Nevada Test Site Oral History Project

University of Nevada, Las Vegas

Interview with Wendell D. Weart

April 18, 2006
Albuquerque, New Mexico

Interview Conducted By
Mary Palevsky

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

Mary Palevsky: *Wendell Weart, thank you very much for meeting with me this afternoon, and if we could start by having you just give me your full name?*

Wendell Weart: My name is Wendell Weart, because my signature name is W. D. Weart, my middle initial is D. I was born in Brandon, Iowa in 1932, September 24, 1932, and went to Cornell College in Iowa, not to be confused with Cornell University, and got my bachelor's degree in geology and mathematics at Cornell, and then went on to University of Wisconsin to get my Ph.D. in geophysics.

I suppose the reason I got interested in earth sciences is largely due to the fact my brother, who was ten years older than I am, was a geologist, and I accompanied him on summer field trips as he gathered information for his thesis. So I became quite intrigued with the world of earth sciences, and as I went on I felt like I'd like to apply a little more of the physics to the interpretation of geology, and that's why I went into geophysics.

I graduated from Cornell College in, let's see, 1953, entered the graduate school at University of Wisconsin, interrupted my graduate work to take a job at Aberdeen Proving Grounds in Maryland; worked there for about three years, and was just getting ready to return to the university to complete my thesis work when I got a letter from a gentleman at Sandia Laboratories, neither the gentleman nor the institution was I familiar with, asking me if I'd be interested in coming to work with them and they would help me finish up my thesis work and we'd go from there. So since the stipend they were offering sounded a lot better than the fellowship I would've got at the university, and I was married and had children by then, I said sure, let's look into it.

Now when you went to Aberdeen, let me just interrupt you, were you in the service then or were you—?

No, I was a civilian. I went out there to work on some earth science programs and air blast programs from large explosions, and interestingly enough, it was while I was at Aberdeen that I had my first exposure to the Nevada Test Site [NTS]. I went out there in 1958 to do some work on an underground tunnel test. That got me started on my work in the tunnels at Nevada.

Interesting. Which test was that?

It was the Tamalpais event. It was a relatively small event but it gave me my first opportunity to look at some of the atmospheric tests that were still being conducted at that time, although I never had any personal involvement in any of the atmospheric tests, only in the underground.

Well, I have to ask you then, what was your impression of seeing those atmospheric tests?

Well, you know, I guess little boys like fireworks and firecrackers, and this was the biggest set of fireworks you could ever hope to see. Really exciting, because in those days, it was a little more free form than it turned out to be later with all the additional safety requirements, so you could go out and observe at observer posts, and even some of the large events, like the Sedan cratering shot. I was up on top of Rainier Mesa when that was detonated, which was quite a bit below the valley floor where the shot was. The shot went off, you could see the ground rise up and then the fireball break through, and then the cloud go up, and you got to wondering, is that cloud coming my way, because it was so high above you. Of course, it didn't. But now, you know, I say "now," I haven't worked out there actively on the test program since about 1975, but in the later years it became much more safety conscious and that was the kind of thing that you'd never consider allowing people to do. No one went into the forward area when they were conducting a test there. So it was quite a different perspective than people later in the program got of these **[00:05:00]** testing operations.

But while I was out there with the Aberdeen Proving Grounds group, Ballistics Research Laboratories, I had an occasion to talk with a lot of the other people who were doing tests out there; found out later, in fact, that one of the people I had met and who offered to let us use some of his electrical cable was one of the people at Sandia in the group that I ended up work with. That gentleman was named Bill [William] Parret and he's passed away now. But it was interesting how my career started there, and then I continued on after I came to Sandia.

Thanks for that. So you got this offer for Sandia to what? They would support you in finishing your dissertation?

Yes, they did support me as I finished my dissertation, which had to do with seismic wave propagation. And the reason I went there, they were interested in having a geophysicist—and there weren't too many of our breed at the time, not too many graduate schools teaching geophysics and seismology—at about that time our country was engaged in discussions with the Russians about a test ban treaty, and in particular an atmospheric test ban, which meant all the tests would be underground. Well, if the tests are underground, it's the ground motion that's generated from those that's one of the primary mechanisms by which you can detect a test, and so we were naturally very interested in learning more about this field of ground motion, seismology, and so I was hired at Sandia to look into that.

Now what was Sandia like in those days? That's still in the first decade of its existence, I guess.

Well, yes, I joined Sandia in 1959, and Sandia was a little over a dozen years old by then. But Sandia was a very burgeoning laboratory then. We were rapidly hiring people because we were building up very rapidly, and that was an interesting period of time in a sense that a lot of things seemed to be going on simultaneously. Shortly after I arrived at Sandia, there was a moratorium on testing, and all testing ceased, but during that time work didn't stop. It allowed me a chance to do my thesis because otherwise I'm not sure I would've had the freedom to do that with all the other things going on.

But anyway I did complete my thesis, and one of my first big underground tests on which I was directly involved was Project Gnome, which was very soon after the Russians abrogated the moratorium. I was the scientific person responsible for the near field ground motion studies on Project Gnome, so I spent a lot of time down there on that event and on the subsequent reentry, again because of my geologic background, to look into that event and try and figure out exactly what went on underground, because it didn't behave as we had expected.

Oh really. So tell me a little more about that.

Well, let me go back to Rainier, which was perhaps the first of the underground contained events, and on that event the containment concept was to use a buttonhook, which would cause the tunnel to be slammed closed opposite the detonation before the shock could come around the buttonhook in the tunnel and squirt on down the drift. That was the same concept that was used on Gnome, but it didn't work there. It did vent. It got loose into the tunnel and eventually after, oh, three, four minutes, it circumvented the large plugs that were at the base of the shaft and started to ooze steam up the shaft and out into the atmosphere. And since it worked once but it didn't work this time, we wanted to see if we could understand what went wrong. And in fact, I think we did develop a reasonable explanation because the Gnome setting was in a stratified, a **[00:10:00]** bedded geology, and as the pressure in the cavity from the detonation caused the cavity to grow, the gases in the cavity opened up these bedding planes which were horizontal and allowed the gas to squirt right out into the tunnel. So even though there was a line-of-sight in the drift, it was not the line-of-sight that contributed to the venting; it was the geology that was responsible for the venting.

Now Gnome was in New Mexico, right?

Yes.

And what was the reason for it being there?

Well, it was a Plowshare event. It was the first of the big Plowshare tests and it had several purposes. One was to see if they could use the heat that was deposited in the salt, resulting in a molten pool of salt, extract that heat by pumping water down, turning it into steam, going up a pipe, and driving a turbine to make electricity. There were some other purposes. One was a Vela Uniform-type purpose, and that was to find out what kind of seismic waves and seismic signature would be generated by having a detonation in another kind of rock. This was in halite, which is just table salt, and all of our experience in Nevada was in alluvium or volcanic tuff.

So that was another purpose of Gnome. And there were still a couple of other scientific purposes. Los Alamos had what is called a neutron wheel at end of this long horizontal line-of-sight pipe. It had heavy metal foils on the rim and it spun at a very high rate of speed. And when the shot went off, the neutrons that were generated came down this vacuum pipe and they would hit the foils at different points as this rapidly-spinning wheel went around, and that allowed then these foils to be activated by the neutrons. People then would go back down quickly and recover these and measure the neutron cross-sections. And still another of the purposes was to see if the copious number of neutrons generated by this event could be used to create much higher atomic number elements, which they would promptly capture through a vertical line-of-sight pipe and get it to the surface and analyze.

