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

Interview with
Charles E. Violet

May 22, 2004
Danville, California

Interview Conducted By
Mary Palevsky

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

Mary Palevsky*: And we're recording.*

Charles Violet: My name is Charles E. Violet, sometimes known as Chuck Violet. I was born in Des Moines, Iowa in 1924. My family moved to St. Louis when I was four. And they moved to Kansas City, Missouri when I was six, and then I remained there through grade school and high school. My main interests in my formative years were music. And I went to—or rather I took lessons and played the cello from about age eight and through elementary school and high school, and became fairly good at it and went to numerous music festivals which are very prominent in the Middle West and won some prizes playing cello and so forth. And then I went to junior college, Kansas City Junior College, for about a year-and-a-half. Let's see, I graduated from high school in 1941. And at that time the war, World War [II], in Europe was underway and it was obvious I was going to be participating soon. And so I enlisted in the U.S. Army and I was inducted in Fort Leavenworth, Kansas in March 1943 at age eighteen. And I then later transferred to the U.S. Army Air Forces and entered a program which is called pre-meteorology, and the objective of that or the end point of that would be I would become a meteorology cadet in the Air Force and graduate and become a second lieutenant and be assigned to an operational unit as a meteorologist, air force meteorologist.

Going back for a minute, I think my main interest in my young life, formative years, was music, and if the war hadn't come along I might well have progressed to become a musician. But the war changed that. And also one of my interests was sports—baseball and football—but I was not very good at it, so I just played at sandlot games and so forth.

Anyway, I became interested in science really as a result of the war and realizing that I would like to—I knew I was going to be involved in the armed forces some way or other and I would like to be involved as a technical person rather than as a private in the infantry. So I did go to University of Chicago in their pre-meteorology program. That was in 1943.

And then I finished that course. And by the time we finished, the Air Force decided **[00:05:00]** they didn't need us—or some of us—so we were given several options. And I became an air force communications cadet, and we studied basic electronics and so forth and then studied all the communications equipment in the Air Force. And then later I went to radar school in Florida, Boca Raton Field in Florida, and studied radar. And by that time it was—well, when I was at Boca Raton in the summer of '45 is when we dropped the bomb on Hiroshima. So I had fully expected to go to the Pacific, but the war in the Pacific ended at the time that I was finishing up my training in radar.

And a lot of other things changed. As it turns out, I was shipped to Europe, and went by troop ship to Europe and then by train to Berlin. And that was all very interesting, riding a train at that point in time through France and Germany. The train threaded its way through much destruction and much damage and it was like a single line that was going most of the way through a circuitous route from Paris to—actually we went from Paris to Munich and stayed at a *Luftwaffe* field outside Munich and stayed in some very nice barracks which fortunately our air force declined to bomb during the war, so they were very nice facilities.

And at that time the Dachau trial was going on—war criminal trial—and I could've gone and I didn't. And I've always regretted that I never went to that trial. I mean, it was continuing over a period of time and I did hear comments from my associates, my colleagues, my brothers-in-arms, about some of the things they heard and listened to. And I've always regretted not going to it.

Anyway, I then traveled up to Berlin and I was stationed at the Tempelhof Aerodrome as communications officer for the Ninth Air Force and also an organization calls EATS, which is European Air Transport System.

So after a year I came back. I opted to leave the Air Force. In fact, I was very happy to leave the Air Force. And I started my attendance at the University of Chicago in the fall of 1946. And I spent one year in the college of the university and got a Ph.B. That's a Bachelor of Philosophy, which I don't know if any other university gives that or not. And then I majored in meteorology because I at that time was very interested in science and math. And so one year later—no, let's see, two years later—I got an S.B. from Chicago, University of Chicago, in meteorology.

But by that time I became interested in physics, so I transferred. I wanted to go into graduate school in physics at [the University of California at] Berkeley, so I transferred out to Berkeley. I had to spend one year as an undergraduate to make up my deficit in physics courses, and then entered graduate school in 1950. Was it '50 or '51? I think it was '51, excuse me. Nineteen fifty-one. And then I got a Ph.D. in 1953 in high energy physics.

Who were some of your professors there? Who was there at that time?

[00:10:00] Well, a notable person that wasn't there was [J. Robert] Oppenheimer. He had left Berkeley, but his presence was still felt. And there were lecture notes of Oppenheimer that were available to students, mimeographed I guess you'd say at that time. And so that was highly disappointing. Although I did have a course from [Edward] Teller when I was at the University of Chicago, in beginning physics. That was very good. And I did actually see [Enrico] Fermi one time at a lecture he gave, and he was there at that time. When I was in Chicago as a cadet studying meteorology, we would go to Stagg Field for physical education. And we noticed that there was a certain area of Stagg Field that was something strange happened every now and then. Somebody would come out, a young man would come out with a piece of metal and hoist it up on some kind of pulley to a high level and then trigger it and it would drop and fall to the ground. And none of us knew what that was, and to this day I don't know what was going on, although we commented about it. But that was the location where the first nuclear pile was built. And so I'll never know what the—I've often wondered if it was a subterfuge to confuse spies or whatever, but who knows?

Anyway, let's see, the people who were quite notable then was Edwin McMillan and [Emilio] Segrè and [Luis] Alvarez. And I took a course from Alvarez in nuclear physics; that was very good. That was really—he's very remarkable, very good teacher. Let's see, other notable people. That's all I can think of at the moment.

That's fine.

Well, let's see. So after I got my Ph.D. I went—well, I'll backtrack a minute. Before I got my Ph.D., as a graduate student I met Eileen and I was married in 1951. In '53, after getting my Ph.D., I took a job at Lawrence Livermore lab. At that time the name was UCRL, University of California Radiation Laboratory, and then it had several other names in between, but it's now of course LLNL [Lawrence Livermore National Laboratory].

I joined the testing group at the Livermore lab when I got out there, and the general testing area at that time was under Art Hudgins, and he was the person that hired me. I'll backtrack for a minute. My graduate work, the academic part of it of course was on the campus;

the experimental part of it was at the laboratory on the hill. And there I worked in the area of nuclear track emulsions as applied to high energy physics. And my immediate supervisor was **[00:15:00]** Walter Barkas who was head of the film group there, and he and his group had done a lot of work already in high energy physics and studied mesons and mu-mesons and pi-mesons. And there was a famous man there from Brazil named [C. M. G.] Lattes, who was at the time credited with—well, he and his group, he was part of Barkas's group, discovering—I'm sorry, I'm not sure, a mu-meson or a pi-meson, I'm not sure what.

But anyway, my study was high energy electron and positron interactions in material. And that was done by exposing nuclear track emulsions, electron sensitive emulsions—which had just been invented—to high energy electrons and positrons which we were produced at the synchrotron. And so we had a setup where we had a target and an arrangement which had a target and positions for the emulsions, and we had the use of a high-field magnet which we could separate out the positrons and electrons. And so we used that machine, a converter target, to convert the high energy gammas into electron-positron pairs, and then we could expose plates, either electrons or positrons. So then I could study the interactions of high energy electrons in material, namely in nuclear emulsion, which of course consisted of a number of elements—besides gelatin it had silver. And various interactions could be studied by way of the microscopes and looking at—well, looking at the emulsions with microscopes, high magnifying microscopes. So I observed many interesting interactions: electron scattering, electron-nuclear scattering, the pair production from high energy gammas in the emulsion itself. And that was mainly my work for the Ph.D.

Let's see. Steve Whitewas a member of Barkas's group, and Al Oliver, was—well, Steve was a physicist and Al Oliver was a technician. He was a photographic technician, very good photographic technician.

About the time I got my Ph.D., the laboratory in Livermore was being formed. I think it was the previous fall, in '52, and I got my degree in '53. There was a group from the hill—I think probably under Herb York—and there were a number of other people, like Art Hudginsand I don't know who else, went to the Pacific for the Ivy test. And they did some kind of radiation work at that test as a separate group, as part of the task group and Los Alamos [National Laboratory] structure. Oh, Duane Sewell was part of that group, and they were the group mainly that **[00:20:00]** started—other than Teller, of course—that started the lab in Livermore. Am I going too far already, or should I keep going?

No, no, this is great. This is great.

OK. So I was hired by Art Hudgins to join Steve White's group—Steve White and Al Oliver. Of course I knew Steve and I had talked to him about it before. And then shortly after I became a part of that group, another physicist, Chuck Francis, who I knew, we worked together in the film group in Berkeley, and he came out and joined the group.

So we had this nuclear film group, and when you have a nuclear films group and experiments going on, you always have to have a number of people who are willing to sit down and scan track, which means sitting for quite a while during the day and looking through a microscope. Well, in Berkeley these people were usually graduate students, scanners. In Livermore they were housewives. And so we had to train them to scan tracks, and that means optically following a track that enters the emulsion by moving the X-Y stage on the microscope and then looking at what happens to it. If it interacts in the emulsion, then you would form certain measurements and that's a good event to record.

