Nevada Test Site Oral History Project

University of Nevada, Las Vegas

Interview with Philip Allen

August 26, 2004
Las Vegas, Nevada

Interview Conducted By
Mary Palevsky

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The material in the *Nevada Test Site Oral History Project* archive is based upon work supported by the U.S. Dept. of Energy under award number DEFG52-03NV99203 and the U.S. Dept. of Education under award number P116Z040093.

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Mary Palevsky: *OK. So Phil, you mentioned that there was a correction you wanted to make to our first interview, and why don't you go ahead and do that?*

Philip W. Allen: Yes. In talking about the final atmospheric test that was scheduled on October 30, 1958, the last test before the moratorium began, I gave the name as Madison. That is not the correct name. The correct name, because my memory failed me momentarily, the correct name is Adams. Both of them named after presidents. There is another Madison test at the test site. It occurred later. I don't believe there has ever been an Adams test. The one scheduled on that October 30 was never fired because of bad weather.

And the next subject?

Yes, I wanted to ask you, just say a little bit what you said off the record, which was about how you were able to research that. That's interesting to me. You went back?

Oh, I looked back through my notes that I made, listing the length of time the tests were delayed. Starting with the first tests in [Operation] Ranger, I found in the reading room records of the readiness briefings, and in the readiness briefings, by going through and reading what they said and knowing what happened, I was able to determine the length of delays. It turns out that for the atmospheric tests, the tests that were conducted in the atmosphere, 53 out of a total 105 were delayed somewhat on account of weather.

Fifty-three out of—?

Out of 105 atmospheric tests at the test site. Now, that 105 atmospheric tests is a little higher than the number you usually hear, which is one hundred, even. The extra five were tests that were conducted below the surface but expected to vent, such as Sedan, where the explosion actually occurred 600 feet below the surface, but we expected it to vent, and there was a large cloud. So I counted it and several others that had been intended to vent, but were called underground tests.

So from a meteorologist's point of view, they're not truly underground because you're getting things happening in the atmosphere?

That's correct. The longest delay on account of weather at the test site was Shasta. The Shasta event occurred in August of 1957, and it took seventeen days to find the right combination of weather and device preparation to get the test off. The longest delay that I'm aware of anyplace on account of weather was the event that occurred much later near Hattiesburg, Mississippi. It was delayed approximately a month, almost entirely because of weather. That's about all I need to say about that, I think.

Great. Great. We're going to have to go through, I think, afterwards and look at—sometimes there seems to be a—this happened up at Livermore. Sometimes there seems to be a difference between the name that's listed here and the name that was actually used on the test, as well. Someone at Livermore kept referring to a test—he kept referring to Bravo out in the Pacific as Shrimp, and that was what they called it internally. So I'm wondering—?

I can't answer that question.

Yes, I'm wondering about that. OK, so, great. Now, as I mentioned, I had hoped you would be **[00:05:00]** *able to go back a little bit and talk in some detail about Ranger, because I don't know—I think there's more there than we talked about last time.*

Perhaps so. The weather forecaster for Ranger was a good friend of mine. He was employed by the Air Force Weather Service. He was a civilian. But he and I had taught school at the University of Chicago several years earlier. George Cressman, he made the forecasts using the weather station at Nellis Air Base. The briefings occurred in a temporary building. By temporary, I mean it was left over from World War II and was a wooden building. It was just a block or so from the weather station. Before each test, there was a readiness briefing. He gave the briefings. I did not participate. My function during Ranger was to learn as much as I could about the trajectory made by the clouds after the explosion.

So you got to Ranger—remind me of how you actually got out there.

You mean the reason for which I went?

That's what I mean.

I was working at that time for AFOAT-1 [Air Force Office of Atomic Testing], which is the name of the organization that was learning as much as it could about foreign nuclear explosions. It sampled the atmosphere in a number of different ways, and also measured the atmospheric pressure resulting from nuclear tests. A pressure wave would go generally all the way around the world. It also measured seismic waves through the ground. But my function was to be able to track the debris from a nuclear test.

Right, and you gave me a lot of detail on that last time.

For the Ranger tests, I only remember a couple of the trajectories. But the largest test, I think, made a cloud that held together sufficiently well that it was detected up over Utah and across the Midwest and Illinois, not too far south of Chicago, and out across the Atlantic Coast and was still easily measured as the cloud passed north of Bermuda. We didn't track it any further than that. Obviously the debris from all of the tests contained very tiny particles that would go clear around the Earth for long periods of time. The particles that were in the lower atmosphere would usually be brought down to the surface in rain. Rain is the major cleanser of the atmosphere, and it took all of the debris out of the troposphere, up to the tropopause, which varies from around twenty thousand feet in the winter to fifty thousand feet in the summer.

The other nuclear cloud that I remember from Ranger was from the last one, which was one of the smaller tests. It went southwest, not over Las Vegas but it went over the Spring Mountains. I could watch the cloud. It wasn't very high and it drifted through one of the passes in the Spring Mountains. I don't know exactly where because I was looking at it from the air base, Nellis Air Base, and didn't get a very good view.

That's what I was going to ask you, where you're positioned for this. You're out at Nellis?

[00:10:00] I was always at Nellis. I carried with me a clinometer, which is a little device used solely for the purpose of measuring angles, elevations, so that I could measure the heights of the tops of all of the clouds. I wasn't always correct in my measurement because the aircraft would sometimes report an altitude either higher or lower than I had estimated.

So let me understand this. You're at Nellis in some sort of observation area?

Just standing on the street.

Just standing on the street, with this instrument.

Yes.

And you—? Tell me what happened.

The instrument has a way of—well, I think it simply has a plumb bob on the bottom of it that holds the instrument straight up, up and down, and then you look through a little telescope, and the angle of the telescope relative to horizontal appears on a dial that you read on the side of the instrument. I would get the cloud height by triangulation using that angle. And later I would receive the information from the aircraft that were flying through the cloud to collect samples. They would give me the measured height of the cloud. Other aircraft, longer range aircraft, would fly around the cloud, trying not to become contaminated themselves, and try and keep the cloud in sight. I used their locations to track the cloud. I would put the location on my map of the upper winds and if the airplane seemed to be looking in a direction that the cloud was not supposed to go, according to the winds, I would pass that information to the airplane and tell it where it should go to find the cloud. They would track the cloud as long as they could see it visually without going into it. Then, when they could no longer see the cloud visually, they would track by instrument, detecting the radiation level. But they always tried to stay near the edge of the cloud rather than contaminate the airplane by flying through the main part of the cloud.

So you were not in those airplanes ever. You were—?

I was never in those airplanes. I was always on the ground. I was in the weather station most of the time, where I had the latest upper air wind data, and I would always have a new map plotted with data when it came in. Upper air data are normally taken every six hours by theodolite and following what is called a pilot balloon.

What was the first word you said?

