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

Interview with David B. Thomson

April 12, 2005
Los Alamos, New Mexico

Interview Conducted By
Mary Palevsky

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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

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Mary Palevsky: *So, David Thomson, thank you for meeting with me this morning. Perhaps you could start by giving me a little bit of your background: your full name, place of birth, date of birth, education, and how you ended up at Los Alamos when you did.*

David Thomson: I'm David B. Thomson. I was born in a small town in northeast Kansas, Irving, Kansas in Marshall County, born on October 5, 1927. Born and raised in Irving, and I graduated Irving High School in a class of six seniors in 1945. Our class graduated just at the end of World War II. I had followed World War II closely as a high school student and junior high student and one who was interested in the current events and politics of the time, of the day. And I went to the University of Kansas, enrolling in the fall of 1945, right after Hiroshima and Nagasaki. And they ended World War II, as you all know. I had expected to go into the armed forces, but because of the end of the war, and my flat feet, why, the Army didn't want me after all, and so I went to school right away. I enrolled first in electrical engineering and switched to engineering physics. I had always been interested in electronics and the old pictures of the electron and the atoms, but I didn't know anything about nuclear physics. Our high school teachers did a good job on mathematics and English, but they didn't know anything about nuclear physics either, so I learned that all later on. I graduated in engineering physics from the University of Kansas in 1949. I stayed on for two more years in graduate school and got a master's in physics at the KU physics department. One of my advisors on the campus of the university, at the KU physics department, was Worth Seagondollar [Dr. L. Worth Seagondollar], who was building the Van de Graaf [Accelerator] at Kansas after the war, but he had been at Los

Alamos during the war and worked on the Van de Graafs at Los Alamos. So that's how I knew about Los Alamos and knew to apply to come out here. I applied in the spring of '51. I had an interview in June, late June of 1951, and was hired subject to completion of my master's thesis, which I completed during the summer, and drove to Los Alamos to work full time in September of 1951.

And who hired you and what was the—?

I was hired by a group leader named Vernal Josephson in Group W-5, Weapons Division. And there was a new group that had been established during the summer of '51 to test a particular design for a particular approach to designing nuclear weapons to get more bang for the buck, so to speak, to get more efficiency out of the use of the plutonium and uranium involved. This is one of those early stories, that within a month when I got—I didn't know I was going to work on nuclear weapons tests when they hired me. I just knew I was going to work in the weapons program doing electronics, which is what my training was up to that point, or my principal training in engineering physics. And they sent several of us out to the [Nevada] test site to observe a shot so that we would be familiar with the operation of the test site, which was new and just then getting set up in Nevada. I think that shot was—I don't know the exact date, but I thought it was within a month of when I came to work, so it had to be late October.

Of '51?

Of '51. Three of us went out, and at the suggestion of one of our group members, Hugh Karr **[00:05:00]**, we volunteered to go in on recovery after the shot, to pick up samples. What the J Division did in those days to measure neutron fluxes from each nuclear test, to diagnose the test, they put small samples of gold on the steel rods and poked them in the ground at 100, 200, 400 yards, at intervals out from ground zero, so that you would get an idea of the neutron flux as a

function of distance from ground zero. And they'd pick these up within a couple of hours there was the lifetime decay of activated gold, activated by the neutrons, and they'd observe that decay in counters after the shot, the decay lifetimes must've been of the order of several hours' half-lives. And so I went in with—a security guard drove the Jeep and I picked up the samples, and we picked them up. The shot we observed from a control point, the CP, which was about ten miles. Let's see, the CP in those days was about ten to twelve miles away from the Area 3 ground zero area, but this particular shot was fired in Area 7 because it was an air drop. In fact, we went out to see the site the day before the shot because—I forget who the—a fellow named Byington, I believe was his name, was running the bunker—they had a bunker in Area 7 near where their air drop was to be. And if you have a map of the test site in the early days, it might help us both clarify any detail you may want to clarify. But anyway, bottom line was on shot day, the test bomb was dropped from an Air Force airplane, and I think this shot was fired about a thousand feet altitude. I'm not sure exactly. I don't remember. And my recollection is that the announced yield to the press was 30 kilotons.

*So it's probably this [referring to document]—it's Operation Buster from October to November of '51, and they ranged from, let's see, the one on November 5th is 31 kilotons and the one on November 1st is 21, and the one on October 30th is 14. So it's probably one of those. [Per DOE/NV—209,*United States Nuclear Tests: July 1945-Sept. 1992, *four shots were airdrops in Area 7; Dog=21 kt and Easy=31 kt].*

Yes. Well, all I was going to say is I got introduced to the atomic weapon business rather dramatically, and I'm green behind the ears, fresh off the campus with only a master's degree and four weeks at Los Alamos, or six weeks, maybe. It was between four and six weeks. I've forgotten the exact dates now. And we picked these samples up and we read the meter and we

looked at a map with RADSAFE [Radiological Safety] at CP before we went in on recovery about two hours after the shot.

I should back up a minute and tell you the impression of the shot itself because, though I saw many more later on, that was why we were doing this, to get familiar before we were involved with our own program. And we watched it through the dark glasses. They had the usual countdown and I think Gaelen Felt was the guy who gave the countdowns in those days. He certainly was later on, for the next couple of years, but I think—

Say that name again. Gaelen—?

Gaelen Felt. You'll run into his name someplace in your history. I think he worked in the J-**[00:10:00]** Division office or one of the groups. I don't remember which one. J-Division was the test group that was responsible for conducting the tests. Al [Alvin] Graves was the test director and division leader. I worked for W-5, which was in the Weapons Division in the lab.

Let's see, I was discussing the shot itself. Well, you see the ball of fire through your dark glasses, and you kind of feel a radiation, the heat radiation, you can feel it on your face a little bit from that distance. What were we, ten, twelve miles from the shot, maybe fourteen miles—maybe this particular shot, we were fourteen miles, I'm not sure. I've forgotten the distances. And you wait, and then you take your glasses off after the flash. The ball of fire looks just like the pictures on—well, you saw the picture on the book. [Reference to Thomson's book, *A Guide to the Nuclear Arms Control Treaties*, Los Alamos Historical Society, 2001].

Yes.

The initial ball of fire is—this is a later shot now, but it was the same yield as the one that we were looking at. But this is a later shot. I'll tell you about it in a minute. But that's what it looks like a few seconds after the very initial flash, but the very initial flash is the radiation, the light

that you see from the—and the light from the soft X-rays that are emitted from the bomb. In other words, the bomb puts most of its energy out in soft X-rays, and if it's in the atmosphere, those soft X-rays immediately fluoresce the atmosphere, and that's what makes the ball of fire that you see. And then as it begins to cool and condense, then it begins to look like this; the initial flash is so quick, it's hard to get a good picture of it, but the people at the lab who diagnose these things could take rapid motion pictures and see the real maximum ball of fire. That's the brightness that you see shortly thereafter that is beginning to form the mushroom cloud, the famous mushroom cloud.

Well, then, anyway, you see the flash, and all the energy is emitted instantly, essentially, but then there's a resulting shock wave, and that comes across the desert and you can almost see it from the control point. You can see the shock wave rolling across the desert at the speed of sound, approximately. And then a minute or whatever, I've forgotten the exact time, but approximately a minute after the flash, why, you hear this huge *bang*. You can feel that as the shock wave goes by you, and it sort of—I always likened it to these fourth of July aerial bombs that they shoot up in the air off of a stand. It's a very sharp *crack*. Now, many rebroadcasts of atomic bombs going off, I've heard, sound like there's a lot of *roar* after that, and that's just reverberations. But when the first shock wave comes by from the bomb, it's a very sharp *crack*, and that's the loud sound that you hear when you're listening. At least, that was my understanding and impression of having watched a dozen of these things over a period of two or three years.

