

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

**Interview with**  
**John Hopkins**

**April 11, 2005**  
**Los Alamos, New Mexico**

Interview Conducted By  
Mary Palevsky

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Produced by:

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## Interview with John Hopkins

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## Interview with John Hopkins

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

[The recording begins after the opening of a discussion about the process by which the Nevada Test Site was chosen.]

**John Hopkins:** It's interesting. You'll talk to lots of people who will tell you that so-and-so was the one who selected the Nevada Test Site, and it's much more complicated than that. It was not [John] Clark, it was not Bill [William] Ogle, it was not Al [Alvin] Graves, it was not Harry Truman. It was lots and lots of different people who looked at this over quite a few years, about five years, four years, and many different organizations were involved, many different groups looked at the various sites. At least when you look back at it in retrospect, the selection was obvious. What they looked at was how many people would have to be displaced; what are the problems associated with controlling the downwind population using the prevailing winds; how difficult would it be to get the land; what are the security problems associated with it; what is the weather like in terms of taking photographs of it—is it clear, were there atmospheric contaminants involved. And when you put all of the various areas that were looked at—and the areas that were examined fairly seriously were the White Sands Range, where the Trinity shot was fired; a section of the Carolina coast; an area in northern Nevada; two parcels in the Las Vegas Bombing and Gunnery Range, a northern parcel and a southern parcel, and they ultimately selected the southern parcel—and when you put all of these things together, particularly the safety, security, and the displacement of people, there was no question that the southern parcel won out. Also, the close proximity to a pool of workers, the proximity to Los Alamos [National

Laboratory], the real estate available, the problems associated with eye burn from atmospheric tests, and so on, turns out that there weren't any other serious areas in the running.

As a footnote to all of this, there is another spot that they did not look at at that time, but we looked at in subsequent years, that really would be, at least from an atmospheric standpoint, as good as the Nevada Test Site, and that's the Red Desert in Wyoming, which is sort of south-central Wyoming. There's an interesting area there where there's no drainage, where the Continental Divide splits and forms a basin—the Continental Divide goes on both sides of this basin—and it's called the Red Desert, and it's in south-central Wyoming. Look at it on a map sometime. That has a very low population. By the way, since I wouldn't want to worry anybody who lives in that area, it's not being considered as a possibility, but it would've been quite good.

It turns out that for underground testing, the Nevada site selection was just fortuitously perfect, about as good as you could possibly do if you tried to design the geology for underground testing. Very low water table—which makes it not very good for homes that want to drill a well because the water's about a thousand feet under, nine hundred feet down—and the [00:05:00] soil material is not conducive to cracking and splitting, so it's really very, very good for underground testing.

**Mary Palevsky:** *And then I guess if it had been elsewhere, that they would've had to deal with the fact—it's always struck me that Los Alamos really is in easy distance of Nevada.*

Yes, that was a consideration. It's important, and so that was considered as well. [Norris] Bradbury referred to this as a backyard testing area. Others didn't like that term. But it turns out, it's even closer to [Lawrence] Livermore [National Laboratory], so that it was very easy for them. But of course, when this first came out, Livermore didn't exist.

*So I had mentioned before we turned the tape on, the meeting that I read about in your draft of February 2004. [25 Feb 2004, Draft of Nuclear Testing in Nevada, John Hopkins and Barbara Germain Killian.] I understand you had rewritten a lot of this stuff, but that was extremely helpful to me. Because when I saw Dotty Grier, she mentioned being there in the early days and mentioned being in a meeting that I think—I have to go check her recording—she characterized as the meeting where the decision was made to use the test site. You're saying already that it's more complicated than that.*

Much more complicated than that, and also the decision was not Los Alamos's decision. Los Alamos felt strongly that the Tonopah south site was the appropriate one, but there were many different reviews; it turns out whenever the federal government didn't really want to make a decision, they asked for a new review. And this happened year after year, month after month, and the players were Los Alamos, the AEC [Atomic Energy Commission], the Department of Defense [DoD], the National Security Council [NSC], the Armed Forces Special Weapons Project, AFSWP. There were lots of players involved.

*Now when I read your history and you read other histories, the narrative goes we were testing in the Pacific, geopolitical developments, the development of the Soviet bomb, the Korean War, and all these things made Nevada, or made a continental test site, more—something that people were thinking about, whereas in the beginning, they really were not. Any special insight you have about that, other than what one reads about the need to move it onsite [to continental U.S.]?*

There was a great deal of concern and discussion about whether to test on the continental United States. Before the Sandstone operation in 1948, there was a meeting with very senior-level leaders in Washington, D.C. about whether to test in the Pacific or test in the continental U.S., and it was approximately evenly divided, but the decision was made to go to the Pacific and they

went to Enewetak at that time. I suspect we would not have gone to Nevada when we did if it weren't for both the Soviet bomb and the Korean War.

*Now in your draft, unless there's something I just didn't understand, and much of the science as you can tell me—I don't know when I'm treading into classified areas here. I say in the informed consent that that you always just tell me when you can't tell me something, and that makes it really easy. Do not hesitate.*

OK, no problem. There's not very much of this that's classified.

*I just wondered. But that [Operation] Ranger, the first Nevada tests, were related—what they did at Ranger—when did you get to the lab anyway?*

I got to the lab in 1955.

**[00:10:00]** *All right, we're going backwards. We'll get back to that story. But the science on Ranger had something to do with [Operation] Greenhouse?*

Yes.

*What can you tell me about that?*

Well, Greenhouse was scheduled for the spring of 1951, and there was concern about the device designs. And anything that they did at Greenhouse was big and expensive. And when it was clear, during the summer of 1950, that there were serious worries about the behavior of the devices that they were intending to test in Greenhouse, they wanted to test one or a few devices prior to the spring of '51. And so there was a strong push to find someplace locally—locally meaning someplace short of Enewetak and Bikini—where they could fire a device or two, even just an air drop, perhaps, out over the Pacific.

*So that looks like it was also an element that converges, then, on continental—?*

Yes. It was very important to Los Alamos, and also at that point there was some concern whether the Department of Defense could actually support the tests in Greenhouse. They threatened not to support the tests on Greenhouse because of their commitments in Korea. And this caused an enormous amount of back room deals and lobbying during late August and September of 1950.

*That's interesting, I had never—you write about it—but I'd never thought, with the world we are in now, it's very immediate. How far can you extend yourself? So it makes more sense of that past.*

Well, it took ten to fifteen thousand people, dozens of ships, hundreds of airplanes. It was a big commitment.

*So Ranger—let's go to that. I want to talk to you a little bit about this thing that we spoke of before I turned the recorder on, which is—you say there were heavy hitters at this meeting.*

*There really were, this, I guess it's late August, September meeting?*

August 1<sup>st</sup>.

*September. Yes, September 1<sup>st</sup>. Or his memo is September 1<sup>st</sup>, OK, but so the meeting is August 1<sup>st</sup>, with Fred [Frederick] Reines and Norris Bradbury, John Clark, Enrico Fermi, Alvin Graves, William Ogle, and Edward Teller. And you're saying here "who had called attention during Sandstone to weather prediction problems at the Pacific." So what was the gist of this meeting, and I'll try to get that memo that Dotty talks about, or the minutes that she talks about. [See LAMS 1173, Sept. 1, 1950, Discussion of Radiological Hazards Associated with A Continental Test Site for Atomic Bombs, Frederick Reines.]*

The question was, how big a device could they fire in Nevada safely? And could they do shots without jeopardizing the general public?

*And so the meeting would be—I'm just trying to understand as a non-scientist—all these minds would get around and, what? Look at existing data?*

Well, the only data that they had, really, that were applicable came out of Trinity. That was a fairly low shot. There were concerns about the device fallout. The fallout would be lower than Trinity if the device were higher. Trinity was a hundred feet. And that picked up an awful lot of the desert floor, mixed it with the fission products, and then distributed it downwind for a hundred miles or so, and that was serious. So could they test devices high enough so that the fireball would not reach the ground? What happened if there was something wrong with the [00:15:00] fusing and it went all the way down to the ground and then detonated? There were concerns about that sort of thing, as well. How accurately could they actually drop a bomb? In other words, another look to see what the problems were with testing in a continental environment. Things they didn't have to worry about in the Pacific.

*Now, in the Pacific, they're on land, though, when they do those things, so—*

Yes, but there are two things. One is there's not an awful lot of land, for one thing, and the—by the way, they weren't all on land, but they were in Greenhouse—they'd just wait until the prevailing wind is not over some occupied islands for the next five or six hundred miles and then they'd fire.