So our work there involved training—well, they were usually young women that were doing this as a part of the job. The rest of her job was being a housewife. And not only training them but also supervising them, checking their work to see how they were doing, and the two sort of merged mostly. And then analyzing the data and going through whatever computations, *et cetera*, that this analysis required.

And then we published papers about some at Livermore, with the understanding—as Steve Whitetold me, and Art Hudgins—that I could spend up to half time working on high energy physics at Berkeley. At that time the Bevatron was about ready to come on line, so I'm looking forward to doing experiments with these high-energy particles. It was quite interesting. So that was a good deal, as far as I was concerned.

Interesting sideline might be that when I joined the lab, my original salary was $500 a month. That was in 1950. Of course, I was a graduate student.. No, wait a minute, sorry, no. That's when I joined the lab in Livermore, my salary was $500 a month. As a graduate student in Berkeley, I was getting $125 a month. So I got quite a pay raise going to Livermore. And then after a six months' probation, I got a fifty dollar raise. So that was very nice. But I remember when Steve White told me, in some conversation he just happened to mention that his salary was $700 a month. I thought, Wow, that was really something. Boy, that sounded wonderful!

So my work at Livermore originally was divided into two parts: the continuing work in the high energy physics and then I started working on and planning for the Fonex experiments to be done in the Castle [Operation Castle, 1954].

[00:25:00] *Can you explain what Fonex is? That's something I didn't understand from yesterday. You said Louis Rosen had invented this?*

Yes. Basically, you have a nuclear emulsion and the emulsion is of course made from gelatin plus silver bromide and a few other ingredients. But the emulsion has a lot of hydrogen in it, being an organic substance. So when it is exposed to neutrons, the neutrons will interact with the hydrogen or protons. And in that process the hydrogen turns into a proton—because it loses it electron—and the proton then has a velocity, a momentum, an energy, and it goes through the emulsion until it stops. And as it goes through the emulsion, it ionizes the silver bromide particles, then as light does. And so then those form virtual images, and then in the development process, the silver is converted to metallic silver and you end up with a metallic blob that can be seen optically. So this kind of a collision would be called an n,p collision, neutron-proton collision. And since they're approximately the same mass, that's like two billiard balls on a billiard table colliding. So we can apply—since these are low energy neutrons that we will be dealing with in terms of the fission process—we can use non-relativistic physics and calculate very simply by conservation of energy and momentum, and so forth, all the details of the collision. And so you can measure the track of the proton, and knowing the initial direction of the neutrons, then you know the angle at which the proton went off in that collision. Then you can calculate the angle of the neutron and the energy of the neutron from that collision. So it's a technique of measuring neutron energies.

So for the energies that we were interested in, namely fission neutrons—I should say neutron energy—it's neutron energy from perhaps half a MeV up to high energies. Of course, the fission energy intensity at—well, let me backtrack. The energy that we're interested in, at high energies, would be the 14-MeV neutrons resulting from the d,t [deuterium-tritium] reaction in the bomb. So from a nuclear explosive, one would expect to see—in terms of neutron energy—we would expect to see a fission spectrum, roughly speaking, a fission spectrum plus a peak for the d,t burn. Again, the d,t is a two-body collision and—well, the energy in the center mass is fourteen-point-something MeV. And then if it's heated, if it has a high temperature, then there's a Doppler broadening in the peak, which then gives you—instead of a sharp peak—it gives you a broad peak, depending on the temperature of the burn. And then by measuring neutron energies using nuclear emulsions, one can measure the broadening. Of **[00:30:00]** course, you detect a lot of 14-MeV neutrons, hopefully, and you can measure the energy distribution. And then by the broadening of the peak, you can detect the temperature of the burn, and that was the whole idea of Fonex.

Right. OK.

Before I leave that, just as a detail, the broadening of the peak also is a result of the experimental errors which are generally symmetric with respect to energy itself. It's a matter of it's a convolution process in which you extract the specific d,t temperature broadening from the total broadening, and so you have to do some physics to do that. But anyway, it can be done; it's fairly simple. And the same thing, the same measurements, can be made with the neutron beam with neutron detectors on the long pipe, evacuated pipe, which was used in the Pacific.

So, let's see, that's Fonex. So this was developed by Louis Rosen at Los Alamos. And Steve Whitewent to Los Alamos when he went to the lab and worked with Louis Rosen for a period of time, actually going over and looking at the films that he had received on the Mike shot. And so he came back and started up the film group of which I became a member.

To do this experiment in the Pacific, of course since you're looking at a big explosion, you had to have your detector back a ways in a bunker that will survive the explosion. And this is true of the prompt neutron measurements also from the shot. Well, to physically get around this problem, what one does is you construct line-of-sight pipes from the device—actually from the big wall in the device building—device shack—that holds the bomb. And it's all of course oriented properly so you have the right geometry. Then you have these pipes, these long pipes that are perhaps—I don't know how big in diameter they were, they varied, but they were on the order of a foot in diameter, maybe down to some smaller dimension toward the device. I'm not sure. Anyway, the pipes have to be evacuated because if you leave the normally moist air that would be there—I mean, moisture makes it worse—but if you leave the air in the pipes, then the neutron attenuation over the distance you have to back off is serious. So what is done is you can start these line-of-sight pipes that are evacuated with extensive pumping systems—so they're evacuated—and so then you don't have any air and the neutrons can go down the pipe without being attenuated appreciably. So what I've said so far applies to the prompt neutron work as well as Fonex. And then eventually neutrons impinge—in the case of the neutron experiments—they impinge on the scintillators or other kinds of neutron detectors. And you have detectors, photo cells looking at the scintillators, and then you can take the signal from that and that's your neutron intensity as a function of time. It's a function of time because you can record the neutron intensity versus time **[00:35:00]** with your oscilloscopes. So with Fonex you don't have the time ability. You take the neutrons and you essentially integrate the neutron energy over the time of the blast, over the time the neutrons were produced.

And so, let's see, our pipes ended in a block and we had a converter in the pipe, and at the position of that converter—the converter was to convert—I'll have to backtrack here. Well, anyway, I'll go ahead. The converter was in the pipe and it converted the neutrons to protons. The protons then took off down a second pipe, and that angle is something like 30 degrees or so, down a second pipe, also an evacuated pipe, into a canister that held the nuclear track emulsions. The protons went through a thin window and then into the emulsion. I hasten to add here that my first description of Fonex was just one way to do it, in which the conversion of neutrons to protons took place within the emulsion. However, in this case—and Louis Rosen did this also—you can have an external hydrogenous converter, target, that will convert the neutrons there into protons. And then you can use the nuclear emulsion plates downstream at an angle from the main stream, and the protons come through a thin window and into the nuclear emulsions. And then you measure the tracks of the protons. You pick them up on the surface and measure them as they go into the emulsion. These would enter the emulsion, say, at about ten degrees from the emulsion surface, so they would go in at a glancing angle and by design they would be stopped, say, approximately halfway into the emulsion.

So that was the technique used in Fonex, in this particular Fonex. They used the external target, converter, and then the protons were detected in the canister holding the nuclear track plates.

Now, to prevent all the debris—that was the way to get the information from the neutrons. And of course the neutrons travel much faster than the blast, so they're out of there. They're ahead of the blast wave and there's a lot of time to work with.

And what kind of distances are we talking about here on these—?

Well, it depends on the blast. In Castle, we were setting up to do an experiment on Morgenstern and the pipes were about, as I recall, 5,000 feet. Now, on the Los Alamos shot, the famous or infamous March 1 Los Alamos shot with Shrimp [Castle-Bravo], the pipes were, I believe, 7,000 feet. And that's where they discovered the fact that the line-of-sight pipes were not line-of-sight. And it was due to the fact that no one took into account the curvature of the Earth, and so they made those adjustments. It was built by Holmes and Narver and they used standard survey techniques. They had their surveyors out there and they were looking through—it was built in sections and each section was set up, so what you had is—well, and normally surveyors don't take into account the **[00:40:00]** curvature of the Earth. But at 7,000 feet, one has to. And so what you had is each section was slightly off. But it's kind of amusing that no one ever thought of that beforehand, you know, because it wasn't Holmes and Narver's fault. It was—well, never mind, it doesn't matter.

Yes, that was interesting to me to hear that yesterday, that that could even happen.

Yes, it was interesting to all of us out there when we heard that, you know. How come you're only getting, you know, some fraction of what you should be seeing, you know? We couldn't understand that.

Well, anyway, our line-of-sight pipes were 5,000 feet, I think, and we had a huge array of pumps to pump on these pipes. And we had some really good mechanical technicians that were running that facility.

Now, this probably seems like a really obvious question, but the pipes are running from the building that holds the weapon or the device, is the building in water or on land?

Oh no, it's on land.

And so they're running on land or under land?

Yes, and they're running on top in an array, sort of closely together, and they're running above ground.

Above ground.

Yes, in a straight line above ground, and they're built in sections and every section, there's a place for a connection and there's a big concrete support there and so forth.