Well, we went in—

Can I ask you a question before you go on, about that?

Yes.

You said you were a high school student when Hiroshima and Nagasaki came.

I had just graduated.

Just graduated, and then—well, that's right, because that was June, and then August. And then you're in Nevada, what, approximately six years later, seeing this.

Yes.

What did you think, as a young person, seeing this for the first time?

[00:15:00] Well, see, I went on campus in September, after Hiroshima and Nagasaki were fired, and there are various aspects of all of this. If you want the political aspects, then I can go into it in detail. The political aspects were the two bombs on Japan, horrible as they were, ended the war. And we knew it; I knew it at the time.

That's what I'm asking, what your thoughts were at the time.

I knew it at the time, from reading the newspapers and reading the news magazines and CBS news on the radio, all of which—I avidly followed the war—that the invasion of Japan was going to be a very difficult and costly thing. Because various islands like Iwo Jima that they tried to capture, they captured before, on their way to get closer to Japan had been costly exercises, costly in American lives. And the estimates were several hundred thousand more American troops would die if we had to invade Japan, and maybe millions of Japanese might've died if that invasion had occurred. And so politically, it seemed to me, hey, I'm glad we got that thing. Now, physics-wise, I didn't know the physics of it yet, but we were educated. The press was full of it and the news magazines were full of it and I was just starting in school to major in engineering physics. In fact, I bought an advanced book my first year in college that our head of our department wrote. [James Docking] Stranathan wrote *The Particles of Atomic [Modern] Physics*, and it had an experimentalist description of the development of nuclear physics in the thirties. So

I picked up a lot of that, not all the details, of course, but a lot of the essence of it, pretty quickly. But it was in the newspapers, and it was a dual promise, and I tried to say that in the book [*A Guide to the Nuclear Arms Control Treaties]*. It ended the war, but it also was a threat to the world if we had another world war fought with these weapons. It would be horrible, too horrible to contemplate. But nuclear energy gave promise to solving a lot of problems in terms of energy supply. Now, even though we had gas rationing during the war, we still weren't as aware of the impending likely energy shortages that might occur later on in those days. So it was sort of an academic thing to hear a physicist say, while I was on the campus, for example, that nuclear energy gives a lot of promise for nuclear reactors for power generation. But it was quite obvious, to me anyway, that it was a potential solution to a lot of problems, so I felt that nuclear energy should be developed, but I also felt, as a student of World War II—and I'd watched, as a very young person, watched World War II develop as Hitler took various nations one at a time. I was a very strong supporter at the end of the war of the creation of the United Nations. And I even attended, as a high school student, right after graduation, a church conference while the UN conference was in process at San Francisco in June of '45. We studied the impending United Nations charter, and a small group of us said, well, we're all for that, but it won't work if they **[00:20:00]** have the veto in it. Well, we were a bunch of young students, high school graduates, if you will, and we weren't experienced enough historically to tell you what you had to do in place of the veto. But we sent a telegram—I was an activist as early as that period of time—and sent a telegram from our little study group in this little Emporia, Kansas summer camp—it was a one-week youth camp, church camp—.

Right. Which church?

Presbyterian. We sent a telegram to the U.S. delegation saying, `Don't put the veto in this charter` [laughter]. And of course, it would've taken a lot—political scientists have failed for the last sixty years to come up with a new version of the United Nations that would solve the problem of proper representation and being able to accomplish things, and a proper executive. But I was a world federalist at an early stage. And when I came to Los Alamos, why, there was a chapter of United World Federalists [UWF] here, and of course the world federalists didn't have all the answers either. But they knew what had to be done, and it's never quite been done, but I'm not a big idealist. I've been involved in politics enough to know how difficult it is to accomplish even simple things like the charter for Los Alamos, let alone the proper organization of the international community. But nevertheless, it's a goal. This is jumping ahead here—

That's all right.

But these arms control treaties are showing some of the things you have to solve and some of the political mechanisms you have to evolve if you're going to make the world a safer place.

Well, I don't know, you wanted to get back—

That's great that you added that, because that gives me this personal perspective, but my only question was; as a young person who's lived through the end of World War II, when you see that event that few of us have seen, of this atomic bomb for the first time, what it was like? That was all I was trying to get [understand], from a personal perspective, or are you just thinking about the science or what? What's it like?

Well, I came to work in 1951. The war in Korea was on. NATO [North Atlantic Treaty Organization] had—the Russians had developed their first—they had fired their first fission weapon a couple of years earlier. The battle in Washington over building the hydrogen bomb had already occurred, and Truman had made the decision, which I felt then and still feel was the

correct one, to go ahead with the H-bomb development at Los Alamos. And when I came out here, participating in these tests in Nevada, to me, I looked at them as physics tests. And I recognized and, as I've already indicated, was an activist in the effort for some effective world international organization, if you will, that could keep us from ever fighting again. But we were facing the Russians, the Communists, the Stalinists—and when I saw that first shot, Americans were dying in Korea as we spoke—were also threatening—NATO was just getting started as a response to the Russian threat. And so I felt we had to (a) stay ahead of the Communists in the development of nuclear weapons, and (b) do whatever was necessary to be strong enough to prevent what had happened during World War II, which was Hitler taking all of **[00:25:00]** Europe and almost the whole world before the U.S. responded and was able to push back and win; win both in Europe and in Japan. So there was never any philosophical problem, to me, the U.S. action and the government, and to work for the laboratory and work with nuclear weapons was a very reasonable thing for me to do. And I soon learned—as I learned how the laboratory operated and learned how important nuclear physics was and how much nuclear physics got done here, particularly in the fifties—the laboratory was one of the centers of nuclear physics and on the threshold, on the frontiers of science, and I felt that they were doing it the right way.

I don't know. Back to the bomb itself, we were picking up these samples and we got into ground zero and the meter read 25 R per hour. We knew we weren't supposed to have more than 3 R per operation, and so we didn't want to stay around there looking for that last sample, which we didn't quite find, and we got out of there. But we got all the key ones that we needed. It was a dramatic way to be introduced to the weapons program. But I've always been proud of that particular operation.

We came back home and worked on our project, and we fired four shots on towers, one after the other, two in the spring of ‘52 and then two more in the spring of ‘53, and it was a series of shots with certain parameters being varied for each shot, which are all classified, of course, as to what the parameters were. And we developed that particular approach to the future design of weapons and made that contribution. And then—

These were all Nevada shots?

Yes.

So you’re working on a particular problem that’s tested out in that period of time.

Yes. Right. By our group, Group W-5 under Vern’s leadership. I can give names of other people. John Wieneke was the deputy group leader. I don’t know if you want that kind of information, but I can give it to you if you—

We don’t have to go through it. It’s Vern Josephson, you say?

The group leader. And John Wieneke.

They left the lab in the middle fifties and went to laboratories in California, but I’ve kept up with both of them with Christmas cards through the years, but they’ve both died within the last five years.

So did I understand correctly that W-5 was created to solve a particular—?

A particular set of nuclear tests, yes, to develop a—we’d have to go behind the fence to describe what it was. And within three years after I had hired, we finished our test series, and this one here is the last one. It was called Hamlet and it was because the—

Oh, so that’s Hamlet, you say?

Yes, that was Hamlet and it was May 19 in 1953. And we came home from Nevada after that shot. We were out in Nevada only about two to three months each of those two springs, ‘52 and ’53.