*OK, so I'll get that.*

Most of the ones in the Pacific were not fired on islands. They were fired either on barges or—well, there were, in the early days, tower shots. Sandstone and Greenhouse.

*Right. I just want to go through these notes. What did I have on—? Some of these were just questions about wanting to look at the documents and that. I don't know if we need to talk about them. Oh, yeah, this document, "the desirability of an area in the Las Vegas Bombing Range*

*should be used as a continental proving ground," we can look at these afterwards. I'm wondering if I can get some version of some of these reports. I just needed to ask you if they're still classified or if you—a lot of these—*

Most of those are not classified.

*So most of these I can get from Martha [DeMarre of the Nuclear Testing Archive] in Las Vegas.*

Sure. There's very little of that that's classified nowadays.

*And there was one other thing. And then we talked a little bit about Ranger. And tell me what you have—for me, there's this interesting line sometimes between public relations, the notion of public relations in telling people that there's going to be continental testing, and actual public safety issues, and it's something that people look at and say, Well, was this just PR? How was the safety connected to that? But what kinds of things have you [found]?*

Well, I think it was a very interesting period. There was a great deal of worry on the part of the federal government about whether people would be very concerned about the safety issues that were raised. After the strikes on Japan, there were wild misunderstandings about how dangerous nuclear weapons really were out at some number of miles. And some of this, I think, was rather encouraged, probably, by people in the media and government officials, the feeling that if you set off a nuclear weapon, it would kill everything within tens or hundreds of miles, which was actually incorrect.

So there was a great deal of concern in the late forties about how the general population would view this. What they didn't realize, I think, was that there was a great deal of excitement on the part of the general population to think that perhaps they were part of this national defense activity, something on the cutting edge of technology, something interesting and exciting. And Las Vegas was delighted to be chosen as the base camp area for testing to the northwest in the

Nellis Bombing and Gunnery Range. And Alamogordo [New Mexico] was very disappointed that the Trinity site was not selected. There were all sorts of strange news stories [00:20:00] that came out about the hype of being the atomic city, the location for the U.S.'s atomic testing grounds. All sorts of things were advertised as being atomic, from soap to hot springs. Well, you know, prior to World War II, there were radium hot springs that people wanted to go to for therapeutic reasons, and there was a uranium soap, as a matter of fact, that somebody had made. Now, whether it contained any uranium or not, I don't know, but the name was used. So there was a lot of interest and excitement, and instead of being negative publicity, I would say it was quite positive.

Because of fallout that did exist, though, during the atmospheric test days, I would say a lot of that goodwill evaporated by the early sixties. I don't think it ever got to the point in Las Vegas where there was really hostility on the part of the general population, but there wasn't as much of a feeling of patriotic pride in this. Although the fallout gets an enormous amount of press today and people who are the Downwinders worry about this, it was extremely rare for anyone to get more radiation than the general population guidelines at that time. I think probably the maximum anyone got, and this probably only refers to a dozen or less people, got over 4 or 5 R in the cloud passage.

*It's an interesting problem, doing my research, to see the very strongly-held views about the actual effects of fallout, even from various scientific studies and stuff, and it's interesting to see how someone can absolutely assert that the science says one thing, and then someone can absolutely assert the science says something much more dangerous or much less dangerous. In the course of your research, have you come across that? It's just interesting to me.*

Well, no. I have been exposed to that sort of thing over the years, but we're not putting very much effort into the public controversy about fallout. I know a reasonable amount about what the issues are. The biggest problem and question is, what is the biological effect of very, very, very small doses of radiation? And there's relatively little known about that. There's a tremendous amount of information from mice where they studied millions of mice, for example. And the genetic effects, which worried people a lot in that period, are almost nonexistent. Maybe they are nonexistent. And that's consistent with the examination of the people from Hiroshima and Nagasaki, as well, and that is that people who suffered from the radiation were the people who were actually there, not the offspring of the people who were there.

The doses were very, very small, and it's very difficult to find something that one can really attribute to radiation. Some people, like Dina Titus in her book, *Bombs in the Backyard* [*Atomic Testing and American Politics, 2001*], give the impression that the AEC didn't worry very much about public safety, and that's just wrong. The criteria were different then, less stringent than they are today, but in terms of paying [00:25:00] attention to this and trying to reduce the fallout risk and danger, they cared a lot about it and spent an awful lot of time and effort and money to make sure that the public was exposed to the absolute minimum amount. *Yes, it's an interesting artifact because it's a cultural study as well as a historical study and social study, and of course part of those impacts in Nevada are downwind populations and talking to them.*

One of the questions I would ask her, if I had the opportunity is, how did she get this impression, where did it come from, that the AEC didn't care about this? The AEC clearly spent a lot of time and effort worrying about it. And so I don't know where that attitude comes from.

*Well, maybe we'll, when things move along a little bit, we'll put—*

You're in the position to ask her. She must spend some fair amount of time in Las Vegas. I don't know if she lives there or not.

*Yes, Dina lives in Las Vegas. She's a political scientist. I see her occasionally. No, I was thinking, well, that'd be interesting, a couple of years down the line when things are more developed, we'll have a panel and then you can—I'll invite you and I'll invite Dina and you can ask her. But OK, great. Let me see what else I had here. Oh, this is just an aside, John. You had mentioned, I think when we saw each other before or maybe in here, Betty Perkins and W-76 history. I wonder if you saw that big piece in the New York Times about the W-76 safety issues. [William J. Broad, Aging Warheads Ignite Debate among Scientists, *The New York Times*, April 3, 2005]*

Yes. It wasn't a safety issue. It was a performance issue.

*Oh. But didn't he allude something to safety?*

No.

*A performance issue.*

No, it's just a performance issue. He said it wouldn't work very well. You know, I think he's wrong.

*Oh, that's interesting. That's another subject matter. But I thought about that. I saw that piece the day I hit New Mexico. It was in last Sunday's paper.*

No, it's not a safety issue.

*Then I'll have to read it again more carefully. I guess I saw something about rust or something and began to wonder. So that stockpile looks good, in your opinion.*

I think it looks pretty good.

*Because the other impression I got from that piece was that there was something about retooling it in some way that would require future testing, and I guess that's what one of the controversies was.*

Well, the so-called margin is not great on that. That is, it was made during a day and age when they wanted the minimum weight, minimum size, maximum yield. And so everything is really shaved to the maximum degree, which makes it less forgiving of problems. It wasn't intended to last for fifty years without testing or rebuilding. And it's difficult to have total confidence that something that you haven't tried for a long time would actually work.

*Sure. That's just common sense, actually.*

It's sort of like not starting a car but making sure that it would start when you turned the key.

You can be sure of the first year or two or three or four or five, but how about thirty or forty or fifty years downstream?

*Well, but the point you raised, and I hadn't thought of asking you about this, but that's an interesting question, when all this was being done, we were working for deterrent, right? But I guess you couldn't really think down the line what you were going to do with really dangerous weapons fifty years out.*

Well, again, they're not dangerous in the sense that it's a safety issue. It's, will the darn thing work?

*Right, but I'm saying dangerous if they were to be used, so what happens if they're sitting there fifty years and you have a choice to use or not? I don't know. I haven't really talked a lot with anybody about the logic of the stockpile, you know what I'm saying?*

Well, yes. These are areas that I'm not sure that have a hard and fast answer to them. Someone mentioned this to Harold Agnew one time about losing confidence in the stockpile if we stopped

testing and Agnew said, Oh, that's easy. You just hire people who are confident. OK? You know, if we think they're going to work and the adversary thinks they're going to work, [00:30:00] they will serve their purpose, probably. The danger is if we're not at all sure that they'll work. Or if we're only—what if you're only 80 percent sure? And the problem is not if you have a confidence of 80 percent, say, it's not that eighty out of a hundred would work; it's a 20 percent chance that *none* of them will work.

*Ahhh. Oh, of course, yes, because if something fails, it's likely to fail on them all.*

Yes.

*Yes, that's a little off our subject matter but that's—*

It is, yes.

*But that's an interesting question about, as I said, how people thinking about the stockpile as these weapons are being invented and built.*

*But speaking of design, another interesting thing that you said here [in Nuclear Testing in Nevada draft] and I actually hadn't understood this but it makes perfect sense, is that during Manhattan days, that [Hans] Bethe's theoretical group was thinking about new weapons designs other than Fat Man, and that those were things that were actually tested in the early days, is that right?*

They were tested in Sandstone.