So they built those sections, surveying them per section, as if the Earth were flat, and then when they all got connected over the seven thousand feet, you were off.

Yes. Yes.

And that was discovered by literally physically seeing that you weren't getting a line-of-sight, is that right?

Yes. Well, after it was done they checked the alignment with I believe it was a large gamma ray source with a film on the end and their aperture was only, I believe, half of what it should be or something like that.

OK. Yes, OK.

So that was corrected. Yes, as soon as people began to realize that, it was a straightforward thing to do. But an interesting thing is that it was discovered at the Mike shot—they had some line-of-sight pipes there, and it just strikes me now, why didn't they learn that at Mike? But anyway, they had a pipe, or many pipes, at Mike that went from the source to a bunker. And maybe the distance wasn't that long or the pipes were bigger diameter or whatever. Anyway, there are pictures. See, there are fireball pictures taken routinely of atmospheric shots. And there was a guy, Fran Porzell from Los Alamos, who was their fireball guy, and he was a very interesting guy, very sharp guy. He later left Los Alamos to go to the University of Illinois at Chicago. Anyway, I visited him once and he had so many stories to tell about the early days of the testing. It was just amazing. He said he went out to a shack and, if I may digress—

Please do.

At the Nevada Test Site in the early days—this is before Upshot-Knothole. [1953]. And in those days, you'd start the tests in February when it was very cold in Nevada, especially up on that mountain, on that mesa. And he said he went to the shack where there was a contractor. It was a dark and stormy night [laughter] and he was sitting in the shack and he had a pot-bellied stove in there, keeping warm. And he had lights and so forth, and he was doing some work there. And I remember Porzell stopped in, and he wanted to get warm, I guess, talk to the guy. And he was talking with him and the guy got up to put some fuel in the pot-bellied stove, and he asked him—it was just a chunk of—it wasn't wood; it looked like just a piece of glass or plastic or something—and he said, `What are you putting in there?` And he said, `I'm burning Comp-B.` **[00:45:00]** That's an explosive. So it turns out Comp-B has to be detonated, shocked, in order to explode, but if you just burn it, it just becomes a very benign heat source. But Fran, it took him back a little. He said he was sort of surprised. Anyway, that's just an amusing story.

Yes. Well, yes, I should say so.

Let's see, where were we? Oh, the fireball. OK, the fireball from Mike produced a jet that went down these pipes. So as you saw the pictures of the fireball, you'd see the fireball—we had a number of still shots of the fireball. Well actually, these were shots that were actually made to make measurements with. And there was a jet composed of, oh, hot luminous gases, very hot luminous gases, going down the pipeline. It hit the bunker and made another spherical ball of fire, and that spherical ball of fire was equal to some of the fireballs they saw in Nevada shots. It was really amazing. Well, what happened is—I don't know as I understand this very well—but what happened is, as the initial fireball started growing out of Mike, it shined down on the pipeline and heated it up. And it so it traveled down there to the bunker. Well, of course everybody knew then that that was going to happen if you had the pipeline. So we knew that was going to happen to our pipelines. Well, nobody particularly was worried—well, I shouldn't say that. People took that into account in the design of their experiment, and in the course of the building of the whole thing. And in addition to the jet that follows the pipeline on the exterior of the pipeline, there's also a jet that goes down the pipeline inside the pipe and would, of course, if it arrived to anywhere near our films they would be ruined. So we devised a plug that would move—there was another pipe—there was a plug that would be—spherical plug, a metal plug that would be moved by a very heavy duty spring that would slam this plug into the pipeline. And I guess it was—I forget the orientation. But anyway, you would have to crank it up to compress the spring to essentially arm it. And then it would stay in that position and then it would be released by an explosive bolt that would be triggered by the light from the fireball. Or maybe it was triggered by the prompt gammas. I'm not sure which. But anyway the prompt gammas, I think that was a detector. It would send a current to an explosive charge that would release the spring that would shove this big metal block into the pipe. It was really neat.

Anyway, that was the Fonex experiment at the Castle, and it was really a descendent of the Fonex in Mike.

And so you saw—you're saying Shrimp but someone explained yesterday that what I've got it here as is Bravo for some reason. That's the giant—

Yes, Bravo is the—let's see, there's Shrimp and Romeo and Bravo. Romeo—

I think Bravo is Shrimp.[Shrimp was the name of the device, Bravo the test].

But anyway, Bravo was the event name, right? And Shrimp was the early name of the device. I'm not sure about that. But anyway, Bravo.

[00:50:00] *Bravo's the big one. Bravo's the fifteen megaton, and then Romeo's eleven. Bravo, it says here, 2/28. But you're saying 3/01, so that's the test you're talking about. It must've been the night of 2/28 into 3/01? Or maybe they're wrong here.*

No, no.

They're putting it on the last day of February; you've got it on the first day of March.

Oh, that may be. Oh, February 28. OK. OK, yes.

It was probably the morning of March 1. Early morning, March 1.

Yes, that's what I'm thinking.

But all dates were—

So they're doing the night of 2/28.

Yes. Yes.

So you saw that?

Yes, and that was—oh, let's see, shall we go ahead and talk about that?

Yes. Sure.

Yes. When I arrived, of course, arrived about the first part of February at Enewetak, we of course arrived at the airfield at Enewetak. Enewetak is the main island of that atoll and there was an airfield there, a big airfield. By the way, another interesting sideline is, we had a radiochemist in our group named Harry [Hicks] He was in the infantry, or in the ground forces, at the time Enewetak was taken by our ground forces. And he was a lieutenant. And what I was told, and I think this is true, is that he accepted the Japanese surrender at Enewetak, at least of one Japanese force anyway. The Japanese commander gave Harry his sword, and Harry was the only officer around that he could surrender to. And I'm not sure, there were probably other elements that surrendered separately, but this one commander apparently was looking for some officer he could surrender to and he happened to see Harry. So anyway, that's an interesting sideline on Enewetak anyway.

And Harry was a radiochemist at the lab. So then—[pause]

Yes, I was asking about sort of what Bravo was like, Shrimp/Bravo was like. You were out on ships or were you?

Yes. Well, I was going to say, I was stationed at Tare. We did our work on Bikini atoll, and then everybody at Bikini had to leave the atoll and come back to Enewetak for this shot. And so we did, and we were staying in the tents there at Enewetak. And so early that morning we got up, and it was still dark, to go down to the beach—go to the beach. It wasn't very far down since everything was pretty much at sea level. Anyway, and they broadcast the countdown. And we had our protective eye gear we were supposed to wear—which we did—and I selected to see through my dark glasses just a speck on the horizon. But at the time of the count zero, it was the most amazing thing: the whole sky lit up as if it was—it was light like it was dusk. It was not light like the sun was shining. It was like light just after the sun goes down. It was incredible. I was just **[00:55:00]** amazed. And it lasted maybe only a few seconds and died down. That's the first one I'd ever seen of the megaton type. And so after that, everybody went to the mess hall for breakfast, and as we were moving from the beach to the mess hall, we heard a big report, a big bang. And I thought, well, OK, that was the shock wave. And then later we heard another bang. And then we went to the mess hall and got our food and we heard another bang, and then all through breakfast we heard bangs from that shot. And it was because the shock wave was reflecting from various layers in the atmosphere. And it went on for maybe—quite a while, I mean, it might've been like ten minutes or something like that, I forget. But it was very interesting. I never realized it would be—that effect was something that one would hear.

What was the atmosphere? I mean, were people—well, first, what was your reaction? Was it scary or was it just awesome or—?

It was awesome. It was very awesome, yes. And I think it was awesome to a lot of people. They were there saying, `Oh my!` you know, `Oh wow! Oh goodness me!` Whatever. They'd say, `Wow,` that's a big one! you know. But of course we didn't know it was twice the yield or whatever.

And it was twice as big because of something in the design that had not been understood properly? And do you know about that piece of it?

There's a certain reaction, thermonuclear reaction, that was not a d,t burn, it was not a d,d [deuterium-deuterium], it was not t,t [tritium-tritium] it was some other reaction which escapes me now—I don't remember—that was not taken into account. There are many thermonuclear reactions that take place, like for example in the sun, that's been worked out a long time ago that are—I really forget what the details are.

That's OK, but just generally, they must've thought afterwards, and of course this is Los Alamos, this isn't your job, but people must have thought afterwards about what they had created that they didn't realize was going to be happening.

Yes. Yes, they went back and I'm sure they—sooner or later someone says, Well, what other kind of reaction could we have in these? you know. And so it was a reaction that it's, I believe, what we call a secondary reaction. That is, a certain reaction like a d,t burn gives a proton and a neutron and then these are primary reactions. These are protons or neutrons that can then interact with other materials, and because it goes so fast before the original materials or other nuclear particles are so dense that sometimes these reactions can become very important. And that's true in plasma physics too. And that's something people deal with in, say, in fusion technology. So I think it's just a matter that it was—I don't know but I guessed that it was—and I've heard people mention in the general comments where it was a reaction that was not included.