Oh, see, they call it Harry in here [DOE/NV—209].

Yes, but Hamlet was what we called it in W-Division. Harry is the shot number. They called it Hamlet in the jargon; an obvious name because they hadn’t decided whether to shoot it or not. It was an add-on to the first three, and it turned out it was maybe the best one, but anyway, it was **[00:30:00]** to be or not to be. We fired that, and that was the last shot I saw.

Oh, it is the last shot you saw. And that was—?

Yeah, because we came home and did some other laboratory work and did some neutron physics and within a couple of years, the whole group moved into the magnetic fusion program, which was then called [Project] Sherwood. It was under Jim Tuck. But it was a direct conversion of a bunch of weaponeers, physicists going into what has turned out to be a long-time study, a long-time effort, to bring fusion energy to the world to solve the energy problem. And fusion energy would be better than fission energy because you don’t have nearly as much nuclear waste. And it’s an inexhaustible supply because you get deuterium out of the ocean and so on. And so the lab had started a program in magnetic fusion in 1952 under Jim Tuck and Jim Phillips and a number of other people who I could name. But our group, our weapons group, joined them in 1954.

Just let me understand that a little better. So you figured out, with the last shot, you figured out what you needed to figure out, basically?

For weapons.

For weapons. And so they say, what are we going to do with these guys [physicists], or—?

Well, the group leader knew what they might do with us, and we might end up doing a lot of engineering work, which would be important, but he wanted to do something new and exciting because he had a bunch of physicists, half-a-dozen physicists, who had ideas, and so he said, Well, let's go into fusion. And so we did. And we could've continued in the weapons work, and I know many of our compatriots were in other groups that continued and we kept in touch with them, of course, but—well, I'll let you ask the questions. I'm not quite sure where to—in terms of my own career, why, I said I came with a master's degree—

Right, let's talk about that.

And I had an important job to do in the firing of these shots, using the electronics that I knew, and had quite a bit of responsibility in terms of certain equipment. But when we got through with that, why, I could see that to really have the full option to do different kinds of work that you might be most interested in over the years, that you needed to have a Ph.D., or it at least appeared so if you were a physicist. And so I went back to school for a year, on a leave of absence from the lab. I worked out an arrangement, my group leader had sponsored me to do my Ph.D. thesis in Los Alamos, should I pass all my other courses and exams.

Where did you go for your Ph.D.?

Kansas.

Oh, you went to Kansas.

Yes, I went back to Kansas and did another year of coursework, and then came back to work here; we were working in the fusion program. But I had to go back to take the prelims and the language exams, the German exam. You know, I went back for one exam at a time. I'd study for it, nights, out here and then go back to KU and take the exam. Took me several years to get ready to pass everything, all the prelims, so that I could do my thesis. And the division leader

then decided that I should do the thesis on the Van de Graaf rather than in the fusion program. Fusion was too new and he didn't think it was—there was some political turbulence in the—and other turbulence, and the plasmas had a lot of turbulence in them, if I can crack a joke. Between the two, why, they thought that it'd be better to do the thesis **[00:35:00]** somewhere else in the division. I picked the big Van de Graaf, the time of flight program that was under—my thesis advisor was Dr. Larry Cranberg, and they had the latest equipment in neutron spectroscopy by time of flight at that point. I was very pleased. It took several years to do it, but I was very pleased with the thesis. [Larry Cranberg was the leader of neutron time-of-flight work at Los Alamos and was very helpful to my career.]

Explain to me what "time of flight" is.

Well, you measure the energy of a neutron or any particle by just how fast it's going and how long it takes it to go from one point to another point.

And so that's what that was.

Yes, and they still use—well, Louis Rosen had done some early—I may be jumping ahead because I don't know if you've done all you want to with the weapons work or the bomb testing or—

I'll ask you a question, but go ahead.

But Louis Rosen, who later started LAMPF [Los Alamos Meson Physics Facility] and became—

My dad [Harry Palevsky] was a colleague of Louis Rosen's and worked on LAMPF.

Well, in the early fifties, he did neutron spectroscopy with emulsions, and they used to have what they called Rosen's Harem, a group of ladies who were trained to read track lengths in emulsions, and that gave you the energy of the neutron that hit them. Louis studied nuclear temperatures in 1953. And of course, he did a lot of work at the test site also, getting the neutron

spectroscopy off the bomb tests. But back home, doing pure physics, why—every physicist wanted to do pure physics at the lab. The weapons work was an application of our trade, but what we'd rather do—which we knew we needed to do for the country, to develop the best weapons possible and to stay ahead of the Soviets—but it was based on the science of nuclear physics and we wanted to learn all we could about it. Louis studied nuclear temperatures from a scattering of high energy neutron off of high Z elements; a high Z element being a heavy element like lead or the highest Z would be uranium and plutonium, of course. But we stayed away from the uranium and plutonium in this pure physics stuff that you wanted to publish, for the most part. In those days because you could do that for the weapons people and get them cross-sections for weapons work, but for the publishable stuff, we dealt with non-classified elements. And anyway, Louis did this nice job of looking at nuclear temperatures, which gives you a measure of the distribution of energy levels, of excited energy levels in high Z elements. And so then I did it with Larry Cranberg on the Van de Graaf, and we could do it with much more resolution and intensity-to-resolution ratios, and more rapidly, on the Van de Graaf with the time-of-flight method five years later. But I always give Louis credit for inspiring us to go into the nuclear temperature business. And, well then, twenty years later, after he'd built LAMPF, Louis Rosen and his people out at LAMPF, they made another factor-of-a-thousand improvement in intensity-to-resolution ratio for neutron spectroscopy with the equipment out at LAMPF, and it's just the nature and the way it operated.

So it's a regime of activity that has continued in nuclear physics, that has continued through at the laboratory and elsewhere. But I think the lab has been second to none in **[00:40:00]** understanding neutrons from the very beginning. And there were lots of other kinds of neutron experiments that different people did, and even that I did a little of before I went into

the fusion program. So my career oscillated between weapons testing in the early days and then the fusion work, which involves plasma physics; it's a different regime than the nuclear. I did my thesis in nuclear physics and then went back into high energy fusion studies later in my career. Now I'll let you ask—

Well, the only question that I had at this point about Nevada was you're basically there working for, what, a couple of years, I guess?

Well, I was in the program for three years, and we spent two months in the spring of '52 and two more months in the spring of '53, and those weren't solid months; they were broken into two pieces, in my case.

That was what I was going to ask you about. So you would drive, you would fly, you would—what would it be? Just sort of real mundane stuff. What was it like to get back and forth?

Oh, we'd fly out to [Las] Vegas and stay at Camp Mercury.

Oh, you would?

We lived at Camp Mercury, yes.

Would the scientists basically stay as a group together where they don't talk to other people about—?

Yes, they had bunks. It was a bunk situation. It was a lot nicer quarters than they had down at Desert Rock where the soldiers were. And I went down there—I had a high school friend—one of those six I mentioned I graduated from high school with, a fellow named Gene Osborne, and he was in the Army. They took him right after graduation—he was a little older than I was and he had been drafted in the Army right after World War II—you know, in June of '45. And then he finished that, but he went back in, the reserves, I guess, some years later, and ended up out at Camp Desert Rock with the soldiers who were being used to get acquainted with the nuclear

tests. And you have other histories, I'm sure, that have discussed what they did at Camp Desert Rock. But they lived down there in pretty primitive quarters, and would go into ground zero after a shot with their counters and so on to learn what they could do and to learn how to do it in the vicinity of a nuclear test. They were being trained so that in Europe, if we had to use tactical nuclear weapons to stop the Soviets from taking over Europe, we'd have some troops who were trained if we had to operate in the vicinity of a nuclear weapon being used. And tactical nuclear weapons were developed in the fifties at Nevada. That was part of the Cold War.

