

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

Interview with
Delbert Barth

March 18, 2005
Henderson, Nevada

Interview Conducted By
Mary Palevsky

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Interview with Delbert Barth

March 18, 2005 in Henderson, NV

Conducted by Mary Palevsky

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

Mary Palevsky: *Dr. Barth, thank you so much for talking to me again. I thought a good place to start again would be to recap the program you were hired to do at the NTS [Nevada Test Site] in 1963; to address the question of the models used for offsite radiation exposure, I guess that would be the way you'd put it, at the test site; give a little background about how that came to be and where you were at the time.*

Delbert Barth: Actually, I had completed my Ph.D. at Ohio State University in biophysics and I was working in Washington, D.C. for the U.S. Public Health Service when I was asked if I would like to come to Las Vegas to develop a program for doing a better job of linking sources of radioactivity generated at the Nevada Test Site to levels of iodine 131 [I131] which would be found in the offsite population. In the early days of testing, it simply was not appreciated how important the levels of iodine 131 could be. And so those levels were not measured in the beginning. So what we had to do was to try to reconstruct what those levels might have been by developing models. In order to develop those models, we had to do a certain amount of experimentation and also take advantage of any inadvertent releases from the Nevada Test Site.

In early 1963 there was a planned event at the Nevada Test Site, and we had developed an experiment where we were going to be measuring air concentrations of radioactivity and forage concentrations and concentrations in the milk at various dairies, particularly over in Utah, since most of the radiation that left the Nevada Test Site moved in an easterly or northeasterly direction and exposed certain dairies in Utah. So we had this experiment all ready to go, and at the last minute our administration decided to sign a treaty which would not allow any nuclear

tests to be conducted in the atmosphere [Limited Test Ban Treaty, 1963]. So this particular test was cancelled at the last minute.

So we never did achieve the experiment that we had hoped to develop in early 1963. So ultimately what we had to do was to try to develop a somewhat artificial situation which was having our own forage, our own dairy cows, in a controlled location in Area 15 at the Nevada Test Site. We could expose our own forage and our own dairy cows to different levels, small levels, of radioiodine aerosols, and then to measure the buildup and decay of iodine 131 in the milk of our own dairy cows.

And in addition to that, we always had a crew ready in case any of the events leaked from the test site. And there was one that did leak and exposed some of the dairy cows—well, and the first instance was the Pike event which exposed dairy cows in Las Vegas. There were a couple of dairies here in Las Vegas at that time. So we were able to get some measurements from that. And then we got a lot of measurements from an event called Pin Stripe which released radioactivity [00:05:00] over parts of Nevada and then on into Utah. But it was a relatively small amount and so we were able to get measurements in Nevada but there wasn't enough radioiodine that was transported to Utah to enable us to get the kind of measurements we needed in the milk there.

And so it was the combination of those inadvertent releases, the data that we got from that, plus the data that we got from our own farm that enabled us to construct a model. Because what we were trying to do was to have an improved model for actually estimating what the doses of people in Utah to their thyroids from iodine 131 was back in the fifties when they were conducting tests and the iodine 131 was not actually being measured. And so our whole mission in life was to develop improved models for estimating dosage of human thyroids to iodine 131. So that's what we were trying to do with the program.

A couple of questions about that. Back historically, you're in the Public Health Service, you have come over from the Army—you told me that story last time—and what was your rank in the Public Health Service at that point?

When I transferred from the Army into the U.S Public Health Service, I was a major in the Army, Army Chemical Corps, and I had been trained especially in chemical, biological, and radiological warfare. And so I went from trying to estimate when we actually use weapons, how effective those weapons would be on the people that we were using them on in the Army Chemical Corps, over to the U.S. Public Health Service where the major concern was the *safety* of people from dosages of radiation which they received. So I transferred directly over. I was a major in the Army; I became a lieutenant commander in the U.S. Public Health Service.

That's an interesting point that you raise, and it was actually something I was thinking about before I came over this morning, is that shift from the warfare point of view to another aspect of the Cold War, which is we're testing and the decision had been made that it was necessary to test, and then the safety issues that are raised by that. So in your own mind, was there any, for lack of a better word, psychological shift or—?

Actually I really didn't feel any psychological shift because although I had been trained to use nuclear weapons and to help to control and design the use of nuclear weapons in warfare, we never used them after I was trained in this program. So even though I had the knowledge, we never used it, and so it was very simple to just turn the knowledge around and worry more about safety than about adverse effect deliberately on an enemy. So it was really not a problem.

Then explain to me the genesis of this particular work that the Public Health Service is asked to do out at the test site. We were saying before we started to film, I asked if it was an AEC [Atomic Energy Commission] initiative. What was the sort of administrative genesis of this program?

Actually, the Atomic Energy Commission funded the entire program, and they did it out of their operational budget. It was not really from the research and development budget of the AEC. It was out of their operational budget because they wanted to be sure that they were not allowing too much dosage of iodine 131 to offsite populations, both in Nevada and Utah and in other locations as well. So it was the operational aspect that actually funded our project, and not the research and development program.

[00:10:00] *But you said also that it was not only looking forward but also a model that would look back to what had already occurred.*

Well, the whole reason for this program was to do our best to develop an *improved* model which would predict what the doses of radioiodine *would be* from a release from the test site, as well as what they were in the *past* when we know what the *yield* was but we had not measured the levels of radioiodine, and so we needed an *improved* model in order to estimate those levels.

You said a few moments ago that there was really not the consciousness early on about the iodine.

No. It simply was not appreciated that—what you really need to do in radiological health is determine what the critical organ is in the human body and then also what the critical radionuclide is that doses that critical organ. And it was not appreciated back in the early fifties that iodine 131 and the thyroid were the two—iodine 131 was the critical radionuclide and the thyroid was the critical organ in human beings. That was not appreciated, so they were not measuring those doses back in the fifties.