So that went very big and of course the world knew that the fallout products were dumped on a force of Japanese fishermen [aboard the *Daigo Fukuryu Maru*] and that was very unfortunate. And of course before a shot, I think, as everybody knows the whole area, thousands of square miles of the Pacific, are cleared of any vessels. They didn't see these Japanese fishermen. I guess they didn't see them. If they saw them, then they would've told them to get out. But anyway.

[01:00:00] And then the other thing is, see, the rest of the day we didn't—I don't know why, maybe it was an off day. Maybe it was a Sunday or something, a day off. Anyway, we sort of sat around and laid out on the beach. And we witnessed this huge, big, massive cloud formation that came across the lagoon—well, it came from Bikini. And it was a cloud formation—my meteorology came in handy here—it was what we call an alto-cumulus cloud, and it's similar to the—you know what used to be called a buttermilk sky?

Yes.

It's a buttermilk sky-type of cloud. It's a series of parallel ridges that the ridges are normal or right-angles to the direction of the motion. And that cloud formation came over and was over us all day long. And of course it was very high altitude. It was not radioactive—well, at least not very. It was made by the updrafts that were produced by the explosion. Of course, the explosion made a huge mushroom cloud that went up to the stratosphere, and these clouds were of course below the stratosphere. But whatever the motion was, it created these clouds that were of that type. And the fallout was not a problem, as far as we were concerned. Of course they planned the shot to go where the fallout would not go to any inhabited place.

So that was very interesting. But then it made life very, let me say, in part miserable for the people who had to go back to Bikini to get ready for their shots, which included us. See, our island, as I recall, was Tare and we had our fast diagnostics and Fonex was on that island. And so we went back on the boat, which was I guess the *Ainsworth*, where we were living. We ate, and we slept and ate on the *Ainsworth.* And so we went back on the *Ainsworth* and anchored in the lagoon off of Tare. And the island was quite hot radioactively. And I don't remember any numbers but it's so hot that your duration on the island had to be limited. And so one had to wear protective clothing, which was essentially overalls with booties and a face mask and I think a helmet. And so we had to don these clothes in a very hot, humid climate. And we were transported from the *Ainsworth* to the island, Tare, in LCTs. And so when we got to Tare, they let down the front of it and we'd walk out; that was convenient. And of course, well, everybody had to be careful of the contamination of their recording instruments from the radioactivity, particularly us because we were using film. I say "particularly" because all the sensors for the fast diagnostics, prompt diagnostics, were in a blockhouse which provided ample shielding; it was really shielding for just personnel. But we had a bunker that we didn't get inside of. It's something. It's a big chunk of concrete at the end of this pipeline that we worked at. So **[01:05:00]** we were out in the open most of the time. So we had a certain number of hours to go to do our work and then we had to get out of there. And then we went through a decontamination process at a barge anchored alongside the *Ainsworth.* It was shower, get rid of our clothing, and so forth. And then we came to a place where we boarded the *Ainsworth* from the LCT, and that was a very hairy operation because the LCT, it was tied to the *Ainsworth* but it was going up and down with the water and bumping against the *Ainsworth*. And we had to step from the moving LCT onto the ladder on the side of the *Ainsworth* and scramble up the ladder. Potentially it was a dangerous operation, but it was handled pretty well. There were guys at the bottom of the ladder helping you to grab the—there are guys on the LCT helping you to grab the ladder, and then there are guys at the top of the ladder to grab you—

To grab you to pull you on.

Yes. So all we had to do was climb up the ladders. It wasn't very far. It was maybe about ten feet or something—well, maybe less than that, maybe eight to nine feet, something like that. Anyway, that was rather miserable.

Let's see. We did get ready for the shot—let's see, was I there? I left before Morgenstern was fired.

All right, and I think this must be called Koon. This is the LLNL shot on April 6.

Yes, Koon, that was it. The Koon shot.

At Bikini.

Yes.

So you left, and that was another problematic shot, I understand, when people say it was—

Yes. Well, I'll just mention, the reason why I left is because I used up all my exposure time I could take. So the other guys, Chuck Francis and Steve White and Al Oliver—I forget how it was worked out, but I had to leave first, and probably somebody else did too. Anyway, we had to divide up and plan our time.

Because you had had your full dose for the year?

I had my full dose for that operation. For the year, yes.

Yes. Did you worry about that at all?

No. I could have, but I decided not to.

OK. It was a conscious choice.

Yes. It turns out, after all the operations I've been through, which was several, I ended up with eight roentgen total radiation. But I decided not to worry about it. So, let's see, what were we—let's see, that was April 6.

So you're gone.

And I was gone, so I did not participate in the recovery. But the recovery was done there as similar to what is done in Nevada, as you go in in a helicopter and you land near the bunker and you're equipped with appropriate protective clothing and booties and so forth, face mask, *et cetera.* So you bring with you containers—which are lead-lined for radiation shielding—and you go to the bunker and you extract the canister from the bunker, and then put it quickly into the container that you brought along. So the canister is only exposed a few seconds if everything goes right. And then you carry it back to the helicopter and then you're out of there. So the whole thing should be done as rapidly as possible and hopefully there's no problems.

That the canister is too exposed to the radiation, it's—

[01:10:00] Yes, it had its own shielding also, but it was not as much as you would like to have if you're going to carry it through a radiation field the distance from the bunker to the helicopters. So anyway, that was done and everything went fine for Fonex, and the next several months we did a lot of scanning at Livermore to get that data out. So everything went very well.

[01:10:44] End Track 2, Disk 1.

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

Let's see, wait a minute. It was Plumbbob—did that have a lot of—so Rainier was part of Plumbbob.

Correct.

OK. So why don't I just go to that?

Great. September 19, 1957 [This date is according to DOE/NV—209: *United States Nuclear Tests: July 1945 through September 1992*].

OK. So I was asked to take charge of a new technique for testing, namely underground testing, by my boss Gerry Johnson who was asked by Edward Teller to do something about this. The idea is to develop a testing system that would remove the fallout problem, and that means testing underground. Well, that's one way to do it. The other would be testing in outer space, but anyway.

OK, I was going to ask you what the other one would be.

So that sounded pretty exciting to me and I was in the mood for a change. I had already done a lot of work in testing and had done a lot of work in high-energy physics on the Bevatron and so forth, so I decided I'd like to do that. So I began to try to inform myself about geology and the whole gamut of all the various types of tests that are done, diagnostic measurements that are done on nuclear devices. So L-Division hired a mining engineer, Don Schueler, to work with me in devising the design of this experiment, and we worked also closely with L-6, Bob Petrie and others in L-6, to design this test. And we also worked with the Berkeley mining professor, Lyle Schaefer. He was hired as a consultant, and he was an experienced hard rock miner from way back before he became a professor. Also another person that worked with us is a theoretician named Gene Pelzer who did some theoretical work concerning propagation of shock waves in the rock and so forth.

Anyway, we designed a tunnel which would be going into the side of a mesa, and the mesa we selected would be in the northern part of the Yucca Flat. And we had some geological help deciding the location, and we were told that most of that formation was tuff, T-U-F-F. Tuff is a volcanic deposit, airborne deposit from volcanoes, fine dust particles that are hot and sticky that fall and are compacted and so forth.

So this particular mesa was chosen, and the design of the tunnel would be a tunnel that would go into the mesa from the side and end underneath the mesa cap where the containment interaction would be, where it was most likely to be contained if it's underneath the cap of the **[00:05:00]** mesa itself. So we had to go in at least 700 feet to get under the cap. And we designed the end of the tunnel to end in a fishhook configuration such that when the explosion took place, the explosion itself would send a shock wave through the stone and collapse the tunnel ahead of the shock wave that would have to go around the corner, so to speak, to get out of the tunnel.

Gene Pelzerwas very instrumental in designing that. And so we also had to design a line-of-sight apertures—not tunnels but tubes in the rock radiating out of the shot site into locations where instrumentation would be placed. And so we wanted to do the shot. This would be a proof test to demonstrate that we could explode a nuclear device underground, contained, and still be able to get the prompt diagnostics and the radiochemistry primarily.

So that underground construction was designed and set underway to do. We had an outside contractor come in to build the tunnel.

Which contractor was this?

Well, it was some mining company. It was someone—It was gone out on a bid. It wasn't part of the so-called AEC [Atomic Energy Commission] group of contractors.

OK. Just before we get there, how long of a time were you working just designing this thing before you go out to do it? Are we talking about months or a year or—?