Well, some of these experiments worked perfectly, some worked partially, and some didn't work at all. The fact that it vented into the drift complicated the recovery of the heavy metal foils, but they eventually did get in and recover some information on that. The Plowshare purpose to look at heat generation and steam power generation did not work because the roof of the cavity fell in and quenched this molten salt, and so there was no pool of molten salt to access when the water was pumped down. The seismic experiment worked well. The ground motion studies were successful, and in fact we found that the seismic coupling in salt was much more efficient, much more effective than from the rocks at the Nevada Test Site, so we learned that if you were to shoot a shot in salt, a much smaller shot could be detected than if it were shot in alluvium in Nevada. But then people got to thinking, and the theorists put on their thinking caps and they said, but what if we mined a big cavity in salt and put the shot in that? Called decoupling. And in fact there were shots to investigate that in salt domes down in Hattiesburg [Mississippi] and we determined in fact that yes, shots in cavities would decouple the amount of seismic energy that goes out to the far field.

So anyway, I sort of got beyond my story, which was that was my initial exposure to underground testing, the Gnome event.

Right. Because I did actually quite recently talk to a Holmes and Narver engineer who worked on the engineering of Gnome, so thank you for the detail because that fills in some pieces of that story. So that was the first event that you worked on, then.

Yes, and in the meantime, during the moratorium, one of the interesting things I got to do with **[00:15:00]** some of my fellows from Sandia was to go around the country looking for various rock types in which to detonate high explosive shots, because of this concern about how do different rock types couple energy. So I got to tour some beautiful scenery in the West and look at some exciting sites. But as it turned out, before those things ever took place, the moratorium was breached and we went back to testing and that took people's time. We did continue to wonder about the behavior in other types of rock, and Project Shoal on which I was the science advisor was in granite just east of Fallon, Nevada; that gave us a point on how seismic energies propagated out from a granite-type rock, salt from Gnome, and then the tuff and alluvium from Nevada.

Now I'm sure there are various reasons that you want to know these things, but am I understanding correctly that one of the primary reasons is for detection of Soviet tests, how things would behave in this rock, or just the science itself of how the explosions behaved?

Well, at the time, the interest was in how confident would we be in detecting Soviet shots if they were shot in different types of rock. Turns out that we had a fairly advantageous situation in Nevada because alluvium soaks up so much of the energy, not nearly as much gets out in seismic waves. But of course, if other countries looked for sites of a similar nature or decoupled their shots, they might also be able to hide their nuclear events more easily. But that was the original impetus. Later on, we had an interest of our own in this country as we started to shoot larger and larger shots, worrying about whether the energy that was propagated to distant sites like Las Vegas from the test site, would we cause more severe ground motion and therefore more severe building motions in different types of media? What was the best location and type of rock to shoot in? Not that we had a lot of choice in Nevada. We had alluvium, but mostly the alluvium, because of its thickness, could only accommodate smaller events, and for the bigger events you had to go deeper or further away, like Pahute Mesa, which is volcanic tuff. But we were quite concerned in being sure that we understood how the energy would couple into these distant seismic waves. That of course was one of the reasons we did the study looking for the Faultless site, to see if we could use a site further away from Las Vegas to shoot large-yield events. So we did the Faultless event in an area that we had selected that we thought would be advantageous, and measured the ground motion very thoroughly with seismometers, and then from that predicted what size event we could accommodate without causing some concern for high-rise buildings in Las Vegas. Well, as it turns out, they decided that for events of a megaton and more, probably should get out of Nevada completely, and that's why they ended up in Amchitka [Alaska] for those two really large shots we did.

Right, but there were megaton shots in Nevada, no?

There was one or two. I think Boxcar was one. But we found that from those events we were getting a lot of complaints and a lot of architectural damage to structures in Las Vegas. As time went on and people became more aware of this, we began to get more and more complaints, and finally I think the AEC [Atomic Energy Commission] or Department of Energy [DOE] or whatever you want to call them became concerned that they couldn't even continue 1 megaton-size events in Nevada without getting undue complaints. But there were one or two, I think. Benham was one. But there weren't too many of that size, and when we started to go above a megaton, **[00:20:00]** then we went to Amchitka.

Now I want to talk to you about that, but before, a little back on the timeline, just because I was reading your thing [profile and interview exerpts] from Caging the Dragon*. Can you talk to me a little bit about Marshmallow and the work you did on that?*

Yes. Marshmallow was a shot that had been detonated in a tunnel with a horizontal line-of-sight, one of the first of its kind, and it almost contained, very little seepage of radioactivity, so it was regarded as a success, and it was different than the other shots like Rainier because there was no buttonhook, just a straight line-of-sight pipe in a tunnel. So again, my geologic background was useful to me and I headed up a program to look at the reentry of the Marshmallow event. The reentry occurred, oh, a year or two after, and the report written on it was a classified report. I don't know what's been classified and declassified nowadays, so I won't say anymore about it, but there was a report prepared on that.

I'll see if I can get some declassified version of it from the archive.

And that was what really directed our thinking in the early days about how to do the containment on underground line-of-sight tunnel shots. We did have some that were successful after that and some that weren't very successful. Most of the tunnel shots were DoD [Department of Defense] shots, but Sandia in the early days did a lot of the containment design on those. Sandia had two shots of their own which they were responsible for up in the mesa, Cypress and Camphor. Cypress was well contained, no releases. Camphor tried to push the envelope a little bit too much. It was a much more rapidly diverging line-of-sight pipe, the mechanical closures were closer. It did not work as well. Not a whole lot got out to the atmosphere, but a lot got out to where the experiments were. It made it very difficult to retrieve and recover experiments.

Now so I understand the organizational issues, you've got DoD doing these shots and then the labs, Los Alamos or [Lawrence] Livermore [National Laboratory, LLNL] usually are designing them, but in the case of Camphor and Cypress, Sandia scientists—

Well, the DoD shots were designed, they were done primarily for phenomenology, experimental results on structures or components you'd put in there. They were not weapons development. So the Department of Defense would say what they needed and what they were going to expose in the line-of-sight pipes, then the laboratory, one or the other laboratories, would provide the device, the nuclear component. And in the early days Sandia did a lot of the containment design, and then phased out of that and DoD used their own consultants and contractors, but because everyone was really involved in this endeavor to try and provide containment, all of the labs were sort of involved and we were a small community. Well, each lab may have had a dozen, two dozen people, and after Baneberry they got even bigger, but before there were a relatively small number of people who would all get together and trade ideas on how to do containment. Everyone thought they knew the best way, and when you got them all together later on, we decided that maybe none of us knew what it really was that was basically responsible for containment. But there were a lot of ideas that developed over time that seemed to make sense and seemed to work and so people would utilize those again and again and often had success but **[00:25:00]** occasionally still had failures even then.

I remember reading when I was doing the preparation for this interview a comment you made, I think in response to a [James E.] Carothers question [in Caging the Dragon*] and we can look at it if I don't get it right, the notion that if it worked you thought you knew why it worked and then you had something that should have apparently worked the same way and it didn't, and only by that happening did you realize maybe you didn't understand why it worked.*

That's right. We of course got to the point as our ability to do extensive calculations, do better theoretical evaluations improved, that we tried to use that ability to calculate how these things would behave. But of course Mother Nature being what it is, they sometimes fooled you.

As a layperson and an outsider, what is just so interesting to me is that you've got these—although it says in this book [DOE/NVOO-209] I know this book is not completely accurate—that Marshmallow was a relatively small shot, but that you've got these tremendous amounts of energy underground in non-uniform, geologic environment, that there must be so many variables to what would happen—

Well, there are, and we did come to a conclusion that the geologic setting can make quite a bit of difference, and in the later years it led people to avoid certain kinds of geology for certain types of shots. We learned that big shots are sometimes easier to contain than little shots because you use the energy to help you do containment, and so the more energy you have, providing you recognize the scaling effects, the more energy you have, perhaps the easier it is to contain.

