding on itself kind of things?

Yes, yes.

No, no, I don't think so. At least not in my judgment.

OK. Thanks for—yes, OK. OK, and then the other question that came up from the—not only your interview with [Jim Carothers]—generally with the interviews that I've read that Carol gave me of Carothers, and I discussed this a little bit with Duane Sewell this morning, was Baneberry [Operation Emery, December 18, 1970], because Baneberry was the one that vented, where the containment wasn't proper.

Failed.

Pardon?

Failed. The containment failed, yes.

The containment failed in 1970. And I wondered if you had any particular insights or thoughts about that. Were you involved in that test?

No.

OK, then we'll let that one go. Just because it's interesting to me, reading the oral histories, how he talks about that himself.

We haven't said anything about underground testing.

No, we haven't, and should we? Why don't we talk a little bit about that?

Yes. It's a good question you raise about Baneberry and failure of containment. Baneberry was the one that they finally, as I recollect, became sensitive to the fault structure in Nevada.

Correct. Correct.

Yes. It's true that we weren't sensitive to that before that, or if we were, we certainly didn't give it the importance that it turned out to have.

I had a question generally about underground testing, from what I've been reading. One of the moves to go underground is that you don't want the atmospheric testing, you don't want the fallout.

Radiation. To get out the radioactive products, to get out, right.

Right. So the notion is that they will be contained in various ways underground. But then it seems to me, and Baneberry maybe was an example of this, of how a whole new set of problems or questions arise having to do with exploding weapons underground, which maybe in their application would never be the case. In other words, you would be not using the weapon underground but you need to test it underground. So that just is interesting to me, that a whole bunch of other questions start arising about how to actually carry out an underground test that may have nothing to do with—

That's true. Nothing to do with—well, yes, that's an interesting point because one of the things which underground testing certainly led to was much greater interest in geology than had existed before, and that was a kind of an unseen byproduct, shall we say. It's a good point. Yes. It isn't so much that we were having to adjust to an environmental climate that's different from the atmosphere, as it was that it stimulated us to be more interested in that climate or in that environment. That's a good point.

Yes. Well, that's the impression I was getting and I sort of wondered about that.

Yes. Yes. Well, you know, the test program developed a containment group [Containment Evaluation Panel, CEP] just on that account, people who did nothing but study that question, and also it provided more support for K-Division. K-Division was the group that basically got into geology and that sort of thing, geophysics.

[00:15:00] Carol Gerich*: Earth sciences. Yes.*

I wasn't very much involved with that, or I didn't have much contact with that, other than I personally thought it was generally a good thing, let's put it that way. Just quite aside from the nuclear business. You know, it's interesting that in a place like this, you'll find a lot of expertise that can transfer into other situations, into other applications, neighboring kinds of sciences, that kind of thing. And it was easy at times to drift off into that when we didn't have any great first-order priority, like during the moratorium. And during the moratorium, some groups had to apply some degree of discipline to keep focused on our objectives because many of us didn't really believe that it would go on and on. I mean, many of us believed that testing would resume. Some people didn't think that it would resume. That was a problem. But yes, that's the past.

Well, I don't know. Underground testing. Well, it certainly made a lot of problems go away. Because you weren't out in the open; you weren't flashing the atmosphere and dropping

radioactivity on the school grounds twenty miles away, that kind of thing. But I liken that period to marching the troops along the borders. In the seventeenth and eighteenth centuries, that was a popular thing for the various leaderships, the nobility in Europe. When they got annoyed by the guy next door, they would march the troops along the border. And the more troops you marched and the more they shot up in the air, the angrier you were, and that was their recognition. I mean, at what point do you actually start the war, that kind of thing? Underground testing had a lot of that to it.

Mary Palevsky*: In what sense?*

Well, I first became sensitized to this during the Vietnam War because it is very interesting. If you plot—which I did, for a study for the National Science Foundation, believe it or not—if you plot the number of troops in Vietnam and you plot the number and yield of Chinese tests, you find a great concentration of very high yield shots just when we had the most troops in Vietnam. As soon as we pulled out the troops and the numbers started going down, they quit testing. Significant? I don't know. It leads me to think that they may have very well sent a message, not just this way alone but through some neutral country to say, `Look, you know what comes next if you don't get out of there.` I really believe that. I think it was—in that context, Vietnam was a nuclear war.