Well, I must've started, probably about a year in advance for myself. And then, let's see, so this was very interesting in that I found that as director I needed to contact many various groups of people who did various things in the lab and also outside the lab. There were many contacts outside the lab. And one thing that was needed at the very outside was to get core samples from the tuff, because we needed to know the properties of the tuff in order to make reasonable calculations about how the shock wave were propagated in the tuff and how the initial ball of fire which would be in the room with the device. I mean, well, the exploded device would be some kind of a fireball that would engulf the wall and vaporize the wall and melt the walls and then compress the walls and shatter and so forth. And all those processes would have to be understood fairly well, not in precise scientific terms, but still we had to make scientific calculations to try to predict what was going on in the closure of the tunnel. And also one needed to do calculations concerning the effects of the device—mainly the effects on ground motion—so there was planned and we did close-in ground motion measurements **[00:10:00]** with accelerometers and strain gauges, mainly accelerometers in arrays around the device, and also farther out we'd be doing seismic measurements.

And in fact we were doing seismic measurements and as it turned out, seismic measurements were made virtually throughout the world from this shot because one reason is that because of the fact that it was International Geophysical Year that year. Nineteen fifty-seven turned out International Geophysical Year [IGY]. We didn't plan it that way but it turned out that happened to be the case. So it was rather serendipitous. We then made a real effort to include seismologists locally in the United States, also worldwide. And the worldwide contacts were done with the help of the—well, it was an agency of the government in Washington that does scientific work. In those days, it was before NOAA [National Oceanographic and Atmospheric Administration]. It was some agency that funded scientific—maybe the National Science Foundation, or whatever.

Anyway, we convinced the AEC that in order to do this, what we wanted to do was to incorporate this with IGY. We needed to get the time of detonation announced ahead of time on an unclassified basis so that it could be used worldwide by scientists. And so it had a real scientific component to it. And so we were able to convince the AEC of doing that, and it was done. And we, you know—in terms of the seismic measurements—we also interacted with Professor Byerly at Cal Berkeley who was, oh, you probably would say it would be correct to say he was a world-renowned seismologist for many years. And for many years he manned the seismic station at Berkeley, and of course did a lot of measurements on the western part of the United States because of course most of the earthquakes in the United States are in the West. And he was very interesting to talk to, and he didn't really get into any of the details, but he certainly gave us blessings about using it and he—I think, I suspect, although I don't have any idea about this—he interacted with members of the seismic net, domestic and worldwide, and also Washington. He was a fairly influential person in Washington too in the scientific circles.

Anyway, we used a device of known yield that was given to us by, or loaned to us or whatever, by LASL [Los Alamos Scientific Laboratory].

[00:14:15] End Track 2, Disk 2.

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

And a device in which measurement had been made on and radiochemistry had been done on—it was tested originally as an air shot in Nevada, so it was a well-known device. And let's see, so we had the device and we had our prompt diagnostics people at the lab doing prompt diagnostics, at least prompt gammas. I don't think there were any neutron measurements made, but they did prompt gammas, which would allow a measurement of the alpha of the device to be made, and that could be compared with the known alpha and so forth. Also the other big question is, could you do radiochemistry when the products were tied up underground in the rock?

So I and I think my associates, definitely my associates, learned a lot about drilling. So I mean, the only way to go there would be we'd have to drill in from the mesa. Well, the easiest way would be to drill in vertically from the top of the mesa into the site, which was presumably a more or less spherical site that had collapsed and made a chimney, which is a well-known phenomenon with underground explosions. A bubble is made underground and then the top of the bubble, by force of gravity, collapses and then makes a chimney because the collapse progresses upward until it finally stops. If it doesn't stop, well, it could erupt. Well, either way, if it stops, it probably would make a crater—the ground above it would sag and make a crater at the surface—or if it vented, it would still make a crater. In this case, no crater was made at Rainier.

This is a test operation that was conducted and—during Plumbbob, all the shots were tower shots or balloon shots. And our shot, Rainier, was toward the end of the program. Anyway, let's see, we had our diagnostic experiments in readiness and we had the ground motion people who were making close-in measurements ready—and that was done by various contractors—and the seismologists were ready. And the shot was detonated, I forget, it was around the middle of the day sometime. I forget when it was. And we were all, of course, very interested, very—I wouldn't say we were uptight, but we were very anxious and excited about if it would blow up or not, be vented or not. And it didn't vent. And at ground zero there was this shock wave that you could see radiating out on the slope of the mesa, and then there's a lot of dust coming up from the shock wave. So on the slope of the mesa there's a big cloud of dust which then flowed down the slope of the mesa and dispersed into Yucca Flat. Of course none of that was radioactive or anything, so—

That's from the earth shaking and the dust coming off the surface.

Yes. Yes. The shock wave would hit the surface and the surface would move from the shock wave and then that motion, a lot of the loose particles on the surface, dust and so forth, would be ejected, propelled, into dust.

Did you expect to get a crater there or did you not know or—?

[00:05:00] No, we had a lot of confidence that it would first be contained, and we had a lot of confidence that there would be no crater. We really over-designed from our standpoint, the depth was probably twice—I'm not sure, but I know it's at least twice what we calculated the depth would be for crater formation.

There had been previous shots in Nevada conducted by Los Alamos that were not contained, but they were crater shots that were done in conjunction with weapons effects tests. That may be Upshot-Knothole, but I'm not sure. But we did have some data from those shots about crater formation. And of course there was a lot of other data available—not a lot, but there was data available from various explosions that had taken place and the meteorite collisions which had formed in the past in geological times and had been more or less preserved—which had been studied quite a bit. And actually as an offset of this program I think there is a whole class of geologists who study meteor craters. They look at craters on the moon and craters on, Venus or whatever, as well as the Earth, and it seems to me that would be really a nice way to earn a living. It seems to be fascinating. Anyway, there's some group, or maybe more than one group that actually got money out of the AEC to study meteorite craters, and just as part of this.

So that was a success, then, for that first test?

That was a success. And the seismic data was all good and our close-in ground motion measurements were good. And, oh well, I was just talking about the radiochemistry. That was something that was a big unknown. So we hired EJ Longyear Company from Minneapolis, who was an old, old company in the mining business, and they were diamond drillers. By that, their drill bits were made of diamonds—the cutting part of the bit were made of diamonds—and it was designed to go through hard rock to get samples. I think, as you probably know, the cutting edge was in an angular ring around an axis, so that as it cut a core would come up in the middle of it into a cylinder. And then you had the capability of then, while your rig was still in the ground, you could draw up cores through the rig, through the drill stem, on up to the surface. And you could accumulate cores and they could be then logged appropriately and stored appropriately, which is all a known procedure and technology for core drillers and so forth.

So as we say, we did a lot of core drilling before the shot on the site, so we had some experience there. But we did hire them to do the drilling for the radioactive samples, and one of the questions was—these diamonds are small diamonds in a ceramic matrix, I believe—and the question is, how would drilling into a hot glassy substance, how would that affect the diamond bit itself? Well, it turned out—you know, you normally drill with water or mud for lubrication—and it turned out, I don't think that was a problem because I think the lubrication, the water and so forth, the drilling mud helped.

But anyway, we were—the drillers had never, of course, ever—they were good at **[00:10:00]** what they did. They were very good at what they did. And they'd never been on an operation at a test site. They were just constantly amazed at what they were doing, you know, all these people, these crazy scientists and these other people from AEC that were around. But we were able to communicate with them quite well and explain to them what we were doing. And they, of course, they needed to explain to us what they were doing, what their concerns were. So it was a very interesting interchange of two technologies coming together that had never been anywhere near to each other before.

That's interesting. And then once you get those core samples up, you analyze them with radiochemistry, you're saying, and then you understand what kinds of things?

Well, it's different, of course, than air samples in that the rock, which is—well, you want to get into the glassy area of the debris because that's where most of the fission products are, and whatever samples they put in to monitor certain reactions. So as you say, you take the samples from the bit and of course they're hot and they had to be properly handled. And these guys learned a lot about radiation. And anyway, then they're transported by air to Livermore in properly shielded containers, and then to the radiochemistry building and so forth. And then they had to be dissolved, and that takes—they're hot—well, and these techniques were—well, dissolving rock, in this context, was not at all known, so they—but the chemists knew how to dissolve rock. So they had to go through that and then do the radioactive chemistry, which means counting, a lot of counting, of decay particles from various—and then you go through the procedure of isolating various isotopes and looking at decay schemes and so forth. So that took time.

[00:12:44] End Track 3, Disk 2.

[00:00:00] Begin Track 4, Disk 2.

Radiochemistry results were very good. They were very much consistent with the known yield of the device. And I can't go into any detail about that.

Right. I know. Yes.

So that was a very satisfying, very satisfying event, experience I had at—and from that, then, all testing became—except for Christmas Island, all testing at Nevada, of course, became underground. And they changed from—well, let's see, Plumbbob which was the—no, wait a minute, let's see, Rainier was part of Plumbbob.

Right.

