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

Interview with
Richard Garwin

July 21, 2004
La Jolla, California

Interview Conducted By
Mary Palevsky

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Richard Garwin: I didn't have anything to do with establishing it in Nevada, so my relation is somewhat limited, but maybe there are some things I can tell you.

Mary Palevsky: *Well, I noticed you said that—I think the way I thought about talking to you today, there were several issues that you raised in your articles that I think are important to understand generally about the whole nuclear weapons situation. But you did say that you were going to Los Alamos during the decade of the fifties, I guess, and I was just wondering if there was any weapons work that you did on there that was actually related to the test site.*

Oh, well, yes, I began my work at Los Alamos in the summer of 1950, after receiving my Ph.D. in late 1949. And I spent, as I recall, three months there the first year and four months in '51 and five months in '52 and probably three months the other summers until '59, when we went for a shorter time, if at all, because we spent the next year in Geneva. I was a visiting scholar at CERN [Conseil Europeén pour la Recherche Nucleaire]. And then during the 1960s, I was there [at Los Alamos] probably half the summers, more or less. From the very beginning, I worked on nuclear weapons. I had been a graduate student of Enrico Fermi's, and he was my thesis sponsor. So the first summer I was there, Fermi and I shared an office, which was very interesting because he had been there during the war, from 1944 until the end of 1945, and had played a very important role in the nuclear weapons program, first with his reactor in Chicago, and then they stayed there after Los Alamos was opened. He came only the summer of 1944, instead of March '43. He had been helping to design the Hanford reactors that would actually make the plutonium that would be used in the bomb. But he had gone back every summer as a consultant to Los Alamos, and so he continued to be involved. And he was highly regarded. In Rome, he was called “the Pope.” And in Los Alamos, of course, this is not a Catholic country, I don’t think they called him “the Pope.” But he was a major resource for building the bomb, and he was an excellent physicist with a very good grasp of experimental things as well as theoretical things. So when people needed a solution to anything, they would, as a last resort, go to Fermi and he would tell them how to solve their problems. So it was very interesting, and when I first went to the classified report library, I read all of the weekly progress reports from the various groups during the war, and then I knew everything there was to know about nuclear weapons, so I had some of my own ideas and he initially helped me work them out.

And when did the Nevada Test Site open?

Fifty-one.

Fifty-one. Yes. So, this was in 1950. And in—you’re sure it was in ’51?

Yes. [Truman accepted AEC recommendation re establishing Nevada Test Site on 12/18/1950; Early January 1951, the decision was made public; first test in Ranger series, 1/27/1951]

So I, of course, read all about the tests in the Pacific and the plans for the test series in 1951 in the Pacific, where various major things were tested by the [Operation] Greenhouse series, like Greenhouse George, which was the first demonstration of burning of thermonuclear fuel, and Greenhouse Item, which was the first boosted fission bomb. And, let’s see, King, which was the largest fission bomb ever tested, at 500 kilotons.

Now, were you there for those tests?

No, I never saw a nuclear explosion. I went in 1951, probably, to Hawaii for a couple of days to talk with people who came back from the test site. They were having some problems. But I didn't want to take the time to go out because I had a wife and a young child and we were there in Los Alamos only for the summer, and so it didn't seem worthwhile for me to do that.

Can you explain to me—you talk about it in one of your articles—what it means, what you just said, "boosted"? What does that mean, actually, in layperson's terms?

[00:05:00] Oh, yes. A fission bomb assembles a supercritical mass of material, so you start with two or more subcritical masses, and in the Hiroshima bomb, these were solid uranium-235 pieces, so gunpowder, not explosives, gunpowder is used to propel one of them down a gun barrel, right up against the other. And when they're in close contact, then a neutron source, an initiator, gives neutrons, and so if you start with a hundred neutrons, then within a few billionths of a second, those neutrons have caused fission and you get two hundred neutrons, four hundred, eight hundred, sixteen hundred, and so on. And after many doublings or fifty generations—factors of e—you have a good fraction of the nuclei are fissioned, a couple of percent in the Hiroshima bomb and 30 percent in the Nagasaki bomb, and that's terminated because the energy released provides pressure that blows things apart. And since the multiplication is every hundredth of a microsecond or so, then you need a lot of pressure, a lot of speed, to move the heavy masses apart in a hundredth of a microsecond, which is what's required. So that's the normal course of a fission bomb. But with the booster, after the fissioning has been going on for some hundredths of a microsecond, and maybe a few percent of the yield has been produced, it's sufficiently hot inside the bomb to cause thermonuclear reactions between the deuterium and tritium, and that gives a very short pulse of neutrons. Doesn't produce much energy, but it may multiply the number of neutrons present by a factor of X. And so the energy is being produced at a rate, and then suddenly it's being produced at X times that rate, so a boost in the level of the fission activity. And that means that you need *more* pressure in order to disassemble the bomb, to stop the fission reaction, and so it increases the yield, by a considerable factor. And of course, it makes it a lot *safer* to get a given yield because you need less fissile material. It's less critical when you start. So anyhow, all of our nuclear weapon primaries of all nuclear weapons in the U.S. stockpile are boosted fission weapons.

Now, that was the first test. I didn't have anything to do with the design of it, but I did have to do with the *diagnostics* on these things, and especially in the 1951 series, which was the first I could affect because it takes *time* to affect these things. And I don't remember whether there was a 1950 series. Because these were *big* operations in the Pacific and they tended to mount them every two years. So in thinking about these things—no?

'48 and then '51.[In Pacific: Operation Crossroads, 1946; Operation Sandstone, 1948; Operation Greenhouse, 1951; Operation Castle, 1954] .

Yes. In thinking about these things, I invented a new technique for finding out exactly what was happening at various places within the bomb, and that was to put there relatively rare materials. The first we used, or I proposed, were nickel and arsenic, but they use all kinds of rare earths or whatever. And after the nuclear explosion and the tremendous fireball and everything is all mixed together, they would go out with airplanes and filters and get radioactive samples from which they would determine the yield, from the amount of fission products compared with the amount of plutonium or uranium remaining. And also the neutrons in the nuclear reaction would activate, convert the stable isotopes into very particular radioactive materials, and that would tell you how much neutron exposure they had had. And so you can look inside a nuclear weapon at the time of exploding, just a few centimeters apart, and this was much more important later when **[00:10:00]** we had the thermonuclear weapons and we really had uncertainties and needed to diagnose what was happening in the thermonuclear fuel here, there, and separate the energy of the primary from the energy of the secondary.

So that was one of the first things I did. And the second was to look at how nuclear weapons would actually be employed on the battlefield. People were talking about getting lots more nuclear weapons, and so battlefield commanders would use them. And I asked myself, how long after a nuclear explosion could you use another nuclear weapon and not have it affected by the first? So it turns out that there are two things involved. One is the time and the other is the distance. And the time is determined locally by the continuing release of “delayed” neutrons from the fissions. It’s important for the control of reactors but has nothing to do with nuclear weapons. But almost 1 percent of the neutrons in the fission product process come a second or ten seconds or a minute after the radioactive materials are created, whereas the fission process itself is a fraction of a trillionth of a second. It’s very short. And the time between fission generations is a few billionths of a second. So there are these things that happen. And that’s important for the control of reactors. But after a nuclear explosion, you have an enormous number of neutrons, and they continue to dribble out. And since gun-type weapons are very vulnerable to having their yield almost eliminated by premature neutrons, they are very sensitive detectors of what has gone on. And even though the cloud has mostly gone elsewhere, together with the debris, there’s still some delayed neutrons. So I looked at that. The neutrons have great difficulty getting through the atmosphere, and so at a great distance, the effect is dominated by high-energy gamma rays. So you have to think of a lot of separate things. Those gamma rays come from the capture of the neutrons in the nitrogen of the atmosphere. So you have an explosion and for every nucleus fissioned, a neutron gets out and gets caught in the nitrogen. One percent of those give rise to an eleven-million-volt gamma ray, which is very high energy compared with most gamma rays, which are a couple MeV [one million, or 10^6 electron volts]. And that gamma ray is a high enough energy that it can cause fission itself. Gamma rays go much farther in the atmosphere than do neutrons. And so miles away, you have photo fission, that is, nuclear weapons will have fission induced in their cores. And then *those* fissions—so that's all prompt processes—and then those fissions in their cores, which are too weak to heat up the weapon or injure it in any way, give you a continuing dribble of neutrons right inside the nuclear weapon, which would pre-initiate it.