Right. So you were in touch with your friend Osborne so you—

I went down to visit him one weekend.

So you knew he was there, then.

Yes, I heard from folks back home that he was there, and so I looked him up and went down to see him one day while he was at Desert Rock. So that was kind of fun.

So then, would you see the troops on their exercises?

I wasn't ever right near them. In our first shot in '53 – it was called XR-3 in our jargon; it had another name, one of your Harry names, or George, I forget what the name was, but in our test series we called it XR-3.

Fifty-three.

It was in March of '53. It was a tower shot and it was about 20 kilotons. And—

OK, so the first two was Annie and Nancy.

Annie sounds like it.

It's 16 kilotons, is Annie, and 24 kilotons is Nancy. They're both tower shots.

But the first one in March—

Yes. Annie.

[00:45:00] —was ours, and it was kind of like the county fair out there. We were doing our thing, setting up the shot and the electronic [equipment]—we had complicated electronics to fire our particular shots between the tower and the bunker—the bunker is about a half-a-mile from the tower, and the bunker is at least one foot, foot-and-a-half-thick concrete walls covered with dirt and then the dirt's covered with sandbags. Inside the bunker, we have an array of oscilloscopes and we're measuring things. And we bring the signals from the tower, from the detectors that are up close to the bomb in the tower, and you use air dielectric cable to come down from the tower and then under. They bring it down and put it underground so it's protected, and then it goes into the bunker.

Say the name of that cable again. Air—?

Air dielectric. And the reason is you can't use just a simple coax[ial] cable, because you want your signal to get down there before the bomb signal wipes it out. And we had pulses coming that mean different things, coming down the cable from a detector that's near the outside of the bomb. And there's a race—your signal comes in at the speed of light down the cable, but not quite the speed of light, and then the radiation field from the bomb, from a given level of generations up from the—[pause.]

Once you get to a certain level, the radiation field from the bomb ionizes and will destroy the cable.

OK, ionizes and will destroy the cable.

And so you're electronic signals that you're interested in, the near time and the short time, are coming down these cables, and this signal, this radiation field, is coming behind it and chasing it. If you're looking at it on your oscilloscope, it's on a microsecond time and we see our various signals from the bomb, and then you see your signal just goes off the screen at the end of the

sweep, if you know a timing sweep across an oscilloscope. And so that was definitely an exciting little thing, to figure that out, and we did some experiments to try to optimize the information we could get from the signals.

Sure. Interesting.

But that was just a typical technical problem, a small one. It's an engineering problem, but it's one of the kinds of things, if you're testing weapons, you have to deal with.

So you had said when you started that it was like a—I forget the exact words, a county fair or something like that.

Oh, yes.

So there's a lot of—

The county fair aspect was because they brought the Civil Defense tests into the tests, They built these two houses. Let's see, the first one was about a half a mile, three-quarters of a mile from the tower, and then the second house was maybe a mile-and-a—have I got that—I think I've got it right, about a mile and a quarter.

I can look that up.

It's thoroughly discussed—in fact, I have a book; I'll have to get it out and show you. You probably have this.

I may have it, yeah, let's see.

If you don't have it, why, you should get it.

No, I don't have it.

Well, look at that. It's written by—

Oh, by—OK.

I know the author because he is the historian for the On Site Inspection Agency [OSIA].

Oh, he's the historian;, [Joseph P.] Harahan.

Yes. In fact, we had him out here two or three years ago to speak. And it has his perspective—*Great. I'll get this.* Defense Nuclear Agency, 1947-1997 *[DTRA, 2002]. OK. Well, and maybe that's why—so your test is Annie, and then they're also doing Civil Defense stuff with it.*

As an add-on to the test. And so we're busy, trying to get our stuff (experiments) ready and making it work and **[00:50:00]** we want that bomb to go off and go off right, and that's what's important. And here's all these Civil Defense people out there, and they have cars with mannequins in them, they have these test houses, all kinds of stuff. And so we said this is sort of like a county fair out there because that had nothing to do with the technology of firing the bomb. But it had a lot to do with observing the effects of the bomb on people and on cities and civil defense, and it was important to do it, and I was glad they did it. Our particular shot, this first shot in March of—Annie shot—

Fifty-three.

Fifty-three. They showed a sequence from that shot in *Time* magazine, and you've probably seen it. They show the sequence of pictures as the radiation from the bomb—that's the nuclear radiation, the soft X-rays and hard X-rays and the heat from the bomb, and it's basically a heat wave from the bomb—sets the house on fire. And then a few seconds later, the shock wave comes along and puts the fire out and knocks the house over. And it's a rather interesting phenomenon. We observed it all the time in one way or another. We observed it when we were picking up the samples in that very first shot I observed. They had a stake in the ground to show how far you were from ground zero, every 200 yards or so, and the stakes were always black on the side facing the bomb and white—it was a wooden stake and they were blackened by the heat wave from the bomb, the radiation wave. And so I've always said, if you're in the vicinity of

where you think a bomb's going to go off and you're not in a cave kind of a bomb shelter, if you're likely to be in the open, cover yourself with a white sheet – that's the least you can do if you can – so that you can reflect the radiation so that you don't get burned. Because of course, citizens at Hiroshima were badly burned, a lot of them. Now, that's the first thing that gets you is that heat wave if you have a line of sight, but you don't want to be in a line of sight with a ball of fire. That's the bottom line. Get down in a ditch or whatever you can do to get out of the line of sight, if you have any idea where it's going to be.

Well, that's interesting, because sometimes when people look back at the Civil Defense exercises we did as students, duck-and-cover, they say, Well, if it was a bomb, it wouldn't make any difference. But you're saying if you got your body out of the way of the heat, it would make a difference.

Well, it might. It depends on where you are, what the physical arrangement of things around you is. But if you're in a bunker, in a good bomb shelter like our bunkers that we had our oscilloscopes in that I mentioned, they were half a mile from the shot, and we'd have been happy enough to stay in there during the shot. They wouldn't let us. But after we had seen a few, and we left the radiation counters inside the bunker, there wasn't any radiation in the bunker, because you had this much dirt and concrete to protect you; if the bunker was physically strong enough to withstand the shock, air drop or a tower shot, you don't have as much coupling to the ground as you would if the bomb went buried into the ground. Now, there are other people that made all these studies of what happens when the bomb is underground, and of course we've fired hundreds of underground shots, and so that's a different phenomenon. So what you do to protect yourself in these things is going to vary a lot [depending on] what the situation is, but certainly a

good bunker with three or four feet of dirt is about as good as you can do, with enough steel strength around you so the bunker doesn't collapse.

[00:55:00] *Right. But they wouldn't let you go into the bunker. You just had to leave your equipment in there.*

Right.

You're saying you wanted to?

Oh, I don't know. We kind of were curious, and I would've if there had been a good reason to. There wasn't a need to. We set it up so you didn't have to. And we'd go in an hour after the shot and get our film out of the cameras and develop it and find out the data and so on.

This question just occurred to me. For those of us who have never seen anything like this, it just seems like it would be so amazing to see even a fission bomb, right? But as you're doing the tests out there for this three years, do you—this is sort of like a human nature question—do you get accustomed to it, or is it that it—?

Yeah.

You do. OK.