*In Sandstone. Tell me a little bit more about that.*

Well, now one is beginning to get close to classified information, but I can say a little that's not. There were ideas about how to improve the efficiency of things like Fat Man, how to make them work better, and there was some experimental work that went on here, testing some of these ideas. And so they wanted to make sure, for Fat Man and for the devices that they were going to

use in World War II, that they had a very high degree of reliability and would actually work and would be available very soon. And so they went a fairly conservative route that way. By war's end, though, they had some ideas, a number of ideas. For one thing, Little Boy in particular was very inefficient and a terrible waste of a lot of uranium. So there was a desire to get away from that as soon as possible and to explore other, newer, modern implosion systems. And one of the reasons that [Norris E.] Bradbury really was opposed to participating in [Operation] Crossroads – Los Alamos only participated kicking and screaming—

*Really?*

Yes. Bradbury felt that was a silly waste of time and effort and if you dropped a nuclear weapon on a Navy ship, you knew what was going to happen; it was going to sink. He thought this was silly. And he considered two separate aspects. Los Alamos's role was not really well defined in late '45. It wasn't at all clear what we were supposed to do: just make more Fat Man and Little Boys for the Air Force, or to do design and development work on new weapons? Bradbury was concerned about what the University of California's role might be. If it was a physics role, then he thought they would be interested in supporting it. If it looked too top-heavy in the engineering aspects, they were not. When Z-Division was split off during the summer of '45 and started its migration to Albuquerque [New Mexico], the University of California was not interested in participating in that kind of work, in the weaponization. They were interested in the physics of nuclear weapons. So when the military became more interested in weapons effects, Bradbury said them, *You go do that. We have no interest in that.*

*So you were saying there were two issues.*

**[00:35:00]** There were two issues with respect to Los Alamos's role. One is what should Los Alamos do, and what might the University of California do, and what should the Department of

Defense do? Bradbury, in the summer of 1946, was not at all sure that Los Alamos ought to be engaged in nuclear weapons testing at all. Then by November of 1946, he had changed his mind, and the way he phrased it is that testing weapons behavior, like the Alamogordo test, is the appropriate role of Los Alamos, but testing of weapons effects, the Crossroads kind of testing, was not appropriate for Los Alamos. The Department of Defense tried to get the University of California, Los Alamos, and the AEC more deeply involved in weapons effects, and Bradbury always dug in his heels. And so that was the reason that the Department of Defense formed AFSWP, the Armed Forces Special Weapons Project, whose first leader was General [Leslie R.] Groves, to look at weapons effects, what do weapons really do in a military environment.

*Now, let's back up a little bit, because I've tried to, from the things that you have here and other things, what can you tell me more about what Bradbury's reasoning behind that was, as far as the—that it wasn't pure physics or—?*

Well, I think that he was—well, let me say that in late '45 and in '46, it wasn't clear what Los Alamos might do. If I can grossly exaggerate, if you ask the military if they want an improvement in any particular thing, they say, no, we're perfectly happy with what we've got. And Bradbury was concerned that the military might view the role of Los Alamos as just making more Fat Men and Little Boys. And he wasn't interested in doing that. He wanted to do R&D [research and development] on new weapons physics concepts.

*OK. And this is because he's a scientist at heart? I mean what—?*

I think it's because—I think probably it's a number of reasons. One is, I think, is because he was a scientist at heart. Number two is he liked the arrangement with the University of California.

Although that's a separate issue, and let me say that until, oh, late winter of 1946, the University

of California was not really interested in pursuing, continuing, their management role of Los Alamos.

*Once the war was over, their piece was done?*

Their commitment expired six months after the end of the war. And they were asked by the Corps of Engineers that ran the Manhattan Project to continue, and their response was not just No, it was Hell, no. We have absolutely no interest in this. It would be an interesting research project to pursue that a little bit further, and Alan Carr [LANL archivist] has done some of this, and actually he wrote an article about how Los Alamos got to be the way it was and what some of the correspondence was back and forth. But by the summer of 1946, the University of California had changed their position 180 degrees. I speculate that the reason for that was that a great champion of nuclear weapons and a nuclear weapons role for the University of California was E. O. Lawrence. And E.O. Lawrence had a lot of horsepower in Berkeley, and I think he convinced President [Robert Gordon] Sproul that U of C ought to stay in this. But Robert Underwood, who was dead set against this, was adamant at least up to the beginning, spring of '46.

*And Underwood was...?*

Underwood was the Secretary of the Board of Regents. And I think for a long time, he was kind [00:40:00] of skeptical of the role of the University of California in this area. And I think Bradbury, who came out of an academic environment, really liked that and thought, Well, there's a lot of good science to be done here, but we don't want to turn this place into a factory. And this was an attitude that carried through with Bradbury until he retired in 1970. He didn't change at all in this regard. But interestingly enough, he also viewed the role of Los Alamos as being

nuclear weapons and not everything for everybody, and he was really reluctant to launch into any project that didn't have nuclear weapons somewhere associated with it.

*Yes. And then we won't go that far up the time line, but things change, then, when Harold Agnew comes in.*

Yes. Yes.

*We'll get there, but not yet. Oh, that's very interesting stuff. So it was to stay on the cutting edge of this new technology?*

Right. Also, by the way, another point having to do with Crossroads and that is that the Los Alamos population at that time was very small. People were going back to universities. If they had come from the faculty of universities, they wanted to go home to that. If they were graduate students, they wanted to go back to a university to get their advanced degree. So Bradbury was not at all sure in the summer of 1946 or spring of 1946 that we had enough people to even assemble a device for the military out at Bikini.

*Do you think that that attitude, his—no, I don't want to say an "attitude," that's not quite right, but his approach had something to do with the fact that if it went more to the engineering and the applications, he would have even less chance of keeping people? In other words, keeping it more on the cutting edge would help attract people?*

I think so. If you back up a little bit and go to, say, the spring of 1945, before the end of the war, Los Alamos was bursting at the seams in terms of population, and we had more than enough work to do. And Bradbury wanted to—not Bradbury, Oppenheimer really wanted to move the engineering aspects, the ordnance aspects, off the hill. I think that in 1946, Bradbury probably favored a small high-tech academic institution because he felt that really would be better, would be easier to develop and maintain. Now, interestingly enough, Livermore would have been

perfectly happy to get much more deeply involved in weapons effects, but the policy was essentially already set by the time Livermore came along and they sort of had to accept the policy that Bradbury had established.

*That's very interesting. So this regime of separating out the effects goes to AFSWP and the military?*

Yes. It was well developed by the time Livermore was formed in the summer of 1952.

*And why do you say you think that they would have been much more interested in that model?*

I would say that Bradbury was fairly far out on an extreme in this regard, even with regard to Los Alamos staff. The Los Alamos staff would've been much more interested in getting more deeply involved in weapons effects if it weren't for Bradbury, and there may be other people who had some influence, too, but I think it was strongly Bradbury. When it came to other things like new, more adventurous ideas—testing underground is one, Plowshare experiments is another, [00:45:00] nuclear ramjet—Livermore was much more adventurous than Los Alamos was in this area, much more inclined to use their intellectual horsepower to pursue some of these other areas. Bill Ogle mentions this in the book, by the way, with regard to the Plowshare program, for example.

*One thing that struck me when I was chatting with Ed Giller, because I'd read this narrative in much less detail than you've just given me, so thank you very much, that helps me understand it—but when he's coming on early—that the military itself viewed this as such a mysterious new weapon that they had to figure out for themselves. So you have a Bradbury saying—I'm just thinking out loud here, but you have a Bradbury saying, yes, you drop it, it's going to sink the ship, but that the military itself—I had a greater appreciation of how the armed forces were*

*trying to grapple with what this whole new thing actually meant as far as how they conducted war or had defense policy.*

Yes. Well, Giller's a very good source on that, and he was in AFSWP in the very early years. He also was a participant, of course, in World War II. He was a fighter pilot, as you probably know. So he's really seen—his whole career was spent in nuclear weapons area and he does have the perspective of the military and he's a very bright guy and a nice guy.

[Regarding] the military questions, there are two things that I think are interesting. One is Los Alamos did make some estimates of the damage to be done at Hiroshima and Nagasaki, and they did a pretty good job of it. But it turns out that the weapons effects are vastly more complicated than anyone thought. The pressure pulse is different. It depends on the terrain. It's very complicated.