So anyhow, the relevance to the test site is that it had just been opened and they were having nuclear explosions there, above ground, and Jane Hall who was, I guess—I don't remember whether she was deputy director at Los Alamos at the time, and her husband Dave picked up on this idea and went out and deployed at the test site a hemisphere of uranium and had some neutron detectors under it to see what the time course of behavior was. They validated this theory, which was important because otherwise if nuclear weapons had ever been used in warfare, they would not have been effective, the later ones. And I later learned that, in fact, people had proposed a missile defense system that would use this technique to extend the **[00:15:00]** range of nuclear-armed interceptors against incoming Russian missiles or airborne nuclear weapons. It turns out that later developments in nuclear weaponry went away from the gun-type weapon used and the implosion-type weapons, because they're built to function despite the much larger neutron background in plutonium. So they're not so vulnerable, and our current nuclear weapons aren't vulnerable at *all* to this kind of effect because it has been revealed by the government that they are not subject to pre-initiation of this type.

So that's the sort of thing I was doing. And then it was also, the first year, decided that the information that people had, the reaction rates between deuterium and tritium, and deuterium and deuterium, went back to pre-war experiments and they weren't very accurate. So I began an experiment to *measure* these things again. When I left in the fall, I turned over the design of the experiment to colleagues. And the laboratory formed a group. Fermi was instrumental in bringing James Tuck, a physicist who had been with the British group at Los Alamos during the war, back from England to lead this group to measure the cross-sections.

I was also interested in other diagnostics for the 1951 tests, and that's when I first met Herb York from [University of California at] Berkeley, and Ernest Krause from the Naval Research Laboratory, and Montgomery Johnson and others who were involved in building big equipment, diagnostic equipment. Kind of pinhole cameras, because you don't have lenses that focus neutrons or gamma rays, to look at the details of the *behavior* of this Greenhouse George burning activity, or some of the others.

So I contributed a good deal in that. And, of course, I got to know a lot of these people at the time. And I was in favor of having a continental test site. Just technically, it was a big pain to have to wait two years or three years to explode nuclear weapons out in the Pacific. It would be much easier if you didn't have to have a whole expedition, but you could do it within weeks or months. If you could do it *safely*, of course. And I devised some other things. And then that was 1950, pretty much. I was busy.

Then in 1951, when I got to Los Alamos, [Edward] Teller, whom I knew from 1950 and also because we were on the faculty at the University of Chicago together, in physics, so I saw him every day or every couple of days there, he told me that he and Stan Ulam had had the idea of radiation implosion—that figures in their still-secret March 9, 1951 paper in Los Alamos—and that he would like me to devise an experiment to show this worked. And as he says, in about a week I came back with a design, not of the experiment but of the full-scale hydrogen bomb. It seemed to me that when you have an experiment, there will always be some dispute. First, you have to make the experiment. You have to show that it's relevant, that it really proves the principle, and then you have to take the next step. So it seemed to me if you could do the whole thing—it was easier to demonstrate in large size than small size—then that would solve the problem. So that's what we did. And in a fantastically short time people actually did the engineering and design and building. So in sixteen months, from May 1, 1951 to the Mike explosion, November 1, 1952, the laboratory did all of this work. And the AEC [Atomic Energy Commission] mounted the usual joint task force, this time, only the *next* year, 1952, for these thermonuclear tests.

So that's what I did in 1951. And I contributed also to the radiochemistry group that did **[00:20:00]** these analyses. I would talk to all kinds of people.

I had a couple of questions. You said about being in favor of the continental test site. Do you have any particular insight into [Norris]Bradbury's views on the test site? I read some things that John Hopkins, at Los Alamos, is writing a history of the test site, and he refers to some memos that I don't think I can see, even now. Bradbury's concerned about safety, et cetera, et cetera, *and* my *interpretation was only under sort of extreme national security kinds of needs should we even be thinking about a continental test site. Do you remember anything about that discussion?*

No, I was not involved in those discussion and, of course, you really *need* to look at all kinds of safety considerations: earthquake, ground shock, and, of course, as Las Vegas has been built up, it gets more and more important—but we have had a moratorium since 1992—but mostly fallout, that is, radioactivity. And there, the information that we have is really not all that great. I know a lot more about this now than I knew then. And as a result of Linus Pauling and other people's pressures, people looked at the influence of radiation, especially the induction of cancer by radiation. And in my books—I don't know whether you've seen my book *Megawatts and Megatons*.

I haven't. I saw a reference to it in your paper.

OK. Well, it's available in paperback, the University of Chicago Press [*Megawatts and Megatons: The Future of Nuclear Weapons*, 2002].

I'll get it.

And so we estimate there that the nuclear testing that has been carried out in the atmosphere, and I think it's 370 megatons of fission, which is a tremendous lot. Mike was ten megatons itself, not all of which was fission, and so 370 megatons of fission is like forty Mike shots. And of course, some of this was from the Russians, as well. Russians and Americans contributed most of it. Most of their *biggest* shot, which was fifty or sixty megatons, was thermonuclear energy and didn't contribute to this 370 megatons. So it's really the thousands of other tests that were done.

So the fission tests, that's when we're talking about the kind of fallout that's dangerous, is that what I'm understanding?

Yes.

Thermonuclear tests, it doesn't result in the same?

No, that's right. It's mostly the fission products. But thermonuclear tests are typically half fission. It's a rule of thumb that a thermonuclear weapon really is half fission because that's the most convenient way to *make* it. Now, we have had some Plowshare experiments, that is, with quite *clean* nuclear explosions, and the Russians, they have in their museum a 120 kiloton explosive for underground rock crushing, and of this 120 kilotons, I think only 0.7 kilotons is fission yield. And so that's very clean, and it would take an enormous lot of those to get even a megaton. It would take fourteen hundred of them to get even one megaton of fission yield into the atmosphere. So it makes a big difference.

But anyhow, I think we figure in our book that three hundred thousand people worldwide is the best estimate of the number who have died from cancer from the atmospheric testing. And that's a lot of people, by one measure, but if atmospheric testing prevented nuclear war, then a lot more people would've died in nuclear war, so you have to ask, what is your judgment on that? And we figure, incidentally, that twenty-four thousand people, more or less, will have died from the fallout from Chernobyl, from that one reactor accident. Because a reactor has a lot more long-life fission products in it than does even a very big bomb, because a reactor fissions a ton of **[00:25:00]** fuel a year, and typically the fuel has an age of two years in a reactor, and so there are a couple of tons, and it's seventeen kilotons of fission yield per kilogram, so seventeen megatons per ton, so about thirty megatons. And that agrees, because I said three hundred megatons will have killed three hundred thousand people. Thirty megatons will kill twenty-four thousand people. One-tenth as many, approximately.

So anyhow, that's most of my involvement with the test site.