Now in layperson's terms, help me understand that a little better. That means that if there's too much of the iodine 131, it deposits in the thyroid, is it—?

Well, first it has to get inside the body, and it gets inside the body by many different ways. If it's in the air, you can inhale it, and once it gets inside the body, it will get into the bloodstream and then it actually goes to the thyroid gland and a certain *fraction* of all that goes into the body is actually *deposited* in the thyroid gland. So you can get it via the air you breathe, the water that you drink, the food that you eat. And the major food of concern is milk because that is the critical exposure route that you have to worry about because that gives you the *maximum* dosage to the thyroid gland in the general situation. Of course, if somebody doesn't drink milk, then that's not going to be a problem for them. But almost always, the major person of concern is a young child drinking milk from a dairy cow which has not gone through all of the processing. Because the iodine 131 has a half-life of eight days, which means eight days after you have a certain level of iodine 131, you will only have *half* that much. So the longer delay time from the time the iodine 131 is produced in the middle of a nuclear bomb to the time it gets into the milk and then the milk is consumed, the lower the dose is going to be.

What about other animals that are eaten? I've been talking to some American Indian people, and there's been some science done on whether, for instance, a rabbit that's eaten in the early fifties by the northern Nevada tribes, is that also something that would be an iodine pathway or however you want to say it?

Yes, it definitely would be. If you are consuming animals that have been exposed and that have radioiodine in their blood, most people would probably not eat the thyroid gland of the rabbit, but there is always a certain fraction of the total that gets *into* the rabbit via the grass it eats or the air that it breathes that will be left in the muscle, essentially, which is what most people eat of the rabbits. Usually you would not eat the thyroid gland. But there would be some, not only rabbits but we were concerned also about deer, and we had a program where we sampled deer

[00:15:00] periodically at the test site and measured the amount of iodine in the thyroid gland of the deer. But this was of course very late and long past the time when the maximum amount of radioiodine was being released from the test site. So what we had to do, again, was to try to develop models for what *might* have been the case back in the early, early days. And our concern, our major concern always was with small children drinking milk from a dairy cow on a ranch where you would not have the decay period of having it collected from the ranch and go to a place where it is processed and then bottled and then taken to grocery stores and sold.

Can you tell me a little bit about the—was there a political impetus for this program? I'm asking you because now there's a lot of publicity and has been a lot of activism on the part of downwind populations about exposures, but back in the early sixties, was AEC being pushed to do this or did this come from their own internally, do you know?

Well, I think it was a combination. I really don't know the answer to your question, but I can speculate and I would speculate that it was a combination. The public health agencies felt that this was necessary and the Atomic Energy Commission wanted to conduct their tests in a way that they could ensure safety. So *they* wanted the information as well. So it was a *mutual* situation where both the U.S. public health agencies *and* the people who were doing the testing wanted to have better models so they could predict what kind of offsite exposures would occur as a result of leakage. Because by this time we had a total [atmospheric] test ban treaty [Limited Test Ban Treaty] and everything had to be conducted underground, so they were concerned mostly about leakage and whether or not any actions needed to be taken if leakage occurred in order to protect health and safety.

So you use, I am assuming, some kind of mathematical model that you have to develop to extrapolate back in time from current time to what exposures might have been?

Well, after we had conducted this program for approximately five years, we came up with our best estimate model which started with the source term. And the source term is a reference for how much radioactivity was injected into the air at what location. Then in the modeling exercise you have to look at the various air currents, which way they blew the material, how high the material went, and how it got spread over various locations. And then as a result of all of that, how much was deposited on the forage which was being consumed by dairy cows and then get into milk and then be consumed by children. So the model had to start with the source term, how much of a yield was it and what kind of a nuclear bomb was it. Because they had reasonably good data for nuclear bombs. What percentage of the radioactivity would be radioiodine?

So I had it a little backwards. So let me see if I've got this right and you can clarify again.

You're doing all these experiments at your location in Area 15, which I want to talk to you a little bit more about in a second, and from that you get certain data that then you go back and apply to this source term to figure out what exposures would've been based on this complex of factors.

Well, the work that we were doing in Area 15, we used *labeled* aerosols. These were aerosols that were tailor-made to a certain particle size and labeled with iodine 131. So we were using iodine 131 itself, whereas when you try to develop a model that deals with real release from a [00:20:00] nuclear weapon, the iodine 131 is there *with* all of the other things. So you have to know what *fraction* of the total yield was radioiodine and of that fraction, how much was gaseous, how much was particulate, how that changed as a function of time and as it moved downwind, and what caused it to deposit on forage and how much of it went on forage at different locations. All of this was part of the kind of models that we were trying to develop. *That's extremely complex.*

Very much so.

So tell me a little bit about the physical setup in Area 15 and how these aerosols would be applied.

Well, in Area 15 we had a water supply there and we just literally cleared the area off by moving all the rocks and we developed a plot of ground in which we planted alfalfa. And this is a common material that is consumed by dairy cows. And so we had our own growing alfalfa in Area 15. We had our own water supply; we had our own herd of dairy cattle; and we had all of the data information on each one of the cows with regard to how much milk they yielded and how that changed as a function of time when they became pregnant with calves and so forth. So we had all of that background information. And what we did was to release aerosols labeled with iodine 131, and had to have very carefully controlled conditions so we would be sure that we weren't going to be harming anybody *away* from the Area 15 site. And the weather had to be just right, and what we usually sought was a very *low*-level wind which would move the materials very *slowly* across our forage grass, which was alfalfa, and then we would cut the alfalfa, and it's called [sp], what you get from that, and we would then feed it to our cows inside our dairy barn there. And we would measure the amount of material which was deposited on the forage. And we were able to tailor-make our aerosols so they went all the way from fairly large particles down to gaseous materials. And so we were able to see how things change as you change the character of the aerosol containing iodine 131. And that was all part of the modeling that we did. And our final report at the end of all of that summarized everything that we had done in the entire study.