And another issue with regard to Nevada testing, and that is the kinds of measurements that the military really wanted to make to find out what the military effectiveness was of nuclear weapons could not really be done in the Pacific. They built a few structures at Sandstone and Greenhouse, but it was fairly primitive compared to what they did in the later days. And the weapons effects really was a much more complicated and difficult issue to get your arms around than the design of nuclear weapons. Los Alamos' task was fairly simple, I think, compared to the military's. And the military also wanted to use the nuclear weapons to give experience to combat units that might be faced with fighting in a nuclear environment, so they wanted to use it for maneuvers. But typically out in Nevada, the military might have ten thousand people, while the laboratories had a few hundred. The weapons effects people out there were probably about the same, generally, as, say, Los Alamos, or Los Alamos and Livermore, that is, the scientists who

were measuring things. But if you add in the observers and the maneuver troops, you get vastly more.

Those were complicated issues. And the pressure pulse from nuclear weapons doesn't look like the way you would calculate theoretically, and it depended on many more things that they sort of brushed under the rug in the early days, like the surface terrain and atmospheric effects and things.

*So then that means the military had to be developing the science side as well as the—I forget what that little phrase Bob Campbell gave me about basically how far will a tank roll. But they're actually having to look at very complicated things.*

**[00:50:00]** Oh, yes. And they had and still have a fair amount of intellectual horsepower themselves, but they've also recruited lots of contractors. A number of companies were formed solely to look into weapons effects areas.

*I think the book I told—the other book I have here that I'll show you, that I mentioned to you, I may have mentioned to you, is by this retired Secretary of the Air Force, and he's, I think, talking about some of the development of some of those contractors.*

Tom Reed?

*Tom Reed's book, yes. [Thomas C. Reed, At the Abyss, 2004, Ballantine.]*

Yes. I know that book. Yes, they had a lot of people, Stanford Research Institute, S<sup>3</sup>, SAIC [Science Applications International Corporation], just a lot of them.

*Right. Yes, so that was interesting for me to hear, and it's interesting the way you're augmenting my understanding of it, of weapons effects. And I think even in some of that early testimony, or maybe it would've been a conversation I had with Phil Morrison or a combination thereof, of*

*talking about it at Los Alamos, predicting what those effects would be at Hiroshima, and then dealing with what they actually were, and they were different than what they thought was—*

They were different. Actually, I think they did relatively well for people that didn't have any more experience of the actual effects of nuclear weapons. But it turned out to be a lot more complicated.

*And I think the point that Morrison makes, actually, which had been lost on me was not a scientific one but the social impact of bombing a place and then having nothing in any part of the city that functions as far as emergencies go, whereas if you're using conventional bombs, you might bomb this side of the city and that maybe something else, doctors, hospitals, things like that, so the complete social devastation of a nuclear weapon as opposed to other kinds of things. Maybe. Maybe not.*

Although actually over Japan at that time, when we were doing the fire bombing, that was so widespread, so thorough, that the damage they did to a city was in many respects more extensive than the—

*So it was equivalent in a certain sense.*

Yes, I would say so. But that's partly because we did such an effective job with the fire bombing. But, you know, if you take—a ten-kiloton [Kt] bomb is not nearly as effective as ten one-kiloton bombs.

*OK. So tell me why.*

Because what you do is you stir up the rubble at ground zero even more with a ten-kiloton bomb, and if you could spread that out over a bigger area. The radius of effectiveness goes as the cube root of the yield; it's not directly proportional to the yield. If you could have that one kiloton spread out in ten-pound chunks of high explosive, it would be even more effective.

*Maybe that explains, when I was talking to Teller all those years ago and asking a question about the hydrogen bomb and bigger and bigger, I think he said something to me like, or he's written it, one or the other, that it's actually more effective if you use smaller weapons, [paraphrasing Teller] So I wasn't all that concerned about us going too big. I don't want to misrepresent what he said.*

That's correct. In the mid-fifties, when we were so enormously successful in Castle in developing the big thermonuclear weapons, for a short time the military thought, great, wonderful, these are marvelous things, until people started to really think about what they would do. If you wanted to destroy Seattle [Washington], you don't necessarily want to start a forest fire a hundred miles away. You want to destroy the enemy's ability to make war. The collateral damage is so great [00:55:00] from some of those big bombs. There was a military Air Force General who was here one time during this period and he asked in a meeting, How big a thermonuclear weapon can you make? And one of the designers said, We can make it so large that you don't need a delivery system.

*What does that mean? You just blow up the world or—?*

Yes. You set it off wherever it happens to be and it destroys the enemy.

*Oh, I see.*

Well, that's not a very useful weapon.

*Right. So you must see an evolution of thinking about these things as new discoveries are made?*

Oh, yes. They actually have done a revolution. In the fifties, early fifties, the idea was to make a nuclear capability of comparable or equivalent to every conventional capability. Every conventional cannon would have a nuclear option. Depth charges would have a nuclear option. Everything down to the bazooka.

*Hand grenades.*

Well, yes, a hand grenade, if you could throw it far enough. The idea was just take the whole spectrum of military weapons above an infantry rifle and a hand grenade and make a nuclear option to that. Also, then, that was, say, 1950 to 1955. Nineteen fifty-five and up, the idea was to have enormous big weapons. Then you didn't have to worry about how accurate your bombers were or your missiles because if you missed by a mile or two or three, you'd still destroy the target. By the end of the 1950s, it became clear that one could get more accurate delivery systems, more accurate missiles, more accurate bombers, and that the collateral damage was not a bonus but a detriment. Like poison gas. If you set a ten-megaton bomb off on the ground in Moscow, it could have a dangerous fallout on California. That's not something you want to do. And so the idea was instead of just trying to aim for a weapon with an enormous yield, use these new, advanced guidance systems weapons platforms and make more surgical weapons, if you can call a hundred-kiloton or a five-hundred-kiloton weapon a surgical instrument. This idea had certainly taken hold by 1960. And then, oh, let's say the early sixties, '61, '62, and then people started taking out of the stockpile the very high yield weapons and substituting lower yield weapons, sometimes more but usually not any more. The total number of weapons probably peaked in the early sixties. And then both the numbers of weapons and the yield went down dramatically.

*And that is probably what Teller is meaning when he says that to me. Maybe he's talking about that transition.*

Yes. I'm sure that's right.

*And because when you see the criticism on the part of even a lot of scientists about the H-bomb, it's the size that they're—that basically it's a weapon of such mass destruction that you—Bethe's paper of '50, whatever, when he's saying, you know, you destroyed a whole culture to save one.*

Yes. Well, you see, the Soviets got up to a fifty-megaton weapon and they quickly discovered that that really has no particular use, and they started going down, as well.

*That's interesting. Let me stop here.*

[00:59:43] End Track 2, Disc 1.

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

You asked how I got here. I started at the University of Washington in 1951 in physics, and all of faculty friends and advisors were involved in the Manhattan Project.

*OK. I'm going to take you back a little farther. So where were you born and when were you born?*

Oh, I was born in 1933 in Palo Alto, California. My father was a professor of economics at Stanford. And then in 1946, he moved to Seattle to form a new group at the University of Washington, and since I was living in Seattle, I went to the University of Washington.

*But you obviously had an interest in physics, in science, early on, or—?*

Yes, I did, and I wasn't absolutely sure I wanted to be a physicist, but I thought it was easier to transfer out of that than to transfer in if I changed my mind. I was also interested in mining engineering.

But at any rate, all of the faculty at that time were in the Manhattan Project, and most of them had been at Los Alamos. John Manley was the chairman of the department.

*I didn't know that.*

Seth Neddermeyer was in the department. His colleague, John Streib, who worked in his group here on implosion, was there. Isaac Halpern, who was a Navy ensign at the time of the Manhattan Project, was there. George Farwell. And actually I was doing work with George Farwell, who was in Emilio Segrè's group here.

Los Alamos sent Harold Argo to interview some people for summer positions, and George Farwell suggested that I talk to him and I did, and so I came here in the summer of 1955 as a summer student. And I worked with John Brolley and Keith Boyer in 1955, and I came back in 1956 and worked with Ben Diven.

*Oh, really?*

Yes, he was in a group that was chaired by Dick Taschek. And then when I got my Ph.D. in 1960, Ben had offered me a job and I came back. I loved Los Alamos. I thought it was interesting and exciting, and still do.

*Were you married at the time, then, John?*

Yes, I was married in 1954. And my wife is a chemist, and she was a summer student, as well.