I look at it as physics experiments. You had to be very careful—there were lots of dangers. I mean just working on 300-foot towers is not the safest thing in the world to do. In fact, I climbed it—we had elevators, but once in a while the elevator wouldn't work, and I was a young twenty-four-year-old—my compatriots who were in the armed forces facing bullets in Korea—so you looked at things a little different in terms of personal [danger]. But we tried to be reasonable as far as personal safety was concerned, and the laboratory was and had safety. Roy Reider was around and in charge of Safety, for example, and he was the safety head honcho at the lab for many of the early years. I don't know if you've heard—

No, I haven't.

John [Hopkins] might've mentioned Roy Reider's name to you in the course of his discussions.

He didn't, but now that you have, I'll add him to my list.

Yeah. And there were others from the Health Division who were responsible for safety. But in one sense, you'd watch the shot on shot day—whether it was your shot or somebody else's—but it figured that if it was your shot, you wanted to see that ball of fire a certain size to know that you'd gotten the yield that you [expected]. And I could tell by [that time]—I'd get my eyeballs calibrated in looking through the dark glasses where I could tell whether the yield was up to snuff or not, and I'd know instantly. But the exact value, of course, took a lot of measurements, and other groups in J-Division were measuring the yield of each shot.

Sure. So approximately how many shots did you see during your tenure there?

Probably about a dozen, all told. Yes. But only four of them were [those] that our group was responsible for setting off.

Right. Well, we're just at about an hour. Not quite. But we can stop here.

[00:58:25] End Track 2, Disc 1.

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

We just discussed that you would spend the second hour talking to me about the arms control issues, international issues that you alluded to earlier as having an interest in since you were a boy. So you hit Los Alamos and there's the World—tell me the name again, the World—

Well, we had a chapter of United World Federalists in town, which is an organization dedicated to trying to revise the United Nations charter so that it could be a more effective organization. And we had quite an active group. When I came, there were maybe only fifty or sixty members, but there had been, I think right after the war, as many as a hundred or a hundred and twenty

people on their mailing list locally. And also right after the war, the person who can tell you about this, of course, is Ed Hammel who was active before I came, 1945-46 era. They had the Los Alamos Association of Scientists [Association of Los Alamos Scientists, ALAS], they called them, and they were concerned about nuclear arms control. I describe it [that period] in the book; [J. Robert] Oppenheimer himself, right after he left the lab in ‘45, became active, lecturing around the country, urging that something be done to control nuclear weapons and to see to it that they didn’t destroy mankind, if you will. And he was involved in the Baruch Plan, which was put before the United Nations. It was an amazing offer by the United States to give up its nuclear monopoly to an international organization, provided it was properly safeguarded; particularly that the Russians agree to the verification of it, as necessary to make such a system work and so that the U.S. could have confidence that it would work. And whether the Baruch Plan, which was turned down by the Russians, actually would’ve passed the U.S. Senate, I suppose is a matter for historians to debate.

But nevertheless, it was the U.S. proposal. The Russians turned it down over inspection issues and openness issues. And arms control faded away for a while because the Cold War came along and the emphasis was on NATO and responding to the Communist Soviet threat in Eastern Europe and threatening Western Europe. And that was why some of the tests, not our [W-5’s] tests but some of the other tests in Nevada in the early fifties, were specifically to develop tactical nuclear weapons, which are still a problem today in the sense that they’re stored. The U.S. and the Russians have agreed long since not to use tactical nuclear weapons and to get rid of them, but the Russians haven’t verifiably gotten rid of all of them yet. There’s thousands of them out there. I’m jumping ahead but that’s—

That’s all right.

That's one of the arms control problems today.

Well, picking up where you wanted to on the testing issues, [Dwight D.] Eisenhower reinitiated the efforts to get some arms control with the Soviets. Truman gave up after the Baruch Plan was defeated, and he had his hands full. He had to worry about the start of Korea and NATO and so on and those issues in Europe. But Eisenhower proposed, and I describe it in the **[00:05:00]** chapters in the book, Eisenhower proposed limitations on arms, nuclear arms that is, and he also proposed the Open Skies Treaty, which was an openness proposal in 1955. And he also essentially proposed the creation of the IAEA, International Atomic Energy Agency, in his 1953 speech to the UN. The IAEA was indeed created and started a mechanism of at least learning about—to promote nuclear power for the Third World countries, among other things, and peaceful uses of nuclear energy worldwide, is what the IAEA was initially set up to do; people from Los Alamos participated in those early IAEA conferences. Now, I mentioned the fusion program. I wasn't part of it in 1958. I was still part of the bunch that—but they took some of our fusion experiments over to an IAEA conference in 1958 and demonstrated how—well, we [LANL] demonstrated the early theta pinch with the Scylla 1 experiment which made the first thermonuclear neutrons in plasma.

Say that name again. The first—you said the—

First theta pinch, which was called Scylla 1. Yes, because there was this series of experiments called Scylla 1, Scylla 3, Scylla 4, Scylla—It's the Greek goddess.

Got it.

Well, OK, that was going on. In other words, in Los Alamos, we had one bunch of people working on the weapons directly, testing in Nevada, and other people doing basic physics, nuclear physics in P-Division, and the plutonium materials people were doing their thing at DP

Site [a plutonium site] and so on. All the other sciences that go to make up—they had to be synergized to do nuclear weapons. The scientists who did all of these different sciences were also trying to carry on their careers and carry on regular research; [Norris] Bradbury was very good about getting money from Congress to do weapons, in which he had a lot of discretionary money [with] which he could do basic research also. And I did my thesis on weapons discretionary money. Today they call it LDRD, but it's weapons-supported research is what it is.

And so that's all going on here at the lab. We're maintaining what I'll call our 'science base' so that we can provide what has to be done. Well, the tests were getting bigger and bigger as you know, nuclear tests and the H-bomb had been fired, and John [Hopkins], I'm sure, told you a lot about some of the big shots maybe in the Pacific that created a lot of fallout. Fallout became a big problem worldwide. The Russians created more fallout than we did, but we both created plenty. And you're in Nevada, so you're closer to the story about the fallout from the test site and the local neighborhoods. And in fact, I'm told that Hamlet shot put out quite a bit of fallout. You may—

It is one that's very controversial: Harry, "Dirty Harry." There are a lot of people who are concerned about what that did to their communities.

Well, we didn't know that at the time, but it became evident later, but we didn't hear as much about that, but that was just simply because we were doing our thing, and we knew there was a lot of fallout from every shot—the amount of fallout depended on the weather. Al Graves and his team at J Division, they sweated out every single shot, and we'd delay shots for a day, for two **[00:10:00]** days, for three days, to try to get the best weather conditions to minimize fallout. And so they missed it on Dirty Harry. I mean I don't know about it, but the people involved with the weather predictions and that sort of thing can tell you all about that.

Yeah, and I have had long conversations with one of the meteorologists and of course, as you know, it's still an area of controversy, to what degree and how much and how much danger was there. So depending on who I'm talking to, there was more or less danger.

Yeah.

But I've had a chance to talk to one of the early meteorologists about all that he went through to avoid things going wrong.

Well, I think it was probably a mistake not to have more film badges in the communities involved. But I don't know. I don't know how many they had, and I'm sure you're running into stories about that.

I am.