Yes, that was something I really don't think I quite fully understand, this whole notion that the energy itself—is it the ground motion energy or is it not that actually helps seal things? Maybe you can tell me.

Yes. Now in talking about line-of-sight pipes, because you have this big open line-of-sight pipe pointing right at the device, and so you naturally would think that the energy of the bomb going right down that pipe would get down to the end and would just keep flowing down and defeat everything. What we found that we could do is first by using the radiant energy of the explosion itself, cause a very fast-acting seal right at the beginning of the line-of-sight pipe, what we call the front end sometimes; and we designed them with certain geometries and certain materials so that as the radiant energy from the bomb flowed around this, it would cause it to implode and squeeze closed. And then, if you could do that successfully at the very beginning, you know, microseconds, then the ground motion which is generated by the explosion starts out by being much faster. It's a hydrodynamic shock wave that travels in the ground and it's much faster than the seismic energy that travels off to great distances, and that ground motion would then get out and squeeze the pipe as it went along, squeezing it closed right in front of the energy that's trying to come down the pipe; what you have then is a race between this ground motion trying to squeeze the pipe closed and the energy trying to come down the pipe. And if you do things right, we think you can succeed and nothing gets out. If you don't do things right, maybe because of the type of geology you're in, that hydrodynamic shock doesn't develop to the extent that it would in another rock type and you lose the race and you don't seal off as well as you'd hoped. So that's why geology that Nature provides is important, but we also think that there are design things you can do to help you a lot with this problem.

And if you lose the race, then that radiant energy and the radiation can do bad things to your—

Well, yes, what happens then is as this cavity is forming, it has tremendously high gas pressures in it, and if you can't close that pipe off, these high gas pressures will flow down that open pipe **[00:30:00]** and it's just like a plunger, and it will take everything with it. And we've reentered tunnels, experiments, where a hundred, two hundred, three hundred feet of massive steel pipe will be all balled up way out here by the force of that flow. So it starts out as radiant energy but the thing that continues the push are the high-pressure gases in the cavity. So all the time, you're trying to get the odds in your favor in this race between the closure of the line-of-sight pipe, using the energy generated by the device itself, from the energy that's trying to flow directly down the pipe.

Right. But you're saying that initial ground motion energy is different than this distant seismic energy.

Yes, it's much faster. It travels much faster through the ground because the pressures are so high. It's just like a supersonic shock wave in air, when you get high enough pressures it travels faster, and then as it dies down to lower amplitudes, it travels at the speed of sound and in air. Well, in the earthen material, the speed in the various rock types, the seismic velocity, may range anywhere from a few thousand feet per second to twenty thousand feet per second. But this hydrodynamic shock very close in is many times faster than that, and that's what enables you to run ahead of this other energy and close off the pipe in front of it.

Interesting. Well then, the other issue you raised is this whole development of all these different closures along the way?

Yes.

Now is it that you were saying that Sandia was—?

Yes, Sandia, for the Department of Defense, was given the responsibility of developing these fast-acting closures, which are mechanical devices actuated either explosively or with very high gas pressure reservoirs, that would drive very massive doors across the line-of-sight. Those were put into the line-of-sight to close it off at distances of a few hundred feet, because even though they're very fast, if you put them too close, they weren't fast enough to beat this energy, and if you didn't beat it, they would never close. So that's what we did, is we tried to develop these mechanical closures. There were also other kinds of closures that were used from time to time, which used the energy of the shock wave itself by making the pipe heavier on one side than another, so that when it tried to close off, instead of doing it symmetrically, it would do it asymmetrically and maybe close it off better. Or one of the labs came up with a design to wrap a spiral band of metal around the pipe to help it close in a non-symmetric way. But these were sort of fine-tuning on the basic principle of using the ground shock to close things off.

But those doors obviously were able to—

In many events, when we reentered, I don't know in the later years if they reentered as many of these events but in the beginning we reentered almost every one, the ones that succeeded as well as the ones that failed, and we found that indeed these fast closures on the ones that succeeded were essential to stopping the fragments from coming down and ruining experiments. But we found on others, if they didn't close before the main energy got there, then they weren't successful and stuff just extruded right through them and on down the pipe.

Now when you say "reentry," did you physically go in on these reentry things or—?

Yes.

What was that like?

Well, it was uncomfortable, particularly for me wearing glasses because you had to go in wearing full face mask because of the radiation. But it was extremely interesting because it's hard to imagine and it's hard to really describe to people the visual evidence of the amount of energy that these nuclear devices going off releases. It can move large blocks of the mountain, **[00:35:00]** and some people feel that in fact containment success or failure may be due to block motion, where the blocks slide across the tunnel. In many cases we found that motion on preexisting faults just offset the drift, and closed it off by putting a new piece of the mountain in front of the fault. So it gave you a real respect for the energy in these devices and what's released.

That's interesting, and I want to ask you more about how that procedure went, because you spoke about having seen these atmospheric tests, and one of the things that people talk about impressing upon them is the huge amounts of energy, but somehow, for the layperson again, everything sort of disappears once it goes underground, but you're saying that you see evidence as a geologist of the—

Yes, while the above-ground tests that I saw—and I didn't see any of the real big ones, but the small ones I saw—they were visually impressive, but they didn't give me anything I could physically get my hands around, my mind around, to give me a firsthand impression of the energy. But when I would go back in a tunnel and see what happened to these massive structures that we had put in there, and to see how it manhandled them and just wadded them up into balls, that was impressive.

I've talked to some of the miners about reentry, so they would physically get ways for you all to get back in there and then—?

Well, usually what we would do is not go back down the preexisting tunnel because it was too radioactive. We'd usually mine a parallel drift, and then we would mine over from that into the other tunnel and look at it in spots, rather than go all the way down, for two reasons: It was too radioactive, and secondly, it was such a jumble of debris from the energy that had pushed all the stuff into a big blob.

So you'd be taking a look at these different perpendicular or whatever into it.

Yes.

How long would that process take? How long would you stay down there, would you say?

Well, because we were often trying to recover experiments, we would try to get back as soon as we could. Now, if everything worked perfectly, we'd be back within a matter of days. But if it vented, then we were going back primarily to see what happened, see if we could resolve the cause of the failure to contain, and that would take a much longer period of time. It would usually be a matter of months, maybe even years. But even then, it would be relatively radioactive, so we could not stay in these areas in which the cavity had extruded on down the drift for very long periods of time because of the radioactivity involved.

So hours or less than hours?

Well, it depends. Every one was different, and some you could stay for, you know, half an hour, something like that. You usually didn't want to stay much longer than that. But because every one was different, and every reentry was accompanied by health physicists with dosimeters, they always knew what sort of radiation exposure you were getting, and they would determine ahead of time how much you could take, and so they made sure that you didn't get more radiation than was allowable.

But as a geologist, with just visual looking at things, you could begin to see, like you were talking about these block movements, you could begin to see what those data were?

Yes, you could see that there had been motion along joints or along fault planes. You could reenter into the tunnel where the stemming was placed, the stemming you'd backfill with sand or grout, and you could get samples of that, and you could see how loose sand had been compacted so tight, it was like a rock now. And so you could determine what had happened to these various areas by knowing what was there ahead of time.

Now before this, when you're setting up the experiment, do you physically—I don't know anything about geology, forgive me, but do you physically see fault lines or different things within the geology as you're preparing for the shot?

[00:40:00] Yes, particularly later in the game when we became aware of how much the geology was responsible for the phenomena, we would send in geologists and they would map the walls and the ceiling of the drift and they would determine where the faults were, what orientation they were, and this kind of thing.