*Oh, how interesting!*

She is a plutonium analytical chemist. So at any rate, she loved it here, as well, and we came back, then, in 1960. I was in the Physics Division, working with Ben Diven. I went, in the summer of 1961, to Europe to a conference. I was measuring cross-sections, like your dad [Harry Palevsky] did. And when I was in Europe in the summer of 1961, late summer, the Soviets started testing. And so I cut a vacation short, came home, and talked to Ben about it. I said, I really want to go out and work on bomb testing. And so he said, OK, why don't you go talk to the people in P-4, who were doing high altitude measurements on spectra from high altitude shots. And I talked to Jim Coon and Harold Argo, the same Harold Argo that interviewed me five years or six years earlier. They welcomed me into P-4 on a temporary basis, and so I went out to the Pacific and participated in Operation Dominic.

Then I came back and worked in the Physics Division for some several years. And then the laboratory had a crash program to look at ABM [antiballistic missile] warheads, and they

[00:05:00] needed some help, and so I volunteered and went out to Nevada in late sixties, I think probably '68, and worked on those experiments for a year or two. And simultaneously, I was doing work in P-Division in preparation for doing some work at LAMPF [Los Alamos Meson Physics Facility]. Oh, I was elected to a Fellowship in the American Physical Society during this period of time for physics work I had done. But in 1970, Dick Taschek became the associate director for research. And he asked me if I would go over and help him for six weeks, as a staff assistant, just helping to set up the office and doing whatever he was supposed to be doing. This was a new kind of position that Harold Agnew formed.

At any rate, the six weeks dragged on and on, and then a year later, Harold Agnew asked me if I would join the Weapons Division as an associate division leader, responsible for research work, and I said I would. But before that came to pass, the laboratory was reorganized and he asked me then if I would go to the Test Division and help them, and I did. And I was there for about a year-and-a-half, and was selected as the alternate division leader, and a few months later, the division leader left and I became the division leader of the Test Division.

*Now, this is J-Division, is that right?*

J-Division.

*And who was the guy that left?*

Brown. Charles Brown. Let's see, the first division leader for a year was Darol Froman. Then Al Graves from '48. Maybe it was '49, the fall of '49. Or '48.

*Barbara [Germain Killian] gave me her charts on that.*

Anyway, it was '48 or—I think it was '48. Forty-seven to forty-eight was Froman, and '48, October to the time that he had a heart attack, which is about '65—Al Graves had a heart attack and died—and Bill Ogle became the division leader and was the division leader until 1970.

Seventy-two, I think, actually. Yes, it was '72. Charles Brown then took over in '72 and was there till '74, and I was there from '74 to '82.

*So you didn't move up through J-Division; you actually—*

Well, I had worked with J-Division during some of these other test series. I went in as the assistant division leader and very—well, in about two years, became the division leader. And I was division leader, then, till about '82. There was sort of a change in the structure during this period, but essentially that's the way it was. And by the way, I *loved* J-Division. Testing bombs is *absolutely* the most fun I've ever had. When I was out in the Pacific on Dominic, I would wake up every morning thinking, somebody's paying me to do this. I loved it.

*What makes it so satisfying?*

Oh, it's interesting, challenging intellectually. There is a lack of unpleasant bureaucracy. We essentially had free rein to do what we needed to do, mindful, of course, that we spent the public's resources responsibly. It was a very high concentration of doing interesting, exciting [00:10:00] work without the constraints and confines of an overabundance of paperwork and rules and regulations. I just loved it. In Nevada, it was a little like owning a hundred-thousand-acre ranch and being able to just decide what you needed to do and doing it without written proposals and an enormous amount of oversight. Now, the world has changed a lot since then, but it was interesting and exciting. And there's a feeling also, I suppose, similar to missile launches at Cape Canaveral. You work hard, get experiments ready to go, and then they go and you're through with that. A feeling of we've gotten to a milestone, it's completed, it worked fine, we found out some very interesting and exciting information, we answered some important questions. It was a great feeling of accomplishment. Also, I think that there is a stronger feeling of camaraderie when a group is working together in distant, remote places. You're working

together as a family. You're dependent upon your colleagues and they're dependent upon you. I suppose the same way a crew on a ship feels in this regard. When something is accomplished, you all share in the accomplishments. You all have something to contribute. It's just a very enjoyable experience and interesting and exciting.

*I would imagine to a certain degree that an experimenter in, sort of at the edge of knowledge kinds of things might have a similar experience.*

Oh, absolutely. Absolutely. But if you as a team do it in a remote and distant place, you develop sort of an *esprit de corps* that you don't get if you just go home at night.

*I see what you're saying.*

So working at Amchitka [Alaska] or American Samoa or Bikini or Enewetak or Mercury, Nevada has a different feeling than doing an experiment here.

*And you were at all those places that you just mentioned?*

Oh, excuse me, I was never at American Samoa, but J-Division did have an activity there. I picked that out just because that was one of the spots where they did work.

*Yes, I wanted to verify that. And what was I going to ask you? So that means, does that mean that organizationally something had happened with, say, the funding of a certain experiment that by the time you're working on it, those kinds of issues are not relevant? You're saying not much paperwork, not much worry about those kinds of things. That's happened previously or up the line or whatever?*

Yes. Well, actually, that was part of my job, too, to insulate the other people in the Test Division from some of this sort of stuff. Probably the best job in the laboratory is a test director, and Bob Campbell was a test director and I was a test director for a year. You have a certain number of resources, which is more than adequate to actually do the job, the minimum job, and you make

the decisions, deciding how to best execute this, and you do it with the help of the group leaders in the Test Division. But you have the authority to spend tens of millions of dollars to get the job done. Relatively few constraints. No pressure to skimp on safety and [00:15:00] security. I was never pressured, for example, to fire a shot when I thought it would not be safe or appropriate. And the same is true of Livermore. And I told my counterpart at Livermore if ever he had a misgiving about a Los Alamos event, let me know and I'll stop it until we get it resolved.

*And who was that at that time?*

At that time, it was Rich Wagner. It really felt very good. A very good position. It's sort of analogous, I think in the United States Navy the best jobs are at the captain level, and there you're the skipper of a ship, and that's got to be the best job in the Navy. When you get promoted, you go to the Pentagon and you get to prepare budgets to submit to Congress and you do staff work. And that's not nearly as much fun as standing out on the bridge of a ship watching the water go by and realizing that you really are in charge of that ship or that operation.

*Now, does that connect, John, up to what you're actually doing, that it's defense work, that it's work that's contributing to the nation's security? Are you thinking in those terms or are you thinking mostly as an experimental physicist?*

No, all of this applies. I think a lot of that just depends upon the individuals involved. But I don't think that anybody can really do this for any length of time without thinking about the implications of what it is that they're doing and why they're doing it. And you certainly couldn't do this if you thought about that and didn't feel that it was the right thing to do. I'm sure that you have the same sort of feeling if you're working on, say, rocket launches for NASA [National Aeronautics and Space Administration] that don't have a national security implication. But in this particular case, I think that it's safe to say that the people who worked on nuclear weapons

or work on nuclear weapons today feel that this is something that is important to do and is making a positive contribution to society and to humanity.

This is an interesting point, by the way, if you look back at Norris Bradbury. I think Norris Bradbury really would have been happier if nuclear weapons had not been possible. But I think that he felt that if they are possible, as they were, we have an obligation to understand and to give technical advice to the diplomats and decision-makers in Washington on what is the best way to handle these. I think the same feeling existed in Carson Mark. And Carson Mark and Norris Bradbury, I believe, were very interested in the broader issues of the role of nuclear weapons in the free world. I'll give you an example.

The arms control issues, I think, really started to a very large extent seriously after Operation Castle when the Japanese fishing boat was exposed to deadly fallout. And the lingering concerns that existed about the role of fallout in the health of the world, I think, were really highlighted at that point. And [Jawaharlal] Nehru, in particular, in India called for cessation of nuclear weapons testing.

Over the next few years, there were diplomatic discussions of this and proposals were made by Livermore, particularly, to test underground, which eliminated the fallout from nuclear weapons, and so eliminated the concerns that highlighted the Bravo issues in Operation Castle in '54. Edward Teller pushed underground testing very hard, and even through the moratorium in 1958 to 1961, he argued that we ought to go test underground and then the people who were worried about fallout wouldn't have anything to worry about and so on.

**[00:20:00]** Norris took a different tack in his communication with Washington. He said, I don't think that the fallout is more than half the issue. The other half is associated with the arms race. And you don't address the arms control issues at all by testing underground. You only address the public health issues. And

therefore, I don't think it's appropriate to test underground as a way of beating the test ban treaty. Now I think that this is an attitude shared by Carson Mark, but probably not by most of the other senior people at Los Alamos.