But I have a question here, and I wasn't anticipating asking you this, but one of the things that is becoming so clear, from my ten months in Nevada, is there's huge argument about just what you're talking about. People who consider themselves Downwinders, sort of awful, unseen-before cancers in themselves or their children. And then people in the test site organization and either the AEC or the DOE [Department of Energy] or one of the labs, will say, Look, the evidence just is not there for these kinds of illnesses. *And one of the things I'm just struggling with myself—I don't have to answer that question* per se*—is to really understand how these narratives could be so diametrically opposed. And then there are people out there now* *with Indian tribes and with long-term studies and their compensation programs. What would you say the state of the science is on this, from a medical standpoint, at this point in time? Because it's really sort of baffling to see the various viewpoints, sometimes very passionately held, of these various groups.*

Well, it would be easier if there weren't money or politics in it. But first of all, I doubt that there are cancers caused by radiation that differ in any significant way from the naturally occurring cancers. And so people who say that there are these strange cancers, I think, are just wrong. Mostly, if people are ill, there's just no benefit and a good deal, somehow, of primitive shame associated with revealing your illness. So if you have a disfiguring illness, you don't go around showing it to people. So people are pretty unfamiliar with the terrible things that cancer does. And here, there's a benefit. Either it's a political benefit or a monetary benefit, helped by lawyers whose job it is to make themselves rich, like Mr. Edwards [then (2004) Democratic vice presidential candidate John Edwards], by using the legal system. And then there are people who are either against the government or they want their own goals. Some of it is personal advancement. Some of it is retribution of some kind or other.

But there are real uncertainties in the effects of low-level radiation, and respectable people who maintain that there are—it's a lot more complicated than was thought in the 1950s, especially to physicists, because there it was thought that you had an electron or some other ionizing particle would pass through the body. It would occasionally disrupt a cell in some way and lead to cancer. Now, of course, since DNA was discovered in the 1950s and the mechanism of heredity and cell reproduction, we know a tremendous lot more. We know that it's damage to the DNA, for the most part, and since DNA was not known, you couldn't ascribe the target for the radiation damage. So it's damage to the DNA. And so pretty early on in the fifties and sixties, it was recognized that there was also an active repair mechanism operating. Because there are very many spontaneous damages in each cell. Every second each cell in the body **[00:30:00]** suffers an insult simply because it has these three billion base pairs of DNA in its nucleus and there are things going on, chemistry, that's *always* damaging the DNA. So totally unsuspected by physicists or maybe others, these cells are little factories with their own inspectors inspecting always the DNA and fixing it, because it's double-stranded, so if you damage one strand, the other one's there as a template to tell you how to fix it. Now, people differ, and they differ from person to person. They differ from time to time, I'm convinced. Sometimes the repair mechanism is going better than others. And so the question of cancer incidents from radiation, in the presence of this enormous background of spontaneous mutation, and especially single-strand mutations, but also double-strand mutations, is very problematical.

So in our book, I spend a good deal of time on this question. And we believe, still as physicists but with more knowledge, that if you have a tiny increment to the background radiation, and this is a very small addition to the spontaneous damage rate to DNA, that there must be, for tiny amounts of radiation, there must be a linear effect. And so people who say, `If the radiation is spread around so a million people get it instead of one, then there will be no damage,` they're wrong, and there will be *at least* as much damage as if it were concentrated on one person. *But* it's very much more difficult to determine what it is because people have a 20 percent probability of dying of cancer *anyhow*, even in the absence of radiation. And that number varies from sub-population to sub-population.

So that's *my* view. Now, it might even be that this linear relationship, it just says that the damage is proportional to the amount of radiation and it doesn't matter how it's spread around. But the theorem doesn't distinguish between a positive coefficient or a negative coefficient, so maybe in this very low level of range, the radiation could even be *good* for you, slightly good for you. We don't believe that, but it's possible. What we *do* know is that it has to be linear.

OK, so there are people who are absolutely dedicated—one of the strongest motivators in human behavior seems to be to attack your enemies. So if there are people who make the mistake or, for that matter, who argue that tiny amounts of radiation are extremely bad and the smaller the amount of radiation, the worse it is per unit dose, that so incenses these other people that they take firmly the position that a tiny bit of radiation does absolutely *no* damage, or that it is definitely good for you. Radiation "hormesis."

So you should be aware of this controversy, and the whole chapter in our book, Chapter 4, which discusses this. And it's very important. But as I say, even granting the International Commission on Radiation Protection, the ICRP, coefficient of one radiation death per twenty-five sieverts [Sv], which is the current measure of radiation dose, and the sievert is 100 Rem [Roentgen Equivalent Man]. So at one per twenty-five hundred Rem, and the average exposure of the American people is about 0.2 Rems per year, and so your probability of ultimately dying from cancer by exposure to background radiation in one year is about 1 in 12,500. And if you're exposed for **[00:35:00]** forty years and then the cancer takes ten or twenty years to develop, then your probability is about 1 in 300. So of the 20 percent normal death rate from cancer, about 1 in 60 of those people presumably die from the effects of background radiation, which is half natural and half diagnostic, medical, and dental. So if you're really interested in reducing deaths from radiation, what you ought to do is to reduce the controllable part of that, and that's, what did I say, that would be about a sixth of a percent. If you have in the United States, say, two million people dying a year altogether, then 1 percent of that is twenty thousand. So about three thousand people a year die from the effects of medical and dental X-rays, diagnostic X-rays. There's more from therapeutic, but that's a different question. And worldwide, this three hundred thousand who died from nuclear testing are worldwide, and so worldwide there are twenty times as many people as in the United States, so we have sixty thousand a year dying from excess medical and dental x-radiation, and other countries have more exposure than we do because we have better rules.

So there are all kinds of things that ought to be done if you have fear of radiation. Fallout, to my mind, is in no way more effective than these other kinds of radiation in causing cancer. I think it's ridiculous for people to be able to prove in court that their cancer was due to this exposure to fuel particles or whatever. You have a public health problem and you have rules and, of course, if people don't follow the rules, then there is a public interest in judging them and assessing penalties. It shouldn't go necessarily to the person who happens to have a cancer that *might*, with some stretch of the imagination, come from this activity of somebody else.

Some of what I've read, because it's of political interest and social interest, let's say, in Nevada— and thank you for that. That's really helpful to me because I'm trying to understand as best I can what some of the elements in this argument are. But they say we have to look at it epidemiologically and you can't say "this particular cancer," but you start seeing clusters in certain areas—

Yes, but clusters really tell you nothing. And I've been involved on National Academy [of Sciences] studies, for instance, of the health effects of background magnetic fields, sixty Hertz magnetic fields, and radiation and whatnot. And the incidence of cancer is so low, by *any* model, that you couldn't possibly get a cluster. The cluster is a statistical anomaly, or it is a genetic, in the case where it's a family cluster, it's a genetic predisposition.

Oh, I should say that we know a lot more about how cancers begin, but we don't know all that much about how cancers propagate. But they begin as a result of several independent mutations in the cell, typically five or eight of them, because cancer is so destructive, one cell mutating to a cancerous form and growing will destroy the whole organism. And so gradually people, species, have evolved to have good defense mechanisms against cancer. Typically, cells wear out. They can reproduce only forty generations or so. And there's a clock in the cell, the telomere, at the ends of some of the chromosomes. And these are non-coding DNA segments, and in normal reproduction of a cell, one of those units is chopped off, and it cannot reproduce that chromosome when there are no telomeric ends. That's how I understand it but I'm not a **[00:40:00]** specialist. But cancer cells have somehow evolved so that their telomeres don't shorten, and so they can reproduce any number of generations.

And then there's the P-53 gene and the corresponding P-53 protein. Each of the genes produces a protein. And as I understand it, that's one that's responsible for apoptosis. When people are being formed from the embryo, all the wonderful things that you see in babies and people, you know, fingers and whatnot, it's hard to imagine how fingers grow out. Well, in fact, they don't really. You're formed with webbed hands, and then the webs die out. They disappear—except sometimes they don't, so you have a congenital malformation—and that comes about from programmed cell death, and these cells that die from apoptosis do so in a very nice way so that they get thoroughly recycled into the body's machinery. So P-53 looks out after the cell, and when things have gone so wrong that there's clearly something abnormal, then it's responsible for provoking the chain of events that leads to programmed cell death. So one of the requirements for most cancers is that the P-53 gene be disabled or that its transcription be inhibited some way.

