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HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Dr. George Voelz, M.D.

Conducted November 29, 1994

United States Department of Energy
Office of Human Radiation Experiments
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FOREWORD

IN DECEMBER 1993, U.S. Secretary of Energy Hazel R. O'Leary announced her Openness Initiative. As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects. The Office of Human Radiation Experiments coordinated the Department's search for records about these experiments. An enormous volume of historical records has been located. Many of these records were disorganized; often poorly cataloged, if at all; and scattered across the country in holding areas, archives, and records centers.

The Department has produced a roadmap to the large universe of pertinent information: *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records* (DOE/EH-0445, February 1995). The collected documents are also accessible through the Internet World Wide Web under `http://www.ohre.doe.gov`. The passage of time, the state of existing records, and the fact that some decisionmaking processes were never documented in written form, caused the Department to consider other means to supplement the documentary record.

In September 1994, the Office of Human Radiation Experiments, in collaboration with Lawrence Berkeley Laboratory, began an oral history project to fulfill this goal. The project involved interviewing researchers and others with firsthand knowledge of either the human radiation experimentation that occurred during the Cold War or the institutional context in which such experimentation took place. The purpose of this project was to enrich the documentary record, provide missing information, and allow the researchers an opportunity to provide their perspective.

Thirty-two audiotaped interviews were conducted from September 1994 through January 1995. Interviewees were permitted to review the transcripts of their oral histories. Their comments were incorporated into the final version of the transcript if those comments supplemented, clarified, or corrected the contents of the interviews.

The Department of Energy is grateful to the scientists and researchers who agreed to participate in this project, many of whom were pioneers in the development of nuclear medicine. ❑

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DISCLAIMER

The opinions expressed by the interviewee are his own and do not necessarily reflect those of the U.S. Department of Energy. The Department neither endorses nor disagrees with such views. Moreover, the Department of Energy makes no representations as to the accuracy or completeness of the information provided by the interviewee.

ORAL HISTORY OF DR. GEORGE VOELZ, M.D.

Conducted on November 28, 1994 in Los Alamos, New Mexico by Darrell Fisher, a health physicist from Pacific Northwest Laboratories, and Marisa Caputo, Oral History Team Leader and Special Assistant from the Office of Human Radiation Experiments, U.S. Department of Energy.

George Voelz was selected for the oral history project because of his research at Los Alamos National Laboratory (LANL) and his work at the Idaho National Engineering Laboratory (INEL). The oral history covers Dr. Voelz's research on the Manhattan Engineer District (MED) plutonium workers, the acute and long-term effects of radiation, his inhalation studies, and his association with other scientists.

Short Biography

Dr. Voelz was born in Wittenberg, Wisconsin on October 13, 1926. He was married in 1950 and has four children. He received his B.S. and M.D. degrees from the University of Wisconsin. He was at Los Alamos from 1951 to 1957. From 1957 to 1963 he served as Idaho Operations Officer for the U.S. Atomic Energy Commission (AEC). He is currently Health Division Leader for the Los Alamos Scientific Laboratory, a position he has held since 1970. Dr. Voelz's field of expertise is occupational medicine.

Dr. Voelz has many publishing credits for his health studies of the MED plutonium workers, inhalation studies of radiation workers, and environmental surveys of various Department of Energy sites.

Medical School; Start at Los Alamos Application Medical Group (1950)

CAPUTO: Today is November 28, 1994. My name is Marisa Caputo. I'm with the Department of Energy, Office of Human Radiation Experiments. I'm here with Darrell Fisher, who is with Battelle Pacific Northwest Laboratories in Richland, Washington, to interview Dr. George Voelz of Los Alamos National Laboratory. The purpose of this interview is to record Dr. Voelz's involvement in human radiation experimentation during the Cold War. Thank you for being here today.

VOELZ: You're welcome.

FISHER: George, would you briefly recall your education, and the events in your life that brought you to Los Alamos?

VOELZ: I went to medical school at [the] University of Wisconsin [and] graduated in 1950. After an internship, a year at the University of Oregon Hospitals and Clinics in Portland, I took an Atomic Energy Commission fellowship in industrial medicine. I spent a year at the University of Cincinnati Kettering Laboratory, [and then] entered into an in-plant training year, [at] Los Alamos with Dr. [Thomas] Shipman, who was the division leader for the medical department—the Health Division at Los Alamos. My coming to Los Alamos was part of an in-plant training program for practical application of industrial medicine.

FISHER: But you stayed longer than just that internship?

VOELZ: Correct. I went through the first year doing various assignments as part of the in-plant training and then stayed on for another three and a half years in a staff position in the Application Medicine Group at the Los Alamos Scientific Laboratory.

FISHER: Who did you work for, and who were some of your associates in the Laboratory?

VOELZ: The group leader initially was Dr. Robert Grier, who left about a year after I arrived. Then, Dr. Harry Whipple was the leader for the Medical Group. [Initially], there [was just] the group leader, myself, and Harry Whipple. I can't remember who came in when Harry became the group leader.

FISHER: What was your association with Dr. [Louis] Hempelmann at this time?

VOELZ: Dr. Hempelmann was at [the University of] Rochester, but he came back here from time to time. His principal contact was Wright Langham. I was introduced to Dr. Hempelmann, I believe, in the first in-plant training year, probably in 1952, when he was having a meeting with Wright Langham about follow-up of plutonium workers, who had been exposed here at Los Alamos during the 1944-to-'45 period. They were discussing the selection of people to have medical follow-ups. I was just fortunate enough to sit in on the meeting and listen to them discuss this project.

FISHER: Was Wright Langham involved in these discussion as well?

VOELZ: Oh, yes. Since he had done the urinalysis procedures in testing the exposure to these people [plutonium workers], he had all of their urine data and [a list of] the groups for which they worked. He really was responsible for the people that were going to be involved in that study. In looking back through the records, it's apparent that Wright had kept in touch with some of these people ever since they left Los Alamos [as there are] letters that we have to them and from them dating back to 1948. Obviously, the importance of following plutonium [workers] was in both Hempelmann's and Langham's minds as soon as they [realized] everybody was leaving Los Alamos after the war.

FISHER: Were there any other principal scientists involved in the plutonium work or follow-up?

VOELZ: Not really. Wright saw to it that the patients [and] the people[1] were going to be followed. Dr. Hempelmann made the arrangements for the medical examinations that were done [in] the city or towns where these people [now] lived. That was done by correspondence. Hempelmann made those arrangements and also got the reports, the x rays, and results of tests. Hempelmann really was sort of the coordinator of that project.

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[1] plutonium workers

FISHER: You published extensively on this follow-up at several different intervals of time. You've continued to follow some of these workers even to the present.

VOELZ: That's correct.

Tissue Studies of Plutonium Workers

FISHER: In the early days of the follow-up of the plutonium workers, was there a tissue collection program at time of death, analyzing the amount of plutonium in tissues?

VOELZ: Not connected with that project. Ultimately, after the U.S. Transuranium Registry[2] was formed up at Richland, Washington, when these individuals came back for their medical follow-up, they were asked whether they wished to participate in that program. That would have been in the 1970s. A good number of those people agreed to participate with the registry. Prior to that, there was no tissue program involved with them. [However], one of the plutonium workers had a lung lesion removed surgically in 1970 to rule out a possible lung cancer. It turned out to be a benign tumor called a hamartoma. No cancer. The tissue removed in this case was sent to Los Alamos and was analyzed for plutonium content. A few other special tissue samples were analyzed at the Laboratory, but they didn't involve any of the plutonium workers.

FISHER: So there was a tissue collection program here at Los Alamos for other populations, but not this select group of plutonium workers?

VOELZ: In the fifties there were a few isolated tissues that were analyzed from people who had died, and there was particular interest in whether they'd had any exposure. Several of these came from people who had worked at the Nevada Test Site. Of course, much earlier there were a few tissue [samples] involved in the plutonium injection studies that were also done here. That goes back to 1946. But those were surgical samples [from patients involved in the injection studies]. A few of those surgical samples were sent here. There were a few samples like that. Then in the fifties, there were a few samples from people who had been working in Nevada.[3] Then, there was the Cecil Kelley accident in 1958. He was a plutonium worker who had died of a criticality accident.[4] Autopsy samples from Mr. Kelley really were the first [from a] Los Alamos [employee]. I believe, I was not here at that time. I left Los Alamos in January 1957. It appears that the Kelley accident got people thinking about the value of doing tissue studies as sort of an additional source of information on what exposures were occurring. After the Kelley accident, there was a growing interest in getting tissues from the local hospital.

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2 The AEC contracted with the Hanford Environmental Health Foundation (HEHF) in Richland, Washington for a National Plutonium Regustry in 1967. In 1970, the name was changed to U.S. Transuranium Registry (USTR). USTR's function was to study post-mortem tissues from exposed workers to determine the pattern of distribution, concentration, and retention of transuranic elements. The USTR currently is operated by Washington State University.