Theodolite. A theodolite is a device that measures horizontal angle relative to the Earth, relative to straight north, and a small telescope that can see an object in the sky, and by measuring the azimuth angle—the horizontal angle—and the vertical angle—the elevation angle—and knowing the programmed rate of rise, one can determine exactly where the object is. And in the case of measuring upper winds, the pilot balloon is a small balloon, inflated to maybe three or four feet in diameter, either white or dark color so that it can be easily seen as it rises through the atmosphere. A white balloon is used in daylight **[00:15:00]** because it can be seen against the blue sky. But it's not so easy to see it against a cloud. And then there are probably several hundred pilot balloon stations throughout the country, and in addition to that, there are maybe a hundred radio sound stations. A radiosonde consists of a larger balloon which carries a radio transmitter. The transmitter is attached to a pressure device that has an arm on it, and the pressure arm crosses what looks like a miniature piano keyboard, and as it goes from each key to the next, that is a specific pressure, and as it goes up, it crosses a number of those keyboards, and you can tell exactly how high the device is by the number of keys it passes. Each time it passes a key, from one key to the next, it transmits a radio signal of humidity, because the device also carries a little strip that is sensitive to humidity, and electronically they can tell how high the humidity is. It also carries a thermometer and it gives a continuous temperature reading that is interrupted at each pressure reading. The pressure reading is simply the shift from temperature to humidity and back to temperature. And the observer needs to keep track of how many—well, this is actually printed out on a chart so that the observer can take it later and at his desk, using a number of charts, a number of tables of information, he can get the temperature, humidity, and pressure. And the radio signal is detected by a radio direction finder that gives him the horizontal presentation of which direction it's going. There are about a hundred of those stations nationwide, and all of that information is available by teletype to all of the major forecasting stations, of which Nellis had one. And so I had all of that information available to me and could use it to see how far away from the actual wind motion the aircraft were getting, and I could tell them then how to find the cloud back where the wind said it should be. But the aircraft gave me the width of the cloud, and when they finally got to flying through it, they could give me the intensity of radiation in those puffs of radiation.

What you're saying is so interesting and raises several questions. I'll do them one at a time. You're out at Nellis. So what are you seeing of the actual explosion when it occurs?

The explosions were in Frenchman Flat, and all of the Ranger tests were dropped from an airplane. The airplane would go over and—we were too far away, about sixty-five miles from Frenchman Flat. We could not see the airplane at that point. This is also early in the morning. It **[00:20:00]** was not yet light. But when the bomb exploded, it would be just below a range of mountains, low mountains, so that we would not see the direct flash from the explosion, but it would not be any more than, oh, two or three minutes before the cloud had risen to where we could see it. The fact that the flash was below the mountains did not prevent us, though, from getting a lot of reflected light from the sky and from any clouds that happened to be in the sky. Usually the sky was almost clear. And by the time the cloud got above the mountains, where we could see it, or the fireball got above the mountains, it had cooled off enough that you could look at it with the naked eye.

So you could actually see the fireball rising.

We could see the fireball rising after the first couple of minutes. And there were articles in the newspaper in Las Vegas about being able to read newspapers by the light from the detonation. Those were true, but most of the light was reflected. It was not direct from the fireball.

OK. But you could look right at the fireball without protection at that point.

By the time it got over the mountains, yes. It would be terribly bright.

Well, that's the obvious human question, is what—you had seen war. What was your reaction? How do you sort of gauge the size? You tell me. In human terms of what's actually going on.

The only part of the war that I saw was in daylight, and these occurred at night. There's no comparison. I never saw anything to compare with these. To have such bright light over such a great distance was just amazing and interesting. I don't know what else I could add.

I guess what's interesting sort of to the public and to me and to history is this is this weapon—you're a scientist so you're obviously focused in on a lot of the scientific things, but I'm wondering, do you make a connection with war when you see something like that? Do you think about that aspect of it?

I did not. To me, this was a physics experiment. And an unusual one, yes, but in those days, I didn't get excited very easily and I was more interested in what was happening in the device and in the atmosphere. I really did not connect any of these with Hiroshima or Nagasaki or war, although the connection was obvious.

Well, I think what's interesting is that people have a variety of reactions, and I don't find them to be uniform, at least in my small experience so far. So that's interesting.

After World War II, although as a meteorologist I did not see a lot of the actual fighting, all that I saw directly myself was the invasion of southern France when I had made an early morning forecast and then climbed into an airplane and flew over the invasion point, and I saw plumes of cloud near the ground, far below the airplane, where fighting was occurring. By 1951 when these **[00:25:00]** tests occurred, Russia [USSR] had detonated their first device and we were just making sure that we were ahead of them, that we had more knowledge about nuclear weapons and were in the process of producing a deterrent. And so my interest was, 99 percent at least, scientific.

And 1 percent?

I would pretty much ignore that.

OK, we'll just let that go. Statistically, it's insignificant. But that leads me to my next question, which is great, which is when you're doing your diagnostics and you're looking at the cloud and how far it travels, explain to me how that helps you on the ground, and correct me, the things that come into my mind are, is it to understand how we detect these tests? Is it to understand danger to populations? What are you trying to understand in addition to the actual sort of pure science of how these things move? How are you applying it?

In the back of my mind, as I was watching the clouds, there was the information, of course, that the clouds were radioactive and therefore dangerous. I knew that people had been moved out of the paths of the clouds so no one was being exposed to the clouds while they were of maximum intensity. I supposed that if I had been more of an environmentalist, I would have been worried about the small animals, the chipmunks and foxes and so forth, rabbits—in those days there were a lot of jackrabbits in Nevada—but I didn't. I would hear the readings come back from the Public Health people who were tracking the fallout pattern on the ground, so that I knew where the cloud was passing on the ground. The ground position of the cloud was not always directly under the upper part of the cloud because winds at the surface are frequently somewhat different from the winds up above the surface. And at that point, I would not keep track of the surface positions of the cloud. My job had to do with the long range transport. That referred more to the clouds from, oh, five thousand feet above the surface on up.

And how would that data be used by the people that were going to use it? For future tests or—? That's what I'm trying to understand also.

During Ranger no one was working on the problem of connecting the path of the cloud with the fallout, the intensity of fallout. That was done later, though. Starting in [Operation] Upshot-**[00:30:00]** Knothole, which I believe occurred in 1953. The Weather Bureau section that I was attached to when I was on loan to AFOAT-1, called the Special Projects Section, was under Dr. Lester Machta. And he started the investigation that really tied it together, the entire cloud from top to bottom to the amount of fallout that had been measured. And he did so using the data back through previous tests, including Ranger. All of that wind data had been preserved, of course, and all of the fallout measurements had been preserved. They first had been preserved, oh, by the Weather Bureau and by other organizations. The intensity of fallout had been preserved by the Public Health Service. It took them several months. By the next test series a year or so later, it was possible for the meteorologists, when they predicted the winds for the test, to give that information to Dr. Machta's people, and they could add the information that they would get from the laboratories about the contents of the device and the explosive energy, and putting all of that together, come up with a predicted fallout pattern. And those were usually pretty good. The errors were either a result of inability to predict accurately the upper winds or, once in while, the explosive energy would depart from that that was predicted.