So we think—people think that they know the chain that leads to various cancers. It's not always the same. And it can happen in any order, so that you can have this particular mutation first, and it has to happen in the same cell, and that's why there are such long induction periods in many cases for cancer.

So all this was unknown, could not have been known, in the 1950s when these decisions were made. What *was* known was that radiation caused cancer and people tried to be conservative with radiation and particularly the general public. And, of course, this controversy involves a lot of people not wanting to admit their ignorance or error, especially where it has legal implications.

Right. Yes, this is really helpful, because what I see there, there are people who are health physicists. That was their job at the test site, to understand health issues. And you see people now, medical doctors from various universities, trying to identify or claim that have been identified: these *are the cancers that could have been caused here. The cancer you have is not on this list. Therefore, you don't have a valid suit against us. Then there are other doctors out there currently who are saying, We still don't know. We're doing all these studies. So it's important for me to have some sort of* way *to start to begin to try to discriminate.*

Right. Well, it's difficult. Our book does a pretty good job, and I can tell you I've run into people at parties in Washington or whatever who are *incensed* that we still support the linear hypothesis and have not come over to the "a little bit of radiation can't possibly hurt you at all" position. But also, there's plenty of reason for suspicion. You know, you have a bureaucratic organization. You have the AEC. You have Dr. Shields [Warren] or whatever his name was. And the AEC wanted to get on with its business, whatever that was, so it had various secret activities to try to understand the effects of radiation and how widespread fallout was. And there was Project

Sunshine; I'm sure you've come across that. I know Bill Libby, later a Nobel Prize winner, but chemist at the University of Chicago, and then an atomic energy commissioner, was involved. I don't know that he started the program, but it was collecting baby teeth and maybe even bones from dead children, **[00:45:00]** and to analyze them for the Sr-90, whatever, for persistent radioactivity. And, of course, the AEC maintained that fallout was not a hazard beyond the bounds of the test site, just the way the French maintained that the fallout from Chernobyl stopped at the borders of France. And if you look at maps, you'll see charts of fallout elsewhere, but the French, because of their nuclear power industry, refused to publish any official observations of fallout within France.

However, the test site. The AEC, although maintaining that the fallout couldn't hurt anybody, when we were living in Chicago until December of 1952, I remember going out and wiping off the car, which got very dusty in Chicago, and bringing the rag in to a Geiger counter, and it just went wild. And it was a substantial dose. I never did estimate what the probability of dying from cancer *was* from that. But the AEC would inform Eastman Kodak whenever there was a test. Because Kodak would package its film with black sheets between them, the sheet film, and the paper which makes the black sheets is made from trees. The trees have radioactivity incorporated in them or on them. And so the AEC would routinely tell Eastman Kodak in Rochester, New York when they were having a test, so that Kodak could be aware that there was some hazard from radiation and they should test their paper. But at the same time, they weren't telling the general public. So ordinary people see this kind of behavior, things are being kept from them, and of course governments and corporations and individuals are not going to tell anybody any more than they are forced to tell because it might lead to trouble, to inhibiting their programs. So when people *see* that, they take it as evidence of malfeasance and evildoing.

Right. Right. Do you know the work of this guy, [physician] John Gofman?

Oh, sure. I quote him quite a lot in our book.

You do. I've just begun looking at some of that material, and his thesis was about the low-level radiation, is that correct?

Yes. Well, Gofman, I think, did a lot of good work there. On the other hand, he takes all of the coefficients in the direction of making low-level radiation more effective than it probably is. It's complicated. I just went over this recently because I'm on a National Academy committee on the effects of nuclear weapons, the "bunker busters" or "agent defeat" weapons, if they were used, and what would be the local hazards. And so we had a briefing on what's known about the effects of low-level radiation, and it's still based really on some couple hundred excess cancer deaths from Hiroshima and Nagasaki. You know, 150,000 people, more or less, don't hold me to it, were killed in those explosions, but mostly they were killed by fire or blast or prompt radiation. And if you barely survive prompt radiation, it's so lethal—LD-50 is like 400 Rem. That's enough to raise your temperature only by a hundredth of a degree or so. It's not a thermal effect. But if you barely survive, then—I had this 2,500 Rem incidence of cancer, and so 400 over 2,500 is one-sixth. So your likelihood of dying from cancer will be the normal 20 percent plus another 16 percent or so. So people who have received by far the largest dose possible all at **[00:50:00]** once and survived have a double risk of cancer. There weren't very many of them. Mostly those people got excess doses in the range of a few Rem. And so there are a very small number of people who died from cancer in those populations above the number normally expected. And much of what we know and fear about low-level radiation comes from that. And the people who argue that low-level radiation does nothing say, `Well, how can you compare doses delivered over a long time with this very sharp dose of radiation?` And the answer is that this is a sharp dose but it's tiny. The likelihood of more than one injury to a cell is extremely small, even in the Hiroshima and Nagasaki exposure, and this is already in the linear range. And besides, we say in our books we have these other exposures to radiation where you have children with scoliosis—curvature of the spine—who have been X-rayed many times and they accumulate a lot of modest damage in this way.

So our best judgment is that this linear effect there, that the superlinear effect—Gofman is not a superlinearist, but there are other people who are. Some of them, and I think it's mentioned in our book, some of them in Europe, and they say that the smaller the dose, the more effective it is somehow, and we say that's absolutely wrong because this radiation is exactly the same kind as the background radiation to which we are all exposed. And whatever the effect is, whatever the variability among people, it's just shifting that threshold a little bit in there. And so there's *no* other possibility than for it to be linear. It cannot be superlinear in the low-dose range. But Gofman's work, we say in our book, deserves a lot more respect than it has gotten. People really have to answer his particular arguments. Oh, one of the problems is that the incidence of breast cancer in Japan is much lower than the incidence in the United States, and so there's a question whether induced cancer rates are additive or multiplicative. And it's very complicated. It's hard to think your way through, and I think Gofman overestimates, but his work really should be paid attention to. I think he's quite old now and I don't know that he's doing any of this himself. There may be a school of Gofman that's carrying on, and I have no idea about their competence or integrity.

Right. When I did an Internet search, there's his stuff and then, you're right, there are comments on other websites that publicize it, so I'm not sure what the state of the situation is. But there is a lot of his material up on the Internet, as you probably know. Well, this is helpful. Prompt *radiation is the radiation that comes with the explosion, as opposed to fallout kinds of things, is that the difference? I need to understand.*

Yes. Prompt radiation comes from all of the gamma rays accompanying fission, and that's about as much as—from all of the fission product decay chains afterwards. Now, there's some transition because even the prompt radiation, some of it comes out milliseconds later because of these neutrons that are being captured in nitrogen, and some of it comes out beginning seconds later from the short-lived fission products, as the cloud is moving up. But the prompt radiation is that from the neutrons and gamma rays from fission. And the fallout, of course, is local fallout, especially if the bomb is at a height, either on the ground or—so that the fireball touches the ground, and then a *lot* of material gets scooped up and it's big and heavy and falls out locally. But if you have a bomb which, I guess, for Hiroshima or Nagasaki, the fallout-free height would've been, I think, four hundred feet, and they were at fifteen hundred feet. So there was no local fallout. There can be some if it rains, but there was no local fallout in one case. In the other **[00:55:00]** case, it did rain. But under those circumstances, particles, fallout, under the track of the cloud as it is carried by the wind, and then it spreads, and mostly the deposition takes place over a period of months and years worldwide. I think about 90 percent in the same hemisphere as the test was made and 10 percent crosses over to the other hemisphere.

Yes, one of the fellows I've been talking to was a weather person during the war and then goes on to do weather things at the test site for that very reason, to be able to predict when and how the clouds travel. It's interesting. And complex.

Yes, there are codes. Now everything is done by computer.

Yes, but this was in the old days, in the fifties.