3 at the Nevada Test Site

4 an event in which a fissionable material unexpectedly undergoes a chain reaction

FISHER: You mentioned just a few moments ago that some tissue samples were collected from workers prior to surgery. Can you describe the type of injections that were administered, by whom, approximately when this occurred, and what the purposes for those studies were?

VOELZ: The surgical samples were samples of tissues that had been removed at surgery. The ones I was referring to were some samples that occurred in the plutonium injection cases. [The latter] were people who in some cases had cancer, whereas the surgery was done a few days after the plutonium injection.

FISHER: You're referring to the original eighteen?[5]

VOELZ: Yes. What I'm saying is that there were two tissues [sample types] that, at that time [1945–46], were measured for plutonium. As far as I know, there were no others except autopsy samples in the fifties that gave them a couple more cases. I believe these were from Nevada.

FISHER: Do you remember who was involved in analyzing the information from these tissue samples, and what they were able to learn from these analyses?

VOELZ: I wasn't here and didn't have much information on those samples, but Harry Foreman was working with the use of EDTA,[6] one of the early chelating agents.[7] Harry was interested in plutonium. He was involved. Dr. Lushbaugh[8] was a pathologist, and so some of the tissues came to him. I believe the plutonium analyses were done by the Industrial Hygiene Group, who did the radiochemical analyses[9] for plutonium. Those were some of the principal people involved.

Plutonium Injection Studies (1945 and 1946)

FISHER: In 1952 to 1953, when you first came to Los Alamos, were you aware of the plutonium injection studies, and what was your early involvement with Wright Langham and the analysis of data from those cases?

VOELZ: I worked primarily for the Medical Group and was involved with occupational accidents, injuries, and health evaluations of employees. I became aware of the injection studies fairly early on after I got to Los Alamos. With any [radiation exposure] accident, Wright Langham was usually one of the people that were consulted, because he was most familiar with the plutonium analyses of urine samples. So to estimate what kinds of exposures had occurred, he was involved in the interpretation of urine results. In those interactions, of course, I had a chance to hear what some of these interpretations were based on, and by that time [1953], the Langham Equation for

5 From 1945 to 1947, 18 persons were injected with plutonium and excreta samples were obtained for plutonium analyses.

6 ethylenediaminetetraacetic acid: a colorless compound capable of chelating a variety of metal ions; used in the treatment of heavy-metal poisonings

7 a substance that removes heavy metals from the bloodstream

8 See *Human Radiation Experiments: Remembering the Early Days; An Interview With Pathologist Clarence Lushbaugh, M.D.* (DOE/EH-0453), April 1995.

9 determining the relative amounts of radioactive elements in biological or chemical samples

excretion of plutonium was well known. Since that equation came from these injection studies, there was no—in the position I was in anyway—there was no hiding or suppressing where this kind of information was. It was discussed as technical and scientific information like any other study or paper, although I did not actually see—and there was probably no need for me to see—any published paper at that early time. I was kind of a bystander at that time. So I just listened to all of the things that were being discussed.

FISHER: Since you were an interested bystander, did you have interest in the long-term follow-up of those who lived beyond the initial period of time when they were injected, and were there any discussions about collecting long-term samples from these people?

VOELZ: I was interested. I was not involved except for the meeting that I referred to of Hempelmann and Wright Langham. The people who were in the follow-up study were mostly living elsewhere. They had left Los Alamos in 1946. They weren't brought back here; they were just followed medically. Because at that time, this wasn't so much a research project as it was that both Hempelmann and Langham were concerned about what had gone on here in terms of whether there would be any health effects and whether they had really had any harmful exposures. This [prognosis] was not known at the time, and they realized this was a significant opportunity to follow these people. It was really done as a pure medical follow-up study. It was an observational study rather than a research project. Actually, they did not get urine samples from these folks until the first time they were brought back to Los Alamos for their examinations in 1971 and '72. Although, I think there were urine samples for some of these people in the late 1950s; around 1958. I don't really remember when they would have gotten those samples. The people who were still at Los Alamos from the original group, of course, were getting urinalysis samples on a periodic basis. Those that were here, were being followed regularly.

CAPUTO: When you first became aware about plutonium injections, did you have Q clearance[10] at the time?

VOELZ: I had Q clearance essentially when I got to Los Alamos. So I had a Q clearance all the time. That really didn't make any difference. There were publications that referenced some of this work in the literature by the late 1950s, and it was used in the first committee report of the International Commission on Radiological Protection. The classified report was even referenced in 1960. In some of the British literature it was also referenced during a fairly early period. It was well known in the scientific community; certainly within a few years.

FISHER: There's a lot of interest, as you know, in the plutonium injection study. We'll be talking to Bill Moss,[11] who has done recent research on the data that Langham collected. Do you remember discussions back in the early

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10 a high-level DOE security clearance, comparable the a Top Secret clearance issued by the U.S. Department of Defense

11 a chemist in Health Division at Los Alamos interviewed November 30, 1994. The transcript of that interview will be published as DOE/EH-0459.

fifties about the decision to inject and the motivations for doing this work? For example, did Wright Langham take credit for this project and was he reluctant to discuss this?

VOELZ: I would not say that Wright took credit for it. He certainly participated, in that he was involved in the analyses and in writing reports as, at least, the lead author on the report. To that extent, he was a contributor to the project. I think Wright had a reluctance to discuss the project because he had an uneasiness about it. Wright was not a physician; he was a biochemist.[12] I don't think he really had any judgment whether whatever had been done was right or wrong. As I say, in terms of the use of patients for these studies, I think Langham was just contributing what he knew about his procedure for urinalysis and the chemical management of the plutonium samples and solutions. As for the rest of it, he was just going along with what was proposed by others. I think he had a real uneasiness. I know he had a real uneasiness about the project, [but] not his end of it. I would say that it was not a thing he would have talked about except for people[13] who were directly involved and needed to get permission.

FISHER: In your career, have you been able to ascertain any of the details about the initiation of the plutonium injection experiments; the driving forces behind them? Maybe who was pushing it?

VOELZ: All I know is the release of the various documents and letters that were in the archives at the time. The driving force, here at Los Alamos, was clearly that they had people in the workplace and things were moving very rapidly and they did not have a way of measuring the exposures. There are many things at the time that you take for granted now. For example, plutonium is an alpha emitter.[14] When they first began working with it at Los Alamos, they had no portable alpha instrumentation. If you wanted to see whether a benchtop was contaminated, you would have to take a filter paper, make a smear, go back in the laboratory, and count it. There was no easy way of surveying surfaces. That was being developed and came later on, in 1945. Then one of the first contamination accidents occurred in August of 1944. They realized the vulnerability because they really didn't have a way of estimating exposures.

Dr. Hempelmann, who was in charge of the occupational medical services and laboratory employees at the time, was pushing Dr. Oppenheimer[15] to see whether studies could be done on the biological effects in animals, as well as looking at excretion rates in humans. There were two letters released, one in the fall of '44 and the other one in March of '45, in which Oppenheimer was transmitting this information to Dr. Stafford Warren,

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12 Dr. Langham received his Ph.D. in Pathology in 1942 and his M.D. in 1948, both from the University of Chicago.

13 medical researchers

14 a radioactive material that emits helium nuclei during radioactive decay; it can cause tissue damage if ingested or inhaled

15 J. Robert Oppenheimer, U.S. nuclear physicist (1904–1967)

who was responsible for the plutonium project.[16] The plutonium project looked at these kinds of issues and was done primarily at Chicago.[17] In these letters were suggestions or expressions of the need for getting human excretion data.

I think it's hard to say strongly enough the concern that particularly Dr. Hempelmann and Wright Langham had for the workers. I had a chance in later years, from the 1970s until his death, to work with Dr. Hempelmann. Every time I saw him, he wanted to know what was new with the workers and whether there were any new health effects. This went on until his death.

FISHER: Do you remember what year that was?

VOELZ: It was summer of 1993. Hempelmann spent winter months in New Mexico; then in spring he would go back to Rochester.[18] He called me just a few days before he was leaving that spring to go back to Rochester and said he just had to call to see whether I'd heard from any of the, what he called, "the boys," and whether there were any new developments and whether they were still okay. This experience had a very significant impact on him because he had been involved in this project and was responsible for health protection. He carried that [concern] through to his final days. Fortunately, the experience and longevity of these workers has been quite good. As a matter of fact, it's been exceptionally good. He went to his final resting place feeling pretty good about the job.

FISHER: Your assessment of the long-term effects of these injections has been that there have been no observable long-term biological effects among the long-term survivors.

VOELZ: We have one bone tumor.

FISHER: But this was in the occupationally exposed group.

VOELZ: Yes. I'm talking about the occupationally exposed group. I'm not talking about the injections. Hempelmann did not talk much about the injectees. I never really heard him say much about those cases. His concern was for the people who were occupationally exposed, here at Los Alamos, under his watch. He was the equivalent of the medical director at that time. So those are the people I'm talking about.