Right. Right. OK, so that's helpful. That helps me understand that. So really you're describing this whole new problem that you have to face and there are various factors.

Yes. It was also during this time that, starting with Ranger and the next two or three test series, when the Sandia meteorological people began working on the connection between explosive power and blast through the atmosphere. And they began to predict whether windows would be broken in downwind areas. The blast would be focused by a combination of the vertical temperature structure and the vertical wind structure. Sometimes the focusing effect would be much greater than at other times, and when it was great, tests have been delayed in order to avoid damage to buildings and consequently to people in the (usually) downwind areas. Upwind focusing did not **[00:35:00]** occur; the focusing occurred always downwind.

So, great. That gives me—since Ranger was my question, I think that—unless there's anything you want to add about it.

There are probably other things to be said, but I can't think of them at the time.

Great. You've given me a lot. Thank you.

I might move to the subject of the need for weather support when the tests are held underground. The main reason for considering weather when the test is underground is that there's never a total 100 percent guarantee that the radiation will be contained underground. When testing first went underground, there were fairly frequent small ventings. Not enough to be detected off the Nevada Test Site [NTS], but enough that people that were onsite would have to be kept out of the contaminated area. And the contaminated area, of course, depended on the wind direction. So we always predicted winds. We also predicted clouds and rainfall because there were always aircraft operations, even with underground tests, and if there were accidental venting, we did not want that to occur in rain. Also, there were other activities that were hampered by having rain in the area. So there was always a need for weather support, even though the test was underground.

Right. So you're saying that consideration was always made, weather-wise, once we go underground.

Yes. Before every test, as long as testing occurred, whether atmospheric or underground or in tunnels, there were readiness briefings beforehand, and those readiness briefings included a weather briefing in which everything was discussed that could possibly affect the safety of people and success of measuring the results of the test.

Right. So just chronologically now, we've moved from you having been at Ranger to not being at the test site, then you came back in.

Yes. I worked for AFOAT-1, detecting foreign nuclear explosions, from 1949, before the first Russian test, until completely through 1952. Then for three years, from 1952 through about May of 1956, I worked at the National Meteorological Center outside of Washington, at Suitland, Maryland. Then in 1956, there was—well, during all of that period, the weather support for the **[00:40:00]** tests here in Nevada was handled by the Air [Force] Weather Service mobile unit, stationed somewhere in Oklahoma. The tests during that period were in short series, short being from an individual test up to a series of them, twelve or thirteen, over a period of two or three months. But the entire weather activity was handled on a mobile basis by the Air Force.

In 1956, the decision was made by the AEC [Atomic Energy Commission] to set up a permanent weather facility at the test site. And they asked the Weather Bureau to do that because they did not want the military—the military was sufficiently tightly connected to the testing program that it was considered potentially biased in safety considerations, and they wanted that potential bias to be eliminated, so they invited the civilian weather service, the Weather Bureau.

I got involved because I was friendly with the people working for Dr. Machta who were still connected with the AEC testing program and still connected with AFOAT-1. But I personally was working in the other part of the Weather Bureau, the meteorological center. A couple of the fellows from Dr. Machta's office were visiting in the building where I was one day, and they stopped to talk, and in the course of conversation they said that the Weather Bureau had been invited to set up a permanent weather research facility at the Nevada Test Site. And upon hearing that, I volunteered the information that if the Weather Bureau did that, I would be interested in participating. They took that information back to Dr. Machta, and the next thing I knew, I had a phone call from one of Dr. Machta's administrators that I was friendly with, asking me if what I had said was true and telling me that I was the first person that had shown any interest in being involved. As a matter of fact, he said that he doubted that anybody in the Weather Bureau would want to go to such a place as Las Vegas, Nevada. But my wife had been with me during the series in 1953. She had stayed in Las Vegas with the kids and had liked the climate so well and the casual living that she was ready to go in a minute.

So the Weather Bureau confirmed that I would be in charge, and another fellow came with me. In April of 1956, we surveyed the test site and the surrounding areas, looking for suitable places for weather stations, and made our initial plans, started procuring equipment, and locating people. We consulted with the weather training programs at UCLA [University of California, Los Angeles] and University of Utah and other places and advertised the positions. **[00:45:00]** That was in April, and by June, when I reported for duty in Las Vegas, we had identified four other meteorologists besides me and six or eight meteorological technicians and an electronics technician, and we started the office with those people during the month of June 1956. At that time, we were located in the—the AEC gave us the office building right next to them. They were located at, I think it was 1231 South Main Street, and we were at 1229 South Main Street.

That was interesting to me, what you just said. I think we have some of this from the first conversation, but what interested me this time, so it really was mobile up till then. There weren't permanent weather stations out at the test site until you came.

That's correct. That's correct. One of the first things we did was to make a daily weather forecast, and set up our own teletype network. We had stations at Tonopah and a couple of places on the test site, to start with. We connected in with Nellis Air Base and with McCarran Airport, both of which had weather stations. And several other scientific organizations at the test site tied into that teletype network, and we would every day put a forecast for the test site. That was the first regular forecasting for the test site, and there were more users than we had anticipated, of course.

Interesting. This just seems like such a trivial question, but I'm curious. You come to do this work. Just give me a really brief sense of how that worked. You set up weather stations. Do you tell the AEC? Do you work directly with the contractor?

We work with the AEC in the sense that AEC is providing the funding, and they instructed me on how to work with the contractors. That was primarily with Reynolds Electric[al and Engineering] Company, REECo, and I can remember one of the first people that I talked with in Reynolds Electric Company was Davy Crockett. He was the father of the Crockett lawyer who still practices in Las Vegas. There were several other people in Reynolds Electric Company that we gradually became acquainted with and worked with. They obtained our teletype circuits, which are Weather Bureau circuits but handled by the telephone company, of course, as all communications were in those days. We set up two teletypes. The weather data came in two channels by teletype, so that we needed two teletypes, and a third channel by telephone called a facsimile. People are very familiar with the term "facsimile" these days, but in those days, "facsimile" referred primarily to the transmission of weather data from the National **[00:50:00]** Meteorological Center where I had worked to the forecasting centers. The telephone signal is received at the forecasting centers by a machine that produces weather maps. In those days, it was a big machine about thirty inches across and two feet high, and it produced weather charts that were of about the size of fifteen by twenty inches. And we received those. We here in Las Vegas would get all of the charts that were produced by the National Meteorological Center in Washington, and we would use them for our basic meteorological analysis and forecasting. But we would prepare our own charts for Nevada and Utah, Arizona, and California so that we could insert any additional information that we have, insert such things as mountain ranges and lakes and so forth, that would affect the weather, and have it in sufficient detail that we could forecast the weather to the required amount of detail here.