There was a comment also I wanted to ask you about, it wasn't your comment, in Caging the Dragon *where someone had talked about a tired mountain, and I wasn't sure what that meant. I think it was the notion that if you did more shots in a certain area, it might begin to affect things overall?*

Yes. I suspect, and I'm not aware of this specific comment, but what I would imagine that it means is that as time went along, we were shooting a lot of tunnel events in fairly limited space. And every time you shoot one, it causes this faulting, this block motion, and so you end up making a large-scale rubble out of the mountain from all these cumulative events, so that if you now shoot an event in an area that you might assume would be good, solid rock, because you've disturbed it so much with your previous activity, it now is more like a pile of rubble than it is like a solid mountain. I imagine that that's the—

*That's what he meant, and this was someone, [Ed]Peterson, "To go to the extreme, you can talk about the 'tired mountain,' which would explain things by saying that the structure is just degenerating with time because you've done more and more shots. I'm not convinced at all that that's what it is."[*Caging the Dragon, p. 435*]*

But it was a question I think from Carothers about why things are not so predictable. OK. So you've explained a lot of things to me already. Where do you think we should go next? I do want to talk about Baneberry. In about twenty minutes I'm going to stop this to make a time mark on it, but is there something we should talk about before that?

Before Baneberry? Well, let's see, we've talked quite a bit about containment. We haven't talked too much about the ground motion concerns.

Let's do that.

Because I was chairman of the ground motion subcommittee for the Nevada Operations Office [NVOO], oh, from '63 or '64 on until I retired in 2000. It was an interesting endeavor, interesting scientifically, but also interesting for other reasons, because we found that in fact while we did cause some minor architectural damage, the psychology of the population changed over time. In fact when we looked into whether or not we could even test the kinds of yields at Nevada that we had tested with relative impunity before, concluded that we probably wouldn't be able to without being flooded with so many complaints, it would just be prohibitive. Well, that's because of the changed psychology and the change in the demographic. You know, Las

Vegas is not the town now that it was in the sixties, and we did have our disputes then. We had a very active program to put seismic indicators, ground motion measurements, in all of the high-rise buildings and in some of the medium-rise buildings that we felt were particularly susceptible. We had a team of observers that would go out to these buildings ahead of time and then follow up after the ground motion had occurred to assess them for damage, and we did find that there was a very minor amount of architectural damage. By that I mean, maybe a plaster crack, something that does not affect the safety or stability of the building. But as time has gone on now, really since the end of the high-yield test program, many, many more **[00:45:00]** buildings, much more in terms of high-rise buildings, but maybe not all that much more susceptible because they have built to higher standards in their engineering, to stronger earthquake codes. So it might not in fact be any worse, but the larger number of people subjected to motions that they can feel would just result in a much larger number of claims that the DOE would probably not be willing to accept.

Oh, you mean like insurance claims for damage to buildings.

Yes. Because they did pay out claims.

Oh, they did?

Even in those days, yes, and they got a lot of complaints. They didn't pay that many claims but there were some that they did have to pay, and we think that nowadays we would not be able to go back and fire even the yields that we were used to firing in Nevada without getting much more static from the population.

What kind of yields, would you say?

Well, I don't know what kind of yields we could get away with there now but—

But in the past, what would these—?

Oh, in the past we could shoot, when we went into the high-yield test program, that limited things to 150 kilotons or below, and we didn't have any significant damage from those. So we could shoot those, we felt, without any real concern. When we started to get events like Bilby which was, oh, I forget now but like 250 kilotons, something like that, that's when we really started to get people complaining that they could feel motion. And so as we went to bigger events like Halfbeak, we knew that we'd better pay attention to this; those were in the range of, you know, from a quarter-to-half-a-megaton. And so in that range, you started to realize that you had to pay significant attention to the ground motions that you could cause in Las Vegas.

You're right, 250 kilotons it has for Bilby. So when you say planning for that, I'm trying to link this back to what you said, would it be a matter of it would be any test anywhere on the test site or would you be able to do them in certain ways that you'd get less ground motion or—?

Well, the reason that we migrated up to Pahute Mesa for higher-yield tests was it is significantly further away, and so we thought we could do higher-yield tests up there, and that probably is true, so that tests, more than 150 kilotons you'd do in Pahute Mesa, although we did do some down in the flats that were in the 200-plus kiloton range. But the big ones, you know, like the shots approaching a megaton or that vicinity, we would do in Pahute Mesa. We never did anything approaching that size down in the flats. And because they did want to go above a megaton, which is what they did for Cannikin, that's the reason we looked at Faultless, thinking that might suffice, but even that wasn't satisfactory for the very big ones. Too much ground motion still in the surrounding communities and in Las Vegas.

Now you talked about the way things were in Las Vegas in the sixties and you talked about the psychology. In addition to there being more building, you're seeing a change in the mindset?

You know, as time went on, we noticed that people were less willing to accept things in terms of building motion, swaying structures. People were more likely to look up and suddenly see a crack and say that must've been due to a shot, whether it was or not. And that seemed to get worse as time went on. Now of course we haven't had any really big shots in a long time out there. But back in the early days, the Nevada Test Site was a pretty big element in the economy of southern Nevada. I doubt that even the magnitude at which we had then, if it were going on now, would be much noticed in terms of the economic impact because Las Vegas has just grown so tremendously. So it would now be viewed more as a possible concern if something went **[00:50:00]** wrong than it would testing be viewed as an economic advantage.

I see. So the economic advantages would outweigh or in some sense—

Yes, and the Nevada Test Site sort of grew up with Las Vegas in the early days, but now you see no one has experienced that, and it would be an imposition on people to now suddenly try and do the same sorts of things we were doing then.

But you did mention before we started [recording] some things related to the Howard Hughes people and this ground motion?

Well, in the period when we were shooting some of these larger events, Howard Hughes moved to Las Vegas and was living in the penthouse at the Desert Inn, and he could feel these things, and he tried to put an end to it. He tried to get the AEC to stop shooting these large events. And we had a big public meeting with I think, who was his deputy, Robert Maheu. Of course we never saw Hughes, even then. But we did have a large public meeting with Maheu and we had to present all the evidence that said yes, we do cause some ground motion and people in tall buildings will feel it, but it's not a threat to the building. But he was really, because he had the power, the money behind him, he was a serious threat. But we did manage to convince people that it was OK to go ahead. So he never stopped a shot, but he sure caused us a lot of concern.

I think just generally one public fear would be, do these things—I don't know because I wasn't in Nevada in those eras but, I'm trying to think of—well, and I think a natural thing, if you didn't understand things or maybe you can explain to me why it wouldn't be true, was that, could you set something off? Could an underground explosion cause other kinds of ground motions?

One of the things that we looked at carefully, particularly in the large shots at Amchitka, could they trigger a natural earthquake that was much larger than the event itself, and perhaps create a tsunami? Well, because I was chairman of the ground motion subcommittee, I was the one who got to put together a panel to look at that and to then go and explain to people in Alaska why this would not trigger a larger earthquake or cause a tsunami, and so we were able to proceed. But you're right, that was a very valid concern that people had. And from our study of large detonations on Pahute Mesa, we found that they were followed by small natural earthquakes, but they're always much smaller than the event itself. So we never, on any of the shots that we looked at, did we ever trigger an earthquake that was larger in energy release than the detonation itself.

But that was a question that you as a scientist had to look into in any case?

We had to look into that, yes, particularly when you shoot the shots in areas that are known to be seismically active, like Amchitka, a very active location, and Pahute Mesa, which is not recently active but in the past has been a focus of a lot of seismic activity. So we did have to look at this in a serious way.

And so you were the head of that panel and you, what, you bring in other experts—?