Now this report, was it published or was it submitted or—?

It was submitted to the Atomic Energy Commission and was subsequently published as an EPA [Environmental Protection Agency] document. And that was about probably '69 or '70, in that time frame.

So is that something that I can see in some sort of version, non-classified version?

Oh yes, I have a copy of the report.

Oh great. Two questions from what you just said. You must have had a very specialized kind of staff working for you, then, to be able to do these various things.

Well, we did. I mean we needed veterinarians for help with the cows. We had a couple or three veterinarians on the staff. We needed physicists, aerosol physicists who understood how particles travel with air and how they deposit. And we needed farming people who understood how the grass grew, how fast it grew, because that affected the concentration. So yes, and we needed [00:25:00] chemists and people who could analyze various kinds of samples for how much radioactivity it contained. So we needed a large group of special people. And when we wound up, I think there were eighty-some people in the program when we finally finished the project.

Did you recruit those people from Public Health, from existing—?

When I first came on board, I was the only one and there were a couple of people who were already associated with the Las Vegas laboratory that were assigned to me. So we started off with like three, and then we analyzed the type of skills we needed and then went out and looked for them and interviewed people and hired them.

Who were the people that you started out with?

The one that I remember very closely was Joel Veater, V-E-A-T-E-R, and I'm not even sure where he is anymore, but he left Las Vegas a long, long time ago.

And he was with Public Health?

Yes, he was with the Las Vegas laboratory, which was conducting the measurements offsite in order to document how much radioactivity left the test site. They had a group of sampling equipment set up in various locations around the Nevada Test Site, and when an event occurred they would go out and be measuring with various instruments, but in the early days those instruments did not measure iodine 131. They measured gross beta and gross beta plus gamma and gross gamma exposure, which you get from a very large [00:27:14] collection of radionuclides. And so you really couldn't estimate exactly how much of that was iodine 131 because it would fractionate with time as it moved downwind, because some of it became more particulate, the gases attached themselves to particles and they changed, the character of this aerosol changed as a function of time.

Then did you see any—I'm asking this a little bit because people think about this and people joke about it. When you're having your cow herd – joke about it in sort of black humor, let me say – did you notice any birth defects or anything in the animals themselves?

No, we did not. And one of the events, one of the Plowshare events – the name of this particular event was Palanquin – we had our cows staked out. And one of our sites where we had cows and hay out – this was done in the wintertime so we didn't have any growing grass – we put hay out. One of our stations was on a hot spot on the hot line, as a result of which it was so highly radioactive that we couldn't go back in immediately to get the kind of data we wanted. And our cows were in there all the time, so they were exposed to a large amount of radioactivity, and actually some of them developed gray hair as a result of the exposure to radiation. But we did not see any *other* adverse effects in the cows, other than seeing the hair on their head turn from black to gray.

I know that Palanquin was a Plowshare, but was it—at this point it had to be underground, no?

Well, the whole Plowshare program was to determine how to build a sea-level Panama Canal, and so what they were trying to do was determine how big a *hole* in the ground you could [00:30:00] develop with an explosion, a nuclear explosion, and how much radioactivity was going to be left in that hole in the ground. So always the Plowshare events would release material. And also they were doing nuclear tests out at the nuclear rocket development program [Nuclear Rocket and Development Station, NRDS] and we got measurements from some of those tests.

From the reactors out there.

From the reactors, yes. But that was a different kind of thing from the nuclear explosions. I mean it was still radioactivity caused by the chain reaction that you have for both reactors and for the nuclear explosions, but the character of the source term was different for those two cases.

Reactor releases and nuclear explosion releases.

So you would put animals out near the reactors or—?

Actually, as I recall, we had animals out only on Palanquin. For the nuclear reactors, we just had measurement stations where we sampled the air, and we took various kinds of samples and then from that estimated how much if there would've been forage grass at this given location. I remember one thing that the nuclear reactor people had estimated that particles, high-level radioactive particles were coming out as they were running these reactors. They said all of these would be deposited probably within one to three miles of the location. So when one of the tests was conducted, we sent a team out, and from the weather we knew which direction it had gone, and we were seeking out these particles, and we found the last particle out eighty-four miles. And it was so dark, we couldn't go any farther, so we terminated our study right there, but it became very obvious that these particles were *not* deposited very close to the reactor. They were

a special kind of particles and some of them had almost aerodynamic shapes, and so they kind of floated along with the air and went way out. So we had a lot of those particles that we brought back, and then we did extensive testing to determine the character of the particles that were coming out of the reactors.

And what kind of instruments are you using to detect them that far out?

Well, actually all you need to do to detect a highly radioactive particle is the simplest kind of a Geiger counter. That's all you really need because they were highly radioactive, these particles, and if you got close to it, you could see it, and then you would keep circling till you got the maximum measurement, and then you would scoop up some of the soil and you would have the particle in there.

And how much longer after the reactor run or whatever it was would you go out? I mean how much time would—?

Actually, the very day that the reactor ran was when we were trying to get the measurements because remember, we were concerned mostly about iodine 131 and it has an eight-day half-life, so you want to sample when you're going to get the maximum level.

So you literally are following the particle across the desert.

Yes, we were able to do that on one of the reactor tests.

Do you remember what the reaction was when you told them it went farther than three miles?

[Chuckling] Well, they couldn't very well negate our study because we brought particles back that we found way out there. So they were astounded, that's my opinion, in seeing that the particles went out that far. I'm trying to remember. I believe it was the Kiwi reactor. That was the name of the particular reactor that they had this test and we were available for the test and [00:35:00] were out taking measurements and had heard about these hot particles because at a

previous test, one of our people at the farm wound up with a hot particle on his coat or on his clothing, and this was picked up when he went through a hand-and-foot counter. So we *knew* that the particles were getting away. It was a question of how *far* they were going.

That's very interesting. Kiwi was that reactor. It'd be interesting to later—I'm curious about what that was.