Now that raises another interesting question, for those who will not even comprehend what you're talking about, because everything is done on computer now. So explain to me, when you say "insert the mountain ranges," do you have some calculating method or…?

No, not at that time. That came later, with computers. At that time, it was the maps that we used. The maps were designed specially for meteorological use. They would have circles for towns, and each circle would have a designating series of letters beside it to indicate which town it was. Each circle would be a town that had a weather reporting station in it and would be on the teletype network so that we would get their weather data. The weather data would come in coded form, and we had subprofessionals decode the teletype data and plot it on the weather charts so that for each one of those little circles on the weather, there would be an arrow attached to show the wind direction and symbols to indicate the type of clouds. Numbers in the grid around the circle, a particular corner of the grid was reserved for temperature information, another corner for cloud information, one side of the circle for humidity information, for pressure information and the rate of change of pressure, a place reserved for a symbol to indicate what kind of weather is going on now. Two dots would indicate continuous light rain, one dot would be intermittent light rain, and three dots would be continuous heavy rain, and so forth. The strength of the wind would be indicated by flags on the end of the arrow. Let's see, what else was on there? The cloud type would be indicated by symbols. A cumulus cloud would look like a little dome with a little **[00:55:00]** tinier dome up on top of it. A thunderstorm, instead of having a tiny dome on top of the dome, there would be an anvil on top. It wasn't until the middle of the 1960s that we began to get information by facsimile, information on the photographs of the atmosphere taken from satellites. That was a big help, especially forecasting near the West Coast, where we had very little data coming from ships offshore. But when we began to get photographs of clouds from satellites, that helped a lot in forecasting in the western U.S.

Wow. That's so interesting. Now, back to that, just one more step in the process. So you plot these things on these maps and then you look at it and you analyze it?

That's the correct terminology, yes. The subprofessionals would plot the data on the maps. The professionals would go over the maps and draw lines of constant pressure. That gives you the highs and lows and isobars that people used to be accustomed to seeing on their weather maps. We would shade the rain areas in green and the fog areas in yellow, and where there were showers occurring, we'd use hatching in green. All of that work could have been done by subprofessionals, but professionals did it because they had to know that in order to make the forecast. So the professionals would analyze the charts and then try to anticipate what that chart would look like at either twelve or twenty-four hours. They would draw that and call it a prognostic chart, and label it appropriately for the time that it is supposed to be effective. In those days, the forecasters had to mentally figure out what the pressure system would be doing in the way of changing, where the lows would be moving, whether the lows would be increasing in strength or decreasing in strength—the same way with the high pressure centers—and whether the pressure would be changing rapidly at a particular station or not, whether it'd be going up or down. And having determined what the pressure picture would look like for the area, we would then try to imagine what would be going on in the atmosphere with the moisture. We would draw lines of constant moisture on some of our charts. Putting all of this together required some concept of what the equations of motion in the atmosphere are. Solving the equations of motion takes a computer. Humans have never been able to do it except over a period of weeks to solve one equation. When the computers came, one was installed in the National Meteorological **[01:00:00]** Center. That happened about the time that I went to the Meteorological Center, and when I was there, we used to compete. The manual forecasting that we did was compared with the forecast that the computer did. The computer would analyze the equations of motion in the atmosphere. There are several equations dealing with several aspects of the atmosphere. And the forecasters who were operating the computer study were forecasting using computer information. As long as I was there, we were able to forecast better than the computer, but by the time I had left, two or three years later, the computer was beating the manual forecasters.

So this time frame would've been—?

That would've been in the late 1950s. The satellite data didn't come for several years after that.

That's very interesting. So when you're first at the test site, you are—?

Everything is manual, as far as the forecasting is concerned. We were supposedly a research station, so one of our jobs was to try to come up with equations that would describe the motion of the air across the test site. And about the time I left the test site, computers were being used then. I was there sixteen years. By the time I left, they finally had some equations that would work to forecast there for the test site, the wind directions and the cloud directions, radioactive cloud directions.

That's very interesting. Thank you for explaining that. Let's see, we've covered Ranger, we've covered some description on your part, which was very useful, about the reason for forecasting even when we went underground.

Yes. Well, the main reason was because there was never a guarantee that the radioactivity would be completely contained underground, and when it came out, that would present a problem, of course, because the wind might blow it over people. There were people working at different places around the test site, and Area 51, bless its heart, was just off the test site, to the north, and they were very much interested in if we had any radioactive material vent.

Now, wait a minute. You can't say the words "Area 51" without telling me, did you have actual relationships with them? Did you send weather stuff over to them or were they—? I know so little about it, obviously. Were they sort of a world unto themselves?

They were a world unto themselves. However, they came under the Air Force, which was headquartered at Nellis, and the Nellis weather station, I'm sure, had a subunit at Area 51, although they never said they did. It just makes sense that they would have. And all of our data would almost certainly have been available to them through the Nellis people.

OK. So there was that concern of radioactivity in that general area.

Yes. The Gnome event occurred down in New Mexico, east of Carlsbad about twenty-five miles. It was an underground test in a salt dome underground, and in December of 1961, they **[01:05:00]** had planned to detonate a device. I think it was a small, something like three kiloton, device underground. Before that event, we gave a forecast that if the radioactivity came out, it would blow toward the southwest, which would take it southwest of Carlsbad, toward Carlsbad Caverns. That was not satisfactory. However, on the morning of the event, the morning that it was originally scheduled, the wind was blowing toward the southwest but Frank Buck, one of our forecasters, was making that forecast. I was on the advisory panel at that time. Frank forecast that the wind would change during the morning and shift so that the trajectory of the degree, if it came out, would cross Carlsbad early, and later on in the day the debris would go toward the northwest and eventually go toward the northeast. The northeast was the preferred sector because there were only one or two ranches off in that direction. We told Jim Reeves, the test manager, the forecast, of course, in the readiness briefing, and he decided to just sit there, ready to go, and whenever the weather people gave them the word, they would fire the test. Well, it came along toward noon and the winds were still blowing toward Carlsbad. And they didn't want to test after noon because that wouldn't give them enough time before dusk in case there was venting, they wanted enough time to go out and clear those ranches in the downwind area. So they had made the decision, they would not fire after noon. Well, just before noon, we told Jim Reeves that even though the wind was blowing toward Carlsbad at that time, it would change and blow toward the northeast before the next few hours. So Jim said, Go ahead and fire it. And they did. It did vent, and the cloud took a curved path, starting off toward the northwest and curving toward the north and went into the northeast quadrant as it was supposed to go. So that's the reason [laughter] we had forecasting for underground tests.

Yes, that's a great example. Let's stop here.

[01:08:33] End Track 3, Disc 1.