Los Alamos, of course, and Livermore had their own weather people in order to be able to predict the course of the cloud and the fallout, and to decide whether to shoot or not to shoot, depending whether the populated areas were downwind.

Right, right. This guy was with the Weather Service and then I think the AEC hires him in the late fifties. He coordinates—so that's an interesting and another complicated issue. Well, this is very helpful. I'm just curious—this is a little bit off the subject, but not really. You spoke before, when you designed the experiment for Teller and Ulam, and later when this whole thing came out about whether you were actually the discoverer of whatever the secret was for the H-bomb, at the time, are you thinking in those terms, or are you just thinking in terms of putting together something that Teller has asked you to do?

Oh, well, what I said to [*New York Times* science writer] Bill [William] Broad, and I sent him some letters. Before the article was published ["Who Built the H-Bomb? Debate Revives," *New York Times*, April 24, 2001], he was kind enough to send me a draft. And I said, Well, you know, first of all, I don't like this personalized. He had told me early on that he wouldn't be able to publish anything unless it were personalized because that's the way the newspaper business is. And so, OK, so I talked to him. And then I wrote him and I said, Well, you know, you've got this mostly right but you got *this* wrong. Where I say that the Los Alamos folks were "burned out from previous testing and so I had to do it myself," that isn't what I said. I said that the cryogenics folks, the people who dealt with liquid hydrogen at Los Alamos, were burnt out from prior testing, from the Greenhouse experiment. And that's true, because I had gone over *to* the portion of Los Alamos where they did the cryogenics work and I talked with the leader of the group, Ed Hammel. I was in those days a person, a physicist, who actually used liquid hydrogen in building my particle physics targets at the University of Chicago, and of course later I became a low-temperature physicist myself after leaving Chicago in December of ’52. But Ed Hammel told me that they really had to get back to their physics, that is, their academic physics, because they had taken a couple of years off to do this work. And so he *really* refused to look at this problem on the schedule that I needed it looked at.

And so that’s what I had told Bill Broad. But, in fact, he did not make the change. I had this communication with him, which I will send you, and, of course, then after the Broad article was published, I wrote the people at Los Alamos. There have been so many generations of change at Los Alamos and at Livermore that almost nobody knew of my involvement in this work. And that’s in part because I was there in the summer and then I would go away. And so I had this report, a five-page report, July, end **[01:00:00]** of July 1951, including a big sketch. I gave a secret about it at the fortieth anniversary of Los Alamos, and particularly at the fiftieth anniversary in 1993, and Edward Teller was there and Marshall Rosenbluth and others. So I showed the sketch and I explained what I had done. But what I did, really, was to take the ideas that were current at that time, in June of 1951, and sort through them and put them together. There are a lot of choices to be made and, you know, you can go on a vacation to one place or another place or a third place, and you can fly or you can drive, but you got to make a decision as to what you’re going to do.

And so I said, `Here’s what it would look like if you made reasonable decisions, and if you make those decisions, then here’s how you can actually do it.` Because I knew all of—well, we needed to get high densities of hydrogen, of deuterium, in order to burn the deuterium fuel. And people were aware that just as the burning of deuterium-tritium, that the neutrons could be used to cause fission in uranium, which was an earlier idea, it would be helpful in getting a large yield if you had uranium in proximity to the deuterium. So people were talking about that. And then you had the question of shapes, and the whole idea was to use the energy from a primary bomb to prepare a secondary bomb, that is, to compress it so that things would burn faster and help overcome the big energy loss. So that's what I put together, and we needed liquid hydrogen to keep the thing cool because we needed liquid deuterium for the fuel in order to have sufficient density so that we would have a good reaction rate. And you could get that by very, very high pressure, but then the whole thing would be dominated by the thickness of the pressure vessel and would be very dangerous. So liquid hydrogen was the way to go.

Now, I well knew that there were other fuels possible and, in fact, as late as January in 1952, the option was still held open whether this thing should be filled with liquid deuterium or with a chemical compound, lithium and deuterium, lithium deuteride, which is a white solid material that they use now, have used ever since 1954, in the normal hydrogen bombs. But I was most comfortable—we hadn't done the calculation on the lithium deuteride, so I was most comfortable with the pure liquid deuterium, and since I could design the thing, I did.

There was a person from the National Bureau of Standards, Ferdinand Brickwedde, who was in on some of these things and he told me later that when the thing had been built, the heat leak, which is what you do to try to minimize the heat leak and the boil-off of the hydrogen, had been just what I predicted, maybe even a little less. But *his* job was to oversee the construction of the plants in Colorado for liquefying hydrogen and deuterium at this unheard-of rate. And that's complicated, too, because when you make liquid hydrogen, initially it boils away much more rapidly than later on. It's not like liquid air or liquid helium. And that's because of a peculiarity of the hydrogen nuclei, is their spin one-half, and three-quarters of the hydrogens produced have their spins parallel. The two atoms have their nuclear spins parallel, and one quarter have their spins in the opposite direction. But the ones that have their spins—let's see—and the energy difference, the interaction of one spin with the other **[01:05:00]** is tiny, tiny, is really negligible. However, there's a lot of heat associated with the conversion of one of these to the other because the physical law of symmetry means that if you have a system with the spins parallel, that it can—let's see, ortho-para conversion—well, one of these can occupy the lowest rotational state of the molecule and the other cannot, and since hydrogen is so light, these rotational states differ a lot in energy. And so as the spins can flip, a lot of heat is produced that boils off hydrogen. So one of the things that you need to do in a large-scale liquefaction plant for hydrogen that you're going to keep around for a long time, is to catalyze this ortho-para conversion in order that your hydrogen have better keeping quality.

So there are all kinds of details that are involved in such a program. And after I had designed that, then never a slouch, I felt that it would be good to design liquid deuterium bombs themselves that could be carried. This first design had a very thick radiation case and the whole thing weighed about eighty tons, as I recall. And the radiation case was clearly more than twice as thick as it needed to be, clearly, clearly, and I had one of my few technical arguments with Hans Bethe, who was head of the theoretical megaton group. But in this one case, he wouldn't listen to reason. So when I designed the flyable versions, I had a thinner radiation case, and I was astonished later to learn, when Herb York sent me the draft of his book, *The Advisors: Oppenheimer, Teller, and the Superbomb* [1976], that the AEC had actually built five or six of these liquid hydrogen deployable bombs, and they had mounted them on airplanes, ready to go. They were called Jughead. It was only when, in 1954, we brought out the dry bombs, the lithium deuteride bombs, that the Jugheads were retired.

And so you didn't know until York's book. Amazing.

If I had been at the laboratory continuously, the people might've talked to me or whatever.

Now, two quick questions about that. You're working at IBM [International Business Machines] and you get loaned to Los Alamos, is that right?

Oh, well, no. That's important. In 1952, I decided that I did not like the sociology of high energy physics where—

Oh, you were at Chicago.

I was at Chicago until December of '52.

The sociology of high energy—?

Yes. I didn't *want* to have to tell people six weeks ahead of time what I was going to *do* with the cyclotron, and work with six other people. And so I decided I would do something where I could work by myself. I looked at various fields of physics and decided that low-temperature physics, the physics of liquid and solid helium and helium-3 and superconductors had not much advanced since the end of the war in 1945, and so I would work in that field. Then there was no reason to stay at Chicago, I felt. It was a bad place to raise a family, on the south side of Chicago. And although there were low-temperature physics facilities under the west stands that Earl Long operated, I figured other places could provide such facilities.

IBM was starting up a solid-state physics laboratory at Columbia University. Fermi's colleague, Emilio Segrè, had been asked to visit and consider leading that laboratory. Its predecessor was the IBM Watson Scientific Computing Laboratory that was headed by Wallace J. Eckert, an astronomer at Columbia who introduced the punch card to scientific computing in **[01:10:00]** the 1930s; and then was at the Naval Observatory during the war, where he was responsible for computing the nautical almanac that airmen and ship-borne navigators used to navigate, with their sextants and chronographs and whatnot.