Well, it's very, very difficult to do an adequate risk assessment of what the hazards might be from these particular particles because there weren't enough of them that you have a huge amount of particles in any given place, so in a large area you may have only one particle, and it would all depend on whether somebody was right there and inhaled that or ate it in some way in order to do the hazard assessment. But then you have to calculate the *probability* that that would take place if you're going to do a meaningful risk assessment. And we were doing that only to get a comparison of the particles that were getting out there versus the kind of experiments *we* were doing, because we were *mostly* concerned with nuclear explosions, not with reactors, because the reactors were only in an experimental phase. They had never been installed on any kind of rockets or whatever.

So you mentioned your own aerosols. You must have followed your own particles in the same way to make sure that they stayed within your predictable area?

Yes, we were able to have downwind from our plot that we were releasing—we were releasing them over our growing crops, and so we had stations past the crops to determine what was going there. We were using trace levels because you can measure very small amounts of radioiodine and it wasn't necessary to use a large level of dose which would cause a major problem offsite.

We were careful to design the study in such a way that that couldn't happen.

So after the five-year period, your mission is basically completed there?

Well, that completed the studies that we were doing on radioiodine and our final result was an improved model for tracking radioiodine from its source to milk. And there were well-developed models to determine dose to thyroid once you had the amount that was in the milk and how much milk was being consumed. So those models had been well-developed. But what we needed to do was get a better handle on tracking the radioiodine from the source *into* the milk, and that was the *real* purpose of our entire study. We didn't actually take it beyond that because you know it's just not appropriate to use people in this kind of a study, so we never considered the final step of this. We took the information that was already available in the literature for the final step.

Now in what way was this used, if any way? You report this back to the AEC.

Well, actually the model that they were using at that time was the Pike model which we had developed when we got measurements at a farm called Habbart's dairy farm, which doesn't exist [00:40:00] anymore. It's H-A-B-B-A-R-T, as I recall, but it doesn't exist anymore. And they were using that Pike model and they felt that the model that we had developed was close enough to the Pike model that they didn't have to switch over from the Pike to our model. But actually what we were able to do is essentially confirm that the Pike model was a reasonable one.

So you concurred with that?

Yes.

And this dairy farm was up in Utah, the Habbart farm?

No, no, no, it was right here in Las Vegas.

It was in Las Vegas. OK.

The Pike event leaked and came straight down the highway to Las Vegas.

That's right. That's what you said.

From then on, they never would have a test if the winds were blowing towards Las Vegas. They learned from that. You never can be sure that it's going to be contained, even though you *expect* it to be contained underground.

Is there anything else that you think of you should tell me about that era?

I think we've covered that reasonably well in summary. You know I've left out many, many details, and as a result of all of those studies we probably developed somewhere on the order of ten or fifteen reports summarizing what we were doing and various aspects of it, and then our final report summarized the whole project, all five years of the data that we collected.

Yes, obviously we can't get into all the detail, but is there anything that's coming to mind right now that you think would be useful? A detail?

Actually, right now I think the major problem that needs to be addressed is not the exposure problem but it's the actual effects on people, because over time it has become more and more understood that lower levels of radiation spread over longer periods of time can have an adverse effect. I think that's the area where the research needs to be done now. Get a better understanding of the effects of low levels of radiation spread over longer periods of time. That I think is the most important scientific question now. And this becomes very important when one looks at the Yucca Mountain high-level nuclear waste repository.

Then let's take a move over to that question, again, that we were talking about before we started recording, of the long-term, the Downwinder question, the theories about dangers of low-level radiation, and the at least two camps that I know of about how much danger there is in the low-level radiation. How can you enlighten me about that? Because obviously it's something that I hear many different viewpoints about as I go around and talk to people.

Well, the reason you hear many different viewpoints is because people simply do not understand the mechanisms that are involved, and the more research that is done, the more confusing the results become. I mean that's me talking. And so essentially every major experiment you do, you come up suggesting maybe five or ten more studies that ought to be done to try to understand what you've just done, or a *piece* of what you've just done. So that is the major problem. And I was a consultant to a group at Hanford, Washington where the Downwinders there were exposed back in the early forties—well, in the mid-forties, '45 and '46, to levels of radiation which [00:45:00] escaped the Hanford site and actually got into the location offsite where people were living. And there was a major study over the course of seven or eight years, I don't remember exactly how long, to estimate the doses of radioiodine to people who lived offsite but close to the Hanford location. And I was a consultant and was part of that particular program, and my job was to help to integrate all the information that we had on source term, on transport, on how much would get on forage and then how much would get into cows, and then how the milk was processed and how it got to children. All of these factors had to be integrated and quality assurance put on the entire thing. And my job was responsibility for how to put all of that together and get the appropriate quality assurance and quality control on the program. It was finally concluded and final reports came out estimating the exposure, and the estimates really came up with some kind of a median dose which would be expected at a certain location at a certain time as a result of a certain source term that was generated. And from this, you can then calculate what the dose was at various locations. And that final report came out, and that report was used as the basis for a major epidemiologic study to determine whether or not there were thyroid diseases that resulted from the exposures coming from Hanford. And of course the Downwinders were absolutely convinced that there *were* adverse effects because they would

point to friends, relatives, who developed thyroid disease, and they were absolutely convinced that it was because of radiation coming from the test site. I mean from the Hanford site. So what they did was to develop the thyroid disease study, which was a major study that was conducted. And the design of the study was called a retrospective dosimetry study where they would draw samples randomly from various locations throughout the entire region of the study and then from wherever they drew them randomly, that they would use the doses that had been estimated from. It was called the HEDR Project, Hanford Environmental Dose Reconstruction Project. And those results came out negative. And the people who lived there, the Downwinders, simply never believed those results, and they felt that the reason they came out negatively was either because the study wasn't designed right or the doses were not calculated correctly. So right now, there is a court case going on in the State of Washington, and a group of the Downwinders have sued for adverse effects, and so everybody is looking back at all of these things, including the design of the epidemiologic study and the estimate of the doses to try to determine whether or not there really were adverse effects. In my own estimation, I felt that I didn't like the design of the epidemiology study. Instead of a retrospective dosimetry study, I felt that a case control study should've been done.