And then just so we can close this circle, how does this relate back to the underground testing? The number *of tests?*

Yes. You know, do you *really* need eight hundred or so tests? Really? Do you really need eight hundred tests? Do the Soviets really need, I don't know, whether they matched us or not exactly is beside the point. Six hundred? Seven? What difference does it make? It's a little difficult to **[00:20:00]** buy that. You are allowed to do it, continue testing, because from the standpoint of

politicians, it's a useful lever. They can tell you, when things are getting politically better, to slow down and turn off, which they did. At many periods during this era of underground testing, that's where we would have a kind of hiatus. We called them hiatuses, in fact. And then it would start up again. And you usually find that when you look back at the political situation, that some disagreement arose, a walkout by the Russians, whatever you want to call it, and we'd start testing again.

So you're moving away from this sort of pure *notion of either you need to test this effect or you need to test this design or—*

Oh yes. No, that might've been the case early on, no doubt about that, but as you went down the line. You know, there's another thing. By about 1970, maybe a little earlier, certain kinds of tests were abandoned. These were tests that were addressed to rather significant nuclear effects. They just quit doing those because we didn't need them anymore. And they were in some sense sensitive internally because they had a higher risk of fallout or whatever, so we didn't do those anymore because we really didn't need them. So we stuck with our good, safe underground, deep underground, wouldn't-get-out tests, but nevertheless we were still firing them off, you know, like firecrackers.

Real big troops along a geopolitical border.

Real big troops along a geopolitical border, right. This is the way I see it.

Yes. It's very interesting. We should probably begin to wind up because you've already given us more time than we asked for, which I really appreciate—

This is very different from what you've heard before, isn't it?

Carol Gerich*: Yes. Yes. I'm writing down my notes here.*

Mary Palevsky*: I know that Carol [Gerich] had a couple of questions, but before that, I always ask people if there's some other little thing that popped in their mind that we didn't get a chance to attend to that you would like to add before we close.*

Well, if there is any one thing that I would say, it is that people should become more realistic. If anyone thinks that we will never test again, they're out of their minds. It's a little like the arithmetic of infinity. I can always name a number larger than a number you can name and vice versa. OK? I can always give you a time period longer than you can name by which we will test again, or detonate a bomb, I don't care which. So people should disavow themselves of the idea that we will never explode another atomic bomb or thermonuclear bomb. I just can't believe that. It's not in the nature of humanity right now. Maybe not for many, many centuries, unfortunately. So it's not something that you should turn off or turn on just by a switch. You can't do that.

You said before about the two thousand years of social development that we need to go through. Do you ever worry that this event that you're talking about, this future nuclear explosion which will occur in one way or another, will be a result of—?

Oh, very definitely. I have grandchildren.

Do you think about that it could end that history, our two thousand years, that we might not have two thousand years? That it could end civilization and things like that?

Oh, well, I think that's about as probable as an asteroid ending civilization. Is that a good answer?

OK. That's a good answer.

About as good as I can give. I think we are tougher than that. When I see what happens in other parts of the world in terms of practical extinction of various racial groups, and then they come **[00:25:00]** back. Nature is that way. It reseeds itself somehow. You will have to do a pretty

thorough job of extermination to eliminate humanity at this point. You might have very high mortalities, no doubt about that. But would we live long enough to reproduce and to keep the race going in any kind of sensible way? I don't mean just going back to pure barbarism or anything like that. Yes, I think so. It might get tough but I think there are a lot of other things that could make it just as tough going on in the world.

Well, we'll wrap it up, and thank you very much.

You had a couple of questions?

Carol Gerich: *Yes. Mine are much more practical and much less profound. Bruce is interested in the difference of Livermore and Los Alamos at the test site. Now you mentioned that they tended to be more academic.*

That was on the hill, yes.

Carol Gerich: *Yes, on the hill.*

Well, in one context, let me give you a quick answer. Rather quickly, I would say within a few years, our ways of testing or manners of getting things done at the test site converged.

Carol Gerich: *OK, in just a few years.*

In just a few years. They leaned our way and we leaned their way.

Carol Gerich: *Was Los Alamos involved at all in Redwing?*

Oh yes, sure. Oh yes, like I said earlier, we had Enewetak and they had Bikini and they fired about as many shots as we did.

Carol Gerich: *OK. But the story that I read, it was Johnny Foster's [former LLNL director] experiments that were really the core of Redwing. Is that incorrect?*

Not entirely, no. There was one big shot that I had a great deal to do with in the design of the measurements, and that was an A-Division experiment. And there may have been a couple of

other A-Division experiments, but I can't really—if I looked at that record, I probably could pick them out.