*Now when did Carson Mark, when did his era end as that was—?*

It ended really with Harold Agnew coming in. There was some incompatibility between Carson Mark and Harold, and so Carson stepped down as division leader and retired soon after.

*Because I would imagine from what you just said that the attitude of Harold Agnew was different than—*

Oh, quite different, yes. He chafed under Bradbury, I think, and felt that Bradbury was too conservative. He felt that Bradbury was not cooperative and collaborative enough with the military. Let me give you another example. This occurred probably about '57 or so. The Army had finally sweet-talked Los Alamos into letting them fire an artillery shell in the Grable event. And they wanted to take an Honest John, which is a surface-to-surface missile, fairly simple one, with a warhead out to Nevada and fire it with a nuclear warhead on it and see that everything goes bang. I tend to be sympathetic, by the way, with those kinds of experiments because it's amazing how many problems you find when you can test part A and part B and part C and then you put it all together and it doesn't work as you anticipated. Anyway, the Army developed a proposal for this. They massaged it and it went through several iterations and then to the Military Liaison Committee [MLC]. And they looked at it and the Joint Chiefs of Staff made suggestions and corrections and massaged it, and it went over to the Atomic Energy Commission and they looked at it and sent it to the Division of Military Applications who negotiated back and forth with the MLC and the Army on this. Ultimately, they sent it to Los Alamos because we had developed the Honest John, and Bradbury immediately wrote back and said hell, no, this is

the dumbest thing I ever saw, telling the Army that if they really want to do this, to find their own test site and do it themselves. And that was the end of that.

But Bradbury really wanted to keep the military systems people at arm's length, and Harold wanted to have much more interaction with the military systems people, to work much more closely with them to find out what is it that the military really needs and how can we be most useful. But Bradbury's attitude, if I can oversimplify it a bit, was that look, we'll build the best weapons we can build and then they can use them.

And actually, the role changed probably by about 1960. Up to 1960, the lab did R&D work on new weapons concepts, and then they, with Sandia's [National Laboratories] collaboration and cooperation, they put it into a weapons system and that was a weapons system [00:25:00] for the military. After that period, the military had their own delivery systems and they said, Well, it's more effective, more sensible, for you just to tailor-make nuclear weapons for our existing systems. And so we were, after 1960, making weapons for specific weapons systems because it made more sense to tailor-make the weapons. But before that, we were really driving the R&D process.

*Right. And that makes sense in the evolution of a new, well, a really new technology, that it's the buildup of the knowledge and then—*

Yes. Exactly. But I would say that Bradbury's attitude didn't change, and this frustrated young Turks like Harold Agnew.

*Well, yes, it's interesting what you say, and other people have said this, too, and other people have discussed it. Herb [Herbert] York discussed it with me. He may have discussed it, mentioning Rich Wagner. I'm trying to think. I have it in my book, this whole notion that we would have been better off if this thing wasn't possible.*

I think Herb thinks that we would've been better off, and particularly if thermonuclear weapons weren't possible.

*Yeah. Well, surely Bethe said as much. He told me that [he hoped] they wouldn't be possible but they were. And I was just re-reading something that Bob [Robert] Wilson told me. I had forgotten this, but I had been to a talk, oh, it must've been '96, and Hans was giving a talk on the Manhattan Project and was saying we succeeded here, we succeeded in fixing this problem, we succeeded with this problem, therefore Trinity worked. And the next day, Wilson said to me, I would've defined success if it hadn't worked. So that's an old, you know, originating difference.*

I'm a little surprised at that with regard to World War II because most of the veterans of World War II, I think, would agree with Bethe, and that is that this is such an awful war, we've got to do something to stop it, and we're faced with an invasion of Japan, and it was awful up to now and it's going to get worse. So nuclear weapons did shock Japan into surrendering. I'm quite sure. And they gave them an excuse to surrender, as well. And that's equally important. But Bethe's view, then, was, I think, that OK, we had to explore whether thermonuclear weapons were feasible. We would've been better off if they had not been feasible. We weren't trying to solve a specific problem at that point. And I would think Bethe would probably say I'm really disappointed that they worked as well as they did, and I'm disappointed that Castle worked as well as it did. But that's nature and you sort of had to explore that. I think that he would say that we didn't try hard enough, early enough to reach some accommodation with the Soviet Union [USSR]. Our hearts weren't in test ban treaties as much as he would've liked to see. That's the response I think you'd get from Bethe.

My view is that—Teller was probably a little bit more hawkish—well, certainly more hawkish than Bethe. I think that the Russians were working very hard on this. I don't think we would've gotten them to the bargaining table. Once it was clear that thermonuclear weapons were going to work, and the Soviets had really the first team on that in [Andrei] Sakharov and the others, who were outstanding scientists, they were going to get nuclear weapons when they got them. It turns out Edward [Teller] was correct, I think, that the Russians were working on them, and [00:30:00] not just as a response to our program, and they would've gotten them whenever they got them.

But I think that nuclear weapons made the world safer for the last half of the twentieth century. I don't think we *really* ever got close to using them, although the most critical time was during the Cuban missile crisis. Fortunately, the leaders of both countries realized that a nuclear war would be awful. I don't think after that we ever got anywhere close to using them. And it probably prevented a potentially awful conventional war in Europe again. I don't think that Bethe would've agreed with that, really, but who knows?

*And now there's two questions that come out of that for me. Let me start—*

That's also, I'm sure, the way that John Manley would feel.

*I'm jumping way ahead, but let me ask you a present question because presently with the Cold War over but the persistence of the fact of nuclear weapons and this whole terrorism concern in Iran and North Korea, one way to look at that history is to say, well, all that development now puts us in this terrible position with other potential enemies, whereas at the time we were focused on the Soviets. So the persistence of the reality of nuclear weapons into the present time and how that was developed, how do you see those connections?*

Say a little bit more about this. How—how does that—?

*Well, the question, I think, is if there had been more control earlier, but you've just given a narrative of why that wasn't possible, because of the Soviet—. Were we overenthusiastic in any ways which now make the world more dangerous, let's say, in terms of numbers of weapons or anything like that? Because we've established, haven't we, that nuclear weapons are a good thing to have. Other people desire them. I'm being very simplistic here.*

Well, I think—yes, OK. Nuclear weapons are a useful thing to have if you feel threatened. And that's one of the things that makes it so difficult to control nuclear weapons. There is a feeling, of course, of haves *versus* have-nots, and that was codified in 1969 in the nonproliferation treaty where it was OK for these five countries to have nuclear weapons but other people could not. And certainly it's true that countries like India chafed under that. Also, it was clear to the rest of the world, other potentially really very powerful countries like Japan and Germany, that the permanent members of the Security Council were nuclear powers. Some of the world viewed the United States as being heavy-handed in one degree or another. France, I think, has felt that way for years. Other countries have benefited from the nuclear umbrella; Japan is probably the most important single one in this regard.

You raise a very interesting point, and I'm not sure there's a simple answer to it. I really don't think there is. I think it's worth a great deal more thought.

*Yes. I've been thinking about it.*

What would the world have looked like post-World War II without nuclear weapons? Very [00:35:00] interesting. I really think that nuclear weapons thwarted a more extensive takeover on the part of the Soviet Union in Europe than would have existed had nuclear weapons not been around. And I think it's kind of frightening to think of what World War II might've turned out like if we'd really had to invade Japan.

*Well, that's a big question that, everyone was talking about ten years ago with the fiftieth, and I think historians and others are still arguing about that; how weak was Japan, would an invasion really have been necessary, all those kinds of things. But we don't have to get into that. That's not—*

It goes on and on. I really think we would've had to invade, and I really think that the technical excuse to quit and the shock to the emperor made it possible not to. The Japanese, it would be hard to find a Japanese [person] who would say that Japan would have surrendered without an invasion. And certainly not in the Japanese military.

*Right. I read all that stuff.*

They had five million soldiers. Only about two million were in the home islands, but lordy, they wouldn't have let them just sit around. Anyway, I think it would've been awful if we had to invade, and of course we had just gotten through invading Okinawa and that was just an awful mess. Did you ask General Giller this question?

*No, I didn't. We went right from his work—no, maybe I will as a follow-up.*

Yeah, follow up. I'd be interested in his response.

*Yeah. We didn't go that way. We went mostly from his World War II experience, which was quite amazing, into how he, going back to graduate school and how he got into the AFSWP and all that stuff.*

Actually, another person to ask this question to would be Herb York.

*Oh, I have, and we talked about it a long time.*

What did he say?