FISHER: This is interesting, what you're mentioning. It sounds like, from what you've said, he distanced himself somewhat from those who were deliberately injected with plutonium in 1945 and 1946.

VOELZ: I don't know that he was much involved with the actual operations of that project. He was on the perimeter of it. Looking at the correspondence, [I would say] he obviously was a driver or certainly was interested in the results. But for expressing the need for some data, I think he wasn't directly involved in the carrying out of the study. He didn't have as much emotional

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16 The plutonium project was conducted to determine the biological retention and urinary excretion rates of injected plutonium in man.

17 at the University of Chicago's Billings Hospital

18 the University of Rochester (Rochester, NY)

attachment and concern with that [plutonium injection study] as he did with the people who were being exposed in the workplace.

Early Impressions and Challenges (1952)

FISHER: We'll come back a little later to the long-term follow-up of the plutonium workers. When you first came to Los Alamos in 1952, what aspects of the work did you find most fascinating in terms of human exposure to radiation, and the medical management of contamination cases or radiation accidents?

VOELZ: Well, I found it all interesting. Because when I came here, although I had studied various classical occupational types of hazards, I hadn't even heard the words to describe most of what we were dealing with at Los Alamos. Plutonium and tritium and fission products were new types of materials for me. At least during the first year, one of the things that I particularly remember was that Dr. Harry Whipple would take me aside and discuss what was known about these various radionuclides. He sort of gave me a short course on all of the various things that he felt I should know about.

One of the things that was apparent to me was that little was known [about radiation exposure]. Problems that came in as a result of accidents and contaminations [weren't fully known] in 1952. Much of what was known was still being worked on in the laboratory, either in animal studies, or in some cases, the tracer doses to humans. Much of this information was being developed as these problems[19] were occurring.

People like Dr. Whipple and Wright Langham had information that was available not only from the Los Alamos work but from other places. It was finding its way into medical and health physics literature. But the field was moving so fast at that time. If you didn't know what was going on that month, you were sort of behind the times, and then a year or so later you might see a reference to it in the literature. It was really almost a day-to-day communication. If you had a problem, Whipple and Langham usually knew somebody who was working on that particular radionuclide or that particular subject, and you might have a verbal conversation with the person. I was not involved in that [research], except I saw the process. I was involved in taking care of workers and I had supervisors, namely Whipple or Langham, who would get the information that was known. I received a very rapid education.

FISHER: What were some of your major early challenges, in dealing with accident victims as an occupational physician? Do you remember some of the cases that you worked with?

VOELZ: We had some minor wounds in which the question was whether the plutonium needed to be excised. There was an accident at one of the chemical laboratories involving basically one individual, who had a capsule of americium-241 which had apparently pressurized. When he tried to open it, it contaminated both himself and the room outside the hood that he was

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19 exposures and contaminations

working in. Still, in the fifties, and maybe even today, one of the major problems was measuring the exposure under these accident circumstances—the [body's] internal exposure in the case of these accidents. During the five years or four-and-a-half years that I was here during the 1950s, that americium exposure was probably the largest one we had. That involved chelation of EDTA at the time.

FISHER: Were you involved in the administration of the chelating agent?

VOELZ: No, Harry Foreman was the one who did that. I think the other cases that we had probably were more involved with the Health Physics Group. We had a polonium-210 contamination that occurred in one of the buildings. While there weren't any serious exposures to the people, the major problem there was getting control of the contamination; it had spread so rapidly. Recognition of the accident and the contamination moved rather slowly. There was concern about people's movement and whether it got into homes. There was a search for polonium-210 in anybody involved in that accident. The health physics people were very busy. But it didn't really involve the medical department, so I only saw excitement going on and not the action.

FISHER: Did it involve any internal contamination, any measurable uptakes of polonium that you can remember?

VOELZ: Very limited.

Long-Term Follow-Up Studies of Plutonium Workers (1991 and 1992)

FISHER: You mentioned just a few minutes ago, in conjunction with your long-term medical follow-up of plutonium workers, that among the most important findings is one single case of bone cancer. Would you like to take an opportunity to elaborate on the long-term follow-up and biological effects of exposure to plutonium?

VOELZ: The last examinations we did on that group of 26 workers exposed in 1944 and '45 were in 1991 and '92. We've had no deaths in that group since that time. As of that examination, 7 of the 26 had died, compared to approximately 13 or 14 deaths that would have been expected had they died at the rate of U.S. males, adjusted for age and calendar year of death. So our death rate has been close to 50 percent of what could be expected in this group. The one cancer that seemed to have the potentially closest relationship to plutonium, might have been the bone cancer, simply because it's a rare tumor, and finding one in 26 people puts it well in excess of what would be expected.

In another study, we've looked at all the people at Los Alamos that have been exposed to plutonium. That study will be published in the December [1994] issue of *Health Physics*. We just had a news release on it last week from our public information office. In that study, only one individual has [contracted] a bone tumor—the same individual I was talking about in the group of 26. When you put it in the larger context, of all the people who have been exposed to plutonium, it turns out to be well within the expected number [incidence] of bone tumors. [There] doesn't appear, in the plutonium exposed people at Los Alamos, that we have any excess of bone tumors.

I would say that I still reserve judgment whether a relationship between that particular case and his plutonium exposure exists. I try to be conservative in these things. His exposure was on the order of a quarter to a third of the traditional lifetime permissible body burdens. Because he had this tumor, I think we need to continue looking at it. If they were, in fact, related, then we may have underestimated the risk a bit. I think the wisest course is to continue to see what other observations we have on that particular topic.

FISHER: Forgive me if this question is inappropriate, but was that bone tumor removed by amputation, and has it been analyzed by either autoradiography[20] or other scientific analysis to determine if there are any plutonium stars[21] associated with the bone?

VOELZ: It was not removed. It originated in the sacrum,[22] which made it inoperable, and by the time it was noticed and diagnosed, there really was nothing one could do surgically. There were biopsies taken when the first symptoms appeared. This individual is one of the seven that passed away. He died in 1990, and we do have autopsy material on him. He was one of the cases that signed up for the Transuranium Registry. The amount of plutonium that was found to be remaining in his body works out to be about the same as we had predicted there would be by that stage of his life. The data corresponds quite well.

CAPUTO: Has his family filed a workman's compensation claim, or do you not know?

VOELZ: That's an interesting question. I talked with the family and visited with them a number of times when he was undergoing radiation therapy and other treatments. We talked about the relationship between plutonium and his cancer and that we did not know for sure whether they were related. I asked him whether he wanted to file a claim and what his thoughts were on that subject. I was surprised when he said that he really felt very good about his experience at Los Alamos and his work. He wouldn't even think of doing that.

He was a military man at the time he worked here. Most of these fellows [(military personnel)] were what is called SED: Special Engineering Detachment. These were people who had been selected for duty outside their normal organization. Most of the people that were selected to come to Los Alamos to work on the plutonium project were chemists or chemical engineers. They had come out of school and were drafted into service; most of them in the Army. They had some acquaintance with undergraduate chemistry or chemical engineering courses. They had met special educational requirements to be selected.

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20 a technique whereby thin tissues sections are placed on photographic film to record tracks if radioactive particles are emitted

21 radial pattern of alpha tracks from a focal point in tissue indicating plutonium deposition

22 a bone between the lumbar vertebrae and tail vertebrae, composed of five fused vertebrae and forming the rear wall of the pelvis

He [(the bone cancer patient)] was in that group and was assigned to Los Alamos. He said, "I was thankful because it saved my life." I didn't understand that at the time immediately. So I said, "You'll have to explain. Why did your work at Los Alamos save your life?" He said, "The unit I was in basic training, when they pulled me out and sent me to Los Alamos, was caught in the Battle of the Bulge. I'm basically the only one that survived from that unit." He was thankful that he had the opportunity to work with plutonium rather than face what his unit faced in Europe. We forget sometimes the competing risks in life. Life, as it goes along, puts us into situations where we have no way of predicting whether we're better off or not.

It's amazing to talk to these plutonium workers, all of whom have exposure levels that today would create a fair amount of excitement in the regulatory body: radiation protection folks. They [(the plutonium workers)] all are very proud of what they did. They're happy they were here and that they're all in their seventies and still doing well. They're very cooperative, and they have no adverse feelings. You won't find an antinuclear activist among the whole group. In fact, they take it upon themselves to go out of their way to talk about their experience. We've not encouraged that, for various reasons, but they have taken it upon themselves to do that.

CAPUTO: Do you know how old he was when he died?

VOELZ: He was 64.

Therapeutic Injections of Plutonium

FISHER: When you first began your career at Los Alamos, were you aware of the therapeutic application of plutonium in some of those later injection cases, and were you at all interested in the potential therapeutic benefit of administered radionuclides, particularly alpha emitters and perhaps also radiophosphorus or radiostrontium?