Carol Gerich: *Was how they were organized at the test site, Los Alamos and Livermore, different? I know you mentioned that we did our own field testing and we even built a field test group and they used EG&G. Were there other differences between the two?*

Far as I'm concerned, no. I think everything else—the test organization in terms of a test program leader—we traded the job of overall scientific director between the two of us, between the two labs. One year it was one lab and the next year it was the next lab, that kind of thing.

Carol Gerich: *How about the role of the engineers? Was it different with the two different laboratories?*

Only to the extent that we didn't use contractor engineering as much as they did. Now as far as weapon assembly and all that, that was entirely ours and entirely theirs. They had an assembly building, we had our assembly building, and it was done entirely by laboratory people. We couldn't contract for that, just by the nature of the work.

Carol Gerich*: And then my final question is, I'll try this one out on you, I've tried it recently, it seems to work. What was the agony of testing and what was the ecstasy of testing?*

That's a good question. Oh dear. The ecstasy of testing. Oh my.

Carol Gerich: *Was it when the shot went off and you said, "Yes!" or was it, "I got my measurements," or—?*

Well, the first couple of times maybe yes, but after that well, you know: `Yes, it worked. Good. Let's get on to the next one.` Well, if I was going to pick anything out, it would be getting up in the morning and driving out on the test site in spring and the flowers all around. Beautiful! And the air is clear. You didn't get air like that around California. Strangely enough, it

was environmental that hit me; it wasn't the testing or anything, that was the business. But the thing that was the sideline, the side issue, was the environment. Later on in the year, it was just like the Mojave Desert, got pretty dry and all that.

[00:30:00] *And the agony.*

The agony. [laughter] No, I don't know, I can't tell you this one.

Carol Gerich: *Oh, you can't tell me this one.*

This is too raunchy to tell.

Carol Gerich: *Yes, well, I know they did pranks out there. I know they did pranks. Some people told me the agony was the food.*

Oh, the food was great!

Carol Gerich: *No, at the Pacific Proving Grounds. I heard it was terrible at the Nevada Test Site. No? You disagree.*

Well, of course my view probably is colored by the last few times I was there, because the last program I was really *deeply* involved in was Ajax, which was 1967 [Operation Latchkey, November 11, 1966]. That was my shot, if I may put it that way, and it saved my neck. I heard a very interesting thing from my division leader many years later, my former division leader. And that's far enough ago and among people who are sufficiently disconnected. You know, I asked him, one thing I didn't understand, I said to him, I told him I couldn't figure out something.

All the tests that I've participated in before, there are always these problems with getting enough help, with getting adequate funding, with getting the engineering department to do what they had to do, *et cetera, et cetera.* I didn't have any problems at all. Everything just came out fine. Every time I needed anything, I got it. The site was just the way I wanted it.

[He asked], And you don't know why?

I said, No, I don't know why.

[He said], They wanted to hang you. They thought that you were nuts, that you didn't know what you were doing and it wouldn't work.

Carol Gerich: *Oh, so you couldn't use any excuse at all. They'd made everything perfect for you.*

Carol Gerich: Everything went beautifully. I got great answers. It was a great experiment. *That's a wonderful story. [laughter] Wonderful story.*

I never had any more problems after that with those guys. [laughter]

Carol Gerich: *That's a great story.*

Let me tell you, that experience is not unique in humanity, by the way. Others have had the same experiences, even here at the lab, where you try to hang someone and it backfires. But I didn't know it, so I'm clean, I can tell you that story. I'm innocent. [laughter]

Carol Gerich*: That's surprising, because of all the oral histories I've read, I'd never gotten any hint of that. You were always revered. You were determined. You did your due diligence.*

There were a few people who hated my guts. Not many.

Carol Gerich: *Because you spoke up?*

Well, the best way of answering that one is a little story I told you, I think, that after each test there was a post-shot review here at the lab.

Carol Gerich: *Yes. Post-mortem?*

Post-mortem, right. And I used to go to many of them because I was delegated from test division to do that, and I used to sit down near the front and I would ask embarrassing questions, and one time one of the associate directors got so annoyed with me that he made some remark about, let's see, how did he put it? That I was a [pause] I can't think of the exact word**.** That I was an annoyance, you know.

Carol Gerich: *A gadfly?*

A trouble-maker, and Jim Carothers, who was my associate director, division leader at the time, was way in the back. He piped up and said, `Yes, but he's a trouble-maker on our side.`

[laughter]

And that sounds like Jim Carothers, too.

[00:34:24] End Track 2, Disk 2.

[End of interview]