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

The Plumbbob series was an interesting series to us because it occurred after the Weather Bureau had set up its permanent station at the test site, but the Atomic Energy Commission hadn't decided yet that we were fully capable of handling its forecasting requirements. So they asked the Air Force Weather Service to help us and actually be responsible for the weather service during Plumbbob. They came in with a staff that more than doubled our staff, and forecasters that had done this before, and they took over. I filled in and did some of the forecasting for some of the shots. The way that worked was that when the chief weather forecaster for the Air Force had been giving briefings every day for a week or so, he would become tired, so he'd call on me. Well, some of the bigger shots, such as Diablo and Hood, I can't remember their names, the bigger shots had more trouble with the weather and were delayed longer and the Air Force forecaster got tired, so he would call on me and I would come and make the forecast that the shot was actually conducted on.

That's interesting. Because it says here that Hood is, let's see—.

Yes, Hood was the largest atmospheric test to occur at the test site.

Seventy-four kilotons. Yes, that's big.

Yes. But I don't believe that Hood was the dirtiest test. Does it say what it was?

It tells us—here are the names [referring to DOE/NV-209, United States Nuclear Tests: July 1945-September 1992]. *Smoky was big, too.*

Hood was detonated from a balloon, and that's significant. The devices were dropped from airplanes in the early tests. They were put on towers later on when there had been time to construct the towers. For the Plumbbob series, they tried a new way of placing the device. They would suspend the device from helium-filled balloons. They hauled large quantities of helium to the test site. And these huge balloons would lift the device up fifteen hundred feet or so, on some occasions, above the surface. By doing that, they would eliminate from the radioactive cloud, they would eliminate the particles from the tower itself, the vaporization of the tower, and would eliminate any particles from the ground surface that had been disturbed by the fireball. The fireball would not touch the surface, and therefore there were fewer particles in the cloud that were intensely radioactive, so that the radioactive fallout pattern from Hood was nowhere near as intense. Even though it was the largest in terms of yield, it was nowhere near as intense a fallout pattern as for Diablo, for example.

And you were saying one of them was especially dirty? Was that Diablo?

I believe that was Diablo, and I believe that I was the forecaster that day. I don't remember for certain. And there was another dirty one in there and I don't remember the name of it.

Now, there's Smoky, which you—?

Smoky was one of them. Yes. It was from a tower.

Right. And it was forty-four kilotons.

[00:05:00] Yes. So that was a very dirty device. Very dirty fallout pattern. But I don't remember any of the details. I do remember one detail for Smoky, and I did make the forecast that day. When forecasters forecast the upper winds over the test site, for each level of the atmosphere, they give a direction and a speed, but they don't say anything much about the way the wind is curving as it goes over the test site. In the case of Smoky, there was a trough in the atmosphere so that the wind was in the process of curving at the time of detonation, so we made a curved prediction, a prediction that was used by the people who predicted the fallout pattern on the ground, the Weather Bureau people, was a curved pattern. They showed a fallout pattern that started off sort of toward the east but curved and went northeast so that it missed all of the cities to the east. And that's the only time that I remember during atmospheric testing that we predicted a curved fallout pattern. And it verified with the measured fallout.

Just so I understand it a little bit, you predict this, then they look at the weapon they use that day to decide whether, then, given what they expect to occur—?

The people who predict fallout patterns use the predicted winds that the meteorologists come up with. They use the predicted yield of the device and they know which isotopes are likely to be in the fallout pattern. They get that information from the laboratories. And by analyses that have been made of past clouds, they have a pretty good idea of how many of the particles will be above a certain size and how many will be at certain sizes and what fraction of the cloud will be of such small particles that they won't fall. Because the size of the particle determines the rate of fall. The large particles, of course, fall close in to the site of the detonation; the smaller particles fall farther out; and the smallest ones, micron-size particles, are likely to stay in the atmosphere for months or even years before they are rained out.

So when you say Smoky, or one of them, was particularly dirty, is this an indication, from what you know, that they knew ahead of time, or was this—?

They knew ahead of time that it would be fired from a tower. They knew that it would have a fairly high yield. And they knew which isotopes would be in the cloud. And putting all of that information together, they knew it would be a dirty fallout pattern. An intense fallout pattern. The pattern, of course, would be less intense with greater distance because the larger particles fall first and they carry the most radioactivity. The lighter, the smaller, particles are carried farther, so the amount of radioactivity decreases as one goes out along the fallout pattern.

[00:10:00] *So let's move to Sedan because—.*

OK. Sedan had a very interesting fallout pattern. Sedan was a device of something like 104 kilotons, which was quite large. It was buried over six hundred feet in the desert, and it produced a cloud that extended—well, it obviously broke the surface and produced a cloud that went about ten thousand feet into the atmosphere. That's about two miles. It drifted off to the northeast. And the cloud contained a lot of rocks and everything smaller than rocks, down to micron-in-size particles, of course. Well, the rocks and the heavier particles fell fairly close in, and the device actually blew big chunks of desert out in all directions, so that right close in, the fallout pattern was probably two miles wide, a mile in each direction. Some of it even went upwind because the explosion threw the rocks that far. But a large part of that cloud was of dust-size particles, ten, twenty, thirty, forty microns and higher, and those particles fell from the original close-in fallout pattern, leaving a trail of dust, a layer of dust, over the desert extending out at least fifteen or twenty miles to the northeast. The amount that could be seen from the test site was perhaps five miles wide and extended across the northeast side of the test site. The finest particles fell last, so that the top of that layer of dust was of very fine particles. And for months thereafter—this test occurred in July of 1962, July 7, I believe. Yes. July 6. And for months after that, if not even the next year, whenever a dust devil would move across the northern part of the test site, it would be hardly visible until it reached the Sedan fallout pattern, and at that point, dust would be sucked up into the middle of the dust devil and it would become like a solid pencil sticking up into the atmosphere a mile or two, as long as it was over that fallout pattern.

A mile or two.

Yes. And so the appearance of dust devils in the northern part of the test site was remarkable for all of the rest of that year and into the next year, before rainfall had wet the surface of the dust and solidified it to the point where the winds would not pick it up anymore. The upper part of the **[00:15:00]** cloud, consisting of smaller particles, moved northeastward and was seen as it passed Alamo. I think at Alamo, Nevada they had to turn on their lights during that afternoon. The test actually occurred somewhere near noon, and by—I'm not so sure about the time of the test. I think it was later on in the afternoon, there was some other town off to the northeast that reported the cloud going by. Ely, I believe. And eventually even a portion of the cloud—the cloud is probably several hundred miles across by the time it reached the Salt Lake City area, but it was visible in Salt Lake as it passed however many hours later, twelve hours or more later.

I think you said earlier that you're counting things like Sedan in your mind as atmospheric tests, is that correct, because they were cratering tests?

So far as the delays were concerned, test delays on account of weather, I did include Sedan, yes.

There were also—in the second test series, there was one underground test—there were two—and the name of that test series was [Operation] Buster-Jangle. Buster refers to five above ground tests; Jangle refers to two effects tests, the first of which was placed right on the surface, and the second one was placed a few feet underground. It was actually an underground test, so it is not counted among the atmospheric tests. And there was Palanquin, I believe the name was, which was a cratering test, and Danny Boy was a cratering test.