Yes, we talked to as many of the experts in the field as we could, and actually had meetings to look at it, and the consensus, not everyone agreed at that, but the consensus of opinion was that we would not trigger any earthquakes larger than the event itself, and that the possibility of causing a tsunami was vanishingly small.

So these are experts from without, not just within the AEC or that community?

No, university, for the most part.

What kind of dissent did you get? What kinds of concerns did people have?

Well, the people who said we just don't know enough to be able to say this felt that maybe it's not likely but there's some possibility. And that's hard to argue against because, well, some possibility, yeah, there's maybe always some possibility, and that's why I said we concluded finally it was vanishingly small. But we did have a lot of measurements placed to **[00:55:00]** detect earthquakes and tsunami activity should any exist. But luckily, perhaps we were lucky, or perhaps we were smart, but we didn't see any of this activity as a result of the detonation.

Well, since you've mentioned Cannikin, maybe you can tell me a little bit more about what your work there involved, in addition to this, obviously.

Well, that was my principal involvement, to look at the distant seismic effects. Sandia had a role, measuring ground motion measurements. One of the things that Sandia did, and because I was the supervisor of the division at Sandia that did these measurements, we always measured close-in-ground motion, this hydrodynamic shock that I mentioned, from very close to the device out to a few hundred meters, and then we also measured surface motions, put in instruments to measure the surface motion from these underground shots. Those were the two kinds of measurements that Sandia routinely did, related to movements of the earth. We didn't get too involved in radiation measurements and diagnostics like the other labs did. So mainly measuring the motion of the earth, and we did do that on Milrow and Cannikin, the two big shots up there in Amchitka. But my personal involvement, you know, I supervised that group, but my personal responsibility was the evaluation and assessment of the seismic hazard.

Did you go up there?

Oh, yes, yes.

So what was that like?

Cold, damp, windy. It's often said that Amchitka has the worst weather in the world. But interestingly enough, the first time I went up there, we were in a prop plane. It was not a DC-3 but sort of the equivalent of that. And I'm sure the pilot was giving us a lot of stories, but he says, I used to fly up here in World War II and sometimes when we'd get close enough to see the ground, we were actually going backwards and we had to speed up our engines in order to land. But it was a little alarming because you could not see the ground when we landed until we were within just a few feet of it. So foggy. And that was the routine condition. Interestingly enough, on shot day, it was a nice, clear, bright day, one of the few you get maybe two or three a year like that.

Which shot now are you talking about?

Cannikin.

Cannikin. So you were there.

Yes. I wasn't on the island. There were only a very few people on the island. I was in Anchorage, where we had the control room and the visitors' center and all set up, where we had a display of the seismic monitors and everything. So I was there and we were giving public broadcasts from Anchorage. I had been out on Amchitka earlier, but when they did the shot, they evacuated most people back to the mainland.

Now again, as a geologist, when you go out there—

Although [James R.] Schlesinger was up there with his family.

Was he?

Yeah, because there was so much concern about what this was going to do that we had convinced the AEC, and Schlesinger was head of it then, that there wouldn't be a problem. He says, Well, if it will comfort these people, I'll go and I'll stay on the island. And he did.

He did. How interesting. To back up a little bit there, when you're going again as a geophysicist, what's the data you're looking at? You have information about the geology? How does that work?

Well, what we do, of course, because what we're interested in is the magnitude of the ground motion, we go largely on the basis of experience, built up from looking at literally hundreds of events on which we've taken measurements at various yields, and we get an empirical, in a sense, feel for what the ground motion is going to be, and it's different in different rock types. Now close in, we found that we could calculate the hydrodynamic wave with some degree of **[01:00:00]** certitude because the material properties are more predictable at these very, very high pressures, and the little vagaries of nature in the geology don't affect them as much. So close in, we could do a pretty good theoretical job of predicting. Further out, we found that we were really relying on empirical knowledge for a large part. Today, I'm sure that we could calculate even further out much better because our ability to do massive calculations is so much better. But in those days, we did rely a lot on empirical information.

And then how close were you? How successful were you, do you think, in predicting what Cannikin would do and what it did do?

We were pretty good on, for instance, looking at surface motions, because you have to predict with some degree of accuracy what the motions will be in order to set out your gauges and have them ranged properly so that they're not too sensitive and they're swamped by the motion or, conversely, the motions are so small you can't see it with the gauge setting that you have. And usually we were able to do pretty good by that period of time. We had enough experience behind us that we were, say, maybe within 20 percent most of the time.

And any other measurements that you would've—or that's basically—

Well, we had other kinds of measurements, for instance, the hydrodynamic zone that we made measurements, and we used a, most people wouldn't even think of it as a gauge. We called it a SLIFER. It was just a cable which went down close to the detonation point, and you would send a signal down it, and as the shock wave proceeded along this cable, it would crush it, and you could tell what the length of the cable was by the signal that was reflecting back up the cable. So you could actually measure the progression of this hydrodynamic front as it went along. But then as the pressures got too low, it wouldn't crush the cable reliably, and so then you couldn't use that. So then we'd use what we called strong motion gauges, specially built pressure and velocity and accelerometer gauges, to measure these parameters. And then as you got real far out, you'd use something that was more like a seismometer. And of course an event like Cannikin, seismometers could measure that all the way around the world. You could see that worldwide.

Right. Wow. I didn't realize that.

Yeah.

So you had the motion being picked up around the world.

Yes. And of course there's a huge volume of information that's been developed because of the interest in detecting underground tests, Vela Uniform studies. I mentioned it when I started out.

Initially there were very few people and very limited funding for this kind of research. Vela Uniform changed all of that. Massive funding. There are now worldwide systems of seismometers. Most people believe that it would be impossible to do an underground detonation of much more than a kiloton without it being detected, and unless you took evasive measures with it. So we've really progressed a long ways in our ability to monitor underground explosions.

Interesting. As I said, I'm going to pause it in a couple of minutes to make a time mark, but just tell me a little bit about what the atmosphere was like in Anchorage in the control room when this test is actually going off.

Well, it's always a very exciting time because you have everyone focused on zero time, and because there'd been so much speculation about what might happen, everybody was very intent. And they had cameras set up so that you could look at ground zero. You could actually see right above where the shot was at shot time, and everyone was very focused on these television screens. And it goes off and everything jumps around so much because the ground heaves an immense amount, that you lose sort of sight of what's really there at the moment. Then it settles down and hopefully the camera is stable enough you can focus back in. And in this case, you ended up not seeing anything, but as the shock wave went out, you could see squirts of water **[01:05:00]** coming up out of little streams that flowed through the bogs there on the island, because the shock wave would come up, would throw those little streams up in the air, and you'd see these sheets of water go up from it. And then of course, after the visual effects, people start looking at the radiation monitors to see if any radiation is finding its way to the surface. And fortunately on this shot, we did not see any release of radioactivity. Often, as I said, the big shots are better contained than some of the small ones. Going back to Baneberry, the same kind of thing goes on with these smaller shots. We looked at the cameras. Not I, I wasn't on site for

Baneberry, but I looked at all the replays later. Everybody thought Baneberry was a normal, well-contained shot, because it wasn't until about three minutes later that it started to vent in a massive way.

OK. Hold that thought.

[01:06:08] End Track 2, Disc 1.

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

OK, so we're back on, and you had begun to talk about Baneberry, so let's continue where you were and how that all came about.