And there are people here, like a friend of mine that lives down the block, Ed Bemus, who died in summer of 2005, was one of the people in Health Division that was involved with keeping track of the film badges and developing better ones and all of that kind of thing, and some of those people, he could tell you who to talk to, who's still around from the Health Division people here. They tracked how much radiation I got, and I got it mostly from going in on recovery in 1951. I got 2 Roentgens [Roentgen equivalent man, Rem] out of allowed 3. We were allowed 3 Rem per operation. The first year I was here, it was 12 Rem per year, and then they cut it back to 5, and it's been at 5—the maximum you're allowed per year ever since. Studies of radiation and health are beyond my scope. I have a daughter that majored in radiation biophysics as a pre-med. That was her pre-med. In fact, she did that at KU, at Kansas. So she could tell you more about radiation.

It's a very interesting subject matter because scientists disagree fundamentally on a lot of these questions.

Well, I don't think that 3 Rem per operation is going to do you any harm, based on anything I've seen or experienced, but I have no idea of what the genetic—but we live in a high rad—you live in a field of radiation, of cosmic rays. I used to develop neutron counters and we had to worry about minimizing the background count, and so I know that if you have a detector about so big, a six-inch sphere, if you will, or a cylinder—mine is cylindrical—why, you get at least sixty counts a minute that are cosmic rays, that are very high energy, that go through the whole volume of scintillator, which is similar to water. So you get sixty, a hundred counts a minute of cosmic rays through your body, year in and year out, at this altitude anyway. And I don't know the precise numbers, I don't keep track of that sort of thing, but other people can give you the numbers.

Yeah. But I want to get back to the—you started talking about—oh, I think this started because the concern, the international concern about fallout, and this is connected to—

Yes. And some of the big tests at Enewetak, which I know about them because we heard reports on them and—

I've talked to some people that were on those tests.

But there are many others here in the community who actually were involved, and John being one of them, of course.

Yes, and Ben Diven.

Yes, Ben. I worked—

I'm seeing him this afternoon.

Oh, great. I'm glad he's doing it because I worked for him for several years in later years in another set of experiments. But he was involved with tests, more in the physics of it. He **[00:15:00]** developed a lot of good physics, neutron physics, during the underground tests series

and—he did a great job of that. His group, they did a lot of good—they got a lot of good science information from the underground shots.

That's what I understand.

And when they first started that, they didn't know—I'm jumping ahead again.

That's OK.

But they didn't know for sure they could. People thought, oh, we can't get good data if the tests are underground, whereas we can get good data if they're up on a tower. Well, the lab responded to the need and demonstrated that over time they got better physics data underground than they'd ever gotten, because they spent more effort on it, doing physics experiments that were related to the tests or that were, let me call them a spin-off from the bomb tests.

Yeah, I talked about that last year. That was very interesting to learn that aspect of Nevada, that you could do those physics experiments.

Yeah. Well, OK, the politics was that the fallout was getting worse and worse, and Ike [Eisenhower] was trying to get [Nikita] Khrushchev to agree to—he tried to get him to agree to Open Skies inspection; and to test ban with verification—on site inspection being needed because we didn't have enough verification capability, if the tests were conducted underground without an on site inspection. You couldn't tell how big it was or whether it was within a given threshold or whether they'd even fired one or not if it was a thousand miles away from your nearest detector and there are earthquakes in the area and all of these things. A person who you could talk to locally who knows quite a bit about that sort of thing would be Paul Mutschlecner. He's one of my close compatriots in the arms control business, but he works in that arena. He knows—

But let me ask you, on the ground in Los Alamos, I want you to tell me more about this, but you've got the Association of Los Alamos Scientists or whatever that was—

We formed a Federation of American Scientists [FAS] chapter in about '55, or maybe '54, and a lot of the same people that were in the UWF group, the federalists group, joined the FAS, plus a lot of others. And we studied test bans and what it took to detect the tests and what it would take to verify and that sort of thing. And so, as a citizens' group of scientists, we knew the issues and followed them closely.

And I might remark as an aside that Senator [Hubert H.] Humphrey chaired a disarmament subcommittee of the Foreign Relations Committee in those years and he did a tremendous job. I read a lot of his hearings and I give him great credit for getting the experts in and of delineating the issues.

Well, Ike proposed a limited test ban, which would limit atmospheric testing and allow underground, and Khrushchev said, no, he didn't want that. He said, You've got to eliminate all testing or none at all. And I don't want any of that on site inspection in my country. I mean that was a fundamental issue for decades with the Soviets. And the secrecy, to them, and in keeping you out of their country was more important than any arms limitation.

But the lab studied the issues. J-Division knew how to verify yields of shots and the existence of shots in the atmosphere. And then we studied fallout, and our neighbor next door worked on the airplanes that did the sampling, that go through the clouds of atmospheric—Paul Guthals can tell you all about the sampling business. And there were people in J-10 who looked **[00:20:00]** at the high altitude shots, a fellow named Herman Hoerlin who is I don't think is any longer with us, he's no longer with us; and Don Westervelt who was very much of an expert in

that field for many years, from J-10, died just two or three years ago, and he followed these test ban issues rather closely for many years, too. One of the people I refer to in the book.

Well, where were we in 1963? Oh, another thing, you're going to talk to Ben Diven, his boss was Dick [Richard F.] Taschek, and Taschek started in 1959 or '60, the Vela program, and they developed detectors that would look at soft X-rays. Now, I noted to you earlier on that when the bomb explodes, 80 percent of the energy first comes out as soft X-rays, so if it's fired in space, there's nothing to stop the soft X-rays, and it's the soft X-rays that you can detect from a distance. And so Vela was predicated on the assumption that if we put a satellite up there in space and put a soft X-ray detector on it, then you could detect the clandestine firing of a shot in space somewhere a thousand miles away.

In space. A shot in space.

Well, above the atmosphere.

Above the atmosphere. OK.

Yeah. On a rocket up a couple of hundred kilometers or so where the test might clandestinely be conducted. And so, again as a science project, but driven by the need for test detection, Taschek and, I don't know—Ben will know all the details also; he was in the Physics Division at the time it was done—of how the detectors they used were related to, not identical to but related to the soft X-ray detectors we used in plasma physics to look at theta pinches with a vacuum line-of-sight to measure characteristics of the hot plasma that's being developed for— So all I'm trying to illustrate here is the synergism of the science between things that have to do with whether it's pure fusion you're trying to create or whether you're trying to detect clandestine nuclear tests or whether it's you're trying to understand a nuclear weapon being fired somewhere. Let's say it's being fired underground nowadays if it were to be done now, and you have the detectors nearby,

to measure various characteristics of a modern nuclear weapon, there's no boundary of the physics. It's the application that counts.

And OK. So Taschek started a program to develop the Vela system. And they had their prototypes working by the time the [Limited] Test Ban Treaty was signed. And as you know, Khrushchev, after he violated the moratorium—well, Khrushchev and Ike signed a moratorium in '59: no tests. And all the guys in J-Division had to look around—they had to stay on standby because we knew we couldn't trust Khrushchev and you'd probably have to test again someday. And so the various guys in J-Division looked for different jobs and keep their group together so that they could resume testing if they had to. And that was another phase of the laboratory. But it was during that phase that a lot of new ideas were being generated on peaceful uses of nuclear energy, and so there are a lot of people that can talk to you about that. We were already working in fusion-type experiments, and so we'd already had our peaceful use of atomic energy, but these people like me had their eyes on applications that the weapons people might need or for detection or whatever. But I wasn't directly involved with **[00:25:00]** Vela. Vela was one of the bright spots of the laboratory because it worked and they were able to—I think they had the first satellites with a soft X-ray detector on it within a year or two of the signing of the Limited Test Ban Treaty [LTBT].