Right. OK, and that was, essentially all except for Rainier, all tower or balloon shots. One thing I'd like to comment about the operation, from my personal standpoint, is the operation of the safety panel. And these were all air shots. And the panel consisted of representatives from the laboratories, representatives from the AEC and other laboratories that were doing measurements of fallout and blast. And it's interesting that some of the shots—normally you'd think of the shots, the main concern was fallout, which is true, and that's a very serious constraint. Also, another concern is blast, and you don't want to break windows in Las Vegas. And blast is very tricky because, as I was mentioning before, the pressure waves from a shot as they go through the atmosphere can be focused by interactions with the various layers of the atmosphere. So it's possible, and a widely-known phenomenon, that if there is a blast somewhere from, say, high explosives, it's possible that close-in nothing much happens. I mean, say, like a mile away or something, but if somebody's house is two miles away, it could be flattened. I mean, that's an extreme statement but—and so it's all because of the interactions of the sound wave with the atmosphere. And there are models for this and for calculating these things.

So the safety panel has to consider, of course, the fallout situation. Also, it has to consider the blast situation. And interestingly enough, there are some shots which were OK fallout-wise, if I may use that term, but were cancelled because of the blast problem. We'd be breaking windows in Indian Springs. I don't know whether people were really concerned about breaking windows in Las Vegas, but I think that was a real consideration on a few shots. Yes, OK, the shots were postponed for that as well as the fallout. But of course fallout was the main thing.

But I was impressed with how serious all these matters were taken. And Duane Sewell was the head of many of the safety panels in that operation. And I don't know if he was there for every shot or not. I don't know what the arrangement was, but as the head of the safety panel he was acting as an agent for the AEC, Albuquerque. Jim Reeves. Jim Reeves was the honcho for the whole operation. Great guy. And so even though I didn't—I'm not sure **[00:05:00]** if Duane recused himself when it came to a Livermore shot or not, but I don't think so.

Anyway, I was impressed with Duane Sewell, his thoroughness and his technical know-how and his questions and his great concern and responsibility that he conducted this panel with. It was very impressive to me. And lots of times they would adjourn, delay, you know, come back in two hours or come back in four hours, you know, trying to get a shot off. They really worked hard.

And another thing that was impressive is that there was a balloon shot at the very end of [Hardtack II]. That was a Los Alamos shot on a balloon, and Sandia of course was in charge of the balloon facilities and so forth. So I forget the name of the shot. It may be an asterisk; I don't know whether it is or not. Anyway, the balloon was raised and ready to go, and the safety panel met and they couldn't shoot it because the fallout pattern was headed towards St. George. And God, there's so many times that the fallout pattern was headed towards St. George, and of course it had to be postponed. And so I don't know how many days it sat there, and you know we're facing the moratorium. —the moratorium had to end—wait a minute, that was Hardtack.

That was Hardtack, right. [Operation Hardtack II, 1958]

OK. The moratorium had to end on October 31 [1958], and on October 31 it was still hanging there. It never got shot. And that was, I thought, very impressive. A little-known story.

Yes. Yes. You know, and I think it's significant because of all the concern in the area about fallout and about how responsible test site and a lot of people were in that area, so yes.

So let's see, so I'm jumping from Rainier to Hardtack [II]. Now Hardtack—I guess I can jump now.

You can jump.

OK. Hardtack [II] was all tunnels. Shots were named after—what were they named after? Mountains, right?

I don't know. I haven't figured out what you name these things after. [laughter]

Yes, I think—there's Evans and there was Whitney and so forth. I don't know that they were all tunnels—no, I don't think they were all tunnels, but there were tunnels and the balloon shots and there may have been tower shots.

Looks like there was maybe a couple of tower shots. I'm seeing on Hardtack Luna, Mercury, Valencia, Mars. So you got planets in there too. And then there's Tamalpais, which is a mountain.

Yes. Was that in Hardtack?

Hardtack. Yes.

OK.

Rushmore. OK, there you go. Evans. Yes. Humboldt.

Let's see, I was, well it doesn't make any difference. I think I was UCRL test director for Hardtack I and Hardtack II. Anyway, that's neither here nor there.

Anyway, that was the series, everybody was in a rush, of course. And there were a number of tunnel shots, and all those tunnel shots went well. The only problem was Diablo, right? Was Diablo in Hardtack or was it in Plumbbob? I think it was Hardtack.

[00:10:00] *I'm going to look it up [pause—checking* DOE/NV—209: United States Nuclear Tests: July 1945-September 1992*]. Well, if we're going for mountains, it sounds like it would be—OK, it's July 1957, so that makes it Plumbbob.*

OK, it was 1957. That makes it Plumbbob. Rainier was part of what?

Plumbbob. Rainier was part of Plumbbob right toward the very end.

Yes, and then how about Diablo? When was Diablo? It was before Rainier?

And it was a tower shot, yes.

OK. OK. OK, that makes sense. So Diablo was Plumbbob and so Plumbbob was a mixture of tower shots and underground shots.

Right. Right.

OK. OK, so Diablo was quite an exciting event. In fact, too exciting.

What happened?

Diablo, there was no detonation.

Oh, that's the one. OK, that was the one that they had to climb up the tower on?

Yes, because I remember standing out there with a whole group of people with our goggles, you know, and listening to the countdown and all of a sudden, then the announcement comes on, you know, just before they do the ten, nine, eight seconds, you know, countdown, like minus twenty or thirty seconds or whatever it is, they say, `Put on your glasses and turn away`, and so we'd dutifully do that. And so we were there waiting because the countdown—you probably could see—we were expecting to see something, even through our glasses, even though we were turned away. Anyway, nothing happened, OK, and it was dark and nothing happened. It was quite a shock. Then what do you do? Well, then there was a lot of people who had gone into meetings and did a lot of thinking and talking and so forth, as you might imagine. Anyway, it was decided that three guys were going to have to climb the tower. They had to climb it because there was no power to the tower. The tower was a 700—it was a 700-foot tower, wasn't it?

Let's see if it tells. It just says "tower," but it was a seventeen kiloton weapon, device, when it eventually goes off. Oh, these yields are wrong. These yields are not true. They [DOE classification officers] told me that they're wrong. Because it isn't a classified [report].

I'm not sure actually whether it was—I went up on that tower, you know, during that operation, just as a tourist because—that that was our shot and—let's see, it was our [Livermore's] shot, wasn't it?

It was.

It was our shot, yes. And just for the kicks, and I knew people working on that and so forth. But anyway, these guys had to dress up in their—well, first of all, the names of these guys were Barney Rubin who's a radiochemist. He's actually a chemical engineer. Actually I have breakfast with him every Friday morning, with a group of us here. And then there was Forrest Fairbrother who was the device engineer. And then Bob Burton who was from Sandia who was in charge of the arming party. And one thing that comes to mind is sometime after this operation was over and these people were being interviewed by journalists—or maybe before, I'm not sure. **[00:15:00]** This is what I'm thinking. The media came in—this is a big, big deal—and so one reporter asked Bob, the head of the arming party, `How do you disarm an atomic bomb?` [laughter] And Bob said, `Well, disarming an atomic bomb is just the inverse of arming`. So that was amusing. Anyway.

So these guys had to climb the tower—which is either 500 or 700 feet—clothed in protective gear, radioactive overalls, Scott air packs on their back, plus carrying whatever tools they had to carry with them, whatever it was. So they were up there and fixed it and came back down. It was pretty exciting.

And was it known or imagined or speculated what it was before they went up? It was.

Yes, I think they pretty well knew what had gone wrong. But there were many hours that people were poring over drawings and charts, mainly the electrical system, and I don’t remember the details, of course, but it took some time to try to figure out what—

And so they go and fix it and then it’s—

Then it’s ready.

It’s ready and then does it go or do you wait till the next day, do you remember, or—?

Oh, I think it took at least a day. It didn’t go till the next day, I think, or it might’ve been later than that. But it was very close.

So Bob Burtonis really, really a remarkable guy. He’s so cool, as you might expect. He’s head of the arming party for all devices, and it was Sandia’s responsibility to provide the arming party. Bob Burton’s an electrical engineer and there were several people in the arming party. I was in the arming party for Rainier since I was in charge of that, but usually the device engineer and some other people go. It’s usually about four people.

But for Rainier, how does that work? How does the arming work in—?

We just went in through the tunnel, into the device room, and what you do when you arm is you make final connections at the site, and you check all voltages and you make the final connections and then you leave.

You exit.

Yes.

OK, so you know how much time you have to get out and things like this.

Well, there's no really any great time constraint.

Oh, that's right, because you still have to essentially push the button to make it go.

Yes. Yes. So this is part of the sequence in a shot. In a shot day, there's a program or sequence, and at a certain time the arming party goes in—at H-minus-whatever, one hour or whatever—it goes in to make the final connections. And when they, say for the shot, for example, in Yucca Flat they would leave the—see, the control point was at the high ridge between Frenchman and Yucca, and they'd leave the control point by vehicle. And there were a series of stations along the main highway where electrical connections were made and monitored, and they would stop at each station—I'm not sure in detail—they would stop at each station going, and then they would stop at each station coming back, to make sure and check everything. And so "checking it" means every conductor has to read a certain voltage. And so it's a very sensational type of job to do. I mean, you're really focused.

But anyway, Bob Burtonis a very good guy, and along with everybody else he did a very good job.