Eckert was a very capable person, and his own research was the orbit of the moon, and calculating to a very high precision the orbit of the moon so as to fit all of the observations. It's an extremely complicated problem because the moon is influenced by all of the other planets, by the obliqueness of the Earth, by the eccentricity of the Earth's orbit, and to be able to calculate those things for hundreds of years to the precision that can be obtained by occultation of stars, so you're looking with a little telescope at the moon and in a hundredth of a second, a star goes behind the line of the moon. So that was his life's work.

He was head of this laboratory that was the source of computing knowledge at Columbia University and provided some computing capabilities. They had hired people after the war from the MIT [Massachusetts Institute of Technology] Radiation Laboratory to build electronic computers, but now Eckert was told by his advisors, I.I. Rabi and Polycarp Kusch at Columbia University, that the coming thing was solid-state physics. Segrè was a particle physicist. But anyhow, he was on leave at the University of Illinois that year, and I went down to see him because he had not yet decided whether he would or would not take this job. In the end, he didn't. But it was very attractive to me, not to head the laboratory but to go to work there. So I did. In my discussions with IBM, I stipulated that they would write into my employment contract that I would have 30 percent of my time free to work with the government on national security matters, and I just knew that IBM lawyers would—there would be dozens of IBM lawyers frantic all the time about what I was doing—but that I wouldn't tell them what I was doing, so the lawyers couldn't do anything about it. And that worked very well for forty years. Of course, they had to be satisfied with the benefits they were getting from the two-thirds of the time that I was spending with them, or else I wouldn't have persisted at IBM.

[01:12:51] End Track 2, Disc 1.

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

UNLV Nevada Test Site Oral History Project, interview with Richard L. Garwin, disc number two, July 21, 2004, conducted by Mary Palevsky in La Jolla, California.

[00:00:14] End Track 1, Disc 2.

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

Well, so I worked on nuclear weapons in the summers of ’50 to ’52, and then in ’53 I was working at IBM, nominally on physics and applications physics, but one of the first things that happened was that the head of the company, Tom Watson Jr., I guess he was the coming head of the company, had been asked by Jerome Wiesner at MIT to provide one of their “leading engineers or physicists” to work on a winter study, that is, a year-long winter study, Project LAMP LIGHT. This was to extend the air defense of the United States and Canada to the sea lines of approach of Soviet bombers. They had already had designed and partially built the so-called semiautomatic ground environment, SAGE, system, early computers and communication links that would command interceptor aircraft and surface-to-air missiles to destroy enemy bombers, presumably carrying nuclear weapons. But there wasn’t anything to keep them from coming in from the sea, and so the question was, how could you extend the radar detection capability and the interceptors so as to catch the bombers before they could come in from the sea?

I had just gone to IBM, and that isn’t why I went there, so I negotiated that I would participate half-time, which I did for about a year, going probably for Tuesday, Wednesday, Thursday to Boston and working with these folks. It was interesting and I met a lot of people. And that’s probably how I was introduced to the Washington activity, because Wiesner and his co-head for this study, the LAMP LIGHT study, Jerrold Zacharias, who was a physicist at MIT.

They were members of the President's Science Advisory Committee [PSAC], which was in the Office of Defense Mobilization in those days. It wasn't moved to the White House until 1955 or 1956. So they were folks who introduced me to these other activities in Washington. And as part of the Lamp Light project, we had briefings on the Soviet threat, that is, how many hydrogen bombs they had or might have, because we didn't have any hydrogen bombs yet, and what these nuclear weapons would *do* if delivered to the United States. And I had been involved in *building* bombs, designing bombs, and had really only a modest knowledge of what they would do. It was clear that enormous damage would be caused to society, and that people were talking about much larger numbers of nuclear weapons than seemed reasonable. So I became involved in understanding that, and eventually in deciding that there wasn't a good mechanism for *limiting* the number of U.S. nuclear weapons, much less limiting the *Soviet* nuclear weapons, and that there *should* be. It turned out, of course, that there were people, including Jerome Wiesner, who was a passionate arms controller and a very energetic person who eventually would play a leading role in discussions with the Soviet scientists on controlling nuclear weapons. So these folks, through their contacts in Washington with President Eisenhower, who was a very sensible, intelligent person, and other folks in the Cambridge crowd, as I would call them, included Edwin Land, the head of the Polaroid Corporation and the inventor of instant photography and polarizing film. And he was head of an intelligence panel for a study in 1955 or 1956. There was the Gaither Report and there was the Technological **[00:05:00]** Capabilities Panel, so I guess he was the Technological Capabilities Panel. And other folks who worked with him on this intelligence-related activity were John Tukey, a statistician of Bell Labs and Princeton, and Edward Purcell, a fine physicist from Harvard University who received the Nobel Prize for his work in magnetic resonance, and a few others. I was later to work very closely with Land on his intelligence activities for the President’s Science Advisory Committee beginning around 1959 or so.

I don’t know how much you can say about this, but intelligence activities, this has to do with what, analyzing data that you’re getting from the technical point of view?

Well, Land and Purcell and company in the early 1950s looked at the information available to the U.S. government, and they decided that we could a lot better with aerial photography than we were doing. I had been in Korea for a month in 1951 and I saw the aerial photography apparatus that we had there, and it was all designed so that you would have contact prints that you would send out to the troops in the field, so it was big-format film at about eight lines per millimeter, resolution which is what the human eye can see reasonably well. But Land and Purcell did some figuring and they said, Well, if I take a 35 mm camera and I have a very good lens and I get Kodak to make me very fine film, I can work at 400 lines per millimeter, and that’s fifty times better resolution than the Air Force cameras, and 1/2500th the area of film required for the same picture, and that means I could carry an *enormous* load of film in my airplane. So that was the origin of the U-2 aircraft. That came from that little panel, and they persuaded President Eisenhower to have the CIA [Central Intelligence Agency] develop it, rather than the Air Force. This was a covert program run by Richard Bissell at the CIA, later to come a cropper with the Bay of Pigs, and development by Lockheed, and I think the whole thing took sixteen months, and the U-2 began to fly in 1956. And it was supposed to be above the Soviet interceptor coverage. It was to fly over the Soviet Union [USSR] and take pictures. Now, it turned out that even though arguments had been made that the Soviets wouldn’t see it on radar, they did track it from the very first flight and they repeatedly tried to shoot it down, but they didn’t succeed until May, I guess, 1960 when [Francis] Gary Powers was shot down near Sverdlovsk.

But Land and Purcell and their panel had also persuaded Eisenhower that he ought to create a satellite system, a film-return system, that would take pictures, again very high-resolution pictures. It was called the Corona Project, and it first flew in August of 1960, just after Gary Powers was shot down, and it provided enormous amounts of information and photography.

So that's what the Land panel was, primarily. It was all optical imagery, and it advised and contributed to the development of the A-11, A-12, SR-71 aircraft; the MACH-3 supersonic titanium aircraft, also built by Lockheed; and to many of the satellite systems that we have for taking pictures, now no longer returning film to Earth but sending the signals back from there electro-optical system, like a digital camera. So it was that kind of intelligence, not the analysis side of intelligence.