Explain to me what this—

The difference between a case control study is you identify people who *have* thyroid disease [00:50:00] and then you determine their dose, whereas the opposite approach is you determine the dose but you select people at random. So you're going to get some that have no effect and very few that really had an effect, because people would come to them and say, I want to volunteer to be in this study because my daughter had thyroid problems. But they'd say, Oh no, you can't do that. We have to extract these people at *random*

from the population. And my approach would have been to use the people who *had* the problem, which is called a case control study, and the people who designed this said, *Yes*, but the case control study doesn't have the same *significance* that the other one does. It doesn't have the same statistical power. And that's true, it doesn't, but the people would've been a *lot* happier if they would've at least *started* with a case control study and then possibly if they found a positive effect, then go *on* to another one. But the problem there is these studies are incredibly expensive and you're looking at millions and millions of dollars to do this one study.

And right now, as I said, this is in the court and people are trying to either convince the judge that the study wasn't done right and that's why it was negative or the doses weren't correctly established. And I was very much involved with the establishment of the doses, and personally I don't think we could have done a better job with the data that were available. And the problem was very much like it was here at the test site. Back in '45 and '46, they were not measuring the amounts of iodine that was coming out, and furthermore they weren't measuring iodine in forage, they weren't measuring iodine in milk. They were measuring iodine in *sagebrush*. So you have the problem of trying to model what *might* have been in forage grass from what you found, which were very *spotty* measurements, in *sagebrush*. And so we did the best we could with what we had. And I don't think going back over the data again is going to greatly improve the dosages. But I think right now the major problem is adverse health effects, because some people are exquisitely sensitive to radiation doses, and others can take a large amount of radiation and have no effect at all. There's a very wide range of sensitivities in humans to effects of radiation. And we really don't understand that lower level, I mean the effects of low levels of radiation on large numbers of people. How many would really have a

major thyroid disease problem, for example, if the thyroid dose is smaller than what has been established as a, quote, “safe dose” by various radiation groups that study these problems?

Many interesting things, but one very interesting thing is the point about the appropriateness of an epidemiological model, just because a physicist that I consider to be very—that might—he’s a very smart guy, said to me when I was asking him questions like this—he doesn’t have the expertise that you do—but he said a similar thing, you can’t look to epidemiology.

Well, let me summarize. There are so many *uncontrollable* factors in *every* epidemiologic study, that people who know how these studies are done say the perfect epidemiologic study has yet to be accomplished.

Maybe that was it.

Because there are so many uncontrollable factors that, you know, it’s diet, it’s state of health, **[00:55:00]** it’s age of the individuals, it’s what kind of way the human body handles radionuclides when it comes in, and that’s a function of each person alone. And you could very well have factors that range, oh, say from a hundred to a thousand, at least, in sensitivity of people to radioiodine. And if you don’t have a large enough population, when you start doing *random* things, you’re not going to find anything; and they didn’t find anything. But the fact that they didn’t find anything doesn’t prove that there’s nothing *there*. All it proves is that that *study* didn’t find anything.

Very interesting. What year was your involvement in Hanford going on?

Actually I believe it was established originally in 1988 and I was with it all the way into the nineties when HEDR was complete. And then I was appointed to another committee and just last year, 2004, was our last meeting in January, when our committee sunsetted. And it was the—I’m trying to think of the exact name of the committee. It was the Hanford—

Hanford Health Effects Subcommittee?

That's it, yes. HHES. That is correct. And one of those was established at Hanford, one was established at Oak Ridge, one was established at Savannah, and possibly another, Idaho, the Idaho Falls place [National Reactor Testing Station]. And some of those are still going, but they have terminated the studies at Hanford and consideration there. And the purpose of that committee was to relate to the people and get the people's concerns brought to bear. And I think that that was accomplished with that report. So I was involved with the Hanford group all the way from '88 to 2004.

So many questions, but let's talk a little bit more about this—well, I'll have you talk a little bit more about this scientific question, low-level versus not low-level [dangers], because it seems that this is important to the Downwinder phenomenon here around the test site.

Well, it's extremely important because the various international groups, the United Nations program and other national groups, have come up with a specific guidance number which they believe to be safe. But there is never a plus or minus figure attached to that. And it always has built into it long-term and large averages of the information that is available. And as I pointed out to you, you can very well have factors of ten to a hundred to a thousand variations in sensitivity of people, not only to radiation but to drugs and to any kind of thing which can affect a person's health. We're all very much individuals and each person is not exactly like every other person. And so when you take a whole large number of data and you put it all together and average it and you come out with a number and you say this is a safe number, it has a lot of questions associated with it. And the research that has been going on over the years has always been trying to reduce that factor of the plus or minus that really needs to go on to what is [01:00:00] called a safe radiation dose. And this is particularly true for radioactive iodine

because concern has always been over thyroid disease. But there are also possibilities of effect on *immune* system and effects on other parts of the body, even though the radioiodine is concentrated in the thyroid gland because the thyroid gland is a master gland. It controls a lot of things in your body. And so people are constantly trying to do a better job of determining just what effects may be happening.

And one of the big things that is being considered right now is what is called bystander effect. And this is a cell which is located close to another cell that is being exposed to radiation. But the bystander is not actually receiving the dose, but the dose that is given to this cell gets into the body fluids and goes and affects the bystander and actually causes an adverse effect there, even though there was no radiation exposure directly to that cell. That's called the bystander effect, and that is something that is being looked at right now and a lot of publications are coming out on that particular question.

And what about genetic effects?