*Oh, he's quite clear that the bomb needed to be used, and he looks at numbers, you know, the numbers that had died and the numbers that were represented by Hiroshima and Nagasaki, and*

*he says, you know, he says something to the—fifty million died in the war, a hundred and fifty thousand died in the ending of the war, and it was necessary.*

Well, I was thinking of the broader question of what would've happened if we hadn't had nuclear weapons post-World War II.

*No, and you know why I haven't? Because I have just really begun thinking about it myself. And will—both those question, yes, I should, and I didn't ask Giller, no, because it's been formulating in my mind. So next time I see Herb, I will ask him that question.*

Well, certainly, as you well know, Hans Bethe is not an enthusiast over nuclear weapons, but he strongly supports the use of nuclear weapons in World War II.

*Right. And he also, when I spoke to him years ago, made it very clear that it wasn't total disarmament that he was after, but a smaller deterrent than came to be so.*

Oh, absolutely. Well, I think there's no question that the deterrent during the peak of the Cold War was much too excessive.

*What do you think drove that? And let me preface this by asking, because some of the critiques of the scientists at the labs is that there was a technical enthusiasm related to maybe how you—the pleasure, the enjoyment of the testing itself that you described that fed this kind of—*

Oh, that probably had a little—had a role, but I think it was a relatively minor one, because the lab didn't drive, say, in the sixties, the weapons systems. It was the Department of Defense and the Department of Defense planners that drove this, not the lab. The expensive part is the [00:40:00] delivery system, and how many they had and where they were deployed. If you take, say, the submarine-based ballistic missile system, the cost of the nuclear warheads is tiny compared to all of that cost, certainly less than 10 percent. The number of submarines, the

number of warheads on submarines, we had no influence on them whatsoever. It was the Department of Defense and their planners.

So how did we get to where we are? They felt their responsibility was to make sure we could beat the Soviet Union, who had *enormous* quantities of stuff, and they never threw anything away. Their tanks were tread-to-tread, horizon-to-horizon. And the Department of Defense not only wanted to be able to feel that they could probably beat the Soviet Union, but to be absolutely certain they could defeat the Soviet Union. And when you start asking planners how much you need to do that, you come up with a lot of weapons. To hold their war-making capability at risk. In other words, the policy which was decided by the President of the United States, do you want to hold at risk the civilian population of the Soviet Union and thus threaten them with that destruction if they invade Western Europe? Or do you want to hold their war-making capability at risk? The decision, at least past the very early days of the fifties, was not to hold their population at risk, but to limit their ability to wage war. And that takes more weapons. And this is pointed out by the people who were arguing for more weapons rather than fewer. If you cut down to, let's say, five hundred weapons, what are you going to do with them? What are you going to target? In that case, you target population centers. You don't target the Russian tank farms somewhere.

So you're changing the role of nuclear weapons. So the military planners, once they're told that what you have to do is to hold at risk the Soviets' capability for waging war, you come up with twenty thousand nuclear weapons or so. Los Alamos had nothing to do with that. We're just arrogant to think that we do have some influence on that. I think people felt that this is important and necessary, but just because we told the Department of Defense we could make extra nuclear weapons, they decided how many missiles they wanted, how many submarines

they could have, and so on. And once you design the rocket missile system and Congress tells you how many submarines you're going to have, the number of nuclear weapons is fixed. We could tell the services that OK, instead of having, say, 200 KT, we can give you 220 KT, but they weren't impressed by that. They usually aren't, which is sensible. So I would say, after the early fifties, we didn't have any influence on any of those sorts of things. But I think, certainly looking at it in retrospect, we had many more weapons than we needed. But if you went down substantially, you do change your targeting philosophy.

*Right. That's an interesting point that I had no appreciation of. So when you're testing, let's say at Nevada, from a science point of view, you are trying to perfect [00:45:00] something or improve something. That's the reason for the tests, right, if you're not concerned with weapons. I mean I know a lot of the underground stuff had to do with weapons effects. So I'm trying to understand this military science thing as you continue to test. We've taken care of the numbers issue, which is not yours, but there are reasons that those tests go forward in the numbers that they do.*

Most of the tests are not just to see if the most recent change we made is good or bad. They are really complicated science experiments, trying to measure the details of the X-rays or gamma rays that are in the weapon. Let me stand back a minute and say that physicists are taught at a very early age that if you can't work a problem, to change it ever so slightly to one that you can work. People can't work out the theory, don't know the theory, of nuclear weapons in enough detail from the very beginning, ignition of the detonators, to the final burn or thermonuclear reaction of the secondary. They're very complicated. First, what happens is the primary goes off, and then radiation comes out of the primary, and it's that radiation that causes the secondary to go off. What are the details of that radiation and how does it work? How does it actually act upon

that secondary? And so the so-called temperature, the energy of that radiation at various times, and what times are taking place there. And so what you'll find is the calculations are long and complicated and start out with the detonators going and then the explosion. But as they get along, they get quantities in here whose values they don't know. For example, what is the energy of this radiation and how opaque is the material that it has to go through? So they have some quantities that they'll just call, say, X or Y or Z, and the people in Nevada have to measure X or Y or Z so they can put them in the equations. And they may not understand exactly what those are. They can make guesses. They can make estimates of those. But it's the people in the nuclear weapons tests who actually measure that. So they're very complicated nuclear physics experiments.

*Right. And then I imagine that your diagnostic instruments over the years have to improve also in order to do those measurements, is that right?*

[00:48:32] End Track 2, Disc 2.

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

In the early days, meaning Trinity, and actually the early testing period, Sandstone, there was interest in the yield and alpha. Alpha is how fast does the neutron population double. OK? Ben Diven was fortunate enough in working with one of the most brilliant guys here during World War II, Bruno Rossi. He took a look at Trinity, and the person who had responsibility for measuring alpha on Trinity was Bob Wilson. And Bruno Rossi looked at it and said, You know, I think I've got a better way of doing it. And Ben talked to Wilson and Wilson said, Well, gee, that sounds pretty good. I'll give you some of my experimental space and technician help and that sort of thing, and why don't you go ahead and measure it? And so Ben and Bruno Rossi and a couple of other guys from that RaLa [radiolanthanum] group went down. The technique Rossi developed for that is

the technique we used in the last shot in 1992, and it was called the Rossi Alpha. And it turned out that it was very successful, and now it was perfected over time.

But for a long time, they felt, well, let's just measure yield and alpha and we'll know everything there is to know. One of the most difficult things in trying now to simulate things, simulate device behavior with experiments out in the back yard is that you don't get a thermonuclear reaction without a nuclear reaction, and you can't simulate with high explosive anything once it goes critical. So you really have to extrapolate a long ways with calculations. Well, particularly after we got thermonuclear weapons, there was a strong push to get more and more diagnostics to understand how these things worked. What is the basic physics that's going on there, and why don't we get the yield that we calculate we ought to get? Sometimes we get twice the yield and sometimes we get half the yield, and more often than not, we got half the yield than twice the yield.

So the experiments got more and more complicated. And then the theoretical designers wanted to know what was going on at different spots in the nuclear explosion. So they wanted a picture of the nuclear explosion, essentially, tell me what the radiation coming out of this device looks like as a function of where it is in the device. So they wanted a picture of it. Well, OK, so the diagnostic people developed ways to get a picture of it. Here's how we can tell what's going on at various spots in the device. Then the theoretical designers said, well, you know, a picture is nice but I'd really rather have a movie. So we want you to take this picture and measure it as a function of time. What's going on when. So then the diagnostic scientists worked this out. And then the theoreticians said, well, it's nice to have this movie but we don't want it in black-and-white; we want it in color. So that means what's the energy of these various things that are coming out, where high energies might look like they're yellow and low energies look like

they're blue or they wanted to know what the energy was. So diagnostic scientists worked out these things. But sometimes you'll see pictures either from the Pacific or from Nevada with long lines-of-sight pipe, looking at various things. That's why. They're trying to see what is going on as a function of time in these various areas. Now, their detector might only [00:05:00] last for, say, a hundred microseconds, and so all of the data have to come out in a hundred microseconds and be recorded. So the detectors are important. How do you really interpret what you're seeing in these detectors. They're like very, very complicated experiments in an accelerator, but an accelerator that has only one pulse. It's got to work the first time, or you start all over again.