So anyhow, I became aware of those folks, and they had much broader views on such **[00:10:00]** things than I did as, you know, a simple developer and tester of nuclear weapons. So I became interested and decided—and they felt they had a lot of self-confidence. After all, they talked to presidents and maybe to Cabots and Lodges and God. And so feeling that you could make a difference, it made some sense to develop some views on these things, and certainly sensible about nuclear weapons. And in 1962 I worked with Spurgeon Keeny and Jerry Wiesner, when Jerry Wiesner was science advisor to President Kennedy, to introduce permissive action links on all seven thousand of the nuclear weapons we had deployed to Europe. Up to that time, anybody who had control of the nuclear weapon could explode it, so a battlefield commander, a corporal, or whatever could decide that it was time for his weapon to go off, and maybe he would have orders, maybe he wouldn't. But this was a way to lock up each nuclear weapon or to lock up its firing mechanism with an electromechanical combination lock so the weapon could not be fired without not just authorization but also the combination, the number, that would come from higher command. And that was pretty audacious. They did it in, I think, in seven months or so, and it cost about twenty thousand dollars per weapon, which doesn't sound like very much, but seven thousand times twenty thousand is a $140 million, which was a lot of money in those days.

So anyhow, so that's the sort of thing we did. But by 1958, there was a technical committee conference under UN [United Nations] auspices on the test ban, so these were technical issues of a nuclear test ban, or something like that. I was not a delegate, but I was a delegate in Geneva for the U.S. government at that time to the ten-nation conference on surprise attack. There were five Western nations—there was Britain, the United States, France, Italy, and Germany. And this, of course, was 1958. And then there were five Eastern Bloc countries: the Soviet Union, the Soviet Union, the Soviet Union, and so on. In our six-week conference—which we had had some preparation in Washington, of course, of our delegation—we agreed on the *name* for the conference. We never did agree on the agenda. But the test ban folks really went farther than that and had good technical discussions.

So this was the fall of '58 through January or so of '59. And our delegations were in the same quarters and so, since I knew all the people on the test ban delegation and I didn't have anything much to do on mine, I spent much of my time with them, looking at verification means for detecting tests, finding errors in the principal reference that people used, and trying to help in general. In fact, I had done some calculations. I hijacked the computer at CERN, at the nuclear research establishment there, in order to do some calculations on the response of seismometers, because it was a big debate between the United States and the Soviet Union on what kind of seismometer would be best, and so I showed the response of our seismometer, their seismometer, at long distances to big underground tests.

I met *other* people there. Dick Latter and Albert Latter, they had invented the big hole scheme for decoupling underground explosions. Hans Bethe had said initially it wouldn't work, but then he looked at it more closely and it does work, but it has to be a very big hole, which plagues us even to this day. But yes, [Stanford physicist, Wolfgang] Panofsky was a leading member of that delegation. And I was a kibitzer.

After that, of course, I was a consultant to the President's Science Advisory Committee and a member of its Strategic Military Panel. And then when President Kennedy was **[00:15:00]** elected in 1960 and took office in '61, I was a member of PSAC, not from the beginning but I think '62 to '65, and then again under Nixon, 1969 to 1972. And PSAC was very much occupied with trying to get a comprehensive test ban treaty [CTBT]. In 1963, of course, we had the limited test ban [LTBT], I remember because we discussed it a good deal and then we had a special meeting of PSAC to decide whether to endorse the president's signing the LTBT, because Lois and I were in France and had to come back early. My way was paid, but I suffered financially for having to pay the difference in ticket price to bring her back early.

At this point in time, what's the logic of the test ban? That it will slow the development or—?

At that time? Oh, no. At that time, yes, you had—in 1960—'58, '60, '62, people, of course, could build the weapons that they had already tested, but they could make smaller weapons by the knowledge gained testing. And in those days it was thought about but nobody had built the multiple independently targeted reentry vehicles, the MIRVs, and this would augment the destructive capability of Soviet forces. Worse, it would provide them more warheads so that our missiles would be more vulnerable, because if you have only a single warhead per silo, then neither side can shoot at the other side's silos and count on destroying them all, if they have the same number of silos, more or less, without disarming themselves, and they wouldn't do that. So it would add to stability if you could not have more than one warhead per missile. So that was one aspect of it.

The other aspect was that yes, you could make more and more destructive weapons by testing, and you have a lot of infrastructure and political support when you're spending money on testing. A lot of people are involved and they have an incentive to support the program, so if you could break that link, it would reduce the emphasis on nuclear weapons and you might be able then to reduce the threat and to get some agreements to limit their numbers and retire them.

So it's a grand vision, and part of it was, then, in the 1960s, we were looking at the role of missile defense. Missile defense, of course, if you had a perfect defense, then you would no longer fear retribution, and so deterrence would be inoperable, if you had a good air defense, as well. And if missiles could not access their target, truth be told, there are other ways of threatening societies, if that's what you wanted to do. But it would be simplest to recognize the vulnerability of society, to recognize that all proposals for missile defense that we have seen, and we were the first people who would see them because we were on the President's Science Advisory Committee Strategic Military Panel. So we met two days every month, and every year we would go over the current proposal of the Army to deploy a missile defense and have to discuss with the Army why it wouldn't work, because the radars were vulnerable or didn't have enough traffic-handling capability or whatever. So it was part of this whole effort to restrain the nuclear threat to the United States, recognizing that we would need to restrain our *own* nuclear capabilities as well. So that was the purpose, then. It was an arms control purpose.

Then there were some other purposes. There was the environmental protection, that is, **[00:20:00]** the fallout was really a problem. It was a public relations problem as well as a health problem. And there was a proliferation impact, as well. So when *you* test, you make the ground shake, it's reported in the newspapers, and so on. Other countries' leaders ask, `Well, should we have nuclear weapons tests, as well? Look at all the publicity these people are getting. Free advertising.` And so we thought that that would be helpful, as well.

Now, the purpose, back with the Limited Test Ban Treaty, we would have had a comprehensive test ban treaty, probably, except for this big-hole decoupling approach. It was blown out of proportion.

Explain that to me a little bit.

Well, when you explode an underground explosive, then it makes a cavity, and the motion of the cavity walls gives rise to a strong ground motion in the vicinity, but it goes on to a distance as waves. And that's the same thing with sound waves. You have a puff and something happens locally, but then at a certain distance, that gets converted into oscillatory behavior. If, instead of having an explosive in contact with the ground that makes this cavity that persists forever because of the strength of the ground, you had an initial cavity in strong rock, so that it was not inelastically distorted—it didn't grow but it would just be banged on and expanded a little bit—then the signal that you get is less by about a factor of a hundred, I guess—seventy or so—at great distances. And so an insignificant test like one kiloton tamped, that is, in good contact, could not be distinguished from a test of *fifty* kilotons, which is much more militarily significant, in a big hole. But the hole has to be really quite large. And, let's see, I don't have that number in my head right now. I think it's twenty-five meters in radius, fifty meters in diameter, so that would be about fifty thousand tons of earth per kiloton, and so for a fifty kiloton, you'd have to remove about two-and-a-half million tons of earth. Now, that's not impossible, but you have to put it someplace. But these are very clever people, Dick Latter and Al Latter, and they are public-spirited as well, and I'm convinced that they were just using their minds to ask whether there was anything wrong with this, and they were enthusiastic when they found something because, just like Edward Teller, they're happy to have an accomplishment. But I wasn't happy. I am happy it's found, if it's possible, but rather it would be impossible.

Now, how do you *hide* such a thing? Well, you could mine it in salt, and with salt, you could use solution mining with water, so you'd have a nozzle down there and it gradually eats away at the salt in the cavity that's filled with water. The fresh water is pumped in from an aquifer and the salty water is pumped out, so you could have an almost entirely underground activity that was making one of these things. So the question was, where are all the bedded salt deposits in the world, and can they be monitored, and how will people have access *to* them, and like that.

But we couldn't settle that in time for a CTBT in 1963, and settled on a *limited* test ban treaty which had, unfortunately, *no* limitation on the yield of underground shots, even though it could've been possible to detect, and if there were controversy, one could have required inspection and validations. It took until 1972, with a lot of experience in underground shots, to have the Threshold Test Ban Treaty that limited underground explosions to 150 kilotons, **[00:25:00]** despite the fact that we might not be able to detect—despite the fact that a bigger explosion could, in principle, be decoupled by it. [TTBT signed by President Nixon in 1974, ratified by U.S. Senate 1990] But by then we weren't worried about it too much. However, this is a live question because we signed the Comprehensive Test Ban Treaty at the initiative of the United States in 1996, and France signed it, too. And President Clinton just didn't have the political power to get it ratified by the Senate. Didn't present it really until 1997, and it wasn't brought up for a vote until 1999, and then under the most unfavorable circumstances, where the opponents to the treaty had organized everything really quite secretly and wanted to have an immediate vote. In fact, the vote was put off for three days, and nineteen hours of debate, or one hour of debate repeated nineteen times.