That's right. I didn't know that. Now, that's interesting. It says, "Uncle, First underground test at the Nevada Test Site" in Operation Jangle. But then—now you're making me—

Palanquin.

Yes, but I'm looking for Rainier. Oh, [reading from publication] "First contained underground." Ah, that's the distinction. I was thinking that Rainier was the first underground test. Now you've taught me something.

Actually it was in a tunnel.

Yes, and I talked to one of the physicists that worked on that. But I didn't realize. I was thinking that was the first underground test, and now you've made me realize, even though it's right here in writing, that it was back in '51.

Yes.

You were saying, Palanquin? Palanquin is about what year? That's late in the sixties [1965].

Oh, that would've been after Sedan.

Yes. That was in Northern Nevada.

No, Palanquin was out on the mesa west of Rainier, I believe. I counted it as a single test, one of the—what did I say, 105.

I think you said 105 that you counted as an atmospheric test.

Yes, because it was intended that it leak to the atmosphere.

Oh, OK. And why?

To make a crater. To make a crater, it has to be exposed to the atmosphere.

Right. Well, I don't think we need to look it up, but it's here somewhere. OK.

And there are enough others that qualified as intended leaks that it came up to 105.

Yes. That's interesting. Now back to Sedan for a moment. Where were you when it was detonated? Or it might be I'm asking, did you see it?

[00:20:00] I was at the CP-1 [control point], which was in the pass between Frenchman and Yucca Flats.

Right. So you did see it. You saw it go off.

I saw it go off, yes. I was on the advisory panel at that time, and I've forgotten—probably Frank Buck made the forecast, I think. He was one of my meteorologists. Later joined the staff over at Environmental Protection Agency [EPA].

But that must've been something, I guess. I'm trying to understand what that must be like, to see an explosion of that size, and then sending debris so high.

The rim of the crater rose—people can give accurate figures for this, but tens if not hundreds of feet, and fell back, leaving a ragged rim that everywhere around was considerably higher than the surface of the desert was originally. And it's permanent that way. You have to remember that the—well, the device was six hundred-plus feet under the desert. Enough debris fell back to fill the crater up to something like 360 feet deep. So there's almost three hundred feet of fill right in the middle of the crater. And enough dust and debris fell around the rim to raise the rim tens of feet.

That's very interesting to know because I've been to the test site twice now and we saw Sedan both times, and I think, again for a non-educated observer, you're not thinking that it—you're looking down and thinking, `Wow, that made a big hole.` *But you're saying it was actually probably close to twice as big, and then that's the debris that—*

That's right, to start with. Not only twice as big, it'd be more than that because if the device is six hundred feet down, when it explodes it vaporizes dirt down several tens if not hundreds of feet further, and all of that tries to rise. Not all of it succeeds. And so enough stayed in place and fell back to raise the crater, the initial crater, up to a depth of only three hundred and some feet.

That's so interesting, and then also I don't think I focused on the fact that there's a rim, but next time I go, I'm going to actually look at that. So that's a permanent change in the landscape, essentially.

That's right. That's right. Part of the rim is from the outward force of the initial blast, which just loosened rocks and made them stick up higher, and part of it is from rocks that fell back down.

Right, and didn't make it into the hole but stayed at the side.

That's right.

That's interesting. OK.

You wanted to go to Baneberry.

Yes.

Baneberry started off—at the time of Baneberry, I was a member of what they call the Test Evaluation Panel, which reviewed the drilling records of holes. The idea was to prevent—well, it was to review the information that we had about the geology and the technology of an exploding device, and the reason I was on the panel was because we were having occasional vents, a vent

[00:25:00] being a leak, and radioactive debris would get into the atmosphere, and for each even there had to be a forecast. Actually, I couldn't contribute much to the panel, but I was there.

Couldn't contribute much, in what sense?

To the physics of the emplacement of the device, the geology or the technology of sealing the hole above the device, and that became a very complicated technology. They tried a lot of things before they learned what would contain an exploding A-bomb.

In the case of Baneberry, the drillers had trouble drilling the hole because the hole would not stand alone; it would fill in. And after a while, they were able to get a hole that stood alone. They would case the hole, that is, put in a steel casing down the hole, once they got the hole to stand alone for a while. And they were able to case this, and they thought that by putting in the usual slurries that would harden, they would be able to contain the device.

Well, it turns out, after the device was fired, we had made our usual forecasts, assuming that—the forecaster always assumes it's going to leak. He hopes it doesn't, but he assumes it does, the weather forecaster. So Baneberry was fired only after some delays and the winds came around to be in the right direction, going off toward the northeast. We were pretty confident that the winds would blow toward the northeast at all altitudes, so the test manager gave the word for them to go ahead and fire the device.

After a few minutes, it vented, and then it continued to vent, and made quite a—it vented rather rapidly, making a quite large cloud. I think the cloud was close to a mile high. There were a lot of photographs made of that cloud. And after a few minutes, we noticed that the surface wind, the wind right at the surface, not the winds up above—the winds up above were going where they were supposed to, but the winds right at the surface had changed direction and they were blowing toward the west.

Well, toward the west is where Area 12 camp is. We had been so confident of the direction of the winds that the test manager had left people in Area 12 camp, which was about four or five miles away, during the test. Since the winds had changed and were going in that direction at the surface, we had to evacuate the camp, which required the activities of the security people. They had to waken people who were sleeping, miners who were sleeping during the day, and they had to move the cafeteria people and other people who worked out there out.

But it took us some time. It took the advisory panel some time to decide what route they should take out, because we didn't want them to get in their cars and drive right into fallout or a radioactive cloud. Once we had decided on the route, the word was given, of course, for them to **[00:30:00]** evacuate the camp. And I think the camp was mostly evacuated before any radioactivity got that far west. Within less than an hour. But the people who were exposed to radioactivity were given the usual wash down and treatment for having been exposed, and that included the security people. The security people stayed in there the longest, of course.

A couple of years later, two of the security people died, and their widows sued the government. The trial was finally conducted, I think 1978 or '79. It was in the winter. I think Frank Cluff at that time was the acting test manager. Frank Cluff had been a meteorologist in my shop, so he and I knew each other real well, and he's the one that gave the word to evacuate the camp. Since I was on the advisory panel and since I knew what had happened, I made notes for myself of what had happened and why. So I was invited to be one of the witnesses for the government, and I spent two-and-a-half days on the stand because by the time they got around to me, Frank had testified and I don't know who all else, they still had not reviewed the photographic evidence of the test. Most of the photographs were of the cloud, so it was up to me to describe how the photographs were taken, from what direction, and as a matter of fact, there was a videotape taken of the surface at the time of the venting, and I was asked to explain what was going on in these videotapes, all of which took a lot of time and that was one of the reasons I was on the stand for so long. I was there two-and-a-half days. The attorney for the plaintiffs accused us of having botched our forecast, and I agreed so far as the surface winds were concerned, and he and I had an argument about whether we had botched our upper wind forecast. And I used the cloud as evidence of the fact that we had not botched it, because the cloud spread out in the correct direction. But he was convinced that I didn't know which direction was which on the photograph. So we argued about that. Also, I had to explain why we botched the surface wind forecast, and the reason, as I recall, was that there was a storm off the coast of California, Southern California, that morning that intensified more rapidly than we had expected, so rapidly that its effect spread inland and included the test site, and started the winds in the test site area blowing from the east toward the west instead of continuing to blow from the south toward the north. In order to explain all of that and make it convincing, satisfying all the questions of the attorneys and the judge, why, it took time. That was Judge Roger Foley, I **[00:35:00]** think his name was.

