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

# Interview with Leslie R. Hill

**February 17, 2006**
**Las Vegas, Nevada**

Interview Conducted By
Mary Palevsky

Oral history is a method of collecting historical information through recorded interviews conducted by an interviewer/researcher with an interviewee/narrator who possesses firsthand knowledge of historically significant events. The goal is to create an archive which adds relevant material to the existing historical record. Oral history recordings and transcripts are primary source material and do not represent the final, verified, or complete narrative of the events under discussion. Rather, oral history is a spoken remembrance or dialogue, reflecting the interviewee's memories, points of view and personal opinions about events in response to the interviewer's specific questions. Oral history interviews document each interviewee's personal engagement with the history in question. They are unique records, reflecting the particular meaning the interviewee draws from her/his individual life experience.

Produced by:

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**Table of Contents**

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

**Mary Palevsky:** *Les Hill, thank you very much for meeting with me today, and we were just saying off [the]recording that you would like to start by talking a little bit about your childhood and high school days and how that related to how you came to be working at Sandia [National Laboratories]. So why don't we start there and then see where we get to.*

**Leslie R. Hill:** Thank you Mary for the opportunity to talk about my life at the [Nevada] test site. Thinking back to day one, I grew up on a farm outside Ithaca, New York. It had a giant sand box. It's actually the end of a glacier run that created Cayuga Lake. And there is geological moraine behind our house and as I say, there's a giant sand box about three-quarters of a mile long. The county, the state would come to get sand there. It's a beautiful area. We'd have sand standing thirty feet in the air when it was mined, and then as it dried out, of course, it was very dangerous to be in this area.

My parents wanted me to get off the farm and go to college. And, well, during high school I met a gal, this neighbor gal, and she was the secretary at the Student Council, I was President of the Student Council at a little country school. She was a cheerleader, I was a basketball player; she was a natural-born singer—in fact, when we fell in love and decided that this was something. I didn't worry about being an engineer because if I failed, she could make it for us.

In any case, I was encouraged to go to college. My parents wanted me to get off the farm. As a matter of fact, they quit farming when I left for college. I always wondered if that was a dirty Communist plot, to get off the farm.

I was valedictorian of this little old high school and as such, was supposed to speak for the class—well, the advisor called me up on graduation day and said, What are you going to talk about? I told her, I really hadn't thought about much. I think I'll talk about [Robert] Frost's "The Road Not Taken." She was a little beside herself that I had not prepared a valedictorian speech. But I talked about Frost's "The Road Not Taken" and tried to encourage my colleagues of about two dozen that they had an opportunity to take the road not taken, which I continued to do.

My dad had worked at Cornell University as a carpenter, and there was a lot of pressure for me to go to Cornell. I didn't want to go to Cornell. I was an only child. Because I had about—this was not a thing I had decided to talk about—I had eleven boy cousins who wanted me to buy beer, I'm sure, because they all were younger and lived around Cornell, so that was another reason not to go to Cornell.

In fact, I won an NSPE, National Society of Professional Engineers scholarship that was fantastic, with Armco Steel as the financial sponsor. Now that you've asked, Mary, I'll say they had four scholarships, one for the East Coast, one for the West Coast, one for the Mideast, and the Midwest. I won the East Coast scholarship. It paid tuition, fees, and books. It was fantastic. And it did not suggest that you must go to this college or that college, but you must be a civil engineer. Having grown up on a farm and had that sand pile and all kinds of things like that, a civil engineer sounded, hey, the thing to do.

So I went looking for a college, with this tremendous pocketbook. I elected to go to a very small, unheard-of men's school, which doesn't make a whole lot of sense, but I had the gal who was following me and we were going to—it worked out, I'll just say, she's still my wife and best friend.

*And what is her name again? Elaine?*

Elaine. Right.

*That's right. What year did you graduate from high school?*

Nineteen fifty-nine, a year or two after the end of atmospheric testing. In any case, the Kennedy years. The scholarship was very good. It was a surprise to me. Our school was exceptional because we were close to Cornell and spouses of Cornellians were often our teachers, so we had an exceptional public school, really very good.

I selected Union [College] because I met a professor there who said, `Hey, you don't know if you want to be a civil engineer? What do you want to do?` And I said, `I'd like to take more math.` And if I had not [messed around], with a number of fraternity brothers, I remember we had five guys, four of us would go play golf and one guy would go to labs in the afternoon. In any case, Professor [Gilbert] Harlow took great interest in my future; a little concerned that I had planned to get married and leave school after I graduated and not go to graduate school. He said he'd call universities if I were interested in going and, you know, "never say never," "the **[00:05:00]** road not taken." He called a number of schools, his Harvard and places back in Boston. It was too late. He called Princeton, Cornell, Cal Tech. It turned out Princeton and Cal Tech had the exact same problem: will a person be able to walk on the moon? And I visited Princeton and I saw a setup there and I said, hey, I could do the research before school starts. Well, it didn't work out that way; it wasn't that easy. But I selected Princeton. It turns out the financial aid between Cal Tech and Princeton was almost identical. The disadvantage of Cal Tech, in 1961 I worked for the Forest Service with nine other fraternity brothers, we went out to California to find out what these California girls looked like anyways and it turns out we all came back east and married our gals after our trip. But we had a great

time. I did meet some guys, working for the Forest Service, who lived near Cal Tech and I figured, well, that's sure a way to flunk out of Cal Tech. I'll take Princeton. And it turns out my wife-to-be could transfer to New Jersey easily. I saw this experiment for, "will a person be able to walk on the moon?" that I thought I could really do, it was tractable that I'll go there.

*So the college is Union College, is that right?*

Union College, Schenectady, New York.

*In Schenectady. OK.*

It was an all-men's school. And now their tuition, would you believe, it was in some magazine I saw; fifty thousand [dollars] a year. And NSPE scholarships, they've discontinued that program. They're sponsoring MATHCOUNTS now in schools, taking a very good approach, I think, because they're meeting more people, affecting more students, because at that day they were encouraging maybe just four people, you know, East Coast, West Coast, Mideast, Midwest. So I think what we're doing now, and I'm very involved with MATHCOUNTS in Albuquerque, sort of paying back. [Largely for coordinating that effort for about 10 years, I was elected Professional Engineer of the Year 2005 for the state of New Mexico.]

In any case, I saw this situation in Princeton and decided I'd do it, and did the research for a person walking on the moon. That worked out well and I was encouraged to take the Ph.D. qualifying exam. That's a story in itself, too—but I won't go into it here—I did finish a thesis. Originally it was nineteen pages long and my advisor, Professor [Ahmet S.] Cakmak wanted me to do some confirmatory experiments. It was a three-dimensional problem. I didn't quite know how to do that. It's sort of like things at the [Nevada] test site, didn't know how to do three-dimensional things, calculational things. And this experiment, the only thing I could figure out, was trying stress-freezing photoelasticity. It was a technique that had never been at Princeton

before. One of my professors grabbed a hold of it, in fact, eventually published a series of articles in *Scientific American*. Well, he was on PBS, too, on Public Broadcasting; after McNeil-Lehrer's programs he was often on, driving through Italy in a right-hand drive, open-air MG or something like that. He was studying flying buttresses of Italy and France.

*What was his name?*

Bob Mark. He was not my primary advisor, but he was—well, he had—because I went to him, he was in architecture as well as civil engineering, and said, `I need a large oven so I can do this photo elastic experiment.` He had one in the architecture building and I gained access to the architecture building, you know, morning, noon, and night to check on this stress-freezing experiment. It worked out well and it published well, and then, as I say, he picked up that technique and was on PBS quite a bit.

In any case, I was trying to figure out where to go. There was a lot of pressure for me to go to Grumman [Aircraft Engineering Corp.] at that time because the vice-president of Grumman was a Princetonian. So I was making interesting trips to Bethpage, Long Island, and was sort of like a consultant with them because they were building the LEM: lunar excursion module. I've done that research on the master's [degree] and there was interest in going there. However, having been out west, and going through the winter, like the winter just now back there, I was ready to get out, ready to leave the farm, so to speak. Once I've left, I'm going to really jump and go west.

So we were really interested in going west, and Sandia was interviewing at Princeton. I went to the Sandia interview, met Frank Bell, who is no longer alive. He had been with the World Bank, quite a prestigious position he had, and he decided to get out. His wife was, I think, **[00:10:00]** asthmatic or something and he wanted to come to the Southwest, came to Sandia, and

was now interviewing at Princeton. He was sort of interested in my background, and I remember we were in McCosh which is a building there, it had an elevated balcony and he went running around the balcony and picked up Tom Cook and brought him back to complete the interview. Tom Cook said he was interested in a math-oriented civil engineer because he was interested in containment studies. The prospect of data that we [Sandia] can calculate with whatever containment studies we have, that we'd have real live field data to compare with; it sounded very interesting—he did not have that capability at Sandia. Tom wanted to create a capability. We do have some data, can you get that data that you come up with to agree with the field information we have? So it was a very interesting prospect. And I guess I was so interested that I didn't really pursue much else, because I had Grumman in the hip pocket, and what else do I want to do? Other things I had looked at, teaching at undergraduate school and things like that just did not interest me. Sandia, being an engineering organization, as it was, really did interest me. And so those were the prospects. We'd take a look at Sandia first and see what happens. And, well, Lockheed was there, too, now that I think of it.

Well, by telephone I met a whole lot of Sandians. I called up one day and I said, `Gee, I can't tell who are the actors here`. So I figured out that Tom Cook was there, and Norris Rose who worked for Tom Cook, and found out, you know, and Andy [Andrew] Fuller, the personnel fellow, put this all in perspective, and his secretary was just great and straightened me out. So I ended up working with the personnel people a great deal, ended up recruiting for Sandia. I recollect three dozen that I have recruited—about two dozen from Princeton.

*Now what year did you graduate so you're doing all these things?*

I finished my Ph.D. in 1967, and came to Sandia in '67. In fact, that's an interesting story, all of it, coming back. I had met Tom Cook, and for my interview, Mel Merritt was my host. And I'm

talking to a relatively small group of individuals that I thought in a seminar that they asked me to give would be packed, and there was only like six people there. I said, golly, I wonder if they're interested. And then my host fell asleep. And I said, uh oh, I've lost this. I've lost it because this is pretty boring mathematics and my host is asleep. So I was on the board and I said, well, I couldn't solve the problem in the first place, that I had to use a series approach. Mel's hand went up, and he said, `How many terms did you have to use?`

And I said, `Only a couple, it converged very quickly.`

He said, `I agree with you, it should converge very quickly.`

So he was not asleep. I was very impressed that he was awake and interested and alert. But seriously, Chebyshev polynomials. It was pretty deep mathematics and he was following me all the way. I couldn't believe it.

So there was a one-and-a-half-day interview planned, and it went to two days because Jerry Kennedy and some other people took me around to see some of the field things in Sandia's Area 3 and 5, and so we had quite a tour. Princeton said don't worry about leaving Albuquerque, that there should be no problems. As it turns out I had brought my wife along because for her it was quite a step to be going to the desert and everything and she was a little concerned about it.

*Oh, OK, so let me understand. This is all taking place, this seminar that you gave is taking place where?*

In Albuquerque.

*In Albuquerque. And then you went up to the test site?*

Just very quickly. My wife did come with me to Albuquerque. As I say, it was a very big step for her, and so I wanted her to see, too, as she was part of the vote. We did marry before we went to Princeton, and so we'd been married now four years. We were married in '63, and so it's four

years and it was a big step because we were both leaving the farm, and as I said originally, I think, that she was off a neighboring farm, so it was a big step for us.

It's sort of interesting, a one-and-a-half-day interview turned into a two-very-full-day interview. She had checked out while I was in this interview, got back to the hotel about **[00:15:00]** six o'clock in the evening, and the only flight she could find was two in the morning, so we had eight hours to kill. So we obtained a snack somewhere. This is the White Winrock which is a hotel which is now going down in Albuquerque. It was a Winthrop Rockefeller connection some time ago.

In any case, we went to a movie. They were showing *Hombre*, and for us—they didn't need a screen. They could just project it up there on the Sandia Mountains because you know, we enjoyed it. So we were very relaxed, we had a, you know, just relaxed, just enjoying the movie, and there's some noise behind us, off-color jokes, and so we're laughing at this group behind us. And on the way out of the movie, I looked at the guy and said, `Is that you, Vaughn?` It turns out, it was one of my fraternity brothers, and so those guys, the other guy, Joe Paone in particular, took us to Ned's. We sat in Ned's, drank a little bit, and they delivered us to a plane at two o'clock in the morning. Ended up not having to get a cab and had a great time.

By the way—this is on the side, Mary, I never thought to mention it—on the plane back, because that was a long day and then seeing these guys after the movie, it was a long night at Ned's, I don't know if I fell asleep first, maybe I did, and then Elaine fell asleep on top of me. The stewardess came back and she said, `I can't believe you two slept the way you did in that position. The people across the aisle from you were so noisy and you were sleeping so soundly, I didn't want to wake you up.` And so we slept all the way to Chicago.

When we returned to Princeton, I told my advisor what I found, that if this works out, I'm going to get moving. We arrived in Albuquerque in October of '67; and I called Mel Merritt up and told him, I'm here.

And he said, Come on in and sign in. It's Friday. If you come in Friday the 13th—

And 13 is lucky for me, a number of times it's been lucky, so I [said], It's all right with me.

And he said, For a half-a-day's work, you get two days of vacation.

I said, That's a good deal, Mel. I'll be right there.

And I met Mel on Friday afternoon, the 13th, and he said, Hey, let's go to the [Nevada] test site on Monday morning.

So we immediately went to the test site Monday morning and I saw—

*How did you get there?*

We flew. I don't remember if we went AEC [U.S. Atomic Energy Commission] charter or commercial. But I do remember seeing the lights of the Strip still on and it's getting daylight and I said, boy, this is something that I'll probably see again and again; and I did, you know, those early mornings to the Nevada Test Site.

*Right. So you flew to the test site?*

No, we flew to Las Vegas, rented a car, and went to Mercury.

*Now just to be clear, had you gone for your interview? Had you been up there or no?*

No.

*OK, so this is the first time.*

That's right. My interview was all in Albuquerque, and the first day it was in our Building 806, where I met the science people: Wendell [Weart], Bob Bass, Carter Broyles, Larry Bertholf, and

then had lunch with Byron Murphy and I can't remember if it was Howard Viney or someone, and Jerry Kennedy took me the second afternoon—I don't know if Kennedy took me the second afternoon—I know he took me in Sandia's Area 3 and 5 and we had a good time and enjoyed it. It was very interesting. And on the interview trip, Sandia did a very nice thing for me, because I had told them I was bringing my wife, they allowed us to have a rental car. We came on Saturday, and so Saturday and Sunday we had a rental car; we drove around northern New Mexico and saw things like that, so it was really nice.

*Yes. Beautiful. So you get out to the test site, to Mercury?*

Yes, Mel Merritt and I did, and just had a great time from the standpoint that we saw the CP [control point], Sedan, collapsed craters, N Tunnel, G Tunnel, where they were preparing for Cypress, and it was just a long day of seeing those things; it was a mind-boggling, long day.

So the work for me was, however, initially to try to create a calculational capability for what was going on out there. They wanted to understand better some things that go on, you know. Number one was, well, we create a cavity, when does the cavity collapse? Nobody knows. We have a line-of-sight pipe, we have flow down a line-of-sight pipe, we want to close off that **[00:20:00]** pipe with ground shock, how do we do that? And do we understand the data we have? So that was my job, number one, the flow down the pipes, that we want to protect the experiments, number one, and contain the event as much as we can, number two. And I should really say containment is number one. But they were always pushing each other, experiment protection and containment, the same problem, we would do it each day.

In any case, my work at first was in Albuquerque. I remember Bob Bass spent hours with me, and days. Initially I shared an office with Wendell Weart, and as I told you when we were walking here, Mary, that Mel Merritt recognized the two of us were always making noise and no

one's getting anything done, better separate us. So I had an office, and it was sort of an empty office with an extra desk, and Bob Bass would come in that office. Very tall, he's six-four or six-five, and instead of sitting down in a chair as we are, it was too much for him, he'd sit on the desk, lean up against the desk where he could reach the board. He taught me shock physics, from the standpoint there's not a university in the world, I think, that taught it, at least I did not know one at that time. We used to discuss, hey, this is material no one can get, and it was just interesting. He spent so many hours with me, I couldn't believe it, but what a commitment he made. It was just wonderful. And I, through the years, ended up using him as a mentor and counselor as well as talking about data that he had collected. He had been at Sandia a number of years before me and had done a lot of things.

His teaching me shock physics was incredible, it really was. It was quite clear that the calculations that we needed to do were very demanding, and we were going to have to use computer codes. Sandia had Lagrangian computer codes in which the grid moved with what was going on, and the grid easily distorted, so a two-dimensional calculation was virtually impossible. I found that very quickly and put a triangular zone right at the origin, and that allowed a calculation that was actually a model cratering calculation to run. And in fact it ran all night long. I remember the guy from the computer annex called me up at home and said, Hey, this is still running. What do you want me to do with it? I said, Well, take a data dump and let it go, and I'll be in there. I can't remember when we shut it down. He wanted the system probably because it was eight o'clock Monday morning.