But now, the purpose would be not arms control but non-proliferation. And not non-proliferation because keeping people from testing is the most important thing in the world, *but* because almost all these states are members of the NPT, the Non-Proliferation Treaty. And to have nuclear explosions going on from the nuclear weapons states while telling other people that they can't work on nuclear weapons at all is really sticking your finger in their eye unnecessarily. We've had a moratorium since 1992, and the Russians also. The only tests that have taken place after Chinese tests in 1995 have been the Indian and the Pakistani tests in 1998. And I don't think *those* would've taken place had we had a Comprehensive Test Ban Treaty. There was a big problem in the negotiation in the Committee on Disarmament [CD] in 1995 *because* of India. India did not want to be singularized, and so, let's see what it said, the treaty was written so that it would not enter into force until *all* states with reactors or uranium enrichment plants had ratified the treaty. And India protested about that, would not agree, and the CD operates by consensus, so the people went *around* the CD and the governments introduced the treaty directly into the General Assembly, annoying India.

CD is…?

The Committee on Disarmament, which is a UN organization, although they claim they're nominally separate from the UN, but they're funded by the UN. They maintain they are the *sole* negotiating forum for international agreements. And they are a big bottleneck because their rules are consensus and they negotiate on one thing at a time, only. So we should *better* have had Comprehensive Test Ban Treaty with a *normal* entry into force when one-third of the nations of the world had ratified it or something like that, and then we could've put much more pressure on

India. So it went astray, in my opinion, and, well, also, there was a problem of Mr. Clinton himself, who had so much difficulty with the Republicans and his self-inflicted damages and like that. So it's a bad show, in my opinion.

We had a National Academy committee in 2001, I guess, published in 2002, on technical issues relating to a Comprehensive Test Ban Treaty. President Clinton had appointed General [John M.D.] Shalikashvili as his special coordinator for the Comprehensive Test Ban Treaty. And Shalikashvili had several studies, one of which was contracted to the National Academy. So I was an author of that, looking at the arguments that had been made in the Senate debate. [*Technical Issues Related to the Comprehensive Nuclear Test Ban Treaty*, National Academy Press, 2002].

So your analysis of the Senate debate on the technical issues is that, what, the treaty—?

Yes, there were three chapters in our report, and one was: Can you preserve the U.S. nuclear weapons indefinitely without nuclear testing? And the answer is: Yes, you can, if you have the option of remanufacturing them. That is, melting down the plutonium, making new plutonium stuff, and so on. And that means you need to resist the **[00:30:00]** pressure to make little changes simply because it would lead to easier manufacturability. We have a number of people, the former director of Los Alamos weapons lab, Harold Agnew, of Sandia laboratory, and of Oak Ridge on our panel, and also a nuclear weapons primary designer, Seymour Sack from Livermore. And Sack was one [who asked]: Can you maintain the U.S. nuclear stockpile? And that was, I think, the most important part of the Senate debate. Then there were two others: Will you be able to detect tests that others are trying to hide? So we went over the detection capability for covert explosions in space and the atmosphere and the oceans and underground. And then finally, since there *is* a level of nuclear explosion activity underground that you *can't* detect—I mean I can have just one *pound* of nuclear explosive, not a pound of fission but a pound of high-explosive equivalent yield, and I could explode that in a steel sphere on shock mounts, and there's no possibility of detecting that seismically. So the question was: What would be the military significance of the explosions that would go undetected? And so we looked at that and we judged that there would be no military significance to a country that was capable of hiding these little explosions. And countries that *could* benefit from a little bit of nuclear explosion testing but didn't have the experience would *not* be capable of hiding it. So anyhow, the report's available on the Web.

So that's where we are. And, of course, Nevada is being held in readiness, and some people who don't like the Comprehensive Test Ban Treaty keep beating the drums for instead of eighteen months, we ought to have twelve months of readiness, and by the way, we ought to have something ready to test, and so on. And I've been involved, I mentioned, in an Academy committee on the bunker busters or whatever, looking at some of these arguments. And there are some people who really want these capabilities, and they are quite sincere that they believe they can and hope they can do it without testing, and there are others who really want to get rid of all of these constraints on U.S. activities. And so they *want* to deploy space weapons while this administration is in power, they *want* to have nuclear tests just so we'll put behind us all of these international agreements that constrain us. I think that's a very bad attitude.

I was reading your book with interest. I've been really too busy and I only got up to the second interview, with Teller, when you and your husband were there. Yes, well, Mici, [the late Mrs. Teller] of course, at that time was very ill. I hope I can finish it sometime soon.

Well, yes, I mean I would love to hear what you think. So we're close to 9:30. We should stop.

OK. You have anything else on your list?

Well, the other thing on my list, it may be sort of a long question but I'll pose it and then maybe we can talk about it later. In your Drell Lecture you talk about the possibility of some sort of nuclear attack being likely—[Stanford's Center for International Security and Cooperation *[*CISACI*]* Drell Lecture series. Richard L. Garwin, "United States Nuclear Weapons and Nuclear Explosion Testing," March 9, 2004*]*

Terrorist attack.

Terrorist attack. And you seem to imply—I should get it out, but the implication was that it may be necessary so the world is reminded how terrible these things are. When I read that—this is a comment as well as a question—it was reminiscent of something that actually comes a little later in my book, when I'm talking to Hans Bethe a second time about the decision to use the bomb and he says, basically, you needed to see the center of the city destroyed, as in Hiroshima—his argument against the demonstration—because people need to know just how dangerous these weapons really are. There is no conception of it. It seemed to me that you were saying something similar, and it just struck **[00:35:00]** *me that how that was true then but sort of memory fades and generations pass and people don't now remember how terrible Hiroshima was. And so it seems that maybe these things, in order for us to understand them, Bethe's argument seemed to be, you* actually *have to experience it. You* can't *think about it theoretically.*

Well, we have the ability to have virtual reality now, that is, to show people the pictures, to remind them through plays and video. We have to remind most people. The Congress and the leaders react to what the people feel, but how is that going to happen? There's not a commercial interest in doing it. And so there really are the two things. One, *can* you know? And the other is, how do you bring it to people's attention? So I'm not saying it would be good, but I say that we ought to be ready to take advantage of such an attack and ask what we would be doing if we were able to engage people in thinking about these matters. And so it might have that effect. Countries are not going to use nuclear weapons to attack one another, at least countries that care about remaining in power or whatever, which is one of the reasons why you don't want to invoke regime change as a principle, because if you're going to bring down all these nasty people and kill them anyhow, they don't have anything to lose. They don't have very much to gain either, but they are not necessarily totally rational, any more than *our* leaders are. Resentment is a very big influence on human action. But terrorists, and especially terrorists who are not interested in popularizing their cause, but really ones who want to kill off people who are not of their type or who are too liberal or whatever for their religious views, and who want to destroy modern society, would have no inhibition about using a nuclear explosive. And so the question is, can they get their hands on it? Can they bring it here or elsewhere and explode it? And so that's something I'm very familiar with. And the answer is that there's just an enormous amount of nuclear material and it's guarded routinely. It's like hospitals. You go to a hospital and, you know, people are carrying out *their* daily functions, and the fact that it's *your* life is important to you and, in the abstract, to them, but they don't put the effort into it that would be warranted, you would think.

Yes.

[00:37:59] End Track 2, Disc 2.

[End of interview]