VOELZ: I was not aware that some of the plutonium injectees had gotten higher doses and that the investigators saw a potential for therapeutic application. That I've only learned in more recent times. Los Alamos, and any of the work that was connected with Los Alamos, really did not do anything related with therapy of cancer or other conditions. That wasn't an area I really had any particular interest in.

Move to Idaho (1957)

FISHER: That's very interesting. Dr. Voelz, you left Los Alamos in 1957 and moved up to Idaho. Can you tell us why you made this move and what your motivations were?

VOELZ: I was a staff member of the Occupational Medical Group here at Los Alamos and had an opportunity to take over the medical department of, what at that time was called the National Reactor Testing Station [(NRTS)]. The medical department there was run by the Atomic Energy Commission, Idaho Operations Office. They provided medical services for all of the contractors onsite, or at least emergency care and full occupational medical services for several of the contractors. It was an opportunity to get into a job I felt had a little more promise and opportunity. So I took that job. The medical department was part of several groups up there: five of the groups dealt with health and safety.

Fatal Worker Accident at Idaho's SL-1 Reactor (1961)

FISHER: What were some of the highlights of your experience at Idaho during those 13 years?

VOELZ: The one that sticks out most prominently is the SL-1 reactor accident, in which three individuals were killed. This accident occurred on January 3, 1961, and was probably the major reactor accident in this country. The fact that it killed three people, in a physical sense, made it more significant than the Three Mile Island accident. But from the public and psychological effects of the accident, the aftermath was probably not nearly as severe as Three Mile Island. For me, it involved three individuals who had fragments of fuel elements blown into their bodies. They were killed by the physical violence of the explosion. We were dealing with people who had external radiation fields of four or five hundred rads per hour up to some unknown level. This was a unique experience at that time to deal with radiation levels of that magnitude. That accident eclipses all of the other experiences I had up there.

FISHER: What are some of your recollections of the work that you did after that accident took place—your personal involvement?

VOELZ: There were at that time two physicians: myself and John Spickard at the medical facility. We had really relatively little capability for handling a contaminated accident at our dispensary. In fact, it consisted of one little showerhead that had been installed under the basement stairway of our dispensary. That was it. It was a time to make many decisions. One was: Where would we put these highly contaminated bodies and how were we going to return them to their families? This work—storing and eventually trying to decontaminate these bodies—was done at the decontamination facility at the Chemical Processing Plant. This was a large room, large enough to drive large equipment into it: trucks and forklifts. It had water and other things we needed. It was built out of materials that could be decontaminated, so it served very well for this purpose. That's where most of the work was done.

Actually, the crew that did most of the work came out of my Los Alamos experience. We got a crew from Los Alamos of eight people, who were health physics specialists, and Dr. Lushbaugh, who was a pathologist. They came up about a week after the accident and did most of the cleanup and preparation of the bodies so they could be returned to the families for their funerals. I think, in those days, Los Alamos probably had the most experienced people. They had experience with contamination at Nevada and the Pacific from atmospheric weapons testing, and other sources where there was contamination at higher levels. It was a fortunate choice to be able to get them up there and do that work.

My principal involvement was with the administrative decisions about how we were going to do these various tasks. We still had hundreds of people working in that contaminated environment. We had people who were getting exposed, potentially every day, in some of the operations that had to be done. I didn't participate much in the work that was done on the three casualties. I was fairly busy with the radiation protection people from the various crews that were going into the recovery operation at the facility. There were lots of things we learned in that accident.

The accident occurred about nine o'clock at night, and it was in Idaho in January. It was dipping to 20 degrees below zero. The [dead] individuals were in a wet environment from the water of the reactor being sprayed throughout the room. The first thing we did was determine if anyone survived the accident. One of the casualties had been taken from the room and brought outside. Because his level of radiation was so high, we had no place to put him. We left him in the ambulance, until we could figure out what we were going to do next. The ambulance was sitting out in the desert, amongst the sagebrush. We decided the first thing we would do is see how much radioactive material we could remove by taking off the clothing. We wanted to get this done before the morning traffic came on the site. It was about four o'clock in the morning when we decided to try to take the clothing off under the lights of a couple of automobiles. Because the radiation levels were 500 rads per hour, we decided to work outside at 20 below zero in anti-C [(anticontamination)] coveralls.

FISHER: The source of the radiation was from just the body?

VOELZ: Yes, from the fuel element particles. Fragments had been blown into the body. All of these individuals had serious physical damage and severed limbs or worse. Each one of these areas had been struck by pieces coming off of very hot reactor fuel rods. Another fellow and I decided we'd try first to remove the clothing. We were the first crew to try to get the coveralls off of one of these individuals. The legs on the coveralls had been blown up to his thighs and were all wrapped up into one big roll around his thighs; with the moisture and the cold temperatures, they were just one solid chunk of ice, having sat out there most of the night. The health physicists had given us about a minute's working time [to remove the coveralls]. But we had anticipated this, and had some pretty heavy-duty tools that we could use on these coveralls. We thought we could do it with two crews. Actually, we were able to get the job done with one: this other fellow and me. They had a stopwatch on us, and we got the job done. I remember we went a few seconds over. I think it was a minute and 17 seconds. That gives you an idea of how you have to improvise when you get into accident scenarios. We were able to get the clothing off, and we put him back in the ambulance. By that time we'd arranged where we were going to do the follow-on work. Unfortunately, taking the clothes off didn't really help anything very much. It [(the contamination)] was about the same level as when we started.

FISHER: That [clothes removal] was an effort to remove some of the contamination?

VOELZ: Right. We thought maybe a good portion of the contamination source was in the clothing. It turned out not to be. We did remove quite a bit of activity in the clothing, but he still had these areas that had physical violence done to them that contained embedded fuel element fragments. That was really our major problem.

FISHER: Do you recall the names of the people who helped you on the first evening after the accident?

VOELZ: There were quite a few people. As I said, John Spickard was the other physician. He actually had gotten to the area first. His car was ahead of our car. This location is 50 miles out of town, so we had quite a ride out there. John was in the ambulance with one of the nurses, and they pronounced the individual dead. Then we just sort of abandoned the ambulance in that area until we could figure out what we were going to do next. I don't recall, right off, who the other individual with me was when we took the coveralls off. There were so many others that were in the radiation detection business that were around that I would hesitate to try to name them.

CAPUTO: Did more people get radiation exposure from moving the bodies than from the actual accident itself?

VOELZ: There were five people who went out and looked in the room. The room was reading over 500 rads per hour, probably more like 1,000. The reason we don't know is that at that time, nobody had conceived that radiation levels in an accident could be above 500 rads per hour. As a matter of fact, that accident changed the manufacturers' [upper] level for all the survey instruments: it is now 1,000 rads per hour. That was a result of that accident. Also, we had problems with respirators because it was so cold out there that the valves froze shut. As you were exhaling, you had moisture in the respirators and they did not function properly. Nobody had really thought about the sort of conventional occupational respirators having to be used at 20 degrees below zero. That's been corrected. There were quite a few of these problems we ran into that subsequently the designers have taken into account.

FISHER: Getting back to the other people who were at the accident scene. There were five people who went into that room, initially to survey what had happened to the workers. Were they in the room when the accident happened or not?

VOELZ: They were really going in for lifesaving purposes. This particular reactor was a military special reactor to be used at the DEW line[23] in northern Canada for radar installations. The idea [behind the design] was [to build] a reactor that did not have to need frequent refueling, because there was access to it only part of the year. The reactor seemed like an ideal energy source for the electricity to run those radar sites. It was designed to be operated by few people. A normal crew was three people; the three people that were working at the time of the accident were a normal-size crew. This particular reactor was doing some experimental work, but the basic purpose of the facility was for training of crews to go up on the DEW line.

The five people who went in to look for the crew found two of them. The room was in complete shambles. The floor was littered with little [pieces of] heavy iron; I'll call them balls. They were used for shielding. In the blast, they had blown all over the room. When they [(the search crew)] went into the room, the electricity was out and they just had their own flashlights.

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23 Distant Early Warning—a 3,000-mile network of radar stations maintained by the U.S. and Canada to provide advance warning of hostile planes or missiles

They were skidding around on all of this shielding that was scattered on the floor. Those five particular individuals came out with a dose of about 25 rads for having done this exploratory search for the people. They were just in there a very short period of time. It was not a time to stand around and think of what you were going to do in there. They recognized that they were in a high-dose area, so they simply identified as much as they could and were out of there in a very short period of time. Other than that, I think all of the exposures were essentially within permissible limits. I don't recall any people who got high radiation exposures, but there was a lot of work done to keep it within the regulations at the time.

CAPUTO: And Dr. Clarence Lushbaugh performed the autopsies?