[00:20:00] That was pretty exciting, and also it was a media thing too because it was covered in *Life* magazine when *Life* magazine was still going strong. And so that was quite interesting.

You know, I don't know if this is the perfect time to ask this question, but when you're involved, so focused on getting these experiments, you know, to go right and this just pulls you right in, are you thinking in terms of the larger context of the Cold War, the arms race? You know, is it just a given that you're doing this because it needs to be done for the arms race? Are you even thinking in terms of this being a weapon, or is it pretty much just focused on, you know, the tasks that are ahead of you, or is it sort of a combination of all the different things?

Well, when it comes near to shot time, you're really focused. In fact, I'd say much of the time you're really out of touch with the rest of the world. You're not listening—well, see, in Nevada you could listen to the radio and listen to the news if you wanted to. But of course in the Pacific, you're really pretty much out of touch with anything that's going on in the rest of the world. I never took a radio with me to Nevada. Maybe some people did, but I don't recall many people ever playing a radio in the barracks, you know, where we slept or anything. Of course that might've been a problem. It probably would be because you essentially needed your sleep and you didn't stay there. It wasn't particularly a great place to stay. Unless you were sleeping, you didn't stay in the barracks very much. Usually you're in the mess hall or else—I'm not sure where else. There was some other place to go in camp, a beer hall or something. Anyway, that pretty well covers the ground, Nevada Test Site.

OK.

Well, one more thing about Rainier, is I remember afterwards, a few days after that was the launching of *Sputnik*. And we did hear enough from the radio or the media, whatever, about what times to look for it and what to look for. And so I remember a bunch of us going out and lying down on the ground. It was at dusk. You looked for it at dusk when the sky was rather dark, at least not bright, and also where the sun would reflect off of *Sputnik.* And we used to lay down and look for it at a certain time and by golly, we saw it moving slowly across the sky. Of course, it was very impressive.

So you've got to be impressed as a scientist who understands what it means. Are you worried about it at all as far as what it means in the war?

Yes, well, yes, *Sputnik* was a worry because the Russians were ahead of us, and at that time or shortly thereafter we launched our grapefruit-sized satellite that I think failed, and so that was a low point. But anyway, yes.

OK. So how do you feel about talking a little bit about Plowshare, or do you feel like this enough?

[00:24:37] End Track 4, Disk 2.

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

Plowshare, and seismic detection.

Seismic detection of clandestine underground shots.

Oh, OK. Right.

The moratorium, test ban treaty, however you want to title that business. OK, so we're now talking about Plowshare.

Right. And how you sort of come to be working on that that. That's because the moratorium is in place, or—?

Yes, that was part of it, and a lot of the Plowshare applications that were discussed had to do with underground shots or cratering shots. And so I was, so to speak, in on the ground floor with cratering or underground shots, and so I—well, I was interested in the whole concept of Plowshare. I thought that was interesting and possibly a very good thing to develop, and so I became interested in Plowshare. And there were a lot of ideas being floated around, and one was cratering to make a harbor, or cratering to make a canal, like a second Panama Canal, which might be in Nicaragua or somewhere else, or Mexico. Yes, one site that was considered was the Isthmus of Tehuantepec in Mexico. I remember Gerry Johnson saying he went to Washington and met with some of the AEC bigwigs and also Richard Nixon. And maybe he went to the

White House, I don't know. Anyway, he went to some high level meeting involving Nixon, and they were talking about digging canals in Panama or Nicaragua. But Nixon said, Why don't we try the Isthmus of Tehuantepec? And no one had ever thought of that. To my knowledge, it had never been mentioned. Because, he said, Because in Mexico the government of Mexico is a very stable government, and so of course he's coming on it from the political standpoint. But I thought that was interesting. That idea didn't get very far, but anyway.

In the process, let's see, well, I became interested in the cratering operation and in what was proposed to be a test in a cratering application in Alaska. And in fact I was the test director of that project {Chariot/Plowshare].. And so I and one of my colleagues had occasion to go to Alaska. We flew up to Fairbanks and Nome and then went to—I guess it's called the North Slope—the shore of Alaska that borders the Chukchi Sea above Nome, and that was the site that was chosen for this experiment. And there was a camp there that had been built by Holmes and Narver or one of the AEC contractors that do that. And the idea there was to do preliminary experiments, mainly geological surveys and soil tests and so forth. There wasn't any big, heavy construction there but there was a landing field and we had a chance to look at the site. And the beach there was really beautiful because it has all black stones **[00:05:00]** neatly polished, about the size of a hardball baseball. Flattened, I mean, they were flattened stones but they're about that diameter. And so it was a very beautiful beach. And the tundra was—well, there was tundra. It was very flat and green. It was very beautiful. It had its beauty too.

But let's see, all I did was to get myself informed on the site and the people who were operating it and the design of what the camp ultimately would look like if it became a test. And that's about it for that trip. I remember stopping at Fairbanks and visiting the University of Alaska at Fairbanks and talking with a geologist there who was a very interesting guy. And I learned a lot about Fairbanks and about Alaska and the sociology of Alaska. It's like one huge, big, small town because everybody knew everybody. If you wanted to get anything done, you know who to go to get it done. And it's a really interesting place. As far as my personal experiences are concerned, I don't have anything more to say, I think, because I left the test program before that was—well, it was cancelled, for one thing.

Yes, that was cancelled, right?

Yes. The other big Alaskan experiment—the test I was not on, I mean, I was not in the test program at that time, so—

And you left the test program for particular reasons or—?

Yes, I left the program for a number of reasons. One, the main reason, was it was during the test ban, the moratorium, and that turned out for me and many other people to be very frustrating. And at that time I was head of test division. In fact, I became head of test division almost at the same time as the test ban went into effect. Well, I became very frustrated at the future of what we were doing and what the future would hold for us and for me. And it was sort of interesting keeping a readiness program going, which you've heard about, I guess, and doing some connected research connected with that on detectors and so forth.

Well, and also the dealing with—the administrative part of the job became quite different. When there was testing going on, it was, what shall I say, informal, let's put it that way. And it was like doing business like I was mentioning in Fairbanks: you wanted something done, you knew who go to to get help and get it done. But the structure started to become quite formal because of inactivity and nothing which, I felt, dealing with reality. And so there was a sort of "make work" type of thing. And so, as I say, I felt a lot of the structural **[00:10:00]** stuff was compensatory to compensate for frustrations. I probably do a lot of people a lot of injustice by that comment, but anyway, that was my feeling.

That was your view of it.

Yes. And I'm sure that it was to a greater or lesser extent true of everybody. And so we did a lot of interesting work and created work in Plowshare and in the detection business, and that was interesting.

I want to ask you a question about that, because in some of the critiques you can read of the weapons labs, it's the notion that—let's say, from a political-personal point of view—did you think the test ban was generally a bad idea? Because sometimes people say, well, [for] weapons scientists the test ban was a bad idea because then they didn't have any work to do. But just sort of generally in your opinion, were you having any opinion about whether it was good politically for us to be in a test ban, as far as trusting the Russians and that whole aspect of it?

That's a good question. I did a lot of agonizing about this, and my position was and stayed pretty much—I thought it was basically a good idea if it was done properly, that is, with the inspection. I thought it would be a big step forward if Russia and we and presumably other countries would stop the technology where it was and hopefully matters wouldn't get worse. I mean, we were in a Cold War, the situation wasn't good, and so I felt that if the technology could be put on a stop, that would be a step forward, and even though I realized that it could cause huge problems to me and my colleagues. So yes, I think that's a very difficult time for me in that sense too because I had the general feeling that if we didn't' resume testing, we couldn't go on like this, or at least I couldn't go on like this for a long period of time on just doing readiness exercises. And I wasn't enamored of going on to higher things in administration because that's just me. Because I—you could say I was not mature enough relationship-wise to deal in relationships with people who were difficult to deal with, who had ideas different than mine, *et cetera*. And I ran into a number of people who were at the Washington level that I had difficulty relating to. It was nothing overt. I mean, I think I was generally liked and appreciated and so forth. And so this is all going on inside of me. And it began to wear on me and I decided I would get out, and I'd rather go do research. But I told Duane Sewell. Duane asked me, If we ever need you to come back to the test program, would you be willing to come? and I said, Yes, I would. You know, I was never asked to come back but I meant that sincerely.

So the other interesting work that we did besides Plowshare, and this was related to what we're talking about, the test ban, was the testing of clandestine underground shots. And that was quite interesting. There were the scaling experiments that one did with high explosives, **[00:15:00]** underground contained, that were very interesting. And also the people, the leading lights on the theoretical end of that problem were Teller and his friends, the two Latter brothers [Albert and Richard], who worked at RAND. Have you heard of them?

No, but I'll look that up.

They were at RAND Corporation in Santa Monica. Very smart guys. And of course Teller was really smart. Anyway, they had developed a very elegant theory of the coupling of the shock waves to the medium and the propagation of the wave in the medium out to large distances, and it was really, really elegant and beautiful. And our people at our lab also did theoretical calculations, mainly computer calculations, on ground motion studies too, and theirs had to do more with the close-in measurements, the ground motions from close in. But they all worked together.