Well, what role did we as scientists play in this in the political scene? Well, the role was played by our director, Bradbury, and the hearings—this is a story I tell in the book and which I think that I'll tell again right now—is that the treaty that was agreed to was a limited treaty that allowed underground testing because the Russians wouldn't allow on site inspection to eliminate all testing. And so they're going to continue underground testing, we're going to consider underground testing, but what we're limiting is atmospheric tests, and that gets rid of the fallout

problem, for all practical purposes. And that's a major boon to the Earth, to the country, because the fallout from some of the big shots had gotten really serious and it resolved the problem about Strontium 90 [Sr90] and so on. You've been through all of that.

I'm talking to people about that.

Yes, and so you know more about that that I do, except it was a serious—but the Limited Test Ban Treaty had several reasons for it. One, it would be a first step, the first time that the Russians or the Soviets had agreed to a significant arms control agreement that could be verified by national technical means. And national technical means means we can do it without having to go on their soil to verify, and the Vela satellites were the icing on the cake of our national technical means. They were developed by physicists in P-Division under Taschek and people like j and others, and Ben can tell you in maybe a little bit more detail, and Taschek, if you could talk to him. [Taschek died in December of 2005.]

Oh, I didn't realize he was still alive. OK.

No, he's still here, but I don't know what his health is. But I give him a lot of credit. I had him to speak to our arms control group once a few years ago and he wouldn't take the credit that I thought he deserved.

But you were saying what Bradbury's role was in this.

All right. Well, Bradbury had the whole picture of what the lab could do. We [Los Alamos] were responsible for 80 or 90 percent of the stockpile at that point in time. Still are, I think. And he knew the role of weapons testing. He knew what we needed for future—what the DoD [Department of Defense] had requested for future weapons. He knew how we could verify it. Verifying a Limited Test Ban Treaty was a piece of cake. We had a dozen ways to verify it along with the Vela thing. And most of the scientists nationwide agreed. They supported it. However,

Ed [Edward] Teller opposed it. And Ed Teller had a lot of influence because he was "the father of the H-bomb," a title which I think Bradbury really deserved because it was his laboratory that developed it. But that's neither here or there. Bradbury didn't want to be known as the father of the H-bomb and he was not the one who ran to the newspapers every day like other people did. And because Ed Teller and a few of his closest compatriots argued against the Test Ban Treaty—they said they didn't trust the Russians—there were a few conservative politicians in the Senate who followed his lead on it. And the final vote, I think, was sixteen votes against it. I quote it in my book. In other words, the two-thirds vote for ratification of a treaty was not a piece of cake. It took **[00:30:00]** some doing. And we in the FAS, we wrote lots of letters to all the senators and so on, and the national FAS did and the local FAS did, and we submitted testimony from our local Federation of American Scientists saying that this treaty is in the interests of the United States and the interests of the security of the United States. But it was Bradbury's testimony in the Foreign Relations Committee. In fact, the hearings were joint hearings by the Foreign Relations Committee and the old Atomic Energy Committee—Joint Committee on Atomic Energy—and I think the Armed Services Committee was also in on those hearings. They had one set of joint hearings and the various people testified. And if you read Bradbury's testimony, you'll find that it's a brilliant exposé of the issues and his faith that this laboratory and the two laboratories—Livermore was in the game at that time—and the U.S. science community could do a better job of maintaining its weapons capability than the Russians could under the terms of the treaty. Therefore we would not lose anything in security, even though we were trying to develop new warheads for new ICBMs [intercontinental ballistic missiles]. Most of those had already been developed but some of them hadn't, and newer ones were developed later, of course. And he had confidence, even though some of the early underground tests didn't do so

well on the physics, Bradbury had that confidence that we would, given that set of parameters, and that we, the United States, could do as good or a better job than the Russians could. And then he, of course—and we knew how to detect clandestine tests. And Ed Teller even came up with a proposal that the Russians will test behind the moon and you can't see them. Well, we can send satellites behind the moon. So I give Bradbury tremendous credit for the key leadership, because the other laboratory was opposing the treaty. Well, it wasn't so much the other—the other laboratory supported Teller's technical assertions, but they didn't take a political position necessarily for or against the treaty. But Bradbury had the faith that we must take this first step for peace because we had tried for years to get the Russians to agree to something, and it's a start, and this was the very first start. One of the reasons that I claim that it was important was it helped lay the groundwork for the Nonproliferation Treaty [NPT] which was signed five years later and which people here participated in technically. There's a fellow named Bob [G. Robert] Keepin who came back from a two-year stint at the IAEA and he was one of my compatriots in both the [United World] Federalists and in the FAS. He's suffering from Parkinson's [disease] now. He lives out in Pajarito Acres. But he could give you a lot of history about the early IAEA and how the safeguards program got started but he came back from the IAEA and said they're going to sign this Nonproliferation Treaty and the lab can play a big role in research; in developing technology for the International Atomic Energy Agency to safeguard reactors; and keep nuclear materials out of the hands of people that are not supposed to have it; and to monitor nuclear materials, plutonium and uranium; and if you're going to have all these reactors all over the world making nuclear power, which they need, why, you need to safeguard them. And the lab has been a leader in IAEA safeguards ever since. And I give Bob Keepin a good deal of credit

for the fact that we have that leadership, and Bradbury, of course, supported him 100 percent and—.

Let me ask you, the community as a whole, and this is an outsider's question, this is a question that shows you I'm not a member of this community, but I think a lot of people who think of Los Alamos or Livermore, the weapons lab, the testing program, as weaponeers, basically, the activity that's going towards arms control—Bradbury is a leader at that time—but politically, in the community here, is there a debate about arms controls issues, how is the FAS viewed? Because FAS, I think, sometimes has been seen—

Oh, the national FAS has, in my view, turned sort of to the left in the seventies.

That's my question. Yes.

And those of us who were active in the sixties in the FAS and in getting the Limited Test Ban Treaty, maybe we got active in other things, too. Some of us got active in local government because we, you know, we got our charter in 19—well, we had disposal. The AEC [Atomic Energy Commission] turned the community over to the local citizens and the local businesses in the sixties.

In the sixties. OK.

In the sixties. And that whole process started in, oh, about '62, and everybody got their own houses in '61, '62, '65. I mentioned building Pajarito Acres. We bought land at $25.00 an acre from the AEC, if we would develop it. Undeveloped land.

So you're saying until that time, no one owned homes here?

That's right.

I didn't know that.

Right. No, the government owned everything in the fifties.

You're right. Yes.

Yeah, they owned everything and we rented. But all I'm saying is the community had a lot on its plate, the local citizenry, in the sixties, and so I think a lot of us—we got the Test Ban Treaty, we were getting the Nonproliferation Treaty, and the lab is technically making these things work. And then the underground testing worked, and so security wasn't an issue, either. And people like Ben Diven were doing good physics and certainly getting the diagnostics that the weapons designers needed from the underground tests. So there wasn't an issue, in a sense. But the issue became the ICBM problem and the Soviets' buildup. They kept building up. And you had [Henry] Kissinger opening things up with China, you had Kissinger and [Richard M.] Nixon signing the SALT I treaty [Strategic Arms Limitation Treaty] and trying to get a SALT II treaty, and you know, those treaties were to limit missiles. And that was the danger, but the government was doing—you know, there wasn't a need for—well, there's always been the need for citizen participation. But it didn't seem the urgent need to some people in this community because we're busy with local government and getting that going and that sort of thing, and the national issues seemed to be taking care of themselves pretty well, and the lab's certainly showing all that sort of leadership that it needed to show and so on in the technical arena. And we formed the Los Alamos Committee on Arms Control, a group of us, in 19—didn't get it actually formed till 1986. Well, SALT I was signed under Nixon and Kissinger, and SALT II was proposed by Nixon and Kissinger—and they tried to get the Russians, and then [James Earl (Jimmy)] Carter came to power and he tried to continue it and finally got a SALT II treaty signed **[00:40:00]** about '79. And that was kind of an interesting year in my career because I spent a year on assignment at the Department of Energy [DOE] in the Office of Fusion Energy in Germantown. So we moved back to Washington that year, and that was the year the SALT II treaty was finally