Of course when Baneberry was detonated, it was thought for the first few minutes to be just another routine successful shot, but after about three, three-and-a-half minutes, people started to notice a very vigorous release, dust, steam, going high into the atmosphere. It had taken that long for Baneberry, the pressures in the Baneberry cavity to find an exit and to vent to the atmosphere. Once it got started, it just continued to erode that opening larger and larger until eventually it was just like a huge freight train coming out of the ground there. But because we had a treaty which prevented atmospheric shots or any underground shots that would release radioactivity past the border of the country, and this did, there was great interest in finding out what went wrong. And in fact, they stopped all testing while this committee that I was chairman of investigated the probable causes and came up with suggested remedial action to prevent it from happening again. [Baneberry Summary Report (u) R.H. Thalgott, W.D. Weart (SNL), L.S. Germain (LRL), F.D. Cluff (Compiler), January 1971. NVO-95 AEC (SRD). This is a secret report. There was an unclassified version—Baneberry Summary Report.]

Now were you in Albuquerque at the time or were you at the test site at the time?

I was not at the test site. This was a Livermore shot and I had nothing to do with it. And when they very quickly determined that they needed a committee to look into it, because there's a bit of rivalry between Los Alamos and Livermore, they thought, we'll get somebody from Sandia who will be neutral to chair the group. And so they asked me to do that and I did.

What was the group's name, do you recall?

I don't know what it was. There is a report on that, too, and I know that there's both a classified and unclassified version, so you can probably find that. But that was just before Christmas, really ruined my Christmas that year, because the pressures on us were intense to try and find out what went wrong and how to avoid it, and so I think I got one day off for Christmas and that was it.

Now how quickly did you find out? I'm trying to understand the emergency itself because you're not there. How quickly does the news travel that this thing has happened and it's serious?

Well, it traveled to the control point as quickly as the speed of light allowed them to see the venting because it was very obvious what was going on. And I would say I didn't find out about it until the next day when I was contacted about heading up this group. But for the government, things worked pretty fast because they'd already decided that there wouldn't be any more testing until we understood how we could do it without violating this treaty provision. So that's what really put the pressure on us was we, the government, DOE wanted to get back to testing, but it said we can't until we know more about this Baneberry, because it looked like a very nominal type event, one that we'd done dozens, hundreds of without a problem, and here this one had a massive release. So that's why there was so much interest in getting this resolved as quickly as possible, and so to get this group set up and to start working within a few days just before Christmas was an indication of how serious they were about it.

Did you choose who was on the investigating panel or—?

No, the laboratories provided one from Livermore, one from Los Alamos, and then we had a lot of support people who were knowledgeable, and we had a lot, a lot of people come in to tell us not only observations but ideas, theories, do calculations for us. But the people who were on it with me, Bob Brownlee and Larry Germain, were two of the best scientists in their respective laboratories in the area of containment, so we had a very well-respected group of **[00:05:00]** people to work on this. And I must say, despite the concern over the rivalry between the laboratories, didn't have any trouble with everyone working together on this. And I think we came up with a reasonable story. Different people will have their own opinions on which of the aspects we pointed our finger at may have been more important than others, but I think we came up with a story that was pretty well accepted and which certainly our panel agreed upon.

And it was one of those things where there were an unfortunate combination of more than one event that may have just caught us. One of the things that was most obvious as we studied it is that the device itself was placed in a geologic unit of saturated clay, where normally at that depth we would expect dry alluvium. Well, we know and we knew at that time if we had given enough thought to investigating what was down there, that a shot in wet or saturated material couples energy much better. Just like when you go into salt, it couples energy much better. Well, shooting in water or shooting in a saturated alluvium couples energy much better. Well, in effect, even though we were above the water table, this clay has the ability to hold water and this clay was saturated, so we had very good seismic coupling. Made for a much more intense ground shock. And one way of looking at it is that the effective seismic yield made it look like a much bigger shot than it was. And because we buried shots according to their yield, we thought we had it deep enough for a shot in alluvium. Probably was not deep enough for a shot in a saturated medium. So that was one factor.

Another factor that all this water led to was it created a huge volume of steam from the heat deposited in the clay and the surrounding rock. Normally in alluvium, it's relatively dry, so you don't create as much steam. That meant you had this big cavity down there, essentially a bubble, filled with this pressurized steam, just simmering, hoping to find a way to relieve itself.

And then the third circumstance that occurred was there was a fault which was off to the side and which eventually gave an avenue for this steam to get to. And the steam went over to the fault and then up the fault, and when it vented, that's where it vented, is out this fault. It did not vent directly above or out any emplacement hole and there wasn't any line-of-sight pipe. It vented out this fault off to the side.

So we had the circumstance of the higher-than-anticipated seismic effect, the high-pressure steam, and then the availability of this fault. And maybe all three were necessary, maybe only one was necessary, but the fact is we had them all and they all contributed in some way.

Well, after we understood this picture, what it told us was that we need to pay a lot more attention to the geology around and above the shot point than we ever had before, and that led to the creation of the CEP [Containment Evaluation Panel] and the much vaster amount of knowledge that was gathered before firing a shot in any location.

A couple of questions about that. The clay being moister, am I correct in remembering—I read these interviews from Livermore a long time back, maybe a year ago—that some of that water was actually from the drilling, that water had been used—?

Well, there was a lot of water used in the drilling, but clay is quite impermeable, and so probably most of this moisture that created the large steam pocket was probably there naturally and not introduced.

Oh, OK. Great. And was the fault known before the shot, do you know?

[00:10:00] Not ahead of time.

So when you're saying knowing more about the geology, that fault was one of the reasons for that?

See, after Baneberry, no one would have thought of conducting an event where you did not have samples from the working point, know what the moisture content is, have looked at the surrounding area to see if there were any faults or joints. All the stuff that we did since 1970, we didn't do before then. We just thought, hey, at this depth in the valley, we're going to have alluvium, and that was about the extent of the worry there was about geology.

What's so interesting about that is that when you look back, even as a layperson, you sort of say, well, these are really smart scientists, wouldn't they think about that? But I guess the other piece of that is when I begin to appreciate how complex what you were trying to do in the first place was. I'm asking a question by making a statement.

Well, what you have to realize is that we had been conditioned by years of experience, dozens and dozens of shots that performed adequately, maybe not perfectly but adequately, without a serious release like this, and so, you know, why spend all this money and time to go beyond adequate?

How do you as a group, just generally—obviously, this is technical information, I'll have to see if I can find the unclassified report—but how do you manage this group to begin to develop this information to be able to come to a theory that has these things? Do you assign different experts? How does that work?

Well, what we usually did, the three principals, we worked as a team almost entirely—

You and Brownlee and Germain, you mean?

Yes. And we had other experts that spent quite a bit of time with us: Russ [Russell] Duff who was with S-Cubed [Systems, Science and Software] and people like this. But the first thing we did was to get as much in our record as we could about what actually occurred, the phenomenology. That meant looking at movies. There was an interesting picture taken from a helicopter, because they always had helicopters and they often used them to retrieve data early. And as I say, this was fairly late, so they guy was flying along and he decided nothing interesting was going on, so he was looking at the scenery, and all of a sudden, you can see the shadow and the camera swings back over here and the cloud is already up here. It happened that fast, because it was delayed, and yet when it did occur, it was so vigorous that the cloud—in fact, I've got a picture of the cloud on my den wall, not in here. I'll show you these pictures on the computer before you leave and what's in that room. So it was a truly spectacular venting.

But what we do is we first get all the information we can, any ground motion information, the seismic data, because clearly that was one of the things, and then we did reentry drilling. We went back, and the lab usually does reentry drilling to get samples for yield determination. Well, because the seismic, the ground motion looked like a pretty high yield, we wanted to make sure it hadn't gone much higher yield than it was supposed to. So we look at all this data that was taken at shot time, both visual and instruments, and we then ask, well, what do we know about where the shot was detonated? Why did it look like it had? So we actually got samples. We looked at the stemming record. Even though it didn't vent out the hole, maybe it went up the hole a ways and then out. We looked at the stemming. We look at the drilling history, like this introduction of water, and there was a drilling problem and they did use a lot more water than they normally would. So we look at all these kinds of things and then say, well, theoretically, what would be the contribution of all these various factors to the phenomenology of the experience we had? And gradually we pieced together this picture that this was **[00:15:00]** what it was like, this could've led to much higher seismic coupling, could have led to a much greater volume of steam, all these sorts of things.