And I did one-dimensional calculations, first with Dirk Dahlgren, because of just learning how to—it was all new, you know, shock physics, the codes, and everything. We did an atmospheric calculation, one-dimensional because no ground involved, no containment involved.

This is a real easy calculation and it worked well and we could see comparison with data, it looked very good at later times. And Dirk said, `Let's play something here, you know, there's a certain amount of energy in this program`, and I've never done this. In fact I may see him later today, to allude to this, that we turned the velocities that were going out from this atmospheric explosion, turned them around. Instead of positive velocities, plus Vs, we made them minus Vs because you're still conserving energy because of velocity squared for energy. So let's implode this a little bit and see what happens. Well, it imploded a little bit and it just bounced and went right back up; maybe twice that time period, the data, you couldn't tell the difference. It was interesting: energy was conserved, and that was just something to remember. As long as you have the right energy in the problem, most likely, you'll eventually get where you should be. It was just very interesting to do that simple calculation. In fact, I had the underground one kiloton calculation, one-dimensional, and this atmospheric calculation, and used those as benchmarks. Whenever there was a new technique, new code, I'd compare and say, I won't believe a thing until you can do these calculations. These were criteria that the person had to meet if they came to me, I remember working with other people, that **[00:25:00]** these were forever benchmarks, and just very valuable.

Two-dimensional calculations, as I said, Mary, were very difficult. The zone distortion would cut the time step so that we were not making progress. I did put that triangular zone at the origin of this two-dimensional grid and that calculation ran forever. We made a movie of that. I was not interested, it was not my job, but the people, the developers of that code showed that movie for years because it had a layer in the calculation; the way the pressure waves came up against that free surface and reflected off that. You'd see separations, and if we knew the material properties better, we could've actually taken this to a cratering calculation. But we

didn't know the material properties, and that's one of the reasons I went to [Lawrence] Livermore [National Laboratory], to get involved with them.

But at this time, I needed to run code calculations because there was interest in support for Cypress and Camphor, and Lagrangian codes were just not hacking it. So I talked with Wally Johnson, who used to be at Los Alamos [National Laboratory] and who was at $S^3$, Systems, Science and Software in La Jolla. Chuck [Charles] Dismukes is in that crowd. You want to talk to them also for the history, very good people.

And Wally, as I say, was at Los Alamos, and we hit it off. Maybe because of our Scandinavian background or whatever, I don't know, but he had a code that would run. It was Eulerian in which the grid stood still and material moved through it, and you say, well, there's not a lot of physics here, but it moved material. Well, he had an Eulerian code with some strength parameters put in it called Dorf. Dorf is Ford with the letters jumbled: his sons had messed up his Ford pickup truck emblem and moved things around on it, so he called this code Dorf.

But it ran forever. It was interesting and he was interested in what we were doing, and I don't know, maybe he liked Albuquerque, but he'd worked with me. Again, we hit it off. Maybe it's just that. He came to Albuquerque off and on, on Saturday and Sunday. We found out we could use the computer annex and have it to ourselves, load tapes, run them, Control Data machines, CDC-3600s, 6600s; we had it all to ourselves. We had sixteen hours on Saturday, sixteen on Sunday; come to work, thirty-two hours worked, and we were making progress. We had a little red wagon that we'd bring the printout back to my office with. In another area, Wally would pore over this, and often he'd have to go back to wherever, but I'd have printout to pore over.

And it just was a great relationship and I don't know, all kinds of cooperation. Just support that the people that had the Lagrangian capability, they were interested in what I was doing. They were amazed that that Lagrangian calculation wouldn't work until I put that triangular zone in it, and now I'm getting results with an Eulerian code, and wow! This code does run. But how good is it, you know? Are the numbers good? And I said, I don't know, probably not, but it's running. We did obtain some agreement with Bob Bass's field data, and so we were encouraged that things were acceptable.

That really was a good, good relationship. You know, as I said, Monday morning I'd just come to work, have all this printout to pore over. Gil [Gilbert] Larson, Harvey Ogden, and John Levesque, for instance, didn't come in on weekends, and they were amazed with the data information. Well, one time my wife was in labor, I was in the hospital with her and Wally was at the computer annex. He'd have a question, he'd call me at the hospital, so we were talking back and forth, and my wife couldn't believe that we were talking back and forth. But he was a great help.

Wendell Weart, Jim Plimpton, and Carter Broyles, our bosses, were ecstatic about how we were making progress. I remember we had a meeting in Albuquerque. The center of mass of this discussion seemed to be not in Albuquerque because I was almost a Lone Ranger, working with Marshall Berman a great deal. Marshall would do early-time calculations from zero seconds to twenty-eight shakes. You know what a shake is, right, Mary?

**[00:30:00]** *No.*

Oh, I thought with your—shake is a hundredth of a—boy, I've been retired now long enough, I think—hundredth of a microsecond. So Marshall would do the Lagrangian calculations, with radiation considered in those calculations, for twenty-eight shakes. He would, in other words, get

the bomb energy into the materials around the bomb and the materials around it, whether they were manmade materials or geologic materials. He would have the materials either with a lot of kinetic or potential energy deposited out of the bomb. Then I would pick up that information and run with it. And so we would go from twenty-eight shakes out to a millisecond or two or longer, and that's what we found. We, in fact, went to a few tens or a hundred milliseconds where we did have agreement with Bob Bass's field data, so we were very, very happy with the progress that we were making.

As I was saying, Carter was ecstatic with this information. Our boss was ecstatic that, number one, we were participating with this, and number two, we had this meeting in Albuquerque and that although the center of mass was in La Jolla and Livermore and Los Alamos, Chick Keller, Carl Keller, Jose Cortez, and Barbara Crowley.

[See Photo 2]

**[00:31:34]** End Track 2, Disc 1.

**[00:00:00]** Begin Track 3, Disc 1.

*Go ahead.*

I was saying Carter Broyles was ecstatic with our progress after one of these meetings in Albuquerque, and again the center of mass was not in Albuquerque but the people in Livermore. The people in La Jolla were interested and coming to see us, so we had Los Alamos, Livermore, and what I called jokingly the PCBs—the Pacific Coast Bandits: all kinds of consultants interested in coming to this meeting. Carter was ecstatic about it. In fact, he invited us to his home that evening, and you know he had a beautiful reception. My wife said, `You were really telling me that Carter is demanding and having a whole lot of work for you. Boy, he is articulate, very smooth!` He took us around and showed us paintings and

everything and I couldn't believe how she thoroughly enjoyed the evening with him. So we had some time for relaxation.

And it was about that time that we started calling ourselves the FEWOG, Front End Working Group. Mary, what we had was what I'd say a great start. In two years we had come from nothing to something; we recognized that we could be part of the players around here and have input to the system on containment calculations.

*Right. Just let me stop you for a second to understand it. So in layperson's terms, the problem is there are no calculations that can describe what's happening in these first seconds that the bomb has been detonated underground?*

Well, no. Los Alamos and Livermore were working on this. Now, we, Sandia, before me, Tom Cook, et cetera, had said we ought to be interested, if nothing else, as a third-party. Then we can help keep the system honest, that if we're going to be a party to this and we're going to sponsor tests such as Cypress, such as Camphor, you've got to have this capability. Because we do go to Containment Evaluation Panel meetings in Las Vegas, how can you sit there and suck your thumb? You've really got to be a player.

*Right. But it sounds to me like you're saying everyone's working on it but everyone's trying to figure it out.*

Yes, that's right. Everyone's trying to figure it out better because—everyone's working on this, we have a calculational problem, how far can we calculate and how accurately can we calculate? How do we feel about the results, compared to field data? However, we have the calculational capability but also material properties that go in those codes. How good is that? So there are abundant problems.

*Yes, and complex.*

And the problem was really very difficult, extremely difficult because we were going from megabars to bars. We're going millions of pounds per square inch pressures to maybe even tenths of that, so we're going orders of magnitude, and containment concerns, going over, as I said, twenty-eight shakes to maybe a second or two. The problem goes, when are you not interested in containment any longer? It's very difficult. We were interested in containment from fractions of a second, say a microsecond, to 50,000 years. So orders of magnitude are ten to thirteen to eighteen orders of magnitude in time, in which we're interested and are concerned about the containment of radionuclides. And my work at Sandia, I look back on it and I feel that of all my thirty-three years, about thirty of them were interested or concerned with the containment of radionuclides, one way or another.

*So what you're saying to me just now is something I hadn't thought of—we're getting a little bit off your script, but I want to make sure I'm clear about this. You're thinking about not only the containment of the event, but you're also thinking about containment, as you just said, many, many years out. It's all part of the same process over time.*

It's a continuing problem, and it's going to come out again, because when I get involved in Cannikin and the WIPP [Waste Isolation Pilot Plan] project in southeast New Mexico, that's where we start to hear the long times.

*OK. Very interesting. Thank you for that clarification.*

Well, as I said, in less than two years, Sandia was recognized as a contributing partner in this, and we were elated about that. And Larry Bertholf, the prime developer of the two dimensional Lagrangian TOODY **[00:05:00]** code, heard and noted what we'd accomplished. He just was amazed that Wally Johnson was doing all this for $2,400 a year, and he said, `That's the best $2,400 that we'll ever spend.` And it really was good.

I have to pick myself up here and figure what I want to talk about again. Oh, I should talk about parameter studies. Mary, there were a number of facets of this code, the material properties that go into that code, and how well can this code handle those properties, and then the field data that a person such as Bob Bass or Wayne Cook have to compare with.

We were in a meeting in Livermore, I remember, at a containment meeting in Livermore. We had been thinking about what we can do on a number of subjects, and one of them was cavity collapse. At the test site, I mentioned earlier that time of cavity collapse seemed to be a random number; it had no rhyme or reason when it occurred. And we could look at material properties, stratification, talk to geologists, talk to all kinds of people, and try to decide when might cavity collapse occur.

We decided we'll do some parameter studies, and that's one of the things I should also mention, that we did a lot of parameter studies because primary material properties were not known, so we tried to bracket things, and the codes are not that accurate, so that we tried to bracket the phenomena that we're talking about. I remember cavity collapse was a concern of the system, mostly the vertical-hole people; not what I was doing with Sandia. But we thought, well, we could turn our calculations on end and do a vertical calculation, cylindrically, symmetric, switch everything we had that's two-dimensional, and maybe we could put a plate above a detonation and see if we could get some energy to go up various-sized pipes; do a parameter study on this and stagnate some energy against this plate up there. I recall that we did a calculation of a few-meter-diameter plate by a meter thick, and we tried to stagnate energy against this plate and see if we could blow down the cavity. We could offer this to Los Alamos or Livermore, their tests were mostly vertical-hole—in fact, I think they only did vertical-hole tests—and see if we can offer them something.

We were at this containment symposium in Livermore and my paper was coming up after lunch. I went to the microphone, the podium, and I noticed that post-luncheon lethargy must be setting in because most of the people were way in the back of the room. But Byron Murphy, my director, was way down front. And I didn't know what to make of that, but I started to describe this cavity collapse parameter study, and I came to the end of it and I said, In view of unknown factors and everything like that, that we really can't say definitively this works, although it looks promising, that I guess I'll have to adopt the old surgeon's song, "when in doubt, cut it out." And when I said "the old surgeon's song," Byron must've known it because he drew a finger across his throat. "When in doubt, cut it out." So it was well orchestrated, I'll put it that way.

As I said, material properties continued to be a problem. I went to a number of Gordon Conferences. I remember a Gordon Conference I went to—I should say a number of, I guess, two—one was with Jerry Wackerly and Bob McQueen from Los Alamos who showed some of the messiest viewgraphs you've ever seen in your life. But they had new data, just hot off the presses, so to speak, or hot out of the lab. Just impressive data that they had, very high-pressure data, megabars. Incredible. Lawrence Livermore had some very good people there: Hugh Heard in particular. I remember talking with him quite a bit. They had some really beautiful labs that they were starting to develop. Sandia was there and we had lower-pressure data, very good graphs, very well done engineering work. And I sort of characterized **[00:10:00]** that, that boy, Los Alamos really is doing the work, but Livermore's coming on.

And as a result of that, I was quite interested in what we were doing in Livermore. Jose Cortez was the primary Livermore code man, and he and I, I thought, got along really well in doing our calculations. It was just a future interest that I was interested in pursuing. However, the calculational capabilities that we had in Albuquerque were about to improve from the standpoint

that Sam Thompson, a fellow who was on the same floor in Building 806 with me, would observe my success, well, I think my frustrations, when those Lagrangian codes would not work for me, and then the Eulerian codes did work. He was just impressed because on Monday morning we would come down the hall right in front of him and we brought up our printout with this little red wagon just stacked with printout from this Eulerian code. And I think he just took—he was a very bright fellow—just took note. Eulerian codes are really, really good and these engineers are doing something, and he's a physicist and really in code development. He wrote a mixed Eulerian-Lagrangian code—it was very successful—called CSQ, and it's used even today. And unfortunately he's one of those guys I mentioned to you earlier who has passed away—you've got to talk to people before they disappear, we're all on a one-way road or one-way street. But he was a great guy, and in fact we would go drink beer on Friday evenings, if I were in town. The code developers were always at the Coronado Club drinking beer, but if I were in town, I'd try to join them. Sometimes it seems like Friday evenings I'd do as much work, as far as code development goes, as I could do all week because just talking with Sam, Larry [Lawrence] Buxton, and Dirk Dahlgren, you'd learn a lot there, plus Randy [Randall] Cole and Frank Biggs. Those were just some of the people that were just very helpful.

So I felt like I was in a privileged seat, from the standpoint I was a user and I had the code developers on my right side and the people interested in the results on my left, and there's just a pass over here. It was just neat, all kinds of support and I certainly enjoyed it.

But you know Cypress went off shortly after I hired at Sandia. Well, my first nuclear event was really Gasbuggy. I got on a bus on December 10 or 12, I can't remember, in 1967, shortly after employment, went up to northwestern New Mexico, observed Gasbuggy, and I have

a picture of that with Milo Nordyke and Leo Brady and myself and Al [Allen] Church, I alluded to you earlier, from the arming and firing business.

[See Photo 1]

But Cypress went off, a horizontal line-of-sight pipe. It was very successful. Conservative stemming plan. Two years later we had Camphor. We'd done a number of parameter studies, as I recall. I was trying to recall these things, we had done a number of parameter studies. If I recall correctly, we tried to use a larger pipe, less stemming—I don't know if we were a lot smarter or cocky or what—and closer-in experiments.

In any case, the nuclear event Camphor was June of '71. It contained for about one minute. Then the underground workings were lost. I was pretty depressed and discouraged, and this was coming right after Baneberry. This is a name you're probably going to hear time and again in your discussions here. I personally was quite distraught. Here we have Baneberry and now Camphor. Did we understand—?

*Well, back up a little bit to Baneberry. Did that affect you guys at Sandia, what happened at Baneberry?*

Well, yes, I had to suit up. I had moved to Livermore at that time, you know. I allude to my notes here, I said, this is where Jim Carothers's book *Caging the Dragon* is really appropriate reading, because you've done all this work and it still doesn't work. Really discouraging.

*OK. So tell me a little bit about Baneberry because that's—*

That's not my event.

*Pardon? I know it wasn't. OK. But what did you have to do in relation to it?*

**[00:15:00]** Well, just only, Mary, in visiting the tunnels after Baneberry, there were restrictions. We had a new road, which is an improved road, to go around, and things like that. I don't want to

say they improved for me, but there was a new road and we could go on. Had to suit up sometimes and things like that. Depends on what you wanted to do.

*What was the reaction when you heard about it? Because you're thinking about containment all the time, right, and then this event happens.*

Are you going to edit four-letter words out of this tape? Oh, damn! You know, I really was discouraged. And then we're going to do better with Camphor, and we did not. That was a double damn, or something like that. Yes, it was discouraging. And you know I figured, well, what can I do? I was very involved in this—I wanted to—I thought we had the codes. Sam Thompson is developing codes. We had material properties that were not—oh, I should tell you a little story here. I was concerned with material properties, but I'm thinking back at Camphor. I personally didn't have any responsibilities on Camphor, but I do remember an incident that was quasi-humorous. Carter Broyles had a meeting in his office and we had particular materials that we wanted to have around the device, and no one in the room could assure each other, or certainly Carter, that we had 100 percent assurance that everything was in shape. And I don't know if it's because I was the youngest, the smallest, I don't know; or knew the Livermore device people, or understanding what we were doing from a physics standpoint, engineering standpoint, in fact from a construction standpoint as well, plus understanding the difficulties of what I was doing, or maybe I talked too much, or maybe not enough. In any case, I was elected to go out and see Camphor before they buttoned up the next day. And I remember going, stopping in Las Vegas, picking up some oyster crackers from the Thunderbird because I knew there wouldn't be time for lunch with everything I was going to do with this device physicist from Livermore, Bill Scanlan, who I knew. By the time I got out to, that would be G Tunnel, people were either having lunch or had lunch and were gone, and so I'd missed lunch or—yeah, I

had missed lunch. But the Livermore physicist had hooked up a periscope so we could look in around the device and see that indeed that materials were there. But neither one of us could see certain areas and neither one of us could assure ourselves that things were attached well. So I went in there, and it was tight quarters, and assured ourselves that. I can't remember if I called from there, I don't think I did because I didn't have a cell phone, certainly, this is 1971. I got out and called that things looked good. I remember I plopped down on some sand bags there to have my lunch with those oyster crackers from the Thunderbird, and a few minutes later there's a horrible *kaboom!* And people around me took off running, and I got up and started running. I went about a half-a-dozen steps and I said to myself, wait a minute, if this thing went nuclear, I shouldn't be running, so just relax, guy. And so I just wandered back to my sand bags and I see there's a whole lot of white smoke coming out of the room and the Livermore people are there, and I asked what's going on. They said, `Oh, somebody shorted out some explosive bolts and the fire retardant went off.` You know they didn't want a fire underground ever and they're dismantling because they're going to stem, then have the nuclear event in a few months after the concrete cures. So there was a little event. I **[00:20:00]** remember that *kaboom!* It just was a surprise.

*Well, before you go on, so you had to go some distance, you said you went all the way down the tunnel to where the device was.*

Yes, right. It was, you know, a mile or two underground. And I had to drive from Mercury.

*How did they take you down in the tunnel?*

Well, you hike it, you know; sometimes you hike it. Certainly after hours you hike it. But I was famous, or infamous, for catching rides on a motor, you know, or the "troop train." I'm lazy.

*That's what I'm asking you. OK, so you'd hop on one of the mining trains or something?*

That's right. Exactly, if possible. You know, in this case, I'm not sure what I did.

*No, I just meant generally.*

Well, probably what I did, Mary, was on the way in I probably hiked in because there was nobody going in, and people were coming out for lunch, you know, and then they're not going back in yet. I got down there and the device physicist is there, and everyone else is gone; although in the Livermore area there apparently were guys there working because they were dismantling, trying to get out, and I did not know that when we had to do our periscope thing. So when I came out, I trust I had the oyster crackers, et cetera, but I imagine I ate oyster crackers until I found a lift out, but I'm not sure, because after a while, I had to report to my office; it is a long hike.

In any case, after Camphor, I'm trying to recall, I did this mostly chronologically, thinking my history through, and it seems at this time all I could remember was—I remember going back and I told Bob Bass the story I just told you with kaboom, I told Bob that if I die in the next few weeks, tell my widow what happened.

An opportunity came up after Camphor, if I remember correctly, in which we were headed for Cannikin with obvious concerns about the containment of Cannikin, being a very big test, potentially very controversial. Our prior events of Baneberry, which people were very well aware of, and Camphor, that I was very personally aware of, concerned about or disappointed or whatever, just was a downer. Cannikin came along; Wendell Weart was appointed as chairman of a containment review effort for Cannikin by AEC/NVOO. And Mary, now that I think of it, you'll have to do the separation of AEC, ERDA [U.S. Energy Research and Development Administration], and DOE [U.S. Department of Energy].

*We fix that in the transcripts. We always do, yes.*

Thank you, because I'm alluding to DOE here, something like that, and I should know it was AEC.

*That's all right. We fix those.*

In any case, Wendell was chairman of that committee and he was appointed by AEC/NVOO, NVOO being Nevada Operations Office here in town, and Wendell had on that committee professor of hydrology Paul Witherspoon from UC [University of California] Berkeley, who was very insightful and entertaining. When we went out to dinner, he'd even say, `I will make reservations.` He was a big boy, a big bald fellow. It was wintertime and I remember he wore a Russian-like hat; I think we were in Denver. `I will make reservations. I'll make like we're Russian visitors and we want a private room with lots of beer.` You know after twelve, fourteen hours of deliberation and we're going out for a late dinner, you were ready for some humor, and Paul was really just fantastic at providing it. And you know he became a lifelong friend. I remember seeing him in Spenger's in Berkeley and we talked a number of times, just really neat.

Also on the committee was Paul Orkild, if I remember correctly, from USGS [U.S. Geological Survey]. The final delegate I cannot remember. I can see him, a bearded fellow not **[00:25:00]** much taller than I [Bob Deupree from LANL]. I believe Russ [Russell] Duff from $S^3$ was on that committee, and Russ would be quite articulate if you want to talk to him, for sure. I can't remember the other people. Wendell will remember.

*That's all right.*

In any case, it was quite an intense couple of months. It ended with an opportunity to go up to Amchitka Island [Alaska], and I don't remember, I think we were up there ten days or something like that. The weather was good and it was neat. You'd go to from bunk to chow hall, never have

to go outside, and the chow hall was open 24/7, you know, if you wanted meringue pie at midnight, you could go get it, whatever you want. It was really neat.

Before I forget, for background, Mary, there is a novel, if you're looking for interesting things to read over the weekend, *The Thousand-Mile War*. It depicts life on the Aleutian Islands during World War II. *The Thousand-Mile War*. Very good novel.

Mel Merritt with another fellow, I think from PNL [Pacific Northwest Laboratories] I'm not sure, co-authored the environmental study for Cannikin. I think he did some others, Long Shot and Milrow, too, prior nuclear events on Amchitka.

Well, we were all concerned about through going faults up there. We mapped, we thought there were faults there. USGS people helped us. We were talking about trenching up there for—boy, I'm trying to dredge this back in my memory—for faults there that might go through the emplacement hole or through the cavity itself. I can remember being concerned about that because water was available there and here we're all of a sudden worried about containment from just a few microseconds, as I alluded to before, to fifty, a hundred thousand years. So containment is over many orders of magnitude in time. It's a large problem. Very high pressure failure, or a slow seep type of problem. So containment comes in many versions and demanding time frames to cover it.

*Can I ask you a question about this? I'm maybe going a little off and you can tell me if you want to wait to ask this, but you seem to have had some serious concerns about Cannikin, so the benefits or the needs for it must be a given for you guys, right? But if you feel like, you know, the whole point of containment is if you don't think it's going to work, then you're obliged to say that the problems are too great?*

Well, certainly. [The need for Cannikin had been accepted; we were tasked to judge the containment design and procedures.] And, well, I was very concerned on Cannikin. We had a meeting, I think before we went to Amchitka when we were just starting to conceptualize, what if a fault went through the emplacement hole? What if a fault went through the cavity and the cavity is making water [filling up] already? We could have an interesting situation here. And Dick [Richard] Heckman from Livermore did some calculations—very interested in that—and I can remember we asked a whole lot of questions one day, and the next morning Dick came in with a big stack of printout and plopped it on our meeting table and he said to all of us, `Well, the answer's in here.`

And I remember asking, `Well, what do you think of it?`

And he said, `I don't know, I haven't had time to look at it.`

So we pored over the printout and tried to convince ourselves that indeed that the calculations are giving us some confidence that it should not be a containment problem.

And that brings up the other thing. I just said something that—after our deliberations for let's say six weeks, I can't recall, and then the site visit in which we tried to get what I call ground truth, get a feel for really what's going on there, we came here to Las Vegas and met with the AEC lawyers. We sat there in a beautiful, beautiful office, I hate to say even nicer than this plush office, well, mahogany door and just, oh, beautiful. We were behind these mahogany doors and they really did not want us to say what I just did. I said "no containment problem" or something like that. They said, `What if the word "no" is left off the transcriptions and things like that are lost?` So we tried to phrase sentences so that if a key word were missing, the sentence would be gibberish, didn't make a lot of sense otherwise.

*Amazing!*

**[00:30:00]** You know, if one word in "no containment threat," if that "no" is missing, then it's a containment threat, and people pick up on that, so this type of thing we didn't want to send forward to Washington to be read there. But it was interesting, just very interesting.

*Now what about the test itself? You were back in Albuquerque?*

We were moving to Livermore. We were in Albuquerque because I remember it might've been the last day we were in Albuquerque, because I remember I filled the bathtub up; I figured I'd make a passive seismometer to see this big event in the Aleutian Islands. Will we see anything in Albuquerque? And for the life of me, I can't remember if I saw anything. I think I did not.

*So you knew about the time, then.*

Yeah, I'd made a passive seismometer for Gasbuggy. I was much closer to Gasbuggy in northern New Mexico. And that little stack of rocks fell down.

*Did it?*

Yeah, I had a passive seismometer. You know, I didn't do anything. There was no beer. There was nothing. It was cold up there. It was December 12th or something like that. So I stacked rocks there. I have a picture here, I'll show you, of the guys there. And that passive seismometer worked, but I don't think my bathtub experiment did. At least I was there at the wrong time if it did, because I had done some calculation when this arrival should be.

At that time, as I told you a few minutes ago, I was very concerned that although we have calculational capability, we do not have good, high confidence in our material properties, you know, garbage in, garbage out. If you don't know, why calculate? So you wanted to get as good material properties as possible. And so I was interested, I told you before, that it looked like Livermore had the capability. I know the people at Sandia Livermore across the street where I'd be working had the capability. It was high temperature and not high pressure, but with

modifications we could really work with the Lawrence Livermore people, and that's the type of thing that Tom Cook was interested in us doing. It's one of the reasons he talked to me about coming there, that he wanted greater interaction with Livermore and he was aware of my success of working with the code people at Livermore.

And as it turns out, we had—this is 1972 or 1973, you know, after Cannikin—what did we have? We had budgetary problems, we had headquarters reorganization, and I told you about the please keep separate the AEC-ERDA type of things. We had all kinds of reorganizational changes. At Sandia there was concern. Tom Cook created a Future Technical Activities Committee, FTAC, and he asked me to serve on it. Well, that's another 100-hour-a-week job, but we wanted to find work that Sandia Livermore might have for the future. We wanted to just look at the whole spectrum of things, and as it turns out, some things that were started then, such as combustion engineering, that really provided Sandia Livermore, our Sandia California branch now, with work for a number of people.

I served on that committee, and after a number of weeks, I can't remember if it was six weeks or something like that that we were given maybe two months, my proposals may have been boring but I kept pursing them. And we'd meet with Tom Cook every Monday afternoon in his nice office and talk about these things. He thought mine were boring, but I was promoting radioactive waste management, fission reactor safety, and a change for handling staff members similar to what Bell Labs had done. They had a position of Distinguished Member of Technical Staff, DMTS, and I wanted to invoke that because there were people who were not management material who wanted to be managers and who recognized managers at Sandia were getting greater pay and they wanted greater pay. They didn't necessarily want to be a manager, OK, and so let's recognize those people. I had a fraternity brother at Bell Labs in Whippany who had been

my little brother, one of my five little brothers—how in three years can one guy gather five little brothers? I don't know.

*You did it.*

I worked with people, yeah, I guess. Just an aside. He was at Bell Labs, I talked to him and it's just a program that's been very successful there, he thought, and I thought Sandia should **[00:35:00]** adopt it and they did, so I felt good.

And radioactive waste management today at Sandia accounts for quite a bit of work at Sandia. [I felt we should try to avoid a layoff and rehiring people in a whole new direction. Instead, we should look at technologies that will use the people we have.] I thought radioactive waste management and fission reactor safety were two endeavors we could go into and still use the people that we have, keep them interested.

And so I really did push those things. There were all kinds of things going on, the materials people wanted me to go to Idaho and talk collaboration in beryllium work to the—I can't remember what we were doing with beryllium other than—I'm not sure it was a big FTAC thing. I think the beryllium work came after my FTAC proposals. I was in a materials group. I don't recollect specifically why we were there, but I do remember that everything I did up there was wrong. High for the week was 12 [degrees] below zero, minus 12, and we were staying up late, and I was not dressed warmly enough. We were wining-and-dining guys there to promote our capabilities, trying to find out what INEL [Idaho National Engineering Laboratory] had, et cetera. And in the back of my mind, I was looking for Sandia work too, in addition to beryllium.

Well, I was underdressed, staying up too late, taking hot showers in the morning, still tired, so I was going from being cold, wet, and tired to hot and tired, and just sort of a recipe for disaster. I came down with double vision, and then approximately two weeks later, the lights

went out. I had total blindness. A neurologist in Oakland told me not to do anything, that he diagnosed MS [multiple sclerosis], and he said in cases like this, he suggests that we do nothing, that it will get better, and it did. I, in fact, saw him recently and said the neurologist I have in Albuquerque now wants me to start these injections, and he said, For thirty years you have been doing well and knowing you a little bit, now I don't think I could rely on you to give yourself an injection every day, or even a deep injection once a week, so I wouldn't start it. He said, Once you start it, you have to continue it. And so I've not, and I've been good since 1974. But the lights went out February of 1974.

*Yes, what goes through your mind when something like that happens? I mean, my God.*

Well, you know, you pray a lot. My wife and partner for many years, has been there for me; made me high-energy milkshakes and I got lots of rest.

*But she wasn't up in Idaho when this happened.*

No, she wasn't up in Idaho with me. Getting home was—you want to hear about that story?

*Sure.*

Is there enough tape? This is not related to containment, Mary.

*Well, let's stop. Let me pause.*

**[00:38:48]** End Track 3, Disc 1.

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

I rest, trying not to get too hot, too cold, too tired. And I've done well.

*It's amazing that that was that many years ago and you're doing OK. Wow. How long before your vision came back?*

About three weeks. Oh, I was going to tell you the story. Very quickly. I was in Idaho, and I was looking at the blackboard, and a Sandian, Harry Saxton, wrote Be for beryllium on the board, I

saw two Be's. I went to the men's room, trying to figure out which eye was involved. Couldn't. That was sort of discouraging, to figure out which eye is not moving. So this guy who had written the Be's said `Hey, I want to leave.` So we left Thursday night. He wanted to go skiing. This was the first or second week in January, 1974. He wanted to go skiing, wanted to be home Friday and not be traveling, so he could leave Friday night or Saturday. So he drove to his home. Do you know Livermore?

*Yes, a little bit.*

OK, Holmes Road?

*Yes.*

Harry lived on the other side of Holmes Road from us. I told Harry, `Don't worry about it, I can get home.` I got on Holmes Road, and it's after midnight, and as luck would have it, it was drizzling, and I met a car. I only had to go like a quarter of a mile to get on our street, just a very short distance, and there's a car. And oh, no, what do I do? The headlights are up here and headlights are down here. Which ones are the real headlights? I grew up in upstate New York, as I told you, near Cornell, but Cornell meant Watkins Glen, so racing was in my blood. So I spun the car around, so if he hit me he was going to hit the rear of the car. He went by me and I just limped home. End of story.

*Yeah. Well, you were lucky.*

Yeah. In any case, thank you, Mary. I'll try to speed this up a little bit. As I said, this—

*That's all right. Well, look, you can't get a whole life in an hour and a half. Do you want to go on and talk about those tests?*

OK. So when you're ready, go ahead.

*Go ahead. We can always have a follow-up. Let's just get what you need.*

OK. Well, that's right, we can do this and I can embellish, is that a good word?

**[00:05:00]** *Yes, well, embellish or you're really giving us some of these real-life details that are so valuable.*

Well, that's one of the things that's interesting, because I find in reading dialogues like this, what's interesting, the technical things are interesting. Whatever the person's doing, the little depth of it, the ins and outs of various and sundry things, that's what's really interesting.

*That's right.*

Well, it was interesting, after discovering the MS situation, I was recovering from that and Jerry Kennedy from Albuquerque called me. He needed help, desperately needed help, because he needed a background such as mine, broad background with some mining experience, like I had at the Nevada Test Site. He and his buddy Bill McClain from Oak Ridge [National Laboratory] had failed to complete a report for ERDA, I think it was ERDA, and they needed to get it done, and would I be interested in coming and meeting with his committee in Albuquerque on coal mining? Because they wanted to gather their thoughts and write a report as soon as possible. Well, I had a friend in Livermore, at Sandia, who was a tech writer. I brought him with me, because we were going to do this quickly. We met with the committee in Albuquerque: there was Ralph Carter from Argonne [National Laboratory], a fellow from Pacific Northwest, Bill McClain, and Jerry Kennedy. There may have been someone else, I don't know. And you may, if possible, want to talk to Jerry.

I was interested in doing this work and so, as I say, I brought the tech writer and we met with the committee and we took notes and we wrote drafts until Friday afternoon. I'll never forget our flight back to California; Del Rasmussen, the tech writer, sat on one side of the aisle, I sat on the other. I read the report for technical content, and Del read it for presentation and

grammatical errors, which I'm prone to. We got off the plane in San Francisco, called Jerry Kennedy's secretary Dodie McKelvey made the corrections, and we had the job done in a week. I really, really enjoyed that little effort. So coming off the MS thing, it was just a breath of fresh air, and working with Del was really neat.

You know, I had an opportunity to think a lot about what was going on at Sandia at that time. We had gone through these layoffs, and a few years later just came to, I don't know, what do I say, a threshold. I was motivated to write a letter to my management, to Vice-President Glenn Fowler, as a matter of fact. I wrote this letter and sent it up my line. It suggested that Sandia make a real effort in developing a capability called geophysical engineering. I can remember discussing the letter with Bob Bass and George Barr and that I didn't hear anything back from the letter. Orval Jones stopped me in the hall one day and said, `You haven't heard anything, but let me tell you that indeed we are considering your letter very, very hard and long.` And I felt really good that they did elect to start the earth sciences. Really, that's what I was looking at, was Sandia getting into earth sciences. I called it geophysical engineering. And I would say, I don't know, we could check, it accounts for the employment of a few hundred at Sandia today, so I feel very good about that.

WIPP came along right at that time in 1975; WIPP is Waste Isolation Pilot Plan. It had various and sundry names. In late 1974, we met with the U.S. Geological Survey and Oak Ridge who had that charter originally. DOE thought that Sandia being an engineering laboratory in the state of New Mexico might be the laboratory to pursue the project within the boundaries of New Mexico. To make that story short, the WIPP site evaluation is summarized in our Geological **[00:10:00]** Characterization Report. Each of the two volumes is the thickness of the Albuquerque Yellow Pages. It's interesting, Mary, from the standpoint that both WIPP and Cannikin had this long period of containment that we're concerned with. They were talking about 50,000 years and

beyond, and start at the early times for Cannikin where we're down in fractions of microseconds or, as I alluded to before, milliseconds. WIPP early-time containment is a man-made problem that we have to handle this stuff well.

[See Photo 3]

In 1980, the services, I think it was the [U.S.] Navy, came along with the DNA, Defense Nuclear Agency, the primary sponsor of interactions with Sandia and asked us to create a closure for Nevada. The fast acting closure [FAC] was supposed to close a thirty-inch diameter pipe in a millisecond and create a plug larger than thirty inches, so it's a real cork in a bottle. I was asked to be the so-called technical director of these activities. [That's the type of thing that I did for I think most of my activities in the future. It started with WIPP to make sure the program's parties are informed and the work is getting done.] The short times of this closure and just the sheer masses meant that surely we were going to have to use explosives. It turns out this explosive closure scaled very well, so we could do tenth-scale, little pipe closures, and just get up to thirty inches, which is two-and-a-half feet, you know, that's a big closure. And so it really did—it was calculable; Bob Bass did calculations. It was scalable; Paul Cooper was doing the experimental work with Harold Walling and George Staller. It was just a good project, that we were getting our hands on the thing, you understand what's going on.

[See Photo 4]

I remember we met on a Christmas Eve or thereabouts. Bob Bass had this calculated weakness at the end of the run of HE. When the explosives ended, that's where the calculation ended up with a weakness. So if we started the detonation at one end, we ended up with a weakness at the opposite end. Well, it was interesting. At first, that didn't seem logical, but when you think about it for a little while, we said, maybe it happens because you're driving this

material very hard and then all of sudden you're not driving it, so it's logical. Paul Cooper sectioned his experiments and found the same weakness. I'll show you pictures here that I brought: that here we have experimental weakness and the calculations show the same weakness, so we ought to use this to advantage, take these chips out here, glue them back in so from a shock physics standpoint, it looks like the pipe, but when the explosive relief wave comes along it'll blow those chips away and we'll end up with a bullet-shaped nose, an ogive-shaped nose. Well, this, in fact, functioned very well, and we mounted it to what we called a hard strong pipe or hardened pipe section, and it had telescopic sections. We were able to place the FAC fairly close to a nuclear device. [An effective thirty-inch diameter closure tremendously increased testing capabilities.]

[See Photo 5]

And so going from developing FAC, we wanted to take it to the field and find out what might the field data look like. So number one, Diamond Ace was an event to find out what the **[00:15:00]** field environment might be close in. The date on Diamond Ace was September 23, 1982. And we just measured pipe flow, just to see what this FAC might encounter. In Midnight Zephyr, now this comes very quickly afterwards. We go from September '82 to September '83. [See Photo 6] So with these low-yield tests, all of a sudden you have the opportunity for a quick turnaround. They're smaller, so they're not as demanding of real estate or construction materials. They've got to be less expensive if you can do one every year. It's just a great idea: less expensive, you can change things around for programmatic needs, change devices, et cetera. There's all kinds of advantages, and all of a sudden we're able to calculate things. There was just a whole lot of excitement about the thing, that we go from Diamond Ace to learn what the pipe flow might be like, what the free field environment might be like, to Midnight Zephyr in which

we have a pre-closed FAC there to see how it takes the flow. We closed it explosively outside of the tunnel and brought it in and put it in the pipe string so the effluent from the nuclear device came down the line-of-sight [LOS] pipe and whammed into this FAC to see how things survive. [See Photos 7-12]

And then Diamond Beech which was a full-blown, low-yield, line-of-sight test with a witness plate behind the FAC. So here we have a low-yield test, pipe string, with a witness plate behind the FAC, attached to the strong, hardened pipe section, and it worked extremely well. We had data that compared well with calculations, and I said, we're really on to something. And these things were coming so fast, Mary. For instance, Diamond Ace, '82, September. Midnight Zephyr, September '83. And Diamond Beech, one month slip, October, that was '85, but still they're coming with quick rapidity.

[See Photos 13-17]

And I brought something to the table, that I just referred to. When I'm working with people, especially in Nevada, because they're so strung out, well, in WIPP it was the same thing. We were strung out with some people in Albuquerque, some people in Carlsbad, and some east of Carlsbad thirty miles at drilling sites. I started sunrise services at WIPP. I would have sunrise services Monday mornings because some people are going to catch the plane to go down to Carlsbad. We had sunrise services to talk over problems, what's going on, so the staff is informed. For me, I would bring the Sandia story to them. I make sure everyone knows the news (and rumors), you know, what's going on, and I want to hear from them what's going on so that if I have to brief people during the week, I am aware of what's going on. I'm up to date. And it's the same thing at the test site. I talked to the chow hall manager, he gave us a room to sit in, to have breakfast on Tuesday mornings, I think it was Tuesday mornings. We'd just sit in there,

Los Alamos and Livermore people being there as well, because they were providing advice and they were interested in what was going on, but mostly the Sandia engineers. Well, everyone involved in these tests, Diamond Ace, Midnight Zephyr, Diamond Beech, you know, discuss what's going on, progress and trouble spots.

**[00:18:52]** End Track 2, Disc 2.

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

OK, well, now we have low-yield tests, Mary, that there's a capability that we can calculate, and I'm very excited that that's a capability, and we used it. And as it turns out, I did other things after that, I can't quite recall, I'm sort of leaning back, trying to think about talking with you, and I could remember the thing I really wanted to do, because they're talking about ending testing, and I always wanted to be involved with a cavity event. The cavity events were across the access way in the tunnels, I see Al [Albert] Chabai, Sandia's technical director for cavities, and what are you doing over there, Al, you know, and talk to them. I was very interested. He had the last cavity event, I think Diamond Fortune is coming up.

*Wow. Yeah, we can look. I should know that off the top of my head, what the very, very last event was. Let's see. Diamond Fortune is—[referring to* DOE/NV—209-Rev. 15.]

That's all right, you can tell me.

*And then it has a few more coming down, but they look like they're different kinds of tests. So Diamond Fortune is April 1992.*

Right, and I really wanted to do it, and it turns out there was a prospect for two add-on experiments on Diamond Fortune. Well, let's do this, if I can get involved in it, and they seemed to want the help; same type of technical-director situation, that Sandia would collect the data if we understand what you guys at DNA want. [In particular, Bill Summa, who we have worked

with and on many events and experiments.] My position was similar to what I've had all along since, well, really, going back to WIPP, which was not with DNA; that was southeast New Mexico. But again, a project management type of technical director responsibility.

There were two add-on experiments they were talking about, so let's do these last-chance type of things; RNET, Reusable Nuclear Effects Tests. Here we have something else, Mary, that's interesting, we've gone from testing—I'm backing up now. We're going from proverbial testing that has grown up. I used to go to these meetings at NVOO and we talked about past experience, and those tests were based on experience. I would always chuckle because people would talk about past experience. What is experience if it's not past? I thought that was very redundant—those test beds, higher yields, those were based on experience. Now we come along with low-yield testing, and I'm excited about that because all of a sudden we're understanding more. We're opening up the pipe, we're getting data, saving money, and now, in this last cavity test, the prospect of something else: reusable. Wow! Not only low-yield, but reusable. This is interesting. How are we going to do this?

And so we had two add-ons, one with Los Alamos and DNA, Defense Nuclear Agency, called reusable nuclear effects, RNET, I think it was reusable nuclear effects test. And **[00:05:00]** it was a proposal by DNA, and the primary calculator was Charlie Snell from Los Alamos. And Charlie was doing, I can't remember, but he was doing computer calculations. And his calculational results were not agreeing with the DNA/PAC-TECH people who were doing more engineering calculations. We had a number of meetings in Albuquerque that would start early and go late. A week of them, you know, Monday, Tuesday, Wednesday, and I think it was Wednesday afternoon when I said, `Would Sandia, as a third party, make a difference if we could do some calculations?` And that's been Sandia's position, and maybe I did

mention it earlier, that Sandia's position often has been as a third party. And they did want us to do the calculations, you know, we were at loggerheads. We were just not making progress, and we had to because we were late-time add-ons. We had to make progress. We were going to have a design meeting in La Jolla at PAC TECH's offices on Monday morning, and here it's Wednesday afternoon, could I do a third-party calculation? I went to Sandia late Wednesday afternoon and talked to the finite element people. Not a code that I could handle quickly, I did not have that capability readily available, but our finite element people did. And they did the calculations. I met Charlie Snell on the airplane Sunday night going to La Jolla, and I showed him the results, and I'll never forget how elated he was—excited with the correlations with his results.

So we carried the day. And this is interesting because we carried the day with that design in La Jolla. We finished that meeting by noon, I went to Las Vegas, had a meeting with the seismic people here in town, Leo Brady and his guys, what we were doing there with the Sandia Seismic Net, and that night I had dinner in Mercury with the other program manager, Francois Heuze, about the work we were doing with him. Francois's from Livermore [LLNL], and he had a program called CONVEX, COntained Nuclear Vessel EXperiment. He had been doing experiments in Livermore with HE [high explosives] on a plate, and looking at that plate's survivability as far as, again, a reusable situation; have a cavity, line it with these plates that he was proposing, and you might be able to reuse this cavity. So we had a plate, I'm trying to recollect, I was looking at our photo, I think it's five feet by five feet by one inch thick, and we pulled it tight against the Diamond Fortune cavity wall; it had a center bolt two inches in diameter. Each corner had a one-inch-diameter bolt. Those bolts were thirty feet long and they, as he would say, daylighted from there. They came out into a gallery which went around the

cavity, and we could tighten those bolts from there. I recall we could get up to the 60,000 psi [pounds per square inch] tension or whatever it was in these bolts. So this plate was really pulled to the wall. The center bolt was in a tube of viscous grease so that it would not grip the mountain, so to speak, when we grouted it in; Francois, the designer, wanted it to be free to move. Bob Swift, LANL (was LLNL) and Sandia did calculations for this experiment, and the data from the experiment overlaid our calculational predictions for many, many cycles. Francois was so excited about it, he published, I don't know how many times he published it, at least three times. [See Photo 18]

Well, long story made short, and maybe the end of this monologue, we [Francois Heuze, Bill Barrett, the Sandia experimenter, Bob Swift, LANL [had been at LLNL] and Sandia received an award from the National Academy of Sciences [NAS]. We actually received the award from a committee, the Rock Mechanics Committee, that reports to the National Research Council, the NRC, reports to the National Academy of Sciences. So I lean back in my office at home and say, I have an award from the National Academy of Sciences. But it's really from a committee of NAS. But those results were fantastic. Yes, it is fantastic, results were fantastic, because we have codes that can calculate, Mary, that was neat. Although I'll admit, we were, you know, in steel. We weren't measuring in the grouts and the host rock. That's still a problem.

*Right. It's so complex. It must be a huge problem.*

**[00:10:00]** Yeah, it is. It really is.

*But you get it on the steel, so that's amazing. OK. Well, that might be a good place to stop for now, unless there's something you just want to say at the end. I think that's a high note.*

I thought after starting an endeavor for Sandia which seemed intractable, and in two years being a contributing member of that activity, and after, what, thirty years, receiving an award from a

National Academy of Sciences committee, that was great, too. Great recognition. [I believe it was a first for work done at the NTS.]

*Congratulations on that.*

Thank you.

**[00:10:55]** End Track 3, Disc 2. [End of interview]


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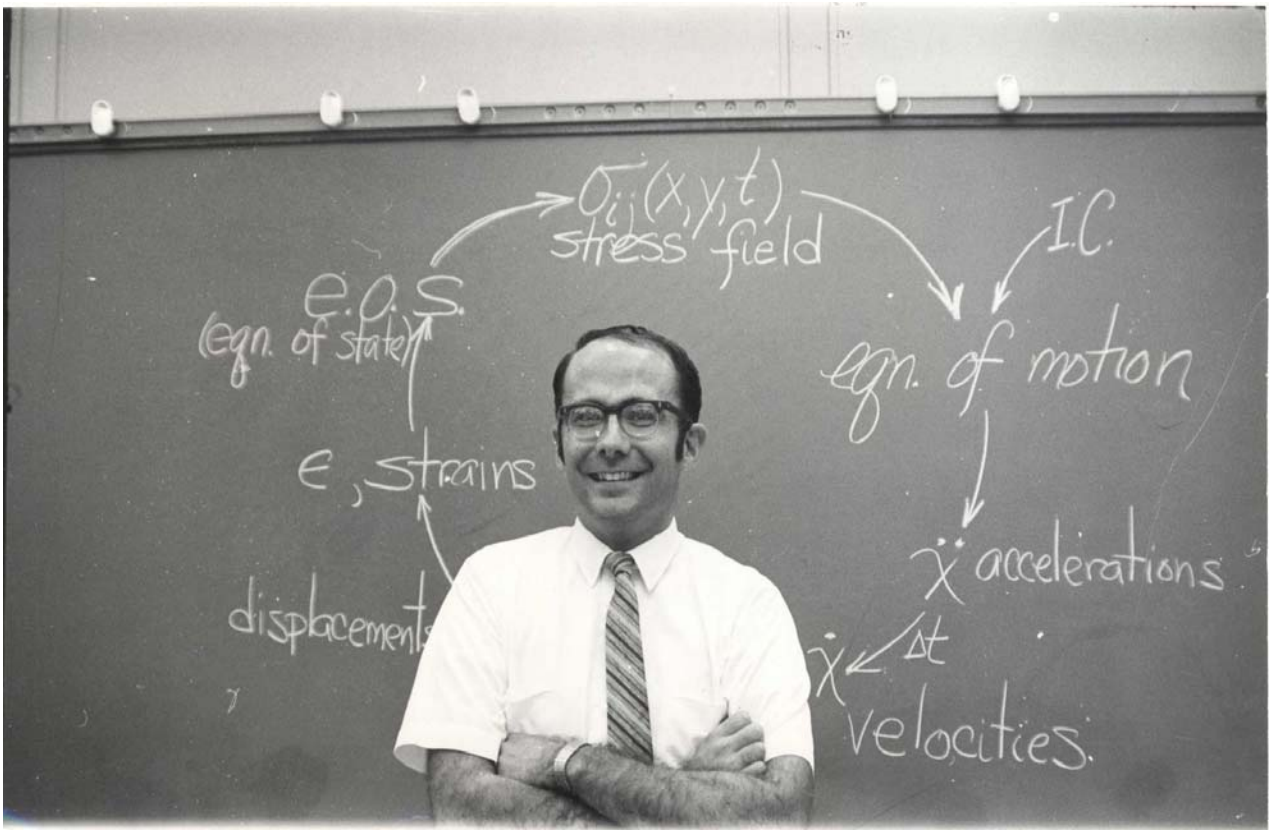


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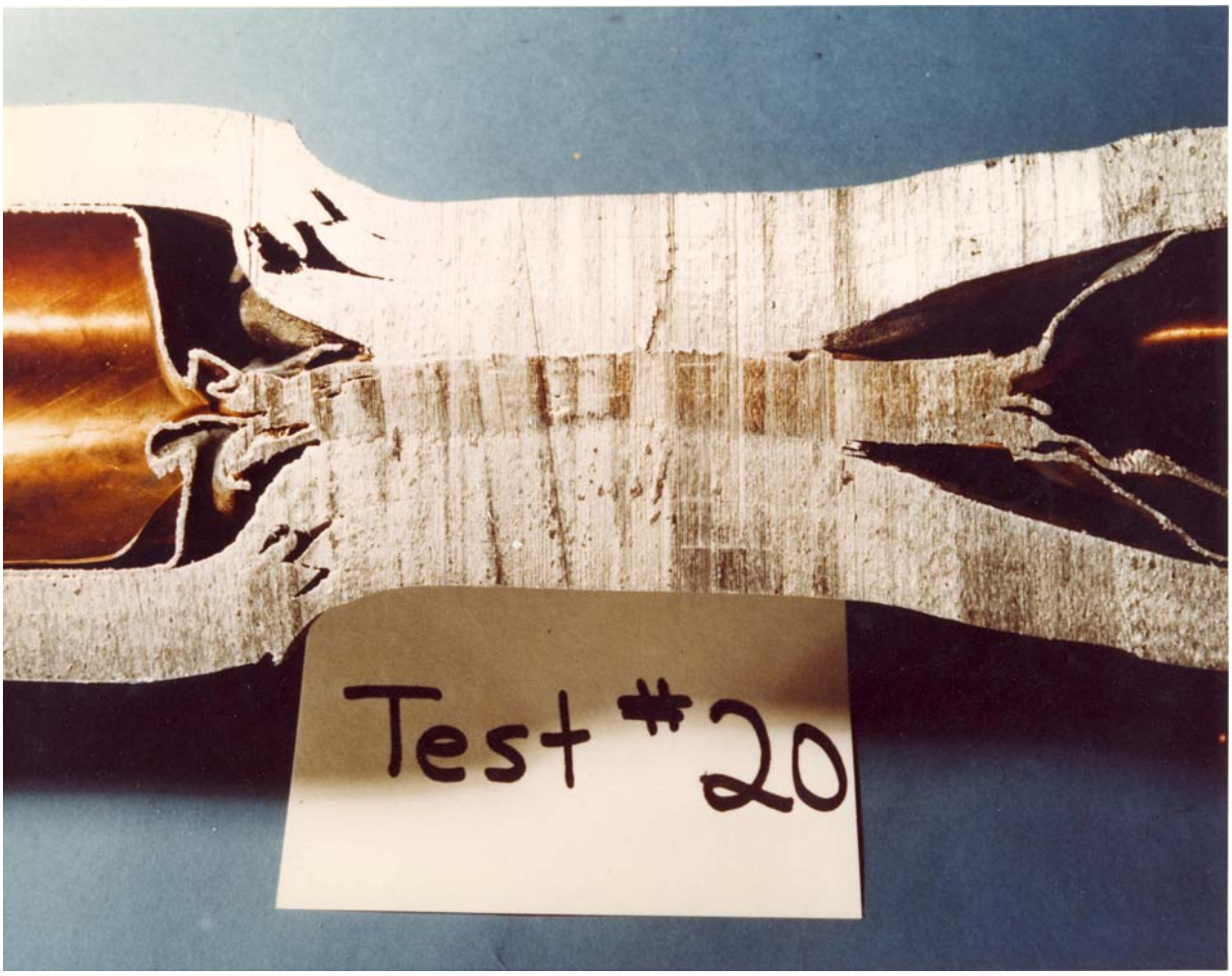


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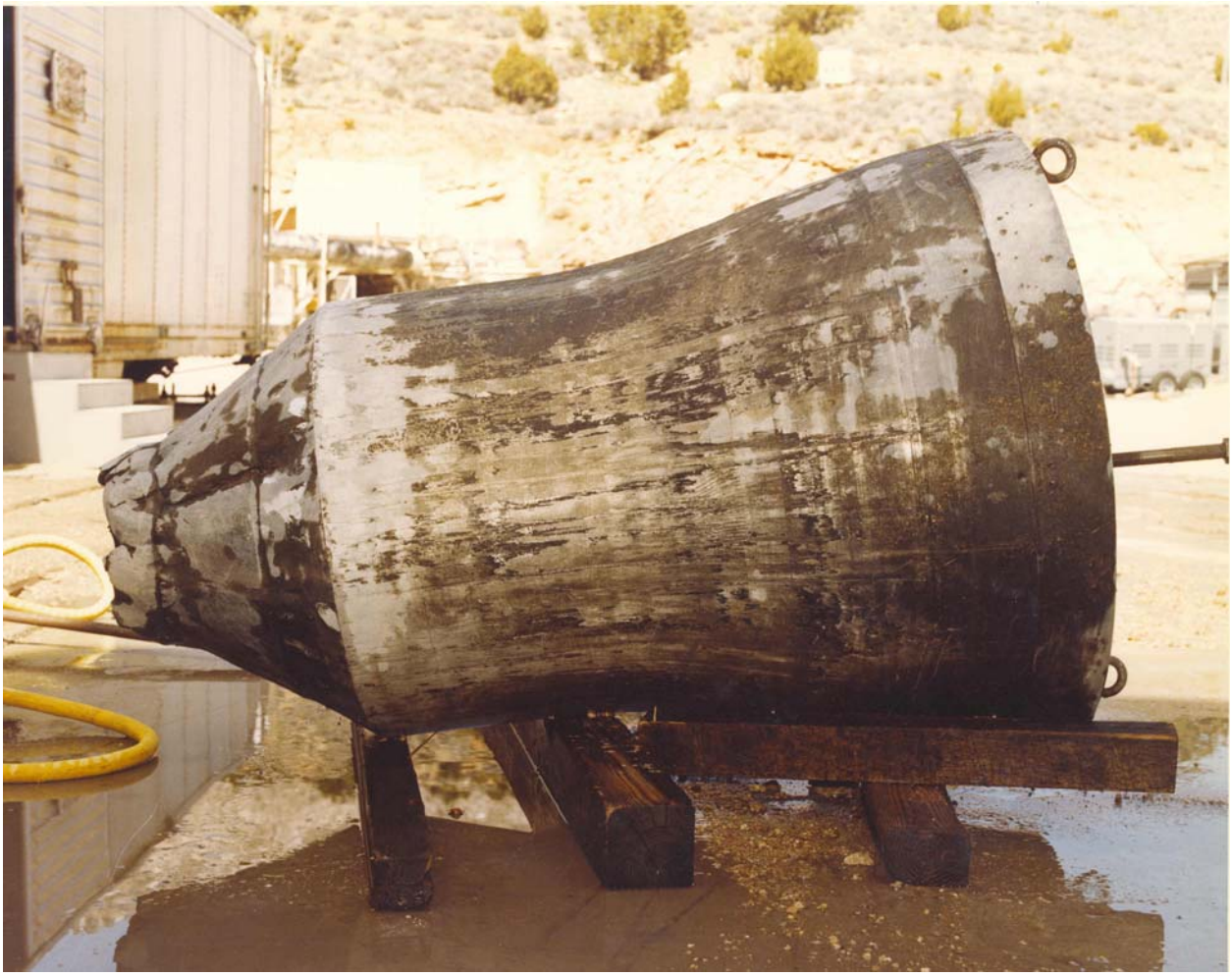

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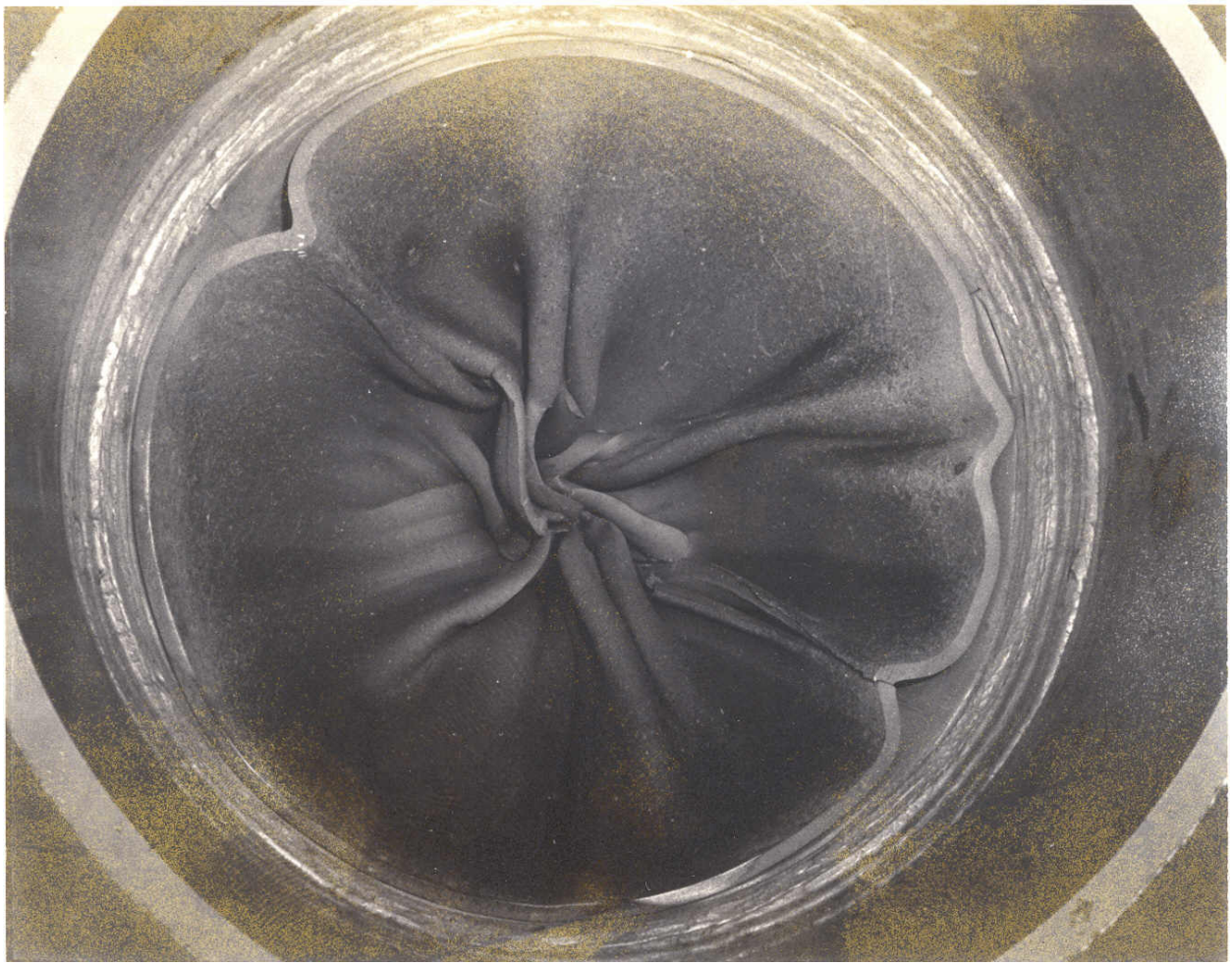
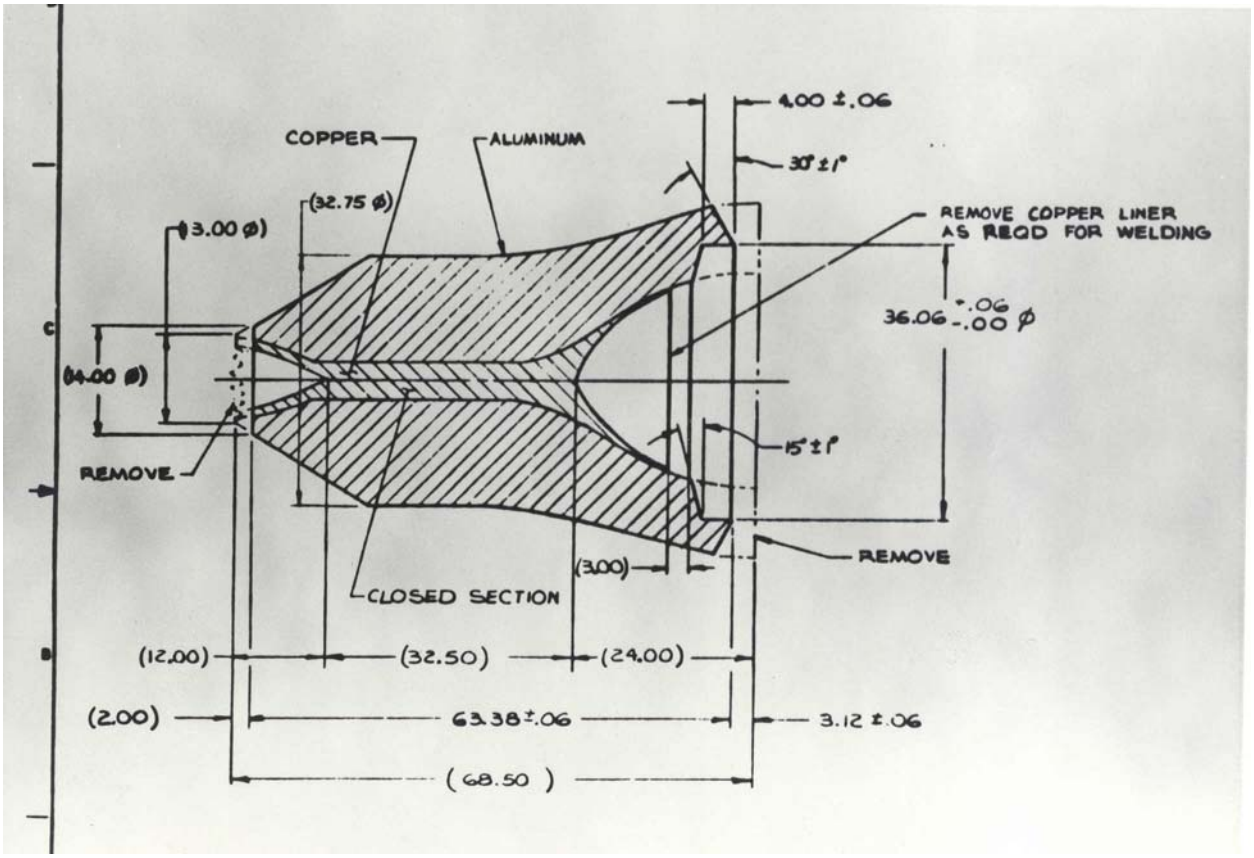


Photo 7: A posterior view of the closed FAC seen in Figure 6.

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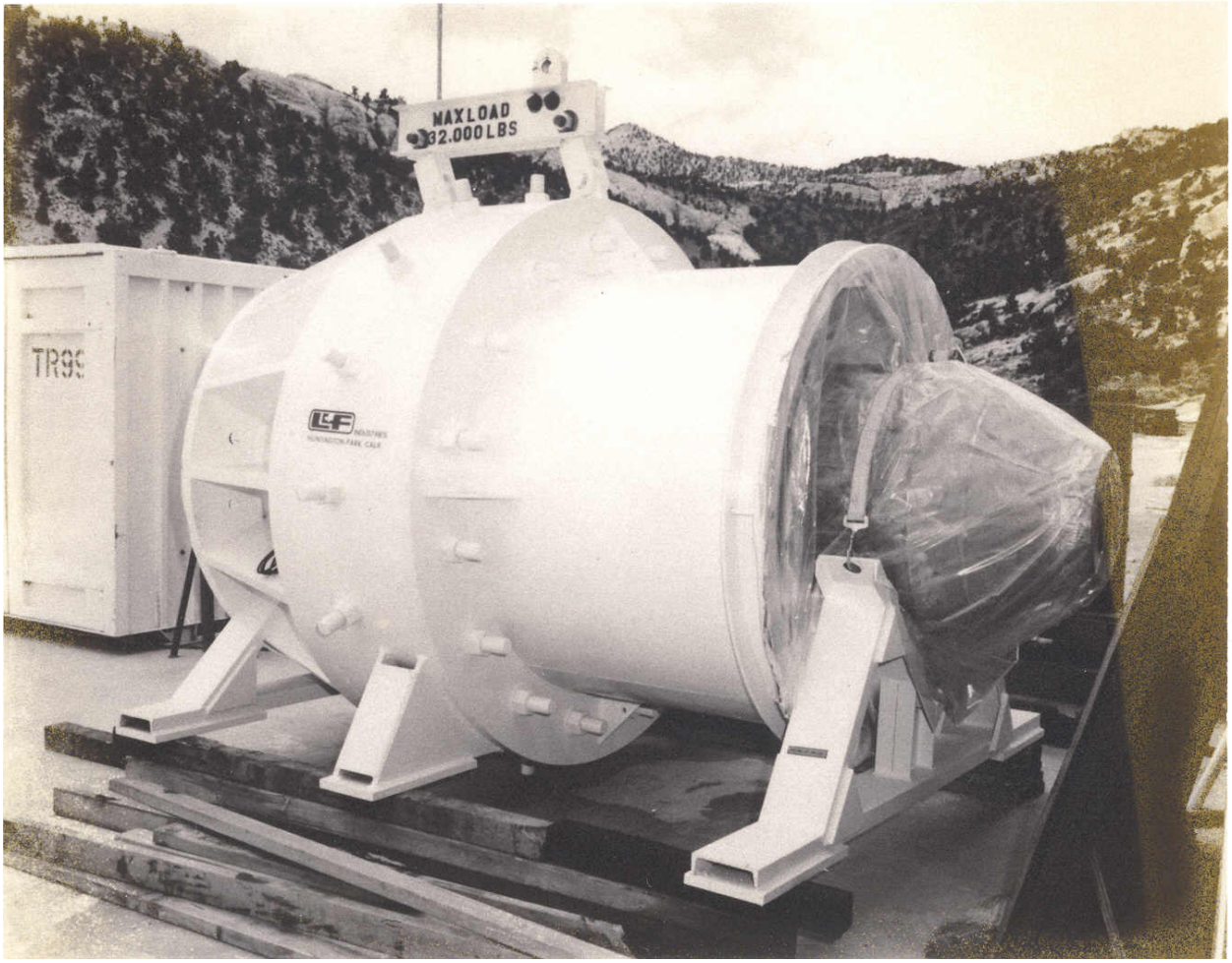


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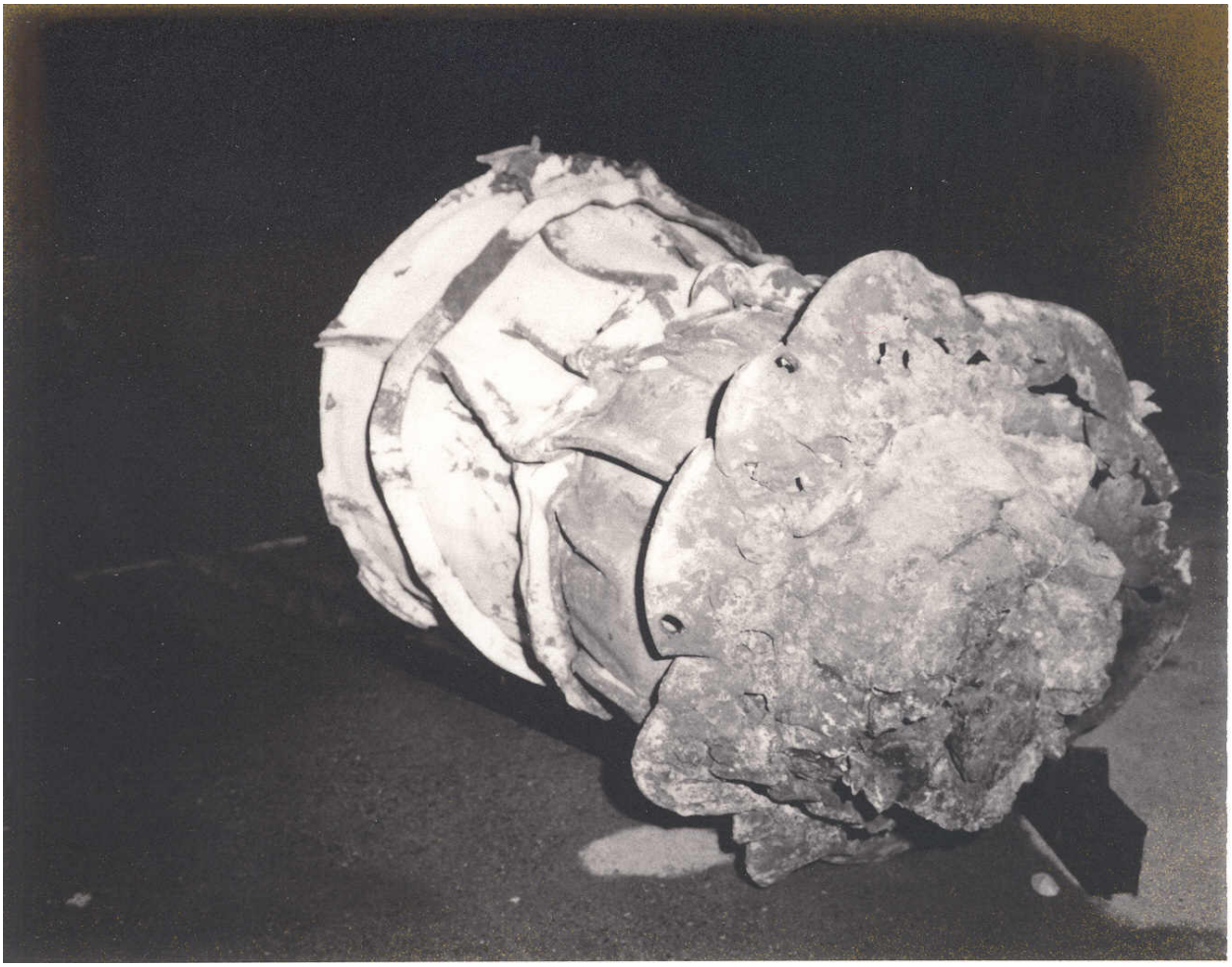

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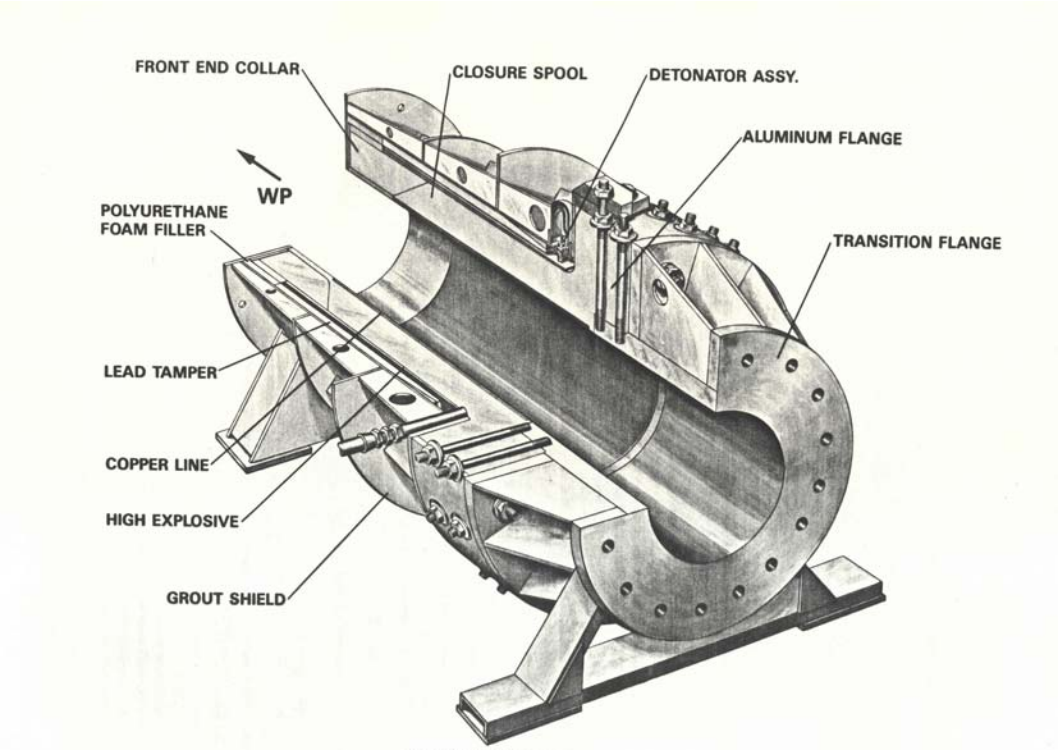


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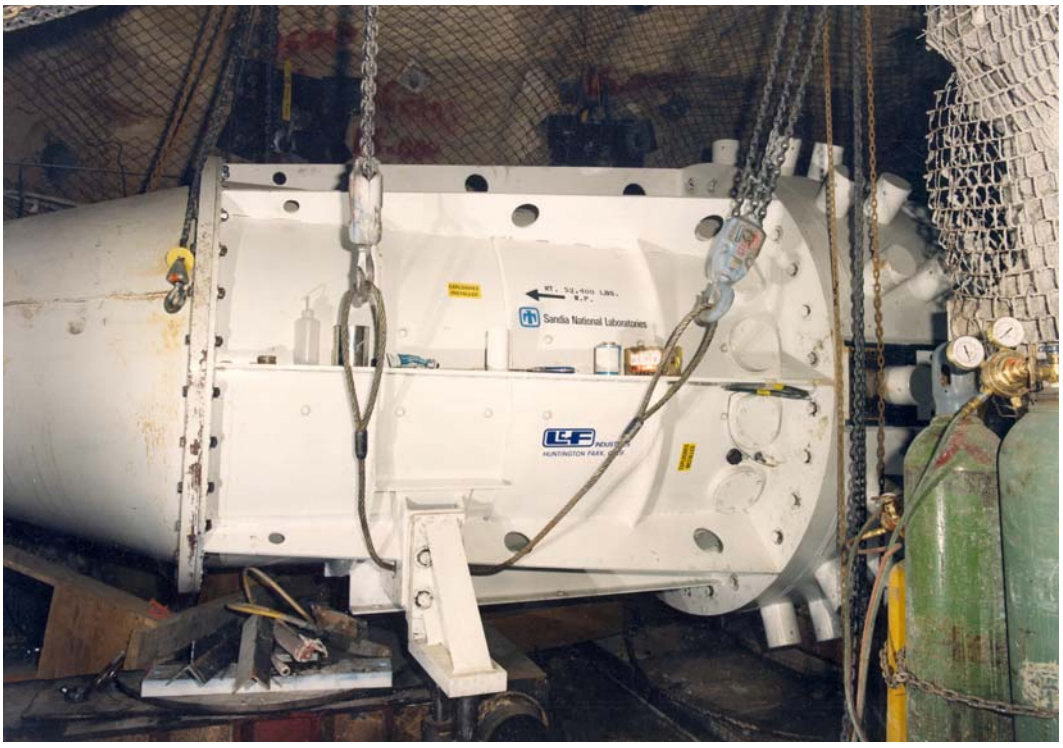


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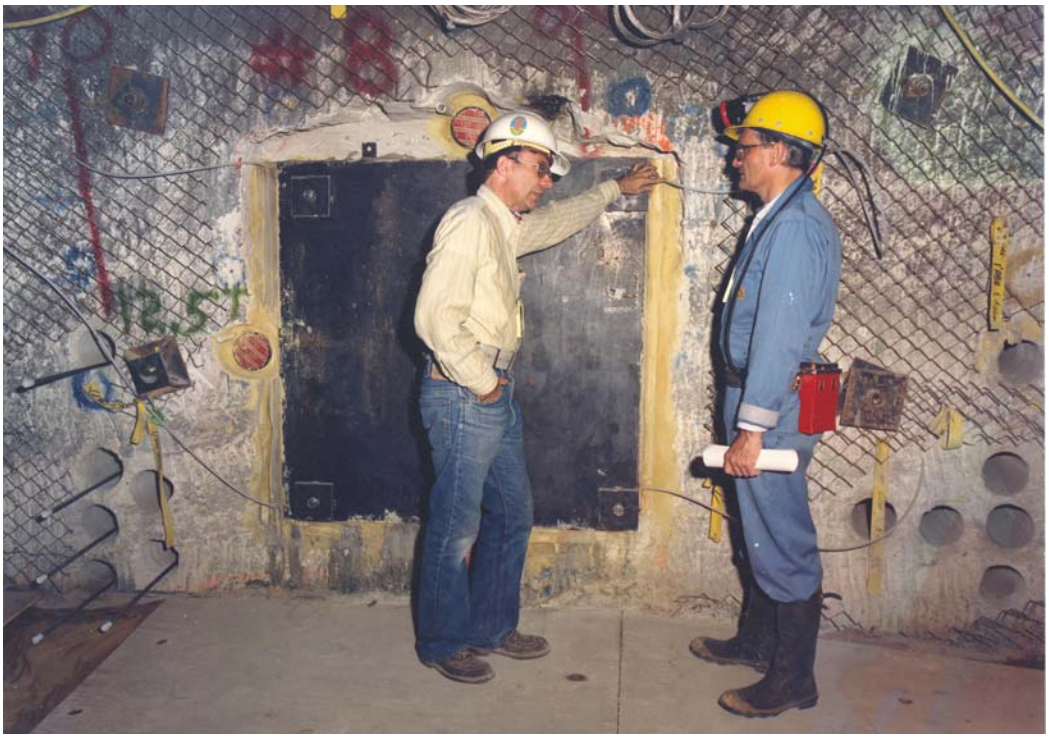
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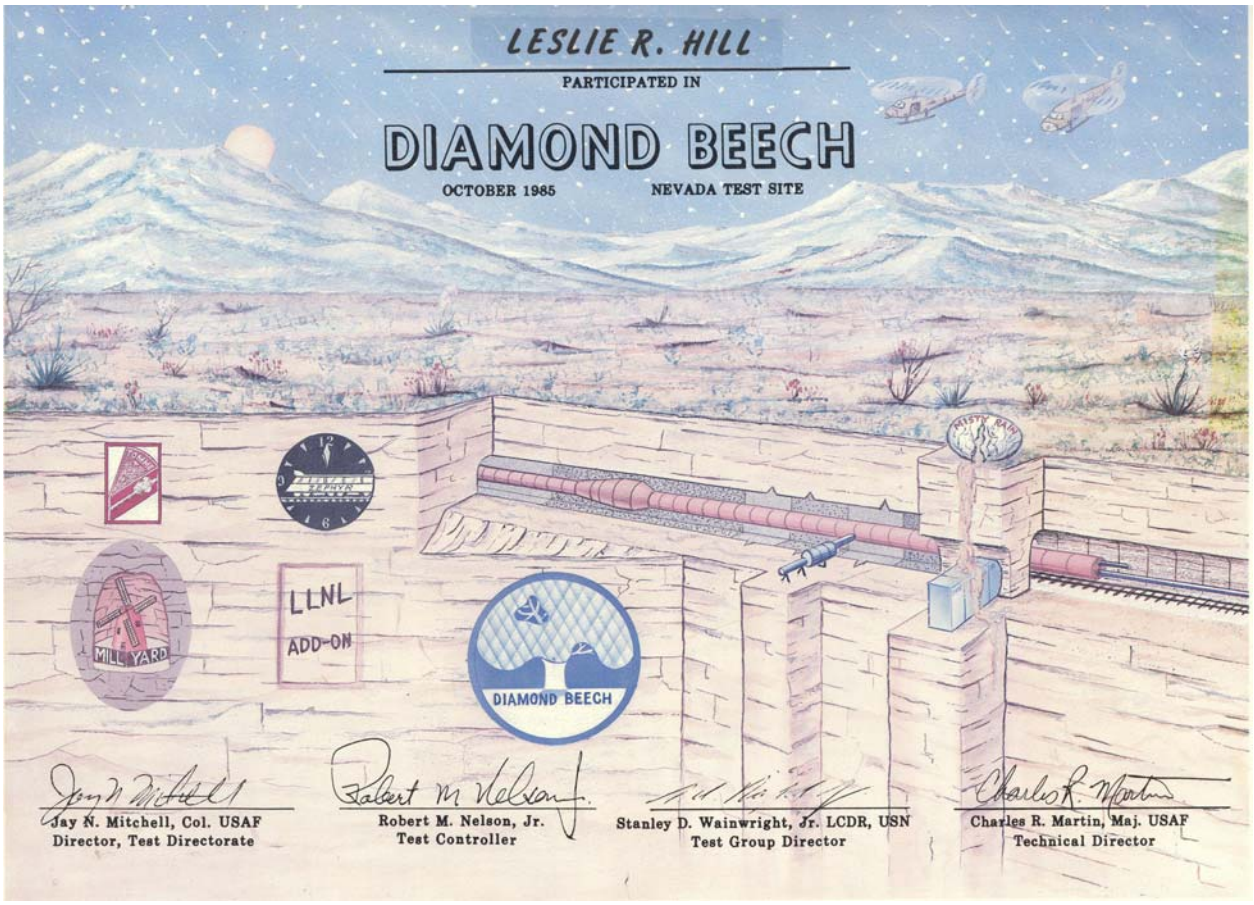


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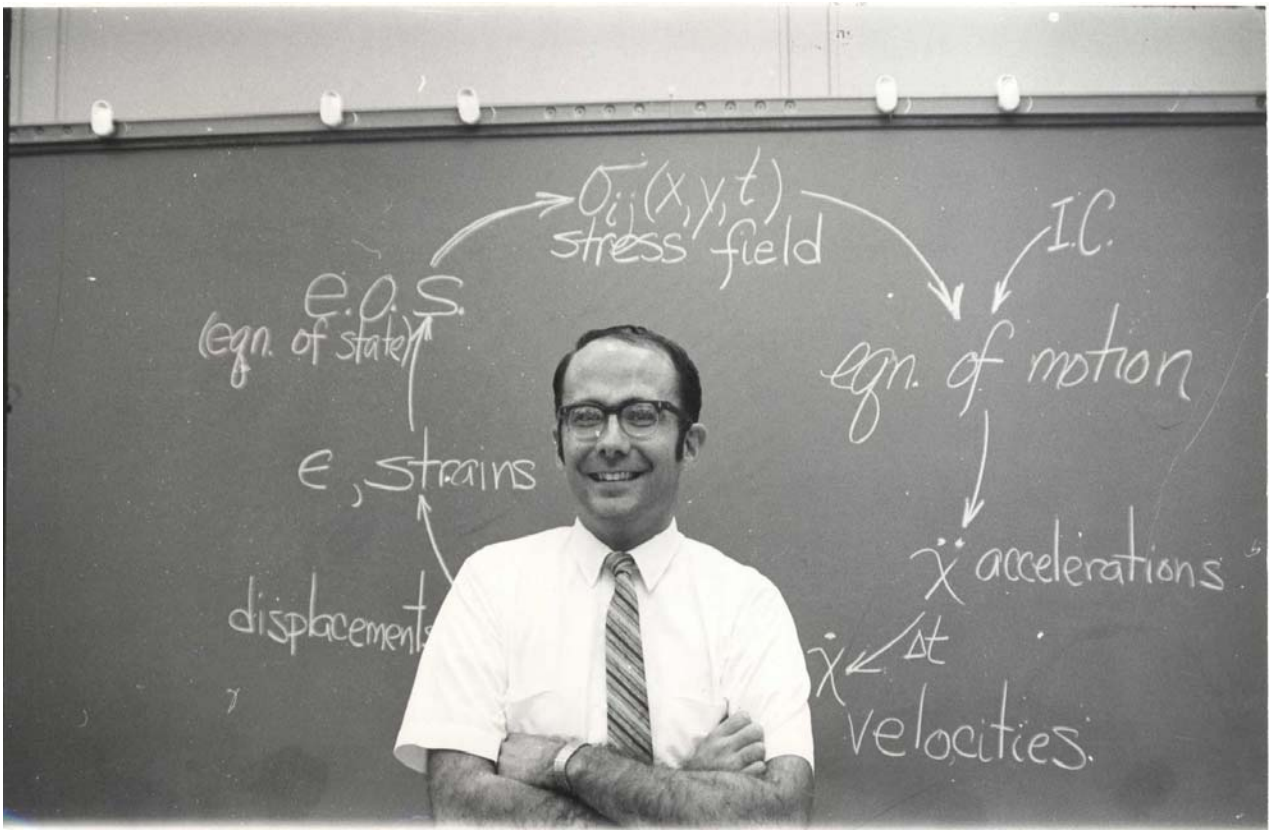


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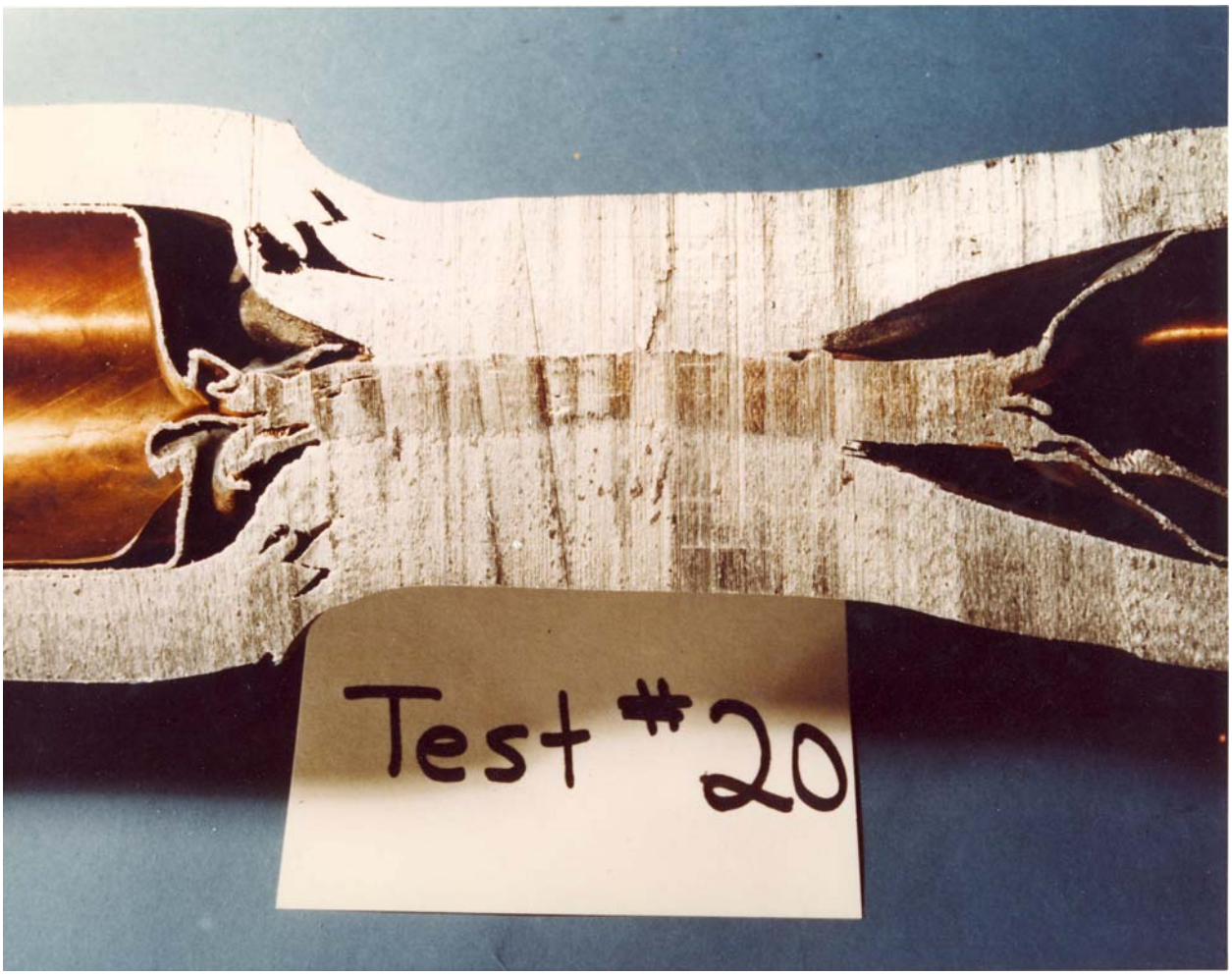


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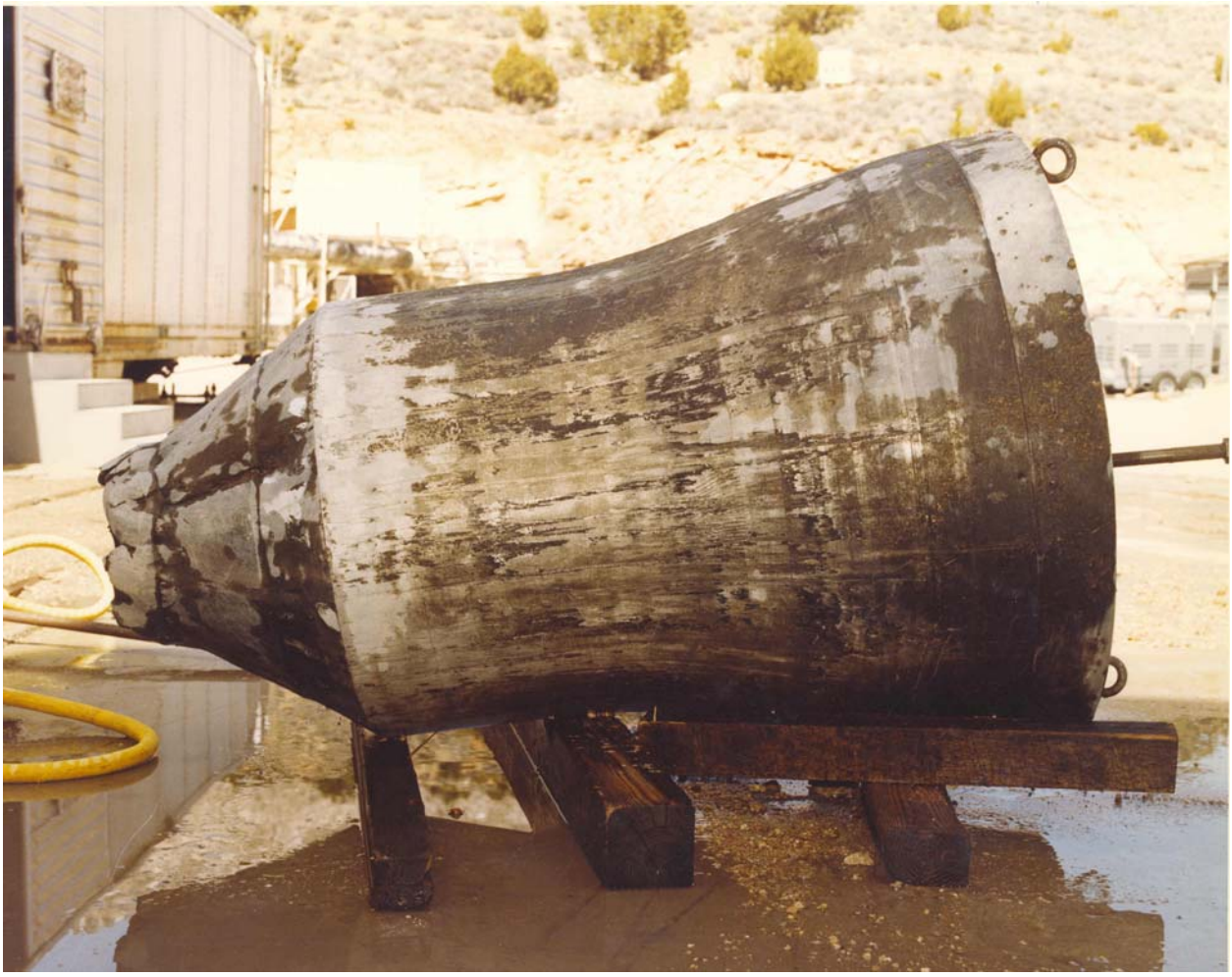

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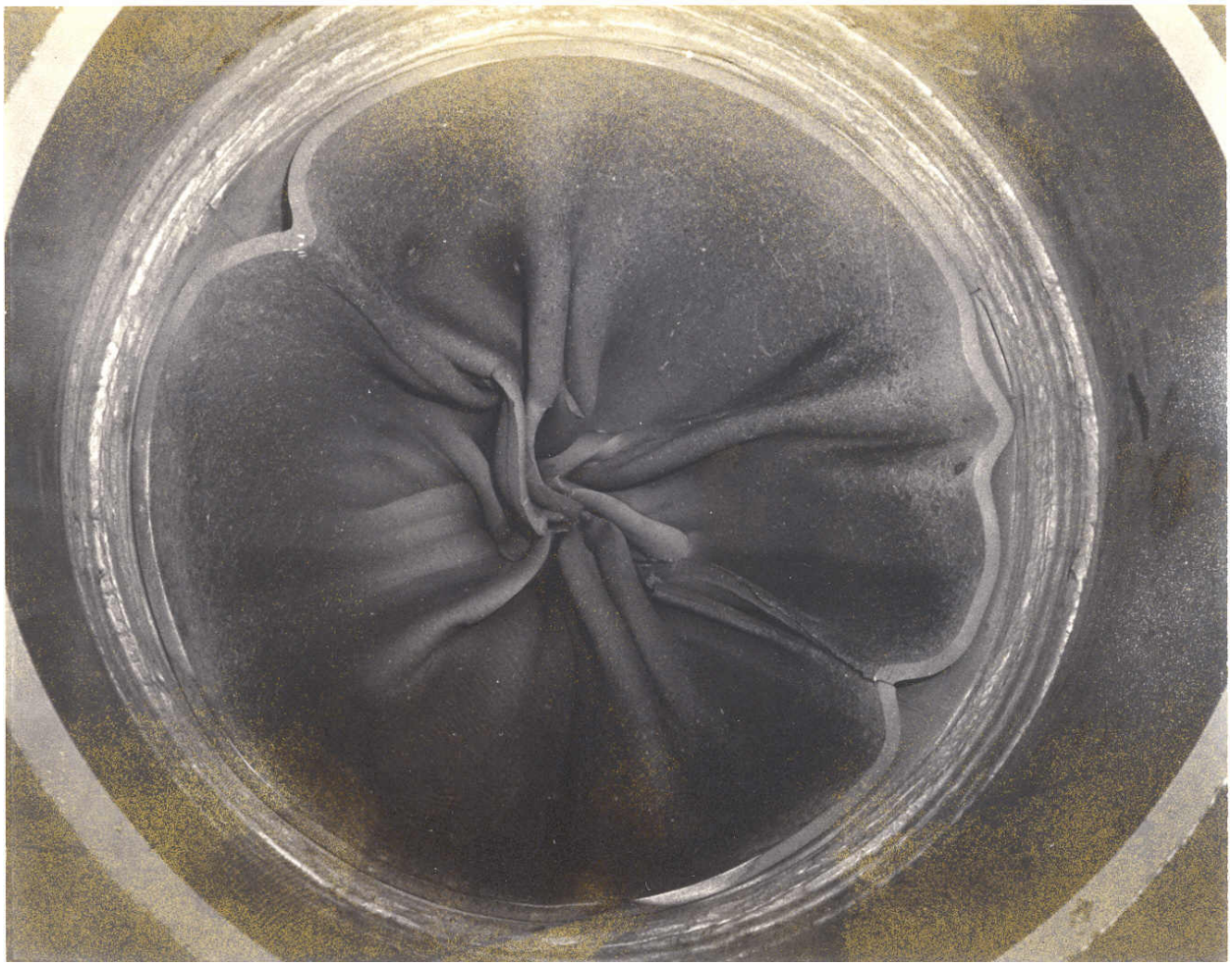

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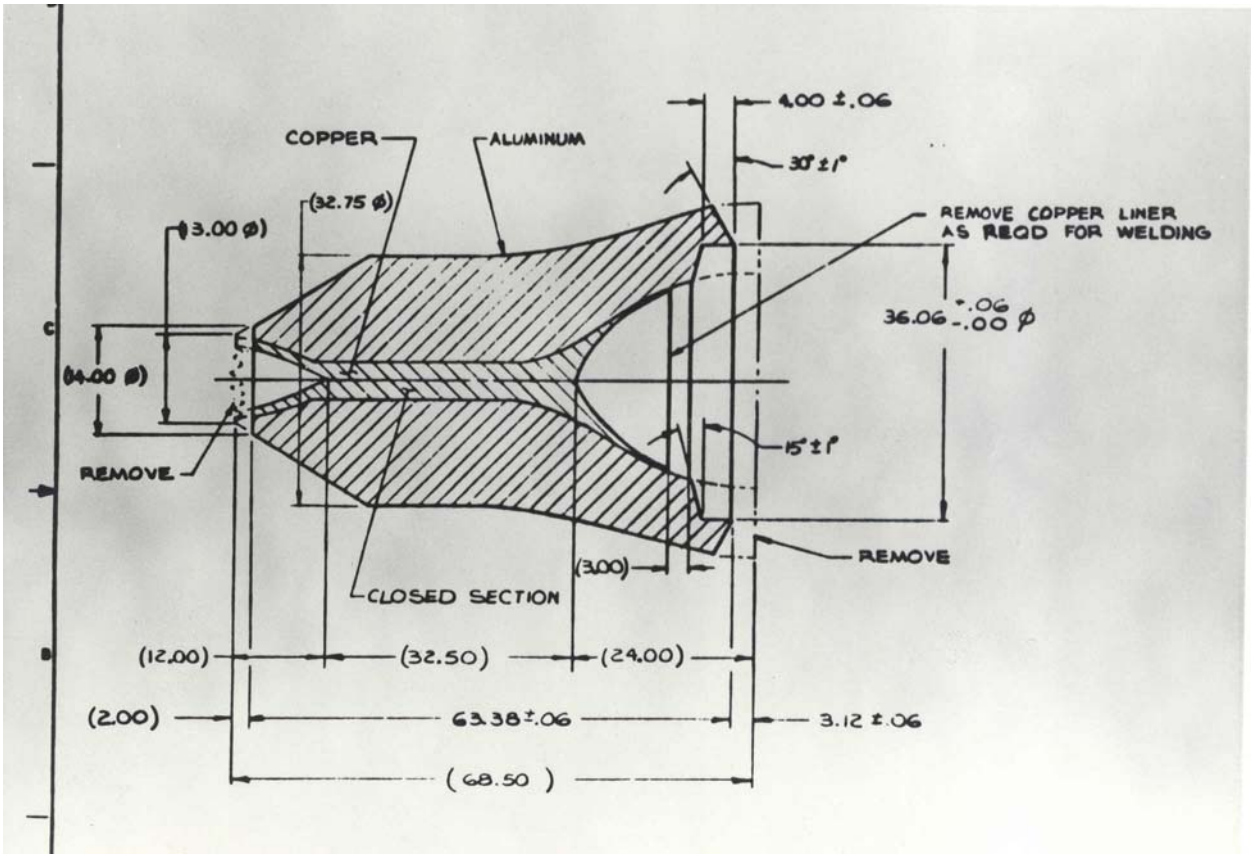


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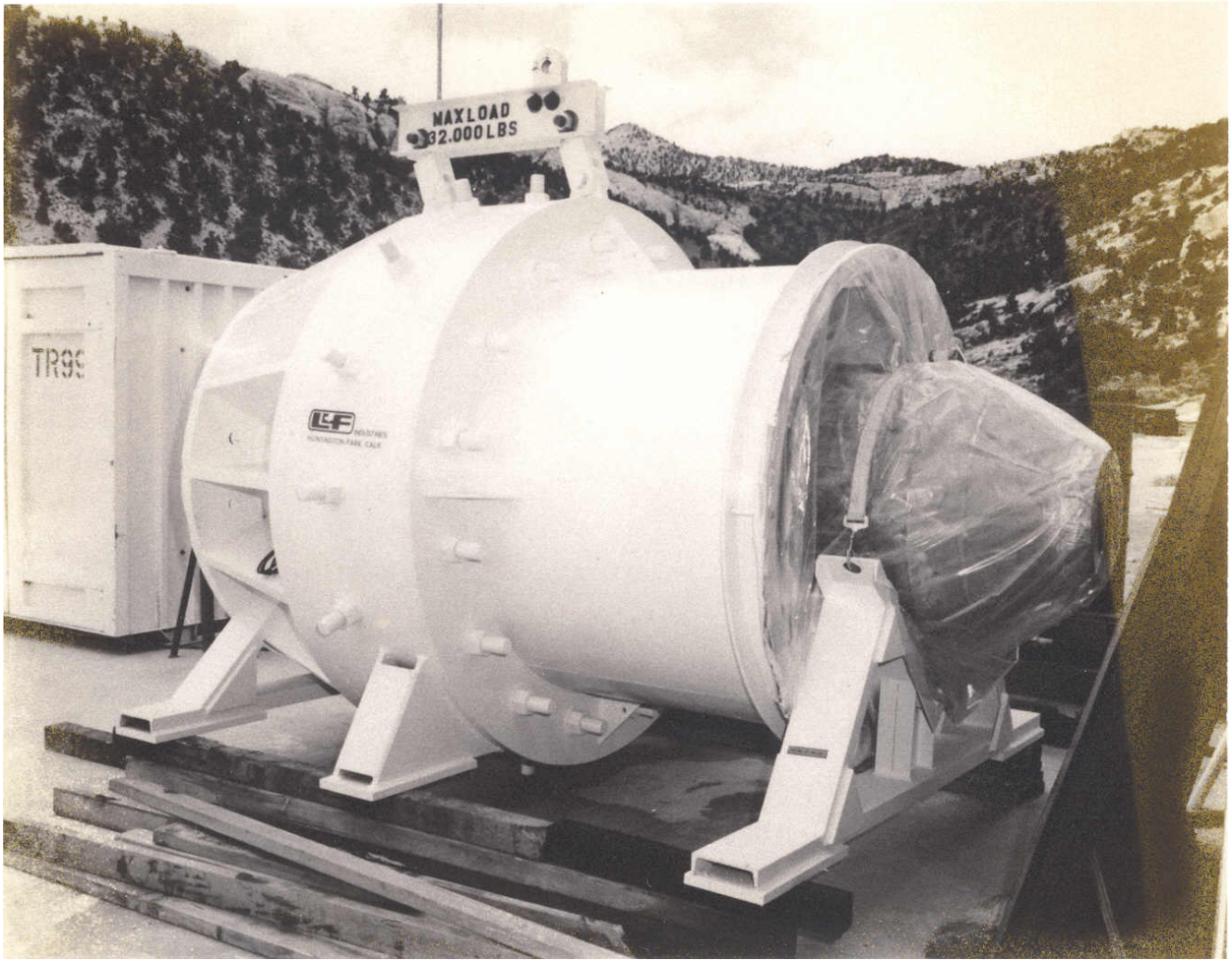


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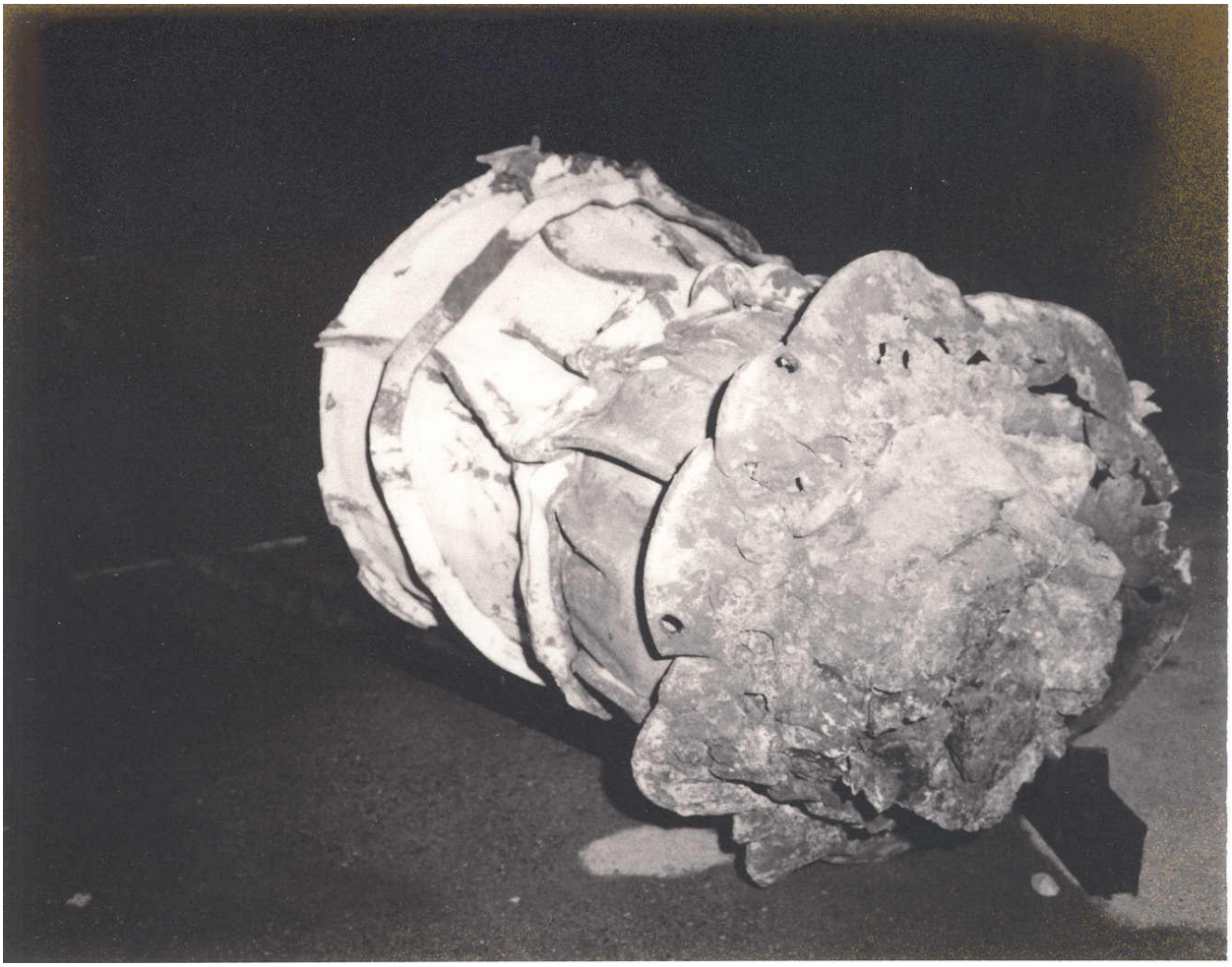

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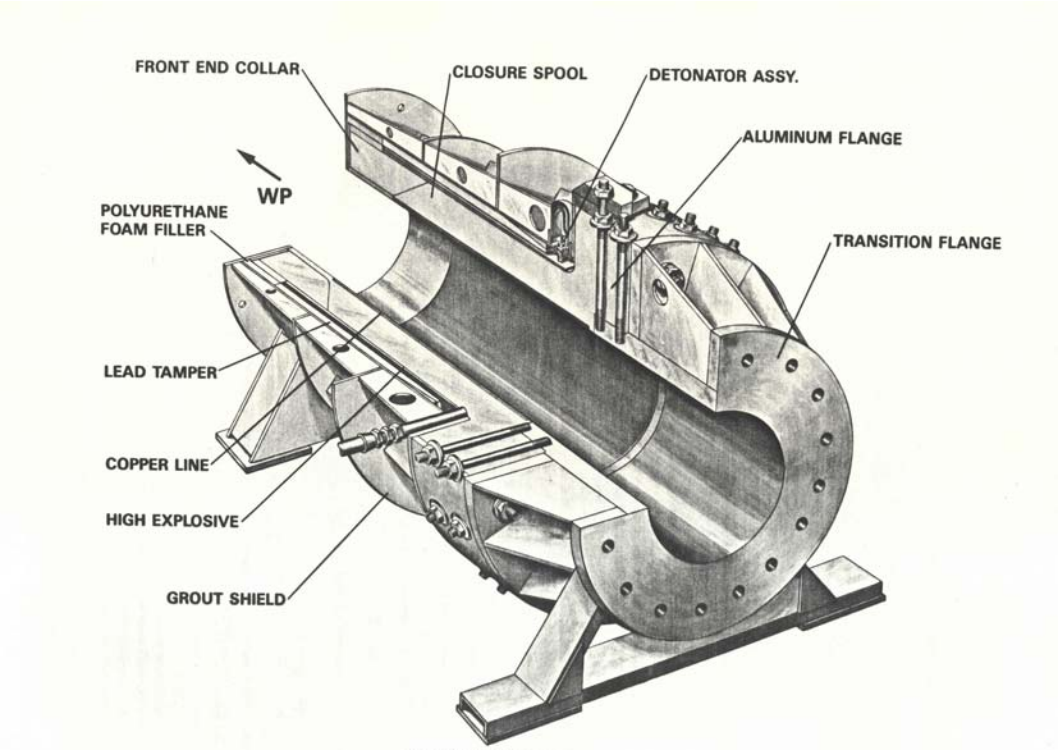


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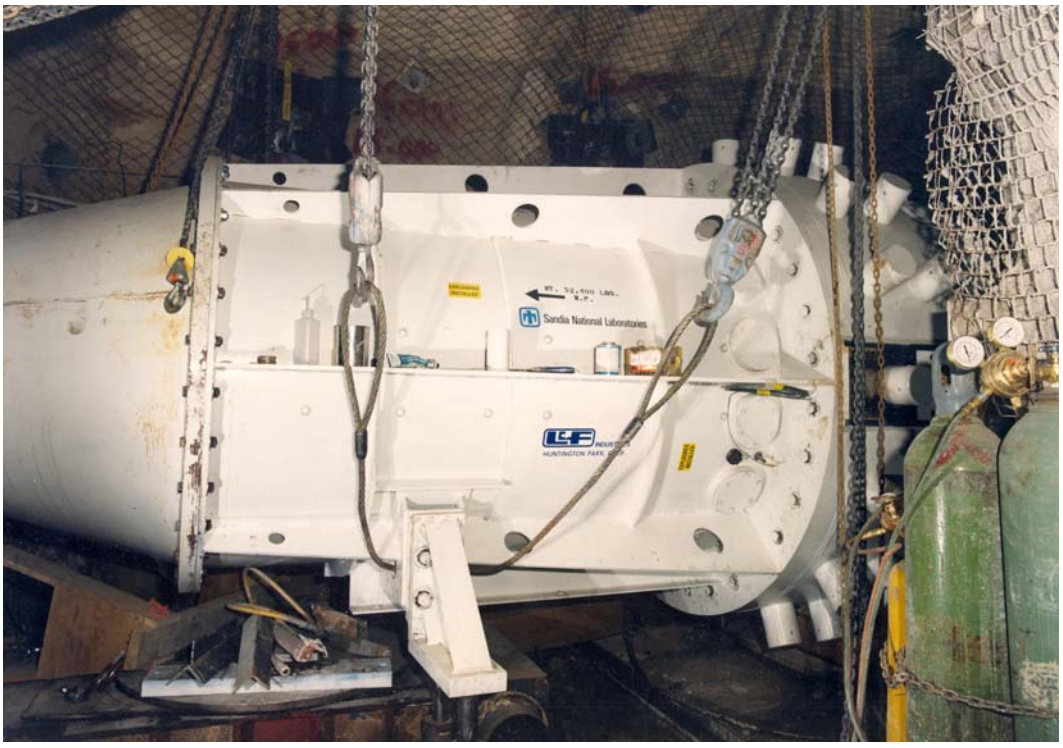


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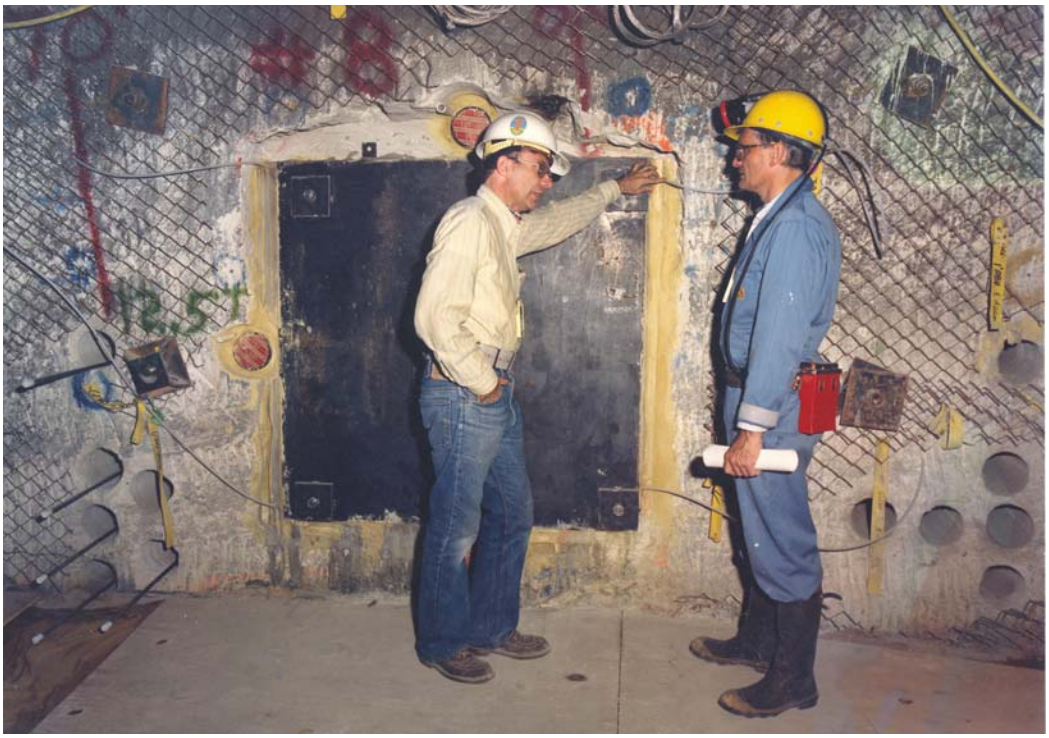
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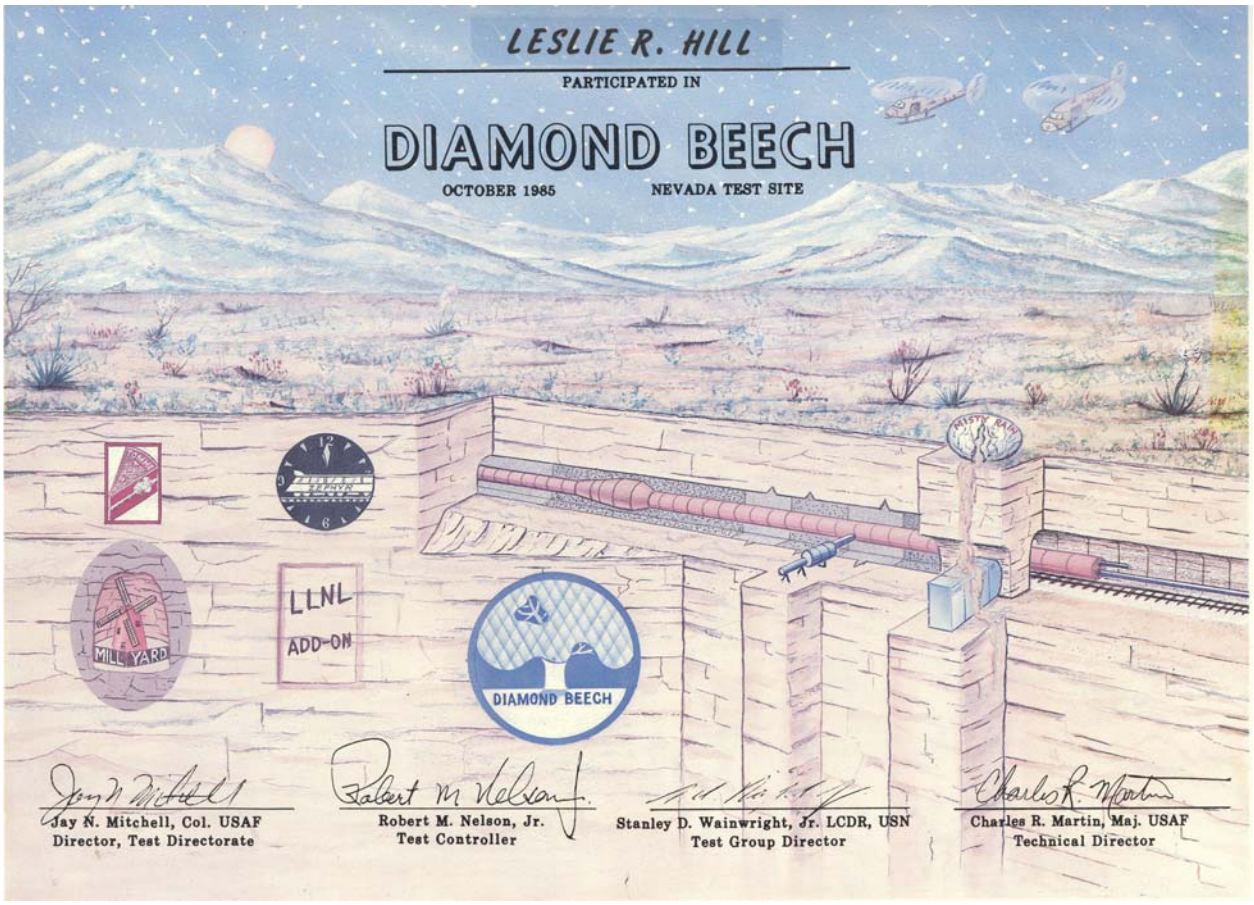


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