Always a problem, and in general people feel as a result of the information that they have, that the iodine 131 does not cause genetic effects. But there again, the question is, how many people have really been studied for this, at what doses, spread over what periods of time? And if you average a whole bunch of no-effects with a *few* effects, the effects disappear. And I think that is what is happening in a lot of the major studies that are looking at various kinds of health effects. You *do* have some people out there who are *exquisitely* sensitive to radiation, but there probably is a relatively small number, and when you average them with people that are very resistant, it disappears. You don't find the effect. And I think that's what's happened at that epidemiology study.

OK, we have to stop.

OK.

There are a couple of things I'd like to talk to you about in this second half. One is the work that you then did at UNLV [University of Nevada, Las Vegas] establishing the Environmental Research Center and those kinds of things. Am I getting that right?

Well, it's wrong to say that I established that. That was established by Dr. Don Baepler. He hired the first director, who was David McNelis, a good friend of mine, and he has his Ph.D. in— actually he was my aerosol physicist for all of the studies out there. He had a master's in physics and he has his Ph.D. now. I believe it's in environmental science or something like that, from University of North Carolina. He lives back East and is working as a consultant still. But it was Don Baepler and David McNelis who built that group. And when I retired from the university, I went in as a senior advisor to the group. And then when David McNelis resigned and moved up to become vice-president of the university for research, I was made the director of that particular group, and I was the director for approximately two years. And then I resigned and went over to Environmental Studies Department and became a professor over there, a part-time professor.

I need to move back a little bit in time to after you leave the work at the test site, a little bit of a time line, because you end up back at UNLV at some point.

Well, actually my work at the test site ended in approximately 1969, and I was selected for a senior position in a predecessor agency of the Environmental Protection Agency, and it was the National Air Pollution Control Administration, I believe that was its title. Anyway, I was selected to be head of the Bureau of Criteria and Standards, and this job was in North Carolina. So I moved to North Carolina and became the director of the Bureau of Criteria and Standards. And it was my responsibility to develop what are called criteria documents for various air pollutants. And the ones for which we had criteria documents were nitrogen oxides, sulfur

oxides, particulates, carbon dioxide, and ozone. And so our suggested regulations were developed under my direction when I was the head of the Bureau of Criteria and Standards of the National Air Pollution Control Administration. That was the title. NAPCA. National Air Pollution Control Administration. And I was a senior official at, the Director of the Bureau of Criteria and Standards. And then the Environmental Protection Agency was formed and I became the director of the National Environmental Research Center in North Carolina. And in that capacity, I had to worry not only about air pollution and its control but also about water pollution and its control, and health effects in general of all pollutants, pesticides problems, noise problems, everything that the Environmental Protection Agency had to deal with. I was responsible for research to [00:05:00] develop information about the various effects of these things on people and then to suggest regulations that need to be developed, standards that should not be exceeded more than some specified period of time.

So that was my position when I was in North Carolina. I was there for approximately three years, and then I was reassigned back to Las Vegas and I became the director of the National Environmental Research Center here in Las Vegas, and it was an Environmental Protection Agency laboratory concerned principally with monitoring and with various aspects of monitoring, to include remote sensing using aircraft, various kinds of measurements for air and water pollution, radiation and how that related to exposures to people.

I was the director there for four years, and then went back to Washington as the Deputy Assistant Administrator for Health and Ecological Effects Research. So I was responsible for all health and ecological effects research for all of the environmental pollutants that EPA has to worry about, and the principal ones, air, water, pesticides, and toxic substances. And when I was there for that period of time, I had five different laboratories reporting to me and approximately

seven hundred people, and a very large budget. Total budget was probably in the neighborhood of seventy to eighty million dollars a year.

So in Las Vegas, when you're talking about monitoring – I'm going back a little bit – it's not only radiation at this point. It's all—

Oh, no, no, it was not just radiation. Well, definitely in those years that I was the director, a major part of the program was offsite monitoring around the Nevada Test Site. So we had that responsibility. That was one segment of our program. But then we had other segments of the program that we were doing: monitoring systems design for various kinds of pollutants, remote sensing and what you need to check when you're doing remote sensing, various measurement techniques for monitoring the different kinds of pollutants. Those were the major concerns that we had at the laboratory when I was the director there.

So that's what we were saying before, off tape, there were monitoring stations, now I'm talking about the test site, in various places—

Absolutely. All the way around the test site, we had monitoring stations measuring air pollution, water pollution, and milk for radioactive materials.

OK. So we're back in Washington, then. You're at the EPA headquarters there.

OK, I was at the EPA headquarters and I had this responsibility that I told you about. And I then was offered an opportunity for an outside-the-service assignment to the University of Nevada, Las Vegas. And I always have loved living in Las Vegas and so—I hated living in Washington, D.C. The job was wonderful but the living conditions were terrible.

Why?

The commuting. It's just incredible. Even though I only lived like ten to fifteen miles from where I went to work, it would take me an hour to get to work. And I'm sitting there you know, just

moving and then stopping and moving and stopping and moving. Usually it would take me at least thirty minutes after I got to work before I could do any thinking about the job. And so when I had the opportunity, I came back. I came back to Las Vegas and went to the university, and what I did there was to start teaching courses in the areas in which I had had responsibility while I was working for the federal government. And after—

[00:10:00] *What year was that that you came back here?*

I came back in, let me see now, it was '78. Nineteen seventy-eight. And I was at the university for the period that they allowed me to be assigned there. And at the end of that period, the federal government offered me a job to come back to work for them from the university that was three levels beneath where I was when I was in Washington. Obviously I was on somebody's list. And that's another story I can tell you, but that's not for the record. I was on somebody's list and I knew that I was and I knew why. But actually it was probably the best thing that ever happened to me because it kept me—I refused to take this reduction in rank by three levels. I said no way and they said, well then you're terminated. So I said goodbye.