I have an interview, and I can't remember who he's talking to, an interview with Carothers from Livermore that's not in this book where he makes some comment about we don't know what happened on Baneberry. What do you think of that?

Yeah, I think we have a pretty good idea. I think we have a pretty good idea. What we don't know is whether if any one of these factors I mentioned had been absent, if it would've contained. We don't know that. But we do know that these factors I mentioned have a—we know it came out the fault. There's no doubt it came out the fault. Well, if there hadn't been a fault, maybe it wouldn't have come out at all. Well, we know that it was shot in water, or a saturated medium, high water. We know that it gives much more intense seismic motion, and we saw much more intense ground motion coupling. So maybe it made it look like it was under-buried. All these things operate in a direction of being undesirable. Can we say any one of them was the primary cause? No. Can we say it would have vented if this one wasn't present? No. But I think if we avoided them all, we would've been safe. Because we have done shots just like it, same yield, same depth, where these factors were not present and it was OK.

Did you have to testify at the trial, the Baneberry trial, of the guys that were—?

No, I don't think so. I know that there was a trial about exposure.

That's right.

But I don't believe I testified at that. I'm sure I would've remembered if I had.

I think you would. I'm just looking because I don't remember what—

Because I wasn't involved in any of the operational—

Correct. I didn't think so. So it says Baneberry, here it says 10 kt.

Yeah, it was not a high yield.

Now explain to me, what were your theories about why there was this delay, that it looked OK. Was it the time it took for the stuff to get up the fault?

Yeah, because it was not a direct release, like up a vacuum pipe or something like that. It just took a certain amount of time for this high-pressure steam to find the easiest path of release, and gradually it worked over to this fault, and we think it actually worked up a ways and then out, and up the fault. And so it was not a direct shot, and it just took some time for it to realize, hey, here's my way out.

How far was the fault from the shaft, do you recall?

Oh, I can't give you a number right now. It was not a long ways away, but it was outside the crater, the subsidence crater, so it was outside that.

All right. And you weren't involved in it, but did you notice emotion on the part of any people that were involved? Concern? Worry? As a scientist, do you step back from that? You try to, I know.

No, I think because, particularly the other two labs who were responsible for conducting these shots on a weekly, monthly basis, they were very concerned because they knew if they didn't find a way to do these test activities better, they might not get to do them at all. And they were interested both from a standpoint of national interest and in doing their job as well as they could. So they were generally concerned. But I don't think anyone was frightened or that kind of thing but they did have a concern that we need to understand this.

So this then leads directly to the creation of the Containment Evaluation Panel, is that right?

Yes. People had looked at tests before. I think it was a Test Evaluation Panel [TEP], but it was not a Containment Evaluation, it was a Test Evaluation Panel. People looked more at the experiment design and it was more in that sense than containment. And they clearly did not have this massive focus on geology that they required after the Baneberry investigation.

[00:20:00] *So we're getting close to 3:30. I don't want to take up too much more of your time, but can you talk to me a little bit about your work on that panel? Or do you think this is a good stopping point?*

My work on—

The Containment Evaluation Panel.

Sure, I can do that. You know, the CEP as we called the Containment Evaluation Panel looked at every event that was proposed to be fired, and on that panel there were experts from a number of disciplines. Some were specialists in calculating the physics of radiation flow. Some were geologists. I suppose if I had any particular expertise, it was ground motion and seismic effects. But one of the things that they all had in common is experience with testing. And so we had people who had been through a lot of this and therefore could call upon their experimental knowledge base. And there were a lot of vigorous arguments that went on around that table. We all sat around a big table. Someone would present the specifics of a particular test to the panel, and we were given an opportunity. In fact, we weren't just given an opportunity; we were polled by the chairman, and for most of that time it was Carothers. He says, `What do you think of this? You have any problems?` Sometimes I had no problem; sometimes I had questions. I'd say, `Well, how do you know that this is the medium in which you're firing? It doesn't look to me like your samples were taken from the working point. They were further up the wall.` Things like this, trying to ascertain how good our knowledge of the circumstances were surrounding this test from the standpoint of containment. We had people later on who became experts in how you seal cables, because a lot of the very slow leaks, small magnitude releases, came up cables, rather than through the ground. So this was the factor that got a lot of scrutiny. And even the experts in the field put a lot of weight on their empirical experience. But because this was such a deviation from how it used to be done, at first there was perhaps over-conservatism in some areas. Then, in time, people learned how to model, calculate things a little better. They may have backed away in certain areas, compensated by doing different kinds of stemming. But it was an opportunity for all these people who sat around the table, I don't know how many there were now, ten or twelve, and all chime in on whether or not they felt this shot was going to be satisfactory. And we could vote it—there was a categorization, A, B, or C, or something like that. If we felt very confident, it would be an A. If we said no, this thing should not be fired this way, in my view, it'd get a C. If there was some uncertainty but probably not enough to be a problem, I'd vote it a B. I don't remember exactly what the categories, but something like that.

Did you work by consensus or did you have to be unanimous?

It did not have to be unanimous. Usually there was consensus, but sometimes there were people who would hold another view, there were divided votes on the panel.

And then it would just be whether yes or no got the most weight or would people have to come back and address some of these things?

Well, often, if there were problems that could be addressed to satisfy someone's lack of information, they were addressed and they'd come back to the panel a second time. Sometimes you couldn't resolve the issues, and then it would go to the manager of NVOO, and Carothers would usually present to him the pros and cons. And I'm sure he gave the manager his own view, and then the manager of NVOO would make the decision, yes, we'll go because the preponderance of opinion is that it will be satisfactorily contained.

[00:25:00] *Where were the meetings physically held, out at the test site or—?*

No, they were usually in town.

In town? And where generally, I know there may not be a consistent answer to this, but this must've then been worked into the timeline of the test designers, so was that early on or later when they—?

Yes, as soon as they had a concept for the test and they knew where it was going to be located, they would come months in advance. But the shot would be reexamined, perhaps not in as much detail, right up until the last meeting before it was fired.

And then post-shot, would there be verification that what was supposed to happen happened?

There would usually be, if it was satisfactory, there'd be a very brief statement, everything went as expected. If there was a release, then it would be examined in detail by the panel.

Anything else about that?

Carter Broyles was the primary Sandia member on this panel in the early days, so if you talk to him, he's one who could tell you about some of the functioning there, too.

And that panel exists till now.

I guess it does.

It does.

I haven't been involved in it since—in about 1975, I was given an opportunity too good to refuse, as they say, by my vice-president, and I was put in charge of developing the scientific basis for the Waste Isolation Pilot Plant [WIPP].

And who was the person who gave you that opportunity? His name?

Glenn Fowler.

Oh, yes, I've heard of him.

He was the vice-president of field tests for Sandia.

So Sandia was directly involved in WIPP, then, or—

Yes, from '75 on until I retired. In fact, even a little bit today, I'm still involved in WIPP. But Sandia did all the science and all of the performance assessment and developed the modeling for long-term behavior for the WIPP, and I was the head of that group at Sandia for all those years. So the first fifteen years of my career I spent predominantly at the test site, and the next twenty-five years of my career I spent predominantly on WIPP. But I've always lived in Albuquerque.