VOELZ: Yes. He came up with a Los Alamos crew and they did autopsies. The principal thing he did was decontamination. We had pretty well decontaminated the intact skin of these people, but the areas where there were wounds were the high-level sources. He had to, what we call, debride or debridement, which is a nice word to say that he simply excised more of the [skin] surfaces and took this hot material out of the wounds as best he could. He also was involved in identifying the right people to make a survey of all of the physical injuries that occurred to these people. Then, in the exercise of what's called *forensic pathology*, you try to figure out where they were standing or what they were doing at the time of the accident. Who was where by the nature of their injuries, and where they must have been located. Lushbaugh was a superb pathologist, and this is the kind of ugly job that you need a top guy like Lushbaugh for. He did a very good job.

CAPUTO: Did he do it by remote control, or did he work on the bodies directly?

VOELZ: There were a few operations that were done with handled tools, but for the most part things were done pretty much hands-on, but very quickly.

CAPUTO: Did he need the families' permission to perform the autopsies, or was it required [by law]?

VOELZ: It could have been done as a coroner's case since it was an unknown and unobserved accident, but actually permissions were obtained from the families. These were military people. Their families lived in Idaho Falls, and so the military people arranged to get the autopsy consents.

FISHER: You participated in some television interviews in conjunction with this accident; some documentary-type interviews. Do you feel that in any way your role or participation has been misrepresented, or any of the facts of the accident have been misstated?

VOELZ: You're correct that there were some interviews done and there were some presentations. There were also some movies made for training films. I'll have to admit that I actually haven't seen the portrayal. When you're involved in things I really don't enjoy reading about it in the paper or seeing it on television, because it disturbs me frequently. I don't really spend much time going out of my way to find what people have commented on. I kind of naively presume they will do their job and do it correctly, and I will go on and do my job and try to do it correctly. That is probably not a good

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procedure. I know about the programs that you referred to, but I've actually never seen them.

Controlled Environmental Radioiodine Tests (1963–1968)

CAPUTO: The CERT test, the Controlled Environmental Radioiodine Tests,[24] at Idaho Falls occurred around 1964—what was your involvement at those tests?

VOELZ: I was in charge, at that time, of both the medical group and the radiation dosimetry [program], which processed the radiation badges at the National Reactor Testing Station. My involvement was [limited to] that as they were planning these tests; it [CERT] was for release of radioiodine on some pasture land just north of the chemical processing plant. This was 50 miles from Idaho Falls in the center of a 900-square-mile government reservation. There were no communities there. The idea was that, as part of planning for potential reactor accidents and emergency responses, this test would provide additional data on iodine release parameters and doses to people, as a result of the environmental pathway through the pasture-cow-milk chain.

Originally, that was as far as the studies were to go. But, as they got thinking about it, the people who were doing the tests and doing the planning said, "As long as we have the milk and can make measurements, why don't we take it one step further? We'll get a few of us to volunteer to drink the milk, and we'll take the final step into the human." Because I was in the medical department, Clyde Hawley, who was the person in charge of the tests, as I remember it, came and talked to me and asked my opinion on this. He had done some calculations a on what the radioactivity intake and doses would be. We went through those and they were quite small. I didn't really see any problem if he wanted to do that; if he personally wanted to do it. Ultimately, he got a total of six people involved as volunteers.

CAPUTO: How did they get the volunteers; do you know?

VOELZ: They were all people that were working with him. They all agreed to do it. They just talked amongst themselves. I don't recall any permission forms, or any particular paperwork that we did with it, but they talked about it. I had the names of the people who were going to be the volunteers. So I reviewed who they were going to be. My concern dealt more with the handling of the milk itself—the question of bacteriological contamination and the fact that we weren't set up as a milk supplier. I told Clyde that I thought we would have to pasteurize this milk and have it in the same condition as milk supplies. He didn't think that would be a problem. He arranged to get a small, little home pasteurizer so they could pasteurize the milk and make sure that we wouldn't run into a bacteriology problem. In reality, we probably spent more time, at least as much time, talking about the bacteriology as we did the radiation.

Part of these tests were to see how fast the radioactivity would go down in the milk supply, if you put the cattle on dry feed. Several of these experiments involved having the cow graze on the pasture for a day or two after

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[24] 1963–1968 studies to examine the transport of radioiodine through the air-vegetation-cow-milk-human food chain

the release and then putting them on [non-radioactive] dry feed. That was one of the pieces of information that we needed for the emergency-response planning. One of the things we learned was that radioactivity in the milk went down very rapidly. I think it had a half-life[25] of something like 12 hours or so, once you got the cattle onto dry feed.

There were a number of tests done. I don't remember the exact number, but maybe five or six. One of them involved organic iodides.[26] There was some question at that time, for certain types of reactors and certain accidents, whether organically bound iodide would have a different type of pathway. One of the experiments involved methyl iodide. There was an accidental release that occurred which almost became an additional experiment. The iodide that was used had been prepared back East by a commercial company. They had prepared the methyl iodide for this one test. When the shipment arrived from their laboratory, it became apparent that one or more of the capsules had leaked. The methyl iodide, as a gas, had leaked out somewhere along the shipment route from back East to Idaho. It had been shipped first by air freight from, it may have been, Boston and then went to Chicago, Salt Lake, and then to Idaho Falls.

One of the interesting parts was that there was valuable cargo on one of those planes. A person had brought out a very important breeding stock of mink from Nova Scotia. I don't know the total value of it, but it was enough so the person had taken the precaution to ship all of these mink in two shipments. Half of his purchase was on the same plane as the iodine, with a guard. He actually had this under guard.

The question was: What was the iodine going to do to the breeding stock of mink and their thyroids? When we first learned of this, we didn't even know what the size of a mink thyroid was. We decided to estimate air concentration by doing a thyroid count on the guard. It turned out that the guard didn't have any iodine in his thyroid—no radioactive iodine. We couldn't measure anything. That seemed strange, because we also measured some of the mink and they had iodine in their thyroids. We asked, "Were you and the mink in the same place?" He finally admitted that he wasn't. The pilots had decided that sitting back in a freight compartment, without seats and windows, wasn't the place for this guard to be; that he ought to come up in the cockpit. This actually was against FAA regulations, because the pilots are not supposed to have anybody ride up in front with them. It turns out that the air supply for the cockpit and the payload weren't the same. Then, we finally did look at the mink and their thyroids. They had some exposure, but the dose did not seem to be enough to be of any particular concern of harm to the mink.

The other problem we had in that whole accident was that, from Salt Lake City to Idaho Falls, the cargo was on a passenger plane, and so we eventually tried to track down all of the people that were on the plane; almost 100 people. By the time we caught up to all of them, they were all over the

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25 time required for half the atoms of a radioactive substance to decay

26 compounds containing iodine

country. We were making measurements in Salt Lake City and Idaho Falls and other places and bringing people back to have their thyroids counted.

FISHER: Was their any detectable activity in any passengers?

VOELZ: Yes, we had some. It was pretty low-level. That plane, unlike the other, was such that some of the methyl iodide did get into the ventilation system.

FISHER: Do you remember what the radioactivity of the shipment was?

VOELZ: I don't know how much was in the capsule. Once they got looking at all the capsules, there was only one that turned out to have had a bad seal. We went back to [the manufacturer in] Boston and looked at the [freight] carrier from the factory. They had delivered it by car to the airport or shipping office. They found some iodine on that vehicle. So, it was leaking from the start. It seems to me it was [leaking] going into these tests so it had to be in the millicurie range.

FISHER: I interrupted you. You were going to say something about the [flight] crew.

VOELZ: They were the [people whose whereabouts] could be located. We knew where they were and who they were. We knew all the other passengers, too, but we didn't know how to locate them. Stewardesses were in the back of the plane, and the pilots were up in front. We used them initially to get some idea of the scope of the exposure. It was detectable, and we had a good idea what the doses were. Then we measured not all, but the majority of the passengers. It was pretty consistent. It must have had fairly uniform distribution during that flight, but not a harmful level.

CAPUTO: Were the people told?

VOELZ: Yes. Oh, yes. It was one of the early transportation-accident type of situations. The methyl iodide was not going to be used—I believe that it was not part of the experiment that involved any human volunteers—except that we got into these accidental exposures of pretty low consequence; low doses. The people who volunteered, the six who volunteered, drank this milk for some number of days. They had measurements of the thyroid doses and iodine in urine, and they were being monitored.

Informed Consent at Idaho

FISHER: In those days, you mentioned there wasn't a consent—

CAPUTO: —written, informed consent.

FISHER: Did you have any kind of a human subjects review prior to this experiment?

VOELZ: I was listed on one of the first reports as an author. I was really acting as a medical consultant in that case. We had other health physics people looking at the doses. So there was a review, in the sense that a number of us on the staff involved in health protection looked at this. There was a written protocol that we reviewed, as I remember it.

FISHER: Human subjects committees didn't really become commonplace until maybe 1967 or later. Do you remember organizing a human subjects committee at Idaho or having a part to do with one?