signed by the Russians, so I went to the hearings. I took vacation time and went down and went to the SALT II hearings. But SALT II was not a sufficient treaty. It was a limitation but it didn't have enough verification. And I thought it was going to be ratified during the summer, during the hearings, but then the Russians kept building up and violating the spirit if not the words of the treaty. They MIRVed. The MIRV [Multiple Independently Targeted Re-entry Vehicle] was developed. It had been developed earlier in the seventies, but they built these big SS-18s which have ten MIRVs on a single missile, and SALT II didn't limit those. And in addition, it just limited the number of SS-18s but not the number of MIRVs you had. And there was no on site verification in it. And by the time the treaty was due to be voted on, the Russians were violating the principle in other areas. They rather suddenly built up their intermediate range missiles in Europe, the SS-20, and it could threaten all the European capitals and so on, and the SALT treaty didn't even touch that. And so the nuclear threat was growing in the late seventies and early eighties. And the thing that saved us, probably, that saved the world from annihilating itself by miscalculation, was a man named Gorbachev. And he came along just in time. And that's my view. And, you know, Reagan, a lot of people thought Reagan was too hardnosed. Well, he was merely trying to respond to this Soviet buildup. Of course, I'm a Republican and I was active, and so I'm a little prejudiced in how I look at some of these guys, but nevertheless, Reagan responded to the Russian buildup with the idea of SDI [Strategic Defense Initiative], which is not in and of itself considered around here to be too viable.

Really.

Well, but limited ABM [anti-ballistic missile] defenses are highly desirable and viable. It's just that the idea of a nuclear shield—he got a little carried away. And he said twenty years, you know, he said it would take—I mean he wasn't really carried away. But Reagan built up our

defenses to counter the Russian buildup of the late seventies and early eighties. Gorbachev came along, and he and Reagan hit it off. They both agreed that all these nukes were terrible and we had to put some limits on them and control them and stop this arms race. And that chemistry between the two of them became an important thing. And because the FAS, we felt, was a little bit to the left of where some of us wanted to be sometimes, though in principle we agree with them, of course, generally, we formed our own Los Alamos Committee on Arms Control in 1986. I was the first president of it, and there were a dozen of us that were involved in forming it, and we've held meetings and we had the experts from the lab in verification and various issues, and we've had speakers. We're mainly a public organization that makes information available to the public. And we lobby for the necessary treaties. And the INF **[00:45:00]** [Intermediate Range Nuclear Forces] Treaty was the key breakthrough in the Cold War. I think more important, even, than the Berlin Wall coming down. The INF treaty was signed in late '87 and it was ratified in '88 and it was the first time the Soviets had ever agreed to significant on site inspections of any kind. And it was part of Gorbachev's openness. He started something he couldn't stop, and Russia became very open and split up, and whoever would've thought that they'd split into all those nations? In fact, the START I treaty [Strategic Arms Reduction Treaty] became the more—it was even—the INF was important because it was the first, and the missiles it limited, the Soviet SS-20s. And we gave up some intermediate-range weapons in response to balance it, but the SS-20, which is a 3 RV [reentry vehicle] per, it was the dangerous one, the short flight time to European capitals from Russia. It was important to get rid of it because that was a hair-trigger. If you only have twenty minutes from the time it's launched till it hits a city, you've got to decide whether you're going to counter-launch in a very short time, and if you make a

mistake, you know, why–. So that's to one who believes in deterrence, and I believe you have to have deterrence of some kind.

And so the INF treaty was signed and entered into force and various people in Los Alamos were involved in helping implement the verification, if you will. And then the START I treaty was signed, and that was really the important thing. That put a lid on the ICBMs. And yes, it took a long time to get it entered into force, but I claim it introduced an element of the rule of law into the relations. Jim Baker, who was Secretary of State, when the Soviets broke up, asked who was going to represent the Soviets in the START I treaty, and he came up with the Lisbon Protocol that put the four former Soviet nuclear states into a protocol that put them all into the START I treaty. And the treaty was there in effect and being adhered to, if you will, even before it was ratified. And it couldn't be ratified until all four parties—the former Soviet parties—had agreed to the Nonproliferation Treaty and that they'd take all their nukes back into Russia and you'd just have one country carrying out the START I treaty. And we now have the Moscow Treaty which was signed by Bush II [George W. Bush] and [Vladimir] Putin which takes us down to even lower numbers but it doesn't have any verification in it, except it has the START I treaty. And the START I verification provides a mechanism of data exchange and inspections and so on that makes the Moscow treaty very viable. It needs to be enhanced with additional agreements; I outline a bunch of those in the book.

Well, we only have a couple of minutes left, but what about the deal over the test ban treaty, the Comprehensive Test Ban Treaty [CTBT]? What's your view on that, because there's controversy here about whether it should be ratified.

Well, our organization took the view, the Los Alamos Committee on Arms Control and International Security (LACACIS), we debated on that, and we support that it be ratified with

some conditions, that the Senate must put in some conditions. And these include thorough verification, full implementation of the verification mechanisms for that, which appear that they can detect anything down to a few kilotons. **[00:50:00]** Something lower than 2 or 3 kilotons fired underground in a cavity can probably be fired clandestinely. But they're not going to get too much out of that. They'll get something. And if that were to go on year after year, then somebody—and we were abiding by it and they weren't abiding by it, let's say the Russians, or some other nation because we got some other bad guys in the world now—why you'd have to do something about it. If we agreed to the CTBT, if I asked the Senate to support the CTBT, it's under the condition that they require that the rest of the world agree to a very detailed verification on site inspection of the existence of weapons themselves. The testing is not what's the most important. It's the existence of the weapons and the weapons material and so on. And that's what you've got to work towards and make progress and have some milestones towards. And so in supporting the CTBT, it's only as an interim step to a more comprehensive system, and if we don't get that more comprehensive system, then my personal view is that we should be able to resume tests if we need to. Maybe we don't need to, but I want to have that option.

Have that option. Yes. That makes a lot of sense. We should wind down, but that actually answered a question for me because in today's world, it seems, testing is an issue, but as you said, the actual existence of those weapons is the main—

And the weapons material, the plutonium, because a bad guy, a terrorist, can get a hold of some U235 and use one of the old Hiroshima or whatever designs and ….

OK. Thanks.

[00:52:28] End Track 2, Disc 2.

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

So that's what you've really got to do, is get the international community to agree to that kind of treaty, and the U.S. government has got to take the initiative in pushing for it. I mean if it were just an issue of testing, I myself, I didn't want a CTBT; I wanted a test ban with a threshold, with about a 10-kiloton threshold, in which we and the Soviets or Russians could test a few limited number of verified, low-yield shots, ten or twenty, every few years, if they needed to.

If they needed to.

And it kept your technology up to date so that you could do the other things that you need to do. But we can make it work without any testing, I think, at the lab. I think we—the Stockpile Stewardship program, I give Senator [Pete] Domenici a tremendous credit for initiating that, and I think it should be implemented, you know, and try to make it work.

OK, great.

[00:01:18] End Track 3, Disc 2.

[End of interview]