And so anyway, it was decided to do a series of scaling measurements in the salt medium. So the idea was to find a site that was all salt and that was uniform, because comparison of theory and experiment is much better if you have a uniform medium. So it was decided that we should go to where the salt domes were. And salt domes there on the southern coast of the United States, they're formed by plastic flow of the salt that was originally laid down in laminar fashion by the ancient oceans. Then sedimentary rock was deposited on top of this, and you get enough rock deposit, they exert enough pressure on the salt so it can flow plastically, and if there's an imperfection in the strata of the sedimentary rock, then the salt can force its way up through this crack and it can grow and push the sedimentary rock aside and continue to move up to the surface.

So in the state of Louisiana near the ocean, you can be, say, driving along and there are plants and so forth, houses and all that, and someone will tell you, `Well, we're living on a salt dome`. They can be huge, and habitable. Oh, I guess you can't plant very much in them, because aside from the surface layer of mud or whatever, from flooding or whatever, you sometimes have to dig down. So at Winnfield, Louisiana we found the site which belonged to E.J. Carey—anyway, it's the Carey Salt Company. And so here we were back in the mining business again. And these miners were just as amazed at us as the hard rock miners in Nevada.

And so, we negotiated with them to use a portion of their existing mine to set up our experiment. And that entailed digging to a site and then you construct a spherical void, and in this case this was about twelve feet in diameter or something like that, in which you would place a high explosive. And so digging a spherical device is quite a challenge, I think, and I don't **[00:20:00]** know how it was done, but anyway it had to be done quite accurately. And then we chose an explosion that had to be housed in a spherical container, and they decided upon a certain kind of lacquer thinner for the explosive because again it's like Comp-B; it won't explode unless it's detonated. And I guess it'll burn also. Anyway, so you just pour the lacquer thinner into this spherical container and you have the detonation. I don't know how many detonators were on it, but they're all designed so as to provide a reasonably spherical shock wave into the lacquer thinner. Then we had a series of people making ground surface motion measurements close in, and then far out. They had seismometers and so forth. And so I started out in charge of that procedure and, let's see, I was not there for the actual shot. But that had significant results for the seismic detection people.

This is in the sixties now?

Yes, this was in the sixties. Project Cowboy, 1959 to 1961. And then there was Project Gnome. I was technical director but I didn't get too much involved before I left. But that's the shot in New Mexico, out of Farmington.

It's a Plowshare, right? Oh no, here we go. It says "multipurpose experiment in salt. Form cavity 160 to 170 feet diameter."

Yes, I think it was partly detection and partly chemistry.

OK, so with the one in Louisiana though, the point is that this then helps people understand how to be detecting if, for example, the Soviets are testing underground when they're saying that they're not. That helps you understand what to look for seismographically?

Yes. Right, because you know various scaling laws as devised by the theorists, the LLNL theorists and the Latter brothers, so you can scale up from that. The only tricky part is converting the efficiency of the nuclear shot versus the efficiency of the—there's source considerations you have to consider. But you can do a pretty good job scaling, and that's been done. Yes, that was a subject of a lot of calculations, a lot of experience. Because we also did some various low-level high explosive shots in that same salt dome and made ground measurements and scaled up between the two and so forth.

Interesting.

Yes, that was very interesting. Scientifically that was quite a very excellent piece of work.

OK. Well, that's a lot. We've been talking for almost three hours.

Yes, that's a lot.

[Laughter] I'll let your brain relax.

One more thing.

Yes, I was going to say, is there anything else?

Well, this is a personal thing and you may want to throw it out.

I like personal things.

But anyway, the lab was deeply involved in providing technical assistance to the diplomatic people in the State Department, AEC people in Washington that were worried about detection of **[00:25:00]** clandestine shots. And so I was told by my boss, Gerry Johnson, that I had to go with him to testify before the Joint Committee [on Atomic Energy]. So I went. He told me what I had to say, I mean, general business, so I wrote down my speech—this is weeks ahead of time—wrote my speech and got it copied and got it approved and Gerry liked it and all that. So we went to Washington and the day before the meeting we went to see General [Alfred Dodd] Starbird, who was the head of DMA [Department of Military Application], another very fine individual, and we talked a lot about the testimony that we were going to be giving the next day. And he talked to me and I think verbally I just summarized what I was saying and he asked me to give him a copy of my speech, which I did, and he took it and we left. The next day we came back, he returned the speech to me and I forget what he said, but he said, `I made a few changes`. He went through that speech with a fine-toothed comb and made numerous changes. Numerous changes. And in general the changes were toning down, making it more conservative, as I recall, than what I was saying of our ability to do this, our ability to do that, how good we were at this, how good we were at that, *et cetera*, *et cetera*. I think that was the main thing he was doing. He didn't change any of the substance, I mean, of the technical data, he didn't change any of that or the results or whatever, technical results. But anyway, I thought that was really interesting because he must've spent hours—maybe not hours, but maybe an hour doing it.

Yes. So let me see if I understand this. You're testifying to tell Congress about our capabilities or your capabilities of detecting Soviet detonations in violation of a treaty, or we're now under a treaty or in tacit—?

Well, there's no treaty here.

There's no treaty yet.

Yes, this is just technical background to hopefully make as a part of the treaty what our prerogatives were, our rights were, if we observed a certain seismic signal. Then, according to the treaty, it would have to be bigger than something in order for us to go on site. Because part of the, what do you call it, the enforcement procedure of the treaty would be the Russians would have to let us on to the site and where we can make on-site measurements. That's another thing. One of the on-site measurements would be, we'd have our drillers back doing drilling, and I remember, while somebody else was testifying, someone raised the question about drilling and someone asked the question on the panel. This is a joint committee. And, you know, the question was, Suppose you, by your seismic detection methods, could say, 'Well, there's a detonation that occurred within a certain area,' say an area of a mile radius or two miles' radius or whatever, and say it was one kiloton or whatever, how many holes would you have to dig to find it? And nobody came up with a good answer, so I remember on our side—I wasn't testifying, and the person that answered that, either he didn't know and I wasn't happy with the answer. So I remember getting out my paper and pencils and starting to do some calculations, because I remembered the diameter of Rainier. And then I calculated the area, and then I knew the area of **[00:30:00]** whatever he was talking about, and then just divided the big area by the little area and I'd say, well, we'd have to probably drill that many holes. So eventually I got a chance to make my comment.

Interesting. And again, now what year was this, probably, when you testified, or you have that?

Let's see, I don't know. [sound of pages turning] I got it—yes, I can look it up too. I think, you know, I got a copy of that whole—you know, the government puts out these big copies of testimonies, and my testimony is in one of those books and I got a copy of it around here somewhere, I think. Maybe I can look it up. [U.S. Congress Joint Committee on Atomic Energy. *Hearings on Technical Aspects of Detection and Inspection Controls of Nuclear Weapons Test Ban.* April 19-22, 1960]

But so it's in relation to the anticipation that there are going to be negotiations for some sort of test ban, and this is the kinds of things that would need to be taken in consideration by the, I guess, the diplomats that are negotiating and the technical people they were—?

Yes, they would have to know why—say, the scientific community says to the diplomat, What we have to have in the treaty is this, you know. Like I said, If we detect with a standard seismometer—that both sides agree on—and they get a certain signal, then both sides would have to agree. If you get that size a signal, well yes, then you have the right to go on-site.

And verify or—

So that would be part of the treaty, and the diplomats would have to know that and the reason behind it and so forth.

Interesting. Right.

So that was during the period that negotiations were going on in Geneva. There were several meetings in Geneva over those years, and maybe other places too. Gerry Johnson was one of the people that went over there, and some of our [people]—Wilson or Herb York might have.

Well, Herb was there during the Carter administration, which is in the early 1970s, and I'm wondering if it's around that era or not, but again—

He was head of DDRE [Department of Defense Research and Engineering] then—

I think he was actually Carter's ambassador to some of those—

Was he an ambassador? York was—?

Yes, to the negotiations in Geneva during a certain period of time.

OK. Well, that's quite reasonable. Yes, those were interesting days. So.

Yes. Well, that's great, because that actually takes us sort of on the ground at the testing sites to what the significance is once you're looking at, you know, international diplomacy and arms control negotiations. That's very interesting. Well, that's all to the good.

Yes.

So I'm sure that if other things occur to you, we can follow up or whatever, but I think I've probably taken enough of your time for the day.

Yes. Well no, I'm very happy to donate my time to this. I think it's great you're doing this. Very happy to do it.

Yes. Yes. Well, I really appreciate—

And if you decide that you'd like to throw some of these things out, I will not be offended.

No. No.

Believe me, I will not be offended. So use your judgment.

Thank you, but I won't. So I think we'll close it there.

Yes.

[00:33:31] End Track 2, Disk 3.

[End of interview]