And I went to the university and became an advisor for the Harry Reid Center. So that's where I went after I completed this in-service training at the university that I was doing. And while I was there, I was teaching at least one course over in Environmental Studies. And after a couple of years, I decided that the best thing for me to do was to leave that position and move over to a teaching position solely. And I've always thoroughly enjoyed teaching. I was very happy teaching. So I went over and worked for Jim Deacon as a teacher in the Environmental Studies Department. And I was there for probably ten, twelve years before I retired and became a professor emeritus at UNLV.

So that was the story of my life, essentially. And after retirement, the first thing I did was to get involved as a consultant, and I told you the major consultancy that I had around the Hanford radiation site, and that lasted from 1988 to 2004.

Right. Let's talk something about Yucca Mountain. In what ways was your work related to that and what kinds of things were happening at UNLV related to that, in the early days?

Well, when I was associated with the Harry Reid Center, the Harry Reid Center had a lot of projects ongoing that related to Department of Energy [DOE] and also the part of Department of Energy dealing with Yucca Mountain. And we were looking at various kinds of possible source terms for materials which could get into drinking water or into the air as a result of the activities that were going on. And this is during a time when they were *building* the Yucca Mountain repository, when they were digging out there, and when they were *disposing* of what they dug up in piles with the possibility of wind blowing those piles and all kinds of various kinds of materials *in* what they had dug up. So we were looking at possible *risks* associated with these kinds of things. That was one of our major concerns at that time.

What kinds of things in the dirt are you talking about?

Well, there are naturally-occurring materials which are considered to be hazardous to humans, various kinds of metals, and I can't go into detail to give you all of them, but there are several [00:15:00] materials in the ground out there that are known to cause adverse effects on humans at high levels of exposure. So there was concern about what was available and what was being disposed of and so forth.

Because once it was dug up, then it could be blown around, is that the—?

Well, unless great care is taken, it can be blown around if all you do is pile it up someplace and you don't make any effort to cover it or to seal it in any way. So that was a concern. Various

kinds of risk assessment problems associated with Yucca Mountain during its construction phase and after it developed. It took the high-level nuclear waste. These were major kinds of concerns that I had when I was with the Harry Reid Center.

Can you talk at all about the questions of the kinds of concerns you were looking out to once waste was established? Because you read in the paper: earthquake, water and all those kinds of things.

Well, actually I was very fortunate because I was invited to sit in as an observer for an organization in California that was doing a risk assessment for the total project. And that had like sixteen different elements in the model; such things as what the climate is going to be out there for the next ten thousand years, how many earthquakes are they going to have, where are they going to be, is there going to be a volcanic eruption sometime during the next ten thousand years, et cetera. So there were sixteen different elements in a very complex model. And I still have all of my literature on that. I was able to sit in on that evaluation. And it was extremely valuable to me, to bring me up to speed with regard to what was known and what wasn't known. And a lot of times, you simply did not know what the input data were because there's no way in the world to estimate what's going to happen for ten thousand years. And now the National Academy of Sciences has said that's not long enough. You have to go beyond ten thousand years. And so you're in a situation where you're trying to estimate what's going to happen to all of these various things. And one of the things that wasn't even discussed in the model—and actually there were three different organizations that did three different models for a total risk assessment—that was never discussed in any detail is the possibility of terrorists digging back into the area to try to recover some of the high-level nuclear waste to use in terrorist activities. That wasn't even considered.

So this whole risk assessment aspect was the one that was most interesting to me and that was the major concern that we had with the projects that we were conducting at the Harry Reid Center while I was there. And we, of course, gave progress reports to the Department of Energy on the work that was ongoing and also on different aspects of assignments that we were given.

[Telephone ringing]

Just let it ring. I'll get the message later. What was I saying?

You were saying that you had various assignments. I got distracted by the phone, too.

Oh, yes, we were given assignments by the Department of Energy and, for example, one of the assignments that we were given was to look at the site characterization plans and determine [00:20:00] whether or not there might be some unacceptable risk associated with the site characterization plans; also whether or not adequate quality assurance and quality control was available for the individual site plans, and there were like 107 of those being conducted by many different agencies. And we were concerned about the quality assurance aspects of the *integration* of all of this information into a total risk assessment package. Because that's what we believe is really necessary at Yucca Mountain. And so we were asked to do a report and we published a very thick report which was delivered to the Department of Energy concerning our ideas on what needs to be looked at for risk assessment and what kind of data need to be collected. That's a very general statement of what we were doing, some of the things.

This question of quality assurance, and you tell me what you can tell me about this, you said earlier—and how did that play out?

Well, it was our feeling that quality assurance, quality control needed to be a total umbrella which came over *all* of the efforts that were going on out there and how the results from each piece was integrated and how much quality you have in conclusions that are drawn from this

integration. And we tried to convince the people who were running the program that this is the approach that ought to be taken for quality assurance. And they said oh no, the way to do this is to have each one of the 107 different contractors that are out there do its *own* quality assurance and quality control on *its* data. And so that was their conclusion and they decided to go that way and they did not accept our argument that they needed a total umbrella over the whole thing. And we offered to do that as a contractor.

I see. So it would not have been, then, DOE necessarily doing that. It would've been an outside agency like your own.

Doing the actual analysis, having expert analysis, and then providing the report to the Department of Energy with recommendation for any additional studies that ought to be done to reinforce the safety of the entire project, not the individual pieces.

I understand. And this is related to your concern about the quality assurance being done by the individual contractors themselves.

We felt that the quality assurance being done by the individual contractors would not give a consistent approach because each agency has a different idea about how to do quality control and quality assurance. And what we wanted to do was make sure that this umbrella would indicate to each agency how they should *do* their quality control and quality assurance and not allow them to do it in the way in which they normally do it. That was our idea and that was what we were trying to sell.

Well, just—go ahead.

No, that completes my statement on that, unless you have a question.