VOELZ: I don't remember that we organized any up there. I left there in 1970, and went back to Los Alamos. When I went back to Los Alamos, there was not an official review committee as such. They operated pretty much the way we had in Idaho. That went way back in the history here. Dr. Shipman, who had been the division leader, reviewed any proposed studies that involved people. And then in 1966, when the NIH came out with their first recommendations,[27] they changed it so Dr. Shipman, another physician, and a health physicist reviewed proposals. There were three people reviewing studies instead of just Shipman. They sort of formed an internal committee from people on the staff. There was an internal committee similar to what we had done at Idaho. When I got back here in 1970 that was still the situation. But I don't think the people here had worried too much about it, because of the lack of studies involving human volunteers from the middle 1960s to the '70s. There wasn't really any of that type of study going on. We started forming a committee when we recognized that the LAMPF[28] project would involve a clinical study of radiation therapy for cancer treatment. We started forming what would now be considered the current kind of review committee with outside members. I think this [review committee] started around 1971.

FISHER: *(to Caputo)* Anything else?

CAPUTO: *(to Voelz)* No, do you have anything else you want to add [about your experience in Idaho]?

VOELZ: No, I don't think so. That really covers about as much as I was involved with [at Idaho].

Tracer Studies on Human Volunteers at Los Alamos (1955 to Late '60s)

FISHER: You returned to Los Alamos in 1970 as leader of the Health Division and then somewhat later became assistant division leader for Environment Health and Safety. In the materials released by Los Alamos, there are descriptions of a number of tracer studies that were conducted on normal human volunteers. We wondered if we could ask you about your involvement or participation in some of these. Were there some studies on uptake and retention of cesium-132, -134, and -137 involving measurements in the whole body counter?

VOELZ: Correct. You asked about my involvement. My involvement was essentially nil, in that I had left here [Los Alamos] in '57. The studies you are talking about really started up around 1955. I think the last reports were written about '67–'68. So, I knew the people who were involved, from the period when I was here in the '50s, like Wright Langham and Chet Richmond. I came back in 1970. By then, that kind of work had stopped. But I reviewed the papers and I am familiar with the studies. I knew many of the people at the Health Research Lab, where this work was done. When I came back in

27 In 1966, the National Institutes of Health (NIH) made recommendations to the Surgeon General's Office for the creation of what are now known as Institutional Review Boards (IRBs). IRBs review and approve medical research involving humans.

28 Los Alamos Medium Energy Physics Facility, an accelerator facility

1970 the Health Research Laboratory staff was within the Health Division. From then [1970] until 1979, I was administratively in charge of that group as well as Operational Health and Safety. But, the way it worked out, most of that work had started after I left and was finished by the time I got back.

FISHER: Were you ever involved in any cesium decorporation[29] studies on human volunteers?

VOELZ: No, but there was some work done here. Chet Richmond reported on that at one of the Hanford Symposia, in which he had looked at Prussian blue[30] in one adult volunteer. There was very limited work done on that. The majority of volunteers at Los Alamos were people who worked at the [Los Alamos National] Laboratory.

Development of Decontamination Technique and Esophegeal Probe

CAPUTO: Were you ever a volunteer for any of the experiments?

VOELZ: Not with radioactivity. I volunteered for a few other things. Some of them were kind of interesting. I'll tell you what they were. When I was up in Idaho, I participated in an experiment that was never written down. One of the problems we had in decontaminating accident victims in the sink that I talked about before, was that the bodies were basically lying in the [contaminated] water. The particulates washed off the top and would settle to the bottom. So when you thought you were done at the top and you would roll the individual over, you would recontaminate what you had worked on before. It was not a very efficient process. One of the things I had thought of was a way to do this by shower, which is what we normally do when people are ambulatory. But these individuals were dead. If you had unconscious individuals, it seems to me the thing to do would be to try to suspend them.

FISHER: Put them in a net?

VOELZ: Yes, and wash them off. I did better than that. I was trying to get a harness to see how long you could stay in it so you could get them off the floor and hose and scrub them down. I volunteered to see how long I could tolerate the particular harness we had designed. It felt like a parachute harness. I thought it was too uncomfortable for the amount of time you needed to decontaminate. So we scrubbed that idea. We only ran through one person; that was me. I think I stayed in it nearly half an hour. But it would have been too long to ask a person, at least a conscious person, to be in that rig. If they were unconscious, it would be even worse because you might not know what [else might be] happening. So we scrubbed that idea.

I also volunteered for an esophageal probe that was designed by Ken Swint. This probe was a fiber optic cable attached to a small sodium iodide crystal. We were trying to measure plutonium in the prominent lymph nodes at the tracheal bifurcation in the chest by placing this detector within a centimeter

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29 removal of radioactive material from the bloodstream or organs, using chemical complexing agents

30 a chemical material used to enhance the excretion of cesium

or two of them. Ken brought this to Los Alamos, and we tested it on two people. He did it first on me. We found the detector was too thick, and the fiber optic was too stiff to get it through the nose. I wound up with a battered nose and nosebleed before we got done. Then we took it down, like a gastric tube, just through the mouth. I was the background control to see how many counts he would get on this detector. As a result of my experience, we decided it was not a neat procedure to do without some sort of analgesia or anesthesia.

We tested the probe the next day on one of the individuals who had plutonium exposure, and who we knew had a lung deposition. [This time], we gave him some medication. He had no discomfort with the procedure, and we were able to make as many measurements as we wanted. When we were done, he said he would have no problem if we wanted to do it again sometime; he would be happy with it. We got some interesting information but it required too much medication to use as a routine procedure. I think the hope was that this could be routinely used as another form of direct counting for insoluble plutonium particulates, but it was too onerous to be used that way.

FISHER: It seemed to be a highly invasive procedure. Didn't you also position the probe with diagnostic radiology?

VOELZ: We used diagnostic x ray to see where the detector was, yes. Those are the volunteer jobs that I've done.

Radionuclide Intake Studies at Los Alamos

FISHER: Do you remember or having any knowledge of rubidium-86 intake studies at LANL?

VOELZ: Yes, there was a rubidium-86 intake study. It was done as a part of a series of studies with the alkali metals that Chet Richmond did. He used sodium, rubidium, cesium—I don't remember what the others were. These were basically tracer studies with just enough activity for the counters to determine the biological half-times[31] for the various materials.

One of the instrumentations and detectors that had been developed here at Los Alamos was the human body counter using scintillation counting fluids.[32] The counter was essentially a cylinder that surrounded the individual. You got very high sensitivity but not very good resolution of any sort. But for the types of studies, with very small amounts of radioactivity, you could make these measurements of the rate of decay, rate of excretion to determine an effective half-life, and the biological half-time. These people were measured in the counters over a period of time and compared also with animal data. Chet did a lot of work on how much difference there was between human half-times and various animal half-times and made some interspecies correlations so you could make predictions without using humans.

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31 time required for half of the material to clear from an organ

32 a fluid that emits light when a radioactive emission passes through it, used to measure radioactivity

FISHER: As an occupational physician, did you review research plans at Los Alamos proposing human administration of radionuclides?

VOELZ: After 1970, that work was all done. We did establish the Institutional Review Board. I was not on that review board, because I was the administrative head, although the Medical Group always had a physician on the review board. While I did not personally do this, there has always been an occupational physician on the review board.

FISHER: There were some strontium-85 studies that probably took place during the same era when you were in Idaho. Also, some ingestion studies involving magnesium-labeled ceramic and uranium-235 carbide, large microspheres of material. Do you remember when these ingestion studies took place?

VOELZ: Let me talk about the strontium just for a minute. Strontium was measured in three adult males. They ingested a small amount of strontium-85 in water. The retention in the body was counted in the HUMCO I counter[33] for up to 6 months. Another study at the time was a skin absorption study. What they did was put a patch containing strontium-85 chloride on the skin of two volunteers; basically like a band-aid. It had a known amount of material in it. Then they made whole body measurements over several days. They also did it for several other radionuclides. They did it for, I believe, sodium and iodine in that same manner. There have been other radionuclides, like tritium, that had been done earlier. Basically, they found that the strontium did not, to any extent, move through the skin. There was little internal exposure to the strontium.

FISHER: Would this have been also during the period you were in Idaho?

VOELZ: Yes. I only know about these by being familiar with the papers that were written and published.

Studies to Predict the Effects of Radionuclide Inhalation and Digestion From Nuclear-Powered Rockets (Los Alamos, 1950s)

FISHER: In the 1950s, when the HUMCO I was first built, there are some references to the use of Los Alamos firemen as subjects. Perhaps it was used after having inhaled or taken into their bodies cesium-134 and cesium-137. Were you aware of these studies, or do you know anything about them?

VOELZ: Don Petersen,[34] who you are going to talk to later, has looked into that whole fireman situation and really knows more details about that. I don't remember specifically the cesium-137; the firemen that I know of were involved with iron-59 studies. The cesium-137 and -134 exposures involved four people. I think they were all people from the [Los Alamos National] Lab.