You raised several questions, but let me start with the one you ended with. This question of surface winds. How do you, at the time, measure that they're changing? How do you realize that this has occurred?

We had wind instruments on one-hundred-foot towers scattered around the test site. That was a permanent network. And they were wired into the control room so that the meteorologist—well, as a matter of fact, there was one wind instrument relatively close to the detonation hole, and its direction showed on a little—what do you call these little red lights? Screen, right in front of the advisory panel, so we could watch it, and as soon as it changed direction, the whole panel knew it. Now we also had balloon soundings being taken, but they were taken from a station at the south end of Yucca Flat, not far away from the CP but not at the CP. And those readings were not immediately available. They were made available within a half-an-hour or so, but not immediately available. So we had both a balloon sounding of the winds and this little instrument on a hundred-foot tower right near the hole.

Next question.

Did you want to add something before I ask you?

Not really. I just wanted to say that what is considered surface is really an unknown depth from the surface up to a few hundred or maybe a thousand feet or so, and it's called a surface wind because it is influenced by the terrain, and it's also influenced by the initial heating from the sun on the surface. So it's not a very deep layer of wind, but it's deep enough to carry a portion of the radioactive cloud.

Right. So thank you for clarifying that, because for a lay person, surface may be what, thirty feet? But you're saying up to a thousand feet, so that's helpful. Does the detonation itself ever influence those winds?

Yes, it does, in the immediate vicinity of the device. It obviously would in the case of an atmospheric test, but in the case of an underground test, a large underground test, it might affect the surface winds because in the case of an underground test, the surface rises several tens of feet and settles back down. In an underground test that is contained, the surface settles back down and only surface dust gets carried away with the breeze. In the case of a venting, there's a spout, usually, a spout of cloud up, but that has only happened a couple of times. Most ventings are slow leaks, so that they do not change the direction of the winds.

Just so you'll know, I've done some reading on Baneberry, but mostly from Jim Carothers at Livermore, who was concerned about why this had happened and so some of his oral histories that he did, we're starting to figure out with the physicists, why did this happen, because he was very concerned about that. So this is interesting, to get this other viewpoint. But it also gives me a little window into what actually happens in the CP, which was my other question. So you're **[00:40:00]** *actually—the members of that panel are sitting there, thinking about, making those decisions. Tell me more about that.*

Yes. This was not true for most of the—let me change that. In the case of Ranger, the advisory panel was at Nellis. In the case of the next series, Buster-Jangle, we used the CP, and we used the CP for all of the tests in Yucca Flat and Frenchman Flat, we did not—well, we used the CP meeting room, panel meeting room, for all tests at the test site. In the case of the Pahute Mesa tests, we would meet as a panel in the CP and then drive to a forward area to where we could see visually the area. Usually we would have a forward CP, a trailer, in which there were TV screens. The TV screens showed photographs—in the case of Baneberry, they showed photographs, actually motion pictures, electronic video presentation, that were taken by helicopter, and I think there was at least one helicopter video at the time of Baneberry. There were other TV screens, but there were TV cameras located on the ground at different angles from the test. And so in any one test, the advisory panel and the test manager would meet several hours before the test to observe the latest forecast of winds and the weather, any information of change in the situation with the device, the stemming of the hole or the instruments connected to the device, the diagnostics, and information from Public Health Service regarding offsite people, information from REECo regarding onsite people, and from the security, which roads are blocked and where people can move. We consider all of those things in each of these meetings prior to the detonation. If the weather is questionable, we ask for additional wind soundings before we decide to shoot, and sometimes in the case of underground tests where daylight makes no difference, we would delay until noon or later. We didn't like to test in the late afternoon because if something untoward happened, there wouldn't be daylight left to deal with it.

In the case of Baneberry, the test occurred, as I recall, around 7:30 in the morning and by 7:35 or 7:40, we knew that the wind was beginning to change and we had given the information to evacuate Area 12 camp sometime within the first twenty or thirty minutes. I don't remember exact times.

Do you have a recollection of sort of how it—I don't know what the proper word is—dawned on **[00:45:00]** *you when people began to realize individually and as a group that something had gone wrong with the test itself?*

Well, there were a lot of gasps. It was almost immediate that it vented. Within—I'm guessing now because I don't remember, but it would be like a minute.

And you see this—?

Yes. And the second place one would look after seeing that it was venting was at the wind measurement that was visible on the tube right in front of us. And as soon as that was wrong, we knew we were in trouble. And we as a panel discussed what had happened and discussed the fact that there were people in Area 12 and discussed the exit route that they should take, all of this before the word was given to actually start the evacuation. During the trial, those things were brought up in the trial and the plaintiffs were arguing that we should've given the evacuation order earlier. You can't blame them for that. I don't remember very much else about the trial, unless you can jog my memory.

No, and I know that UNLV [University of Nevada, Las Vegas] has some papers from the trial, and I think that those things are available to me in paper form, so that's not necessary. I'm actually—did you want to say something?

Well, we were concerned, the advisory was deeply concerned, both before anything happened and after. By before, I mean during these Test Evaluation Panel meetings, days and weeks in advance. I think that hole was given more attention than any other hole up to that time, because it was different. We didn't know what the difference was, and I think we ended up speculating that it was all right. But I'm not a geologist and that gets into the field of geology.

Right. I think this also connects to an observation you made, or a statement you made, that's interesting to me generally with this project, which is—we don't have to get into it, but I'm sure you have some insights about it from being on the panel. All the kinds of things you have to think of from a scientific and engineering point of view once you go underground. You said it very succinctly, but it presents a whole bunch of other problems.

That's right. The cementing of the tuff—cementing of the—I forget what they call it out there, the rocks underground, is important. The depth to hard rock. In the case of Rainier Mesa, the nature of the tuff. It isn't uniform all the way through the mesa. And all of these things are discussed in the Test Evaluation Panel. At the readiness briefing, as they call these briefings three or four hours ahead of the detonation, those things are never mentioned except very briefly because they've been considered beforehand. It's too late for us to do anything about that. We're concerned about what has to be done or can be done or what anybody thinks of doing just in the **[00:50:00]** last few minutes before the test.

So am I correct in observing that you always have this problem of there's things that you don't know.

That's right.

You're working on the knowledge that you have.