No, it's an observation. What you're talking about in organizational terms or administrative terms, it seems to me, is you have got a very complex system and things are going to happen in

between the pieces of that that the individual people in those pieces may not see, so you're asking for a system-wide approach, is my—

That was exactly what we were trying to sell, a system-wide approach to quality control and quality assurance. And it was not viewed favorably by the administrators.

Since we're on Yucca Mountain, what can you tell me generally as of today about how you're seeing how that whole thing is playing out, from the point of your expertise.

[00:25:00] My feeling is that almost certainly the controlling factor in risk assessment is going to be ground water. I think the most important aspect is to determine the possible concentrations of radionuclides in ground water if, in fact, you get leakage of the containers. I think that is going to be the controlling aspect because the fifteen millirem dose which is presently being considered and now being reconsidered is very, very close to what you get from natural background. So it won't take very much to go over that if things begin to leak out of the repository. So I think the most important consideration of all is concern over ground water, ground water flow, and climate, because that's going to affect those things. A lot depends on how much *rain* you get, for example. And the ground out there does have cracks in it. All naturally-occurring grounds have cracks. And so you don't have simple flow. You have very complicated kind of flow through the strata, through the soil, because it will follow the various cracks instead of going right straight through. So there is a major concern over doing the best you can to model ground water flow, the amount of material which could get into the ground water, and how much dosage that's going to give to people at the nearest site where people can use the ground water. That is my judgment on the subject.

And of course another major problem which we never got into to any great extent because the Department of Engineering at UNLV has concerns about this, and that is the

accident situation of things that are being transported very long distances to get them *to* the repository; then concern over how they get picked up at the end where they're going to be put into things to be transported, and then how are they going to be taken out of those things and actually placed inside Yucca Mountain. And those kinds of concerns are being looked at by the Engineering School at UNLV, rather than the Harry Reid Center.

Well, maybe I'll just note—I have one more general question to ask you and then we can probably wrap it up for today, but we should probably just note that it's March 18th 2005 and that just recently in the paper there have been concerns about the validity of some of the things that were being done at Yucca Mountain. To tell you the truth, I've been so busy, I haven't even read the articles. I've just heard the headlines.

Let me summarize for you. It boils down to two things. One is the accuracy of the *data* which have been collected, and two, the quality *assurance* of the data which have been collected. Those two things are the most important. And what we have heard in the paper is that some of the data apparently were manufactured. And if you start with manufactured data, there's no way in the world that you're ever going to be able to use that in a risk assessment approach because you won't be *able* to do realistic alterations in this if you start off with wrong data. So those two things are inextricably tied together, the data itself and the quality assurance *on* the data, and those are the two things that if I *understand* what I've read in the paper, have been *admitted* to have been falsified. But I don't know any further than what I have read in the newspaper. But I understand very much that those are critical elements, particularly because they relate to what I consider to be the major problem, which is water flow and climate and exposure to people from [00:30:00] drinking water.

Now when I read the articles, I'll be able to do it with your really good overview as I try to understand it.

Well, I'm a teacher and I always try to make sure that whomever I'm speaking to understands what I'm saying, and if I'm too complicated, I try to boil it down some more.

Well, that's great. Let's just close with this because we touched on it when I spoke to you last time and I just want to make sure I understand it. This is not a science question. When I go over to UNLV today and I see the EPA buildings, you were saying last time that that part of the campus was something that you were involved in establishing?

I was the director of that entire laboratory there.

That's the laboratory, when you came back to be the director of the laboratory.

Yes. It was called the Environmental Monitoring Support Laboratory, and then its title was changed to National Environmental Research Center, Las Vegas.

Now was it always on the campus?

No. A long time ago, it used to be over across the street from the DOE headquarters. It was in temporary buildings over there. There was one temporary building there on Highland Drive and there was another temporary building down on Charleston. And we also had aircraft and a hangar at the airport where our aircraft were because we were doing aerial measurements of radioactivity over the test site. So that's where it all started, and then when the facility was built on campus at the university, those two were given up, the Highland building and the Charleston building, and everybody moved over to the university.

So when you came back in '72, you were on campus at that point.

Yes. Exactly. In fact, we were on campus most of the time when I was here from '63 to '69. It was only like the first year or two at the most that I was in the Highland office.

I see. Well, the campus must've looked a lot different then.

Oh, absolutely. Oh, yes. We didn't have the chemistry building and we didn't have the physics building. Just empty space out there.

And how does it work—now this is a really ignorant question—but how does it work that a laboratory of the government is sitting at the university? Does that happen a lot?

Well, as far as I'm concerned, that is the best of both worlds because it's a win-win game.

Because if a government laboratory is sitting on a campus, it is inevitable that the scientists from the government lab will do some of the teaching for the university, and many of the students will have part-time jobs at the laboratory to help them get through their work. And so you've got a win-win game. And not just here. You have exactly the same situation at Oregon State University in Corvallis, Oregon. The EPA lab there is sitting right on campus, and they have a very close relationship with the campus. And I did my best, when I was the director of the lab, to build bridges over to different departments in UNLV because I wanted to maximize the possibility of our working together, because it was good for both of us. And actually we were paying rent to the university, and my understanding is that the present president has substantially raised the rent, but the EPA has felt that it was sufficiently necessary to stay there that they have paid the extra rent. That's scuttlebutt. I'm not sure how accurate it is.

Do you think that those kinds of bridges are still existing there on campus today?

My feeling is that it's not to the same extent now as it was in the past. But this is a value [00:35:00] judgment of mine. And I am no longer affiliated with the university other than the fact that I'm a professor emeritus and if asked to give a guest lecture, I have never turned it down. So that's all I can say.

Well, you've given me tons of information, so I think, unless there's some other little tidbit that you want to give me, that we can stop.

I think I have managed, one way or another, at least in generality sometime, to include everything I wanted you to hear.

Thank you very much.

It's my pleasure and yours as well, I hope.

Yes, absolutely.

[00:35:51] End Track 3, Disc 2.

[End of interview]