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33 HUMCO I is the first whole body radiation counter that became operational at Los Alamos National Laboratory in 1956; the sensitivity and noninvasiveness of this new instrument permitted studies at levels 10 to 100 times below established limits of exposure.

34 Don Petersen was interviewed on November 29, 1994; the transcript of that interview will be published as DOE/EH-0460.

FISHER: Were they people who had been deliberately administered cesium, or was it natural-fallout cesium?

VOELZ: They all got natural fallout. But this study [also] involved giving an additional known amount [of cesium]. There were four people that had tracer doses of cesium-137. It was a study done by Chet Richmond. I don't know whether Chet may have been one of the volunteers. They were not the firemen, to my knowledge. All of these studies were really generated or designed to answer questions of the moment. The cesium study was of particular interest because of needing the best dosimetry measurements they could get for the fallout. This was the number one public health issue at the time.

Most of these studies had a particular problem or question that was trying to be answered. The study with the microspheres was tied to the development of nuclear-powered rockets and space vehicles. The safety committee was looking at the safety of these kinds of devices. [They wanted to know whether] an accident like a burn-up in reentry into the earth's atmosphere would be safer with bigger particles, rather than smaller particles [from the burn-up]. In the one case, bigger particles might have [a human] ingestion problem, but not as much of an inhalation problem. If it broke up into small particles, you'd have an inhalation problem. How long would these materials stay in the gastrointestinal tract if there were large particles, and would the density of these particles, since they would be uranium particles, change the [GI tract] transit time? In other words, would the gastrointestinal tract somehow discriminate between the density of these various particles?

Chet and others designed an experiment to try to answer some of those questions. They used two types of particles. There was a low-density ceramic particle that was traced with manganese-54. Then they used some unclad uranium particles. These particles were from about 100 to 200 micrometers. They were [orally] administered to 57 volunteers. These particles, although they were called large, were actually barely visible, but they were very large compared to inhaled particulates. The people generally took three of these particles in a gelatin capsule. They just swallowed them and then their progress was tracked through the gastrointestinal tract. They [(the researchers)] could measure the particles in fecal samples rather easily.

The tag that was put in wasn't a radiation study. The radiation just was the modality that was used to make these measurements in an efficient way. The result was that it confirmed the transit time that others had either studied or estimated. They also found that there was no difference between the high- and low-density particles: they moved at the same rate through the [gastrointestinal] system and none of them were held up in the system. The longest particle in the whole experiment took 96 hours to clear the system.

FISHER: They [(the gelatin capsules)] were highly insoluble to prevent dissolution in the digestive tract.

VOELZ: [Yes]. There had been animal and other studies done prior to this to show that for the amount of time involved, they would stay [intact] in digestive juices. I believe in that experiment, all but one of the people were laboratory employees at the Health Research Lab. That other one was the wife of the principal investigator. You know, in those days, they [(the employees)] felt it was very important work to do. They're very proud that they were participating in this. You get the impression, at times [from the press], that you had to twist people's arms to get these kinds of studies done. It was really quite the opposite. It was a good thing to do, and they wanted to participate in this. That's not always appreciated. Even today, I get calls maybe once or twice a year from people who have some [terminal] illness, and they want to know [how they can donate] their body. They want to make one, at least in Los Alamos, one more contribution to science. [They ask us], "Do we have any idea what could be done with our tissues or organs, or is there some way we can contribute?" I don't think the average person has any idea this goes on until they are faced with some known finite period of time to live and want to contribute for the good of society. I think that's a different view point than most people feel about volunteers. It certainly was evident in the '50s and '60s.

Use of a Particle Accelerator to Explore Cancer Therapy (Los Alamos)

FISHER: The LAMPF[35] accelerator has been used in some number of studies to investigate the potential for accelerated particles in cancer therapy. Have you had any involvement with those studies, and has there been any use of the Los Alamos accelerators for any clinical studies on cancer patients, or any simulated therapy of normal subjects?

VOELZ: My involvement was at the time LAMPF was constructed. It was completed in the early '70s. I had come back as the Health Division leader at that time. My involvement was setting up the [Los Alamos] Institutional Review Board, so we could look at all the protocols and review this [prospect]. We had a joint member from the University of New Mexico's Institutional Review Board, who sat on both boards. The clinical part of the studies was conducted by the University of New Mexico. There was really no one at Los Alamos that was responsible for that part of it [(the cancer clinical trial studies)].

Dr. Mort Kligerman was the director of the Tumor Registry at the University of New Mexico, and was also the principal investigator for that project. They did the workup of patients who were selected for the clinical trial. These people came from various parts of the country. The clinical workup was done at the University of New Mexico, and then they came up here for their radiation treatment. I really was not involved in the actual operation of that study, except for making sure the Laboratory had thoroughly reviewed the way that the beam configuration and doses were measured and controlled. This was done by the staff down at the LAMPF facility. There was a lot of coordination between the staff up here and the University of New Mexico, but the part involving the humans was really done by the University of New Mexico.

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35 Los Alamos Medium Energy Physics Facility

FISHER: Was it a clinically based project, where only patients were treated, with some reasonable expectation of a therapeutic benefit?

VOELZ: Yes. There were also studies done on animals that were basic dosimetry studies. In the early part of the work, it involved more dosimetry and some animal work and then eventually went into the clinical part of it.

Pion Irradiation Therapy at Los Alamos (1974)

FISHER: *(hands document to Voelz)* In 1974, patients were irradiated with pions[36] to investigate normal tissue response. Do you have any recollection of those studies?

VOELZ: I don't think there were any people irradiated other than cancer patients. When you irradiate cancer patients, you have to go through a certain amount of normal tissue to get [to the tumor sites]. In conjunction with the therapy, you had an opportunity to observe what went on with the normal tissue. I think that is what whoever wrote this document was referring to that. It was kind of a natural thing to do. You don't want to destroy normal tissue if you don't have to.

FISHER: It's also important to determine what the therapeutic ratio is—that is, the tumor-to-normal-tissue ratio.

VOELZ: One of the attractive features of pion therapy was that this particle, when it goes into the tissue, it releases the majority of its energy right at the end of the track. So, if you now can, let's say, control the pion beam's width and size, and direct where it's going to hit the cancer target, you can control the energy so that it dumps the major portion of the radiation at the tumor site. This is quite different from x ray or neutrons or other forms of irradiation. So, it was a unique idea to get much higher doses into the tumor without having a very large dose in the normal tissue that it had to go through. One of the important unknowns in that study was just how much energy you could get, and how well you could control the length of that path. It was a very complex dosimetry problem.

FISHER: You were on the Institutional Review Board for that work?

VOELZ: No. We had set up the Institutional Review Board, but I was not on it.

Allegations That the Transuranium Registry has Impeded Settlement of Workman's Compensation Claims

FISHER: *(to Caputo)* All right. Do you have any other questions?

CAPUTO: I was hoping to get back to the Transuranium Registry. I know you have some involvement with that. A man named Jeffrey See has testified before the Presidential Advisory Committee on Human Radiation Experiments. He has expressed concern about Los Alamos and Oak Ridge losing tissue samples from the registry after they tested them, and not testing for the element that is the subject of the workman's compensation claim. There-

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36 a fundamental radioactive particle

fore, Mr. See claims, the sites have thwarted workman's compensation claims through the use of their registry. I was wondering if you could comment at all on that?

VOELZ: I don't know who Mr. See is, but I think probably what he's referring to is a case involving a Rocky Flats worker who has been in the paper and has been frequently referred to. This was a man who had a brain tumor and subsequently had a workman's compensation claim that his brain tumor was related to his exposures at work.

Los Alamos has done plutonium measurements in [human] tissues for many years and was doing radiochemistry work for the U.S. Transuranium Registry since about 1982, or somewhere in there. Prior to that, it had been doing tissues on people that were submitted by pathologists in certain areas of the country who had agreed to collaborate on this type of study. Of course, we always had some samples coming from the Los Alamos population locally.

This particular case with the brain tumor involved a man who had been to Los Alamos in the clinical trials at the LAMPF facility. He'd had irradiation of his brain tumor here as part of that clinical program. He had also, I believe, signed up for the registry. In any event, there was an arrangement, an agreement, when he died that the autopsy samples were to be sent to Los Alamos for plutonium measurements as part of the U.S. Transuranium Registry study. What in fact happened, was the University of New Mexico medical school had also indicated an interest in the brain tissue. Because of his pion therapy, they wanted an opportunity to see what had happened to the brain tissue. The autopsy was performed and the tissue samples that are normally done for the registry were all sent to Los Alamos. They were measured for plutonium.

Some years later, when his workman's compensation claim came up, it was discovered that the brain was not one of the organs that had been measured for plutonium. It [(brain tissue)] had never been received at Los Alamos. As part of the legal proceedings, they questioned where the brain tissue had gone. In checking back, it had gone to the University of New Mexico. By that time, they had done their studies. They did not have any tissue left, except for one small paraffin[-embedded] block of brain tissue, which was used for histological[37] sections.