So these are very, very complicated experiments, and only occasionally did we really do what would be called a proof test. Let's just take one out and see if it really works the way we say to the military. And once in a while, we would take one from the military. Sandia has a way of getting random numbers on these, and so they'd get a random code number and find out that this device is aboard a missile in North Dakota, and we sweet-talked the Air Force into letting us have that warhead, and we made the absolute minimum changes to it to allow us to fire it in Nevada. And that means some very complicated things because there are environmental sensors on board these things that say it has to lift off and it has to have a deceleration going in and so on, but—

*So you remove those kind of weapons aspects to—?*

No, we tried to activate those as it would—we'd try to fool it into thinking, so the only thing we have to do is that we have to tell it when to go off and not have its own sensors tell it when to go off. So we really put a lot of thought into making sure that we don't make any modifications to this device that might compromise its behavior. Then we put it down hole and fire it and see if it works properly. And fortunately, they all have. We also took one, usually maybe, oh, somewhere

between the fifth and the tenth one, off the production line and just take that out and fire it. So we did do that from time to time. And that's comforting.

*It's almost like a quality control thing.*

Yes. It is. A sort of a final test. But most of the things, most of the tests in Nevada or in the Pacific, were not of that kind. They were trying to understand how does this thing really work, what's the physics. If you write the equations for how this is supposed to work, how can we tell you what the unknowns are that have to go into that equation?

*Now, let me ask you this, John, and you tell me if this isn't a good question. In the context of the arms race, as physicists knowing the quality of the Soviet scientists, are you ever thinking or do you have to assume that the Soviet scientists are doing something similar to what you're doing?*

*Is that even something that enters your mind at the time?*

Oh, yeah, sure. And we pay a lot of attention to what they do, what they're doing and what we can tell from their shots and what we can tell from their measurements. When they tested in the atmosphere, we knew a *lot* more about their weapons than we did once they went underground.

One of the advantages of having atmospheric testing is you can keep track of what the competition is doing. But we could certainly tell the yield of their devices, we could tell how complicated their experiments were, approximately what they were measuring, and so on. And we had to assume that they were very good, every bit as good as we are.

*I'm thinking back to the similar situation. During the Manhattan Project, it seems very much in the forefront, the concern about if we can do this, then the Germans can certainly, that sense because you know who those scientists are and you know what their abilities are. So I had never really thought about as you, as a physicist, whether you're thinking in terms of what the enemy is doing.*

Oh, sure, and we play a major role. The analysis of foreign nuclear weapons programs takes [00:10:00] place at Livermore, Los Alamos, and Sandia. So we at Los Alamos are the world's experts at what is actually the technical capability of other countries.

*Now, this is a question out of my ignorance of physics, and you read all the time about measuring alpha, but why do you measure alpha?*

Alpha is very important as a fundamental measurement of how well the fission process is actually taking place.

*So it's a function of the fission?*

Take a look at [Robert] Serber's book [based on Serber's 1943 Manhattan Project lectures. See, *The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb*, 1992] and you don't need to understand every equation there but you'll see that alpha is very important.

*And then explain to me the question of gamma radiation that has to do with—the measure of gamma has to do with what? I'm not a scientist; you can tell.*

Well, several things come out of an explosion when it takes place. One is X-rays, and X-rays are just low energy gamma rays. Or they're both electromagnetic radiation. And that's important for the behavior of a device, and it's also important for the weapons effects. And neutrons come out. And so people like Ed Giller in AFSWP measured at a distance how many neutrons are there on the gamma rays, what's the spectrum of the gamma rays, how about the heat, because nuclear weapons—. Well, if I oversimplify, low yield nuclear weapons generally kill people by the radiation that come[s] out. Medium-sized weapons, say, a few hundred kilotons, usually kill people by knocking down structures and having them fall on them. Most of the people, I suppose, at Hiroshima actually died because the building they were in fell on them. Very high yield weapons do damage, to a large extent, by fires they produce at great distances. And it's

AFSWP that wanted to understand that in detail, and how could you tell and what were the important things about the height of the device. They thought about this before the strikes on Japan, and they wanted to optimize the damage done, and decided for twenty kilotons, eighteen hundred feet was optimum. And that was about correct. But—I'm not sure I didn't get off the track of what you were asking.

*No, you answered my question. We're just—I want to pause for a second.*

[Pause]

Well, I told you that I love nuclear weapon testing and the activities, and I also thought that there is a great deal missing on what went on in that period. By the way, Terry [Terrence R.] Fehner and Skip [F.G.] Gosling have written about the atmospheric test days. They haven't published it yet but they have a manuscript mostly done. I think that's going to be different than what Barbara Germain and I are working on, but I think Barbara and I both felt that this was a very interesting and exciting subject. It was a great time in our lives and it has been neglected by most other people who concentrated particularly on the wartime days at Los Alamos, and so we thought it would be interesting and fun to do.

*And who are the people that have written the book on the atmospheric testing?*

Terry Fehner, who wrote the *Origins of the [Nevada] Test Site* [DOE/MA-0518, 2002], you know, that—

*Yes.*

OK, he and his colleague, Skip Gosling.

*That's right. So they're doing an expanded thing on atmospheric testing.*

*Yes.*

**[00:15:00]** *And how's it going? I mean do you feel—is it what you thought it would be to have to write this book?*

Oh, it's much longer and more involved than I—I thought it would be long and involved, time consuming, but it's much longer and more time consuming. And also, somehow or other I just have difficulty keeping on the subject all week. I have done a fair amount of volunteer for the county. I've chaired a number of committees for them, and that's interesting and exciting and I think worth doing, but it takes time away, and also with my wife's health problems, that also takes a fair amount of time and effort.

*Well, we should wrap up, so if there's anything—I wanted to talk to you a little bit, but we can do it another time, about some of the kinds of problems you would encounter at the test site, but if—on what we've talked about today, is there anything that I missed asking you about?*

No, it seems to me we covered an enormous amount of stuff.

*We've covered a lot of stuff.*

Yeah. Well, I would be glad to talk to you more about that. You have some experts, of course, out there in Troy Wade and—

*Yeah. Well, I'm talking to lots of people but it's always good to physicists.*

OK, get a fresh view.

**[00:16:31]** End Track 3, Disc 2.

**[00:00:00]** Begin Track 4, Disc 2.

Most people have a fairly narrow view of what they do and how it fits in to the overall chart. If you ask the Los Alamosan to draw an organization chart of the Nevada Test Site, it'd be Los Alamos up at the top and maybe Livermore to the side, and then down here somewhere there would be the AEC and DOE [Department of Energy] and REECo [Reynolds Electrical and

Engineering Company] and so on. If you ask DOE to draw that box, there'd be the DOE or AEC in the big box and then the little laboratories. The truth of the matter is, of course, it's in there. I had no appreciation, for example, for how big and extensive the DoD work was out at the test site, although I probably could've dredged up the numbers of people and dollars and so on out of my memory. It wasn't their site. It was our site. And we owned it and the DOE worked for us and the DoD work came in a bit. It's the same attitude that Los Alamos has if you take a look at the Manhattan Project. Around this place, people think the Manhattan Project was Los Alamos. And we were three thousand people out of a hundred and seventy thousand or so.

The big work, and this is what has kept proliferation down, was getting the material. It seems to me if we had given very much thought to what the Germans really could do during World War II, it should've been crystal clear and obvious to everybody that there was no way they could have possibly gotten a nuclear weapon because they couldn't have separated out the U235 or produced plutonium. And certainly they couldn't have done so without aerial surveillance picking this up immediately. Why it came as a surprise to people that they weren't any further along, I don't know. Why people felt until even the last minute that the Germans might be ahead of us, I don't know. We had a fair amount of aerial reconnaissance over Germany in World War II, and the targeteers certainly knew what was where.

But at any rate, they couldn't make the material, and I think they realized this. Japan could not. The Japanese scientists, their Academy of Science, was asked during 1943 whether either the United States or Germany could make a nuclear weapon, and they concluded that we could not in time—neither Germany nor the United States could do so in time to influence World War II. Well, they were half right. The wrong half, unfortunately.

But Los Alamos was where it all came together, but Los Alamos wasn't the Manhattan Project. And Oak Ridge [National Laboratory, Tennessee] and Hanford [Site, Washington] deserve a lot more credit than we give them for what they did in making that material. And a lot of these top-notch scientists, by the way, were not focused on Los Alamos; they were focused on the material production. Harold Urey, Eugene Wigner, these guys really knew what was going on in the piles [nuclear reactors] in Hanford.

*Right. That's interesting. But it's interesting where you started here, which is that depending on who you talk to, the organizational chart is going to look different.*

Yes. Yes. And the DOE played an enormous role out at the test site, and much more of a role than we give them credit for.

**[00:04:24]** End Track 4, Disc 2.

[End of interview]