That's another big difference about test site experience. When I first started working at the test site, we were expected to go out, and of course we worked on a project basis. You'd go out. You'd work three weeks before you could go home. So I spent a lot of time in Nevada, and had Sundays to explore, and worked long hours. In those days they paid overtime, so I got a lot of overtime in those days. But the food was good at Mercury. They had a cafeteria and you'd go through a turnstile and you'd put a silver dollar, because silver dollars were still in circulation then, put a silver dollar in the turnstile and you'd go through, have as many steaks as you wanted. And that all changed when they went to a more year-round-type operation later in the sixties.

Oh, right. So you stayed at Mercury, then?

I stayed at Mercury, yes.

There were, what, like what were the places where you would stay?

We had barracks. When I first went out there, they were wooden barracks, and I went out there first with BRL [Ballistics Research Laboratories] in '58 and then with Sandia in '59, and they put us in these wooden barracks with three bunks in a room. Being the new guy, I got the top bunk with these two old guys who snored louder than any diesel train you ever heard. I finally took to going to bed before they got into the room because if I didn't get to bed first, I couldn't get to sleep. But, yeah, those days, it was quite different then. And then they built some much nicer cinderblock rooms out there, which we stayed in in later years. But Mercury was quite a booming little town in those days. Booming in more ways than one.

Yes. Now when you first went out, were there soldiers still at [Camp] Desert Rock? Were they doing any of those things then or was that ended?

In '58, there were still some soldiers there, but by the sixties they'd mostly abandoned that activity.

[00:30:00] *Right. It's 3:30. I would really love to know about what you did with WIPP but I think that's really off our subject matter.*

Yeah, little different story.

But I did have one question about it was, because I don't really understand the history of that. How did that come about? What was the atmosphere in which that whole program was created?

Well, in the early seventies, Oak Ridge [National Laboratory] had studies going on at Lyons, Kansas in salt beds, in which they were looking for a repository, and thought they had a site near Lyons, Kansas. But it fell apart because of problems they hadn't anticipated, geologic and engineering problems. So they started to look in New Mexico. They were still looking in salt beds. Salt, because the National Academy [of Sciences] had said salt's a good rock to look at and has a lot of advantageous properties. New Mexico has a lot of salt. So they had picked a location that they thought they would use, and then the program was suspended because the AEC thought, as they often do, they've got a better idea: we won't build a repository right now, we'll just store the stuff on the surface. But opposition groups and the [United States] Public Health Service [USPHS], it became EPA [Environmental Protection Agency], says, You're just putting off problems to future generations. You need to find a site now. So after a couple of years, they started back up. But when they did, the AEC thought, maybe the local population would be more receptive if an in-state lab was doing this rather than an out-of-state lab like Oak Ridge was. So they came to Sandia. Sandia said yes, we'll take this on. They came to me because I was the only geophysicist, right background, at Sandia at that time. And that's how we got into it. And when we got into it, we thought we had a general site location established by Oak Ridge. The very first hole we drilled found totally unexpected geology. Instead of nice flat-lying beds, they were vertical. So we had to abandon that location. So basically, my first job was to direct a program to find an acceptable site in New Mexico.

And this is for nuclear waste?

It's for transuranic nuclear waste generated by the Defense program. Not high-level and not even Defense high-level. Only transuranic waste.

So that's a whole different situation than Yucca Mountain.

Whole different thing than Yucca Mountain, yes.

But that is operational, right?

It's operational. They've been operating since '99. Well underway. Very, very successful.

Did you enjoy that work?

Yes, I did. I enjoyed it a lot. In fact, I've enjoyed my entire career. It was a great career. Just full of interesting things and opportunities.

I want to close, but I have one sort of general overview question about testing because it's a question that the public, I think, people and scholars have a lot about, which was your own views *about the necessity of the test program, given the politics, and there's so much debate about that still. Did you think about those things at the time you were doing the work, whether testing was necessary, et cetera?*

No, I must say I really didn't. In those days, the atmosphere was quite different than it is nowadays. We felt that improved nuclear weapons were necessary to successful defense of our country and that the work we were doing was a very important part of that. Things have changed, of course, today, and I'm not really competent to address the issue of, do we still need to do testing today? I have good friends who argue both sides of that issue and who are much more knowledgeable than I am about it. Now, my own uninformed opinion is that if you're going to just rehabilitate existing weapons, perhaps you don't need to. If you're going to really break the mold and develop a new kind of weapon, even though we have excellent computing capabilities today, I'd be much more comfortable if we did a test of it. But that's just sort of my layman's view because I'm not a weapons designer.

[00:35:00] *Right. Well, is there anything else you can—I'm sure there's lots more we could talk about but I think this is a really wonderful overview of your career and a lot of interesting information. Is there anything else that you thought of that you might want to say?*

Well, a lot of the high spots we certainly hit. I think just the difference in the way that testing was conducted over the years. During the time I was involved, very pronounced, going from a much more casual approach to it in which many of the shots that were fired did leak some amounts of radioactivity, a few of them a lot, but then later as we entered the atmospheric test ban, fewer and fewer of them leaked but still seeped. And then Baneberry came along, and after Baneberry, so much more attention was given to containment that very few shots released after that. Really a striking improvement in the degree of containment after Baneberry.

Well, great. Were you going to say something else?

No. I was just thinking that the containment part was really a necessary job I had to do. The work I did with the seismology, the ground motion, the structure response to ground motion was perhaps one that I enjoyed even more, just because it was more closely related to my training, my educational training. And that was one of the few things I got to do in my career that actually employed any of my education in a direct way. Of course everything indirectly was related because understanding how the ground, the earth responds to sudden release of large amounts of energy is basically fundamental to everything we studied: containment, seismic coupling, ground motion, building response. So it was all very valuable background to me in working in these fields.

So I guess what I'm hearing is that as a geophysicist, some things that you did more than others actually were looking at geophysical knowledge, let's say, more directly than other things that you did.

As a seismologist and geophysicist, we looked at waves that propagate far enough away from the source that the ground motions are relatively small. In containment, our primary success occurs well before we get into the seismic fields where the ground motions are extremely large. And so it was a realm that I never actually studied in school, but yet they're closely related, and the very strong ground motions, large-amplitude motions close in, of course lead to the seismic behavior further out. But it's just a little different way of looking at things.

Right. And I imagine it's something that you wouldn't anticipate because it comes from this specific thing that you're doing, which is exploding these very high-energy things in the ground, so that causes you to have to look at—

Yes. That's right. And until I got to Sandia, I had never thought of really seismic waves generated by large explosions. I knew that sometimes in our field studies we used dynamite down a hole to make a seismic signal, but I never really thought of a nuclear detonation as being a source of these immense amounts of seismic energy, which can approach those of large earthquakes.

So are there ways in which having this kind of knowledge informs natural phenomena, would you say, of earthquakes and things? Is there an overlap into that kind of science?

Oh, I think there is, and in fact even in the area of waste disposal now, like Yucca Mountain, there's a big argument about whether or not earthquakes will occur at the Yucca Mountain site, and this gets into the issue of what causes earthquakes, which causes seismic motion. Will the **[00:40:00]** seismic motion jeopardize the repository or will the motion on a fault jeopardize it? They're all very closely related. And just like with what caused Baneberry to vent? We think we know a lot about earthquakes, why they occur, but we don't know the details. We can say, we're likely to have an earthquake here, and we think we'll probably have one in the next fifty years, but I can't tell you it's going to happen tomorrow. So there's a lot we still can't predict, even though our general knowledge is much better. And that's sort of the way it is with containment, in a way. Our general knowledge of what it takes for good containment is much better, but our detailed knowledge is not sufficient to provide 100 percent assurance.

OK. Great.

OK. You want to look at this? [Photos]

Yes.

[00:41:15] End Track 2, Disc 2.

[End of interview]