Dr. Jim McInroy, who was in charge of the tissue [analysis] program at Los Alamos at the time, really had no indication or way of knowing this had happened. The brain was not an organ that was [examined] in all autopsies. It was a special sample, and was not something he would normally expect. He didn't realize anything was missing until this was discovered. At that time, they located the one small paraffin-embedded tissue and the University of New Mexico offered the sample. For plutonium measurements, you need much larger tissue samples. If you took a small sample, such as they had, you would not be able to detect any plutonium in it, and you would come out with essentially a zero, which we would know is not correct. That

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37 relating to the study of the structure of tissue

sample was not measured; it was just not adequate. That sample was probably in the range of a gram or so, and we normally like to have samples of 250 grams or better.

CAPUTO: Didn't you have the other tissue? Did you need the brain sample to derive his exposure to plutonium?

VOELZ: No. The brain really doesn't help us much in that matter, because the plutonium levels in the brain aren't really unusual. Roughly 90 percent of the activity is in three major organs: the lung, liver, and bone. The rest of the tissues contain fairly low concentrations compared to these three locations. The purpose of the registry, of the autopsy studies, is to look at the distribution between these three major organs and within these organs for dosimetry purposes. The brain really was of no particular value to the registry and wouldn't have changed any of the findings. In this case, they did have the results of what had been found in the other tissues. As far as we were concerned, it was a non-issue except for the family. I'm sure they [(the family)] felt that this was an unusual situation, since his brain tumor was the tissue of interest to them. But it didn't really affect our program at all.

Evidence For and Against Links Between External Radiation Exposure and Cancer Incidence Rates

CAPUTO: *(to Fisher)* Did you have more questions?

FISHER: Your research to be published in the December 1994 issue of *Health Physics* identified a trend for higher [incidence] rates of death due to malignant brain tumors, Hodgkin's disease, and esophageal cancer with increasing levels of exposure to external ionizing radiation. Were there sufficient numbers of workers, high enough doses, and sufficient numbers of tumors on which to base a strong conclusion that this was indeed the case? Is there statistical significance to that finding?

VOELZ: The answer is: On a statistical basis, these cases were placed in the various exposure levels. Statistically, it appears to be a significant correlation with dose. In other words, the incidence rates of these three types of cancers are elevated statistically in persons who had radiation exposures. There are a couple of problems with it, however, as explained in the paper. The number of cases are what they are. You never have as many cases as you would like for this kind of study. Yet, for the number of cases and the number of person-years in those exposure groups, it comes out with a statistically significant result.

[However], there are several reasons why you cannot make a judgment whether this correlation is valid. One of them is that statistical correlations still have some probability of being a random event; of being just the way that the events occurred. You can't get away from this potentially random event until you have other studies or other experiences that correlate with this. Correlation is not yet available for these cancers. Two of them aren't really correlated with radiation to begin with. The third one occurs only at high doses. These particular findings don't correlate well with the rest of the epidemiology literature. It makes you think that this may, in fact, be a spurious finding.

The other question is that when we're dealing with work-related exposure, you may have other confounders that are operative that may also be correlated with radiation. Radiation may be a surrogate to some other event going on here. These could be, for example, chemical exposures or other things. In this study, we really only looked at radiation records, because in almost all plants they are better kept than chemical records. We can't rule out exposure history of other substances we aren't aware of that might have produced this result. At least, I'm personally skeptical of the relationship between increased incidence and radiation. But statistically that's the way the numbers came out.

Correcting Misperceptions About the Los Alamos Human Radiation Experiments

CAPUTO: One more question. Cold War human radiation experimentation has received a lot of public attention since last December 1993. We've been working at Los Alamos to retrieve documents relating to the subject. Do you feel there are any misperceptions that need correction for the American public to better understand what occurred, which we haven't already brought out in the interview?

VOELZ: I probably have more ideas along that line than I can remember or express. There are a number of things in the television and newspaper reports that are really quite misleading. One is that—and it's a very easy thing to fall into— many people who have written on this subject always say these are studies on the effects of radiation. That was not the purpose of these studies. As soon as you start talking about effects, you're thinking about higher doses and that something's going to happen to people who volunteered. This is not what people here agreed to. They were told these were low-level experiments, which they were. But somehow, "effects of radiation" seems to be a set of words that always goes together. When you see the stories, it almost always comes out that way. In fact, what was being done here was that we looked at how these materials are handled within the body, how they're excreted, how they're absorbed, how they're distributed in the body. Knowing that information can then be used for calculating doses. It had nothing to do with the effects of radiation. This is, I think, quite misleading if you look at the doses.

We calculated the doses from these experiments to see whether today we came out with the same dosage estimates that people had earlier. In all the studies done here of what we call tracer studies, the highest doses were from the tritium experiments and involved three people. They were all principal investigators, and were experts in their field. Tritium experiments went on for a couple of years, and the total dosage estimates in these individuals ranged from just under 300 millirem to 900 millirem at the highest. Those exposures are comparable to the equivalent of a few years of natural background radiation.

All of the rest of these experiments involved studies from under roughly 100 millirem down to 1 or 2 millirem. Los Alamos, being at a high altitude of about 7,000 feet above sea level, has a higher background radiation level than if you live at sea level. Living here for anywhere from a few weeks to about two to three years would have been equivalent to what the other volunteers would have received. It really is small.

One of the studies, which got a lot of publicity, was on children. I'm not sure it was always well explained that the doses for these children turned out to be 10 to 15 nanocuries, which would be 10 to 15 billionths of a curie. That dose is probably anywhere from 50 to several hundred times less than a diagnostic iodine thyroid test done today. This low dose was possible because we had such sensitive whole body counters at the time for these kinds of studies.

The studies in the children, incidentally, were done because of the iodine in fallout. Scientists wanted to get better information on the dosimetry of thyroids of children, including what the depth of the thyroid was in the neck, so that when you used an external counter, you could take appropriate absorption measurements of tissue and not under- or overestimate these doses. That's why the children were used. They were all children of the investigators.

Those are probably some of the main misperceptions that have gotten into this work. Not every place that did these studies involving humans were the same. All of the studies here, by the nature of our work, and the nature of the problems we were trying to address, were low-level tracer studies. We weren't into therapy or higher doses. We did do some nuclear medicine studies. But these were done initially because we had instrumentation to do these kinds of measurements, and some of the physicians in the area wanted to have things done that were not available to them otherwise. The [Los Alamos National] Laboratory was interested in doing this because the staff thought they could learn some things from these measurements.

One of the things I did not know until these records were reviewed, and we had some people call in on our telephone line, was that this early medical research work was done, at that time, from physician referrals. Physicians brought patients from the Lovelace clinic in Albuquerque. Several people who called in had their work done at Lovelace. They came up here in the late '40s, 1947 or 1948, to have diagnostic studies done on them, because it wasn't available in Albuquerque. So the doctors would call up and make arrangements to bring their patients to have these studies done. You have to remember that in that period, the '40s and early '50s, these were rather unusual facilities to have available.

One of these people that I talked with on the telephone said her mother didn't really believe in medical practice much. She was a young girl in her teens then. She said, "The procedure was nothing, but driving with that doctor up that dirt road, up to the hill was really the hazard. That was the scary part of it." She still remembered that, over 40 years later. Incidentally, she was very appreciative, because they did diagnose a thyroid cancer on her. She had it taken out, and she's now a middle-aged mother with several children and she said, "They would have never done that if they just had the methods that were available in '47 or '48. They knew there was a lump there, but they would never have operated as early as they did." She actually called in and told me the story. That wasn't a complaint. It was just that she wanted to support the idea that there were many people that were helped by this.

The other part of the story that isn't told very well is that scientists have learned a lot of medical science from this. I think somebody should trace back to confirm this. For example, I'm not sure today that without the tracer and radionuclide work that was done, it would even be possible to have any of our knowledge about genetic structures and chemical structures, and all of the new advances in the last 30 or 40 years. I'm not enough of a biochemist, but I doubt that we could have the advances we have in genetics, or even know anything about that whole subject to the degree we do, if we hadn't done all of these tracer studies and been able to put it together. It's just a marvelous tool for the biochemists and the biologists. We're benefiting now in ways that [could not be imagined by] people who are critical of some of the methods that may have been used at the time to do some of these studies, such as written consent or other things now in our regulatory framework. I think without the radionuclide work, we wouldn't be anywhere near where we are today. That story needs to be told.

FISHER: You've had a remarkable career and certainly a distinguished career. Your writings are well read, and I've read them many times.

VOELZ: I would like to have done more. I'm still trying.

Continuing Follow-Up Studies of Plutonium Workers

FISHER: You still have work to do in the continued follow-up with plutonium exposed workers.

VOELZ: Unfortunately, the funding is gone for that. I don't know what we're going to