That's right, and that's true of any frontier. And I got a little disgusted with people in the space exploration business, wanting to have perfect control and perfect safety in connection with the exploration of outer space. It's just not going to happen. And even in the current terrorism business, whatever our homeland security can think of in the way of preventatives are going to be thought of first by the terrorists themselves, and the terrorists are going to skip that and go on to something else. So that I'm not saying that the government shouldn't go through all of these safety endeavors. What I'm saying is that the terrorists are going to be ahead of us no matter what we think of. The best thing to do is to think of ourselves as terrorists and what would we do? Try to get ahead of them that way. And that works most of the time, but can't be expected to work all the time.

I think back to your first point in this little section about the space program, safety versus *exploration. That seems to me to be indicative of really a philosophical divider, a cultural divide, about science and moving forward in those areas. Of course, at the test site you have the added complication, political complication, of what you're doing, which is making weapons, you know, preparing to make weapons in the Cold War, whereas in the space program, it can be seen more purely if it's just space exploration, " purely." But how people evaluate the value of doing it or not doing it, I think that there's a real break sometimes between how the scientist is thinking about those things and how the general public is.*

You certainly are correct. And I'm afraid that in that department, I depart from the scientists because the scientists want the information and they're willing to spend billions and billions of dollars to get the information about other planets and other parts of the universe. Since the existence of habitable planet is practically nil, well, my personal opinion is that there is no place outside of our own Earth that man will ever succeed in inhabiting beyond perhaps mining for minerals. And Mars is farther away than the moon. I think our first efforts should be to exploit the moon before we try to go any farther, because any farther is so terribly expensive, as well as **[00:55:00]** risky. So I'm in favor of holding back on the exploration of space and taking it slow and easy. I'm not saying let's not do it. Let's progress very slowly.

If you really want to get into the regional air pollution study—.

I did. We've got about eighteen minutes left. Let me pause this for a second.

[00:55:46] End Track 2, Disc 2.

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

So my question was about the experience of the trial in the midst of all these—?

Yes, I was invited to testify, and I was not paid for it. Never received a cent for the testimony. But I was interested in having the position of the government reinforced because I felt that we had done everything that we should have done under the circumstances. So I was glad to do it.

I had a rooting gallery of one person, and this sort of affected the way I took the whole thing. This will sound like a large digression but it isn't. Our daughter married an Australian and moved to New Zealand. He was a doctor in agronomy and was in research in New Zealand and made a lot of friends there, of course. One of those friends we had met when we were over visiting them, and he came back to visit, or came to the States—he's a New Zealander—came to the States to visit at just about a week or two before the Baneberry trial started. And he really wanted to sit in on this trial. So I took him with me every time I went. And in between my appearances, since I made appearances on three separate days, we had plenty of time to discuss the whole thing.

But I just felt that the truth would prevail and so I told everything exactly as I remembered it, with the help of my notes, and I may have gotten some things wrong when it came to describing what was on those videotapes, but I wasn't aware of it if I made errors. It was sort of a challenge, too, so I didn't have any hard feelings or bad feelings about it. I just wanted the judge to have all of the information. It was a judge; it was not a jury.

The judge made the decision, but not immediately. After the trial finished, we waited, I think it was five years before the final verdict came down. And the final verdict was, briefly, the plaintiffs did not prove that the deaths of the husbands resulted from the radiation. They did not prove that. However, the judge did decide that the government was negligent in its conduct of the test. I forget whether he specified any part of the government's operation or not, but just in general it was we were negligent.

And what was your response, having been there on the ground, to this characterization? Or judgment, I guess.

Just not surprised. And so it went that way. I was happy that it was shown that the two people who died had not died as a result of radiation because if the judge had decided in favor of that being the cause of death, it would have opened up a huge number of trials. A large percentage of **[00:05:00]** the people in Utah who died of cancer in those days would have said that it was as a result of radiation. A lot of them did go to trial for deaths that they thought was a result of radiation. I have known all along, as have most people involved in nuclear radiation, have known all along that there were no exposures large enough to cause death, but proving it is not easy to do. Proving a negative is very difficult.

Right. I guess I am curious, does it have any personal effect on you to be on the stand in that position as a defendant, in a sense, and when the judgment comes down, that the government was negligent, since what you're describing to me is a lot of care. I mean does that—?

Long before this ever went to trial, the AEC changed its procedures at the test site. What had been the Test Evaluation Panel became—it was reorganized completely and I was taken off of it. It became the Containment Evaluation Panel, and people who knew more about the Earth, geophysicists, were put on the panel. And I'm sure that the government has always done everything that they knew how to do to provide for the safety of people. It's just that this was a whole new area at that time. And we could probably have made it safer. I know we could have because they did it. It's just they didn't do it immediately at the beginning of the test site. But I still don't think that anybody offsite died as a result of radiation. There are one or two people onsite that were exposed. I don't know the details.

Yes. It would be interesting, and not today, obviously, but to talk about that because in my mind, that question of the downwind and the Downwinder population and all of that is directly related to weather questions, right?

Yes. That's right.

Because you've got the radioactive cloud.

One thing that I didn't mention, and it pertains here. I forget exactly when this happened, but after I left the test site. I left the test site in March of 1972. There was a demand that all of the Public Health Service measurements of radiation offsite during the atmospheric testing days, all of that data was reexamined, all the way from the people who made the measurements through the Weather Bureau and its interpretation of the results. They were all reinterpreted, actually, and new maps were drawn about the total amounts of radiation received by different areas downwind. And I can’t tell you the exact results, but what I was told was that not much was changed from the original, which means that if anybody really died as a result of radiation from the tests, it would’ve been hard to prove that that’s exactly what caused the death. My personal **[00:10:00]** opinion is that nobody died from that one cause. There are other sources of radiation in Utah. Well, everywhere. Everything that one touches contains some radioactivity, and especially where mining of uranium has happened, or where the surface soil contains an unusually large amount of uranium or uranium products, and that includes most of Utah. People are, just by living there, exposed to varying rates of exposure. Dairies. The quickest route, apparently, to get radiation from the soil into a person’s body is by way of milk, and a lot of dairies exist in Utah and their milk goes to the children in Utah. And I don’t know whether the people who own dairies have ever had the soil tested where their cattle graze for natural uranium and natural uranium products. That is all a factor in deciding who died from leukemia or anything else. It’s a huge problem and probably there will be people that are not satisfied, no matter what we do.

Yes, it’s a huge topic here, but particularly when you talk about the test site. That’s a definite sort of cultural element of what you all worked on.

Yes. Now, the meteorological work was all considered to be part of what they called safety. They had a part of their budget dedicated to safety projects that included the Public Health Service and part of REECo’s monitoring of the test site, the film badge activities.

Right. And your work fell into that purview.

All of meteorology fell into that, yes.

Any other questions?

No, I think this is fine. We can turn it off.

OK.

[00:13:06] End Track 3, Disc 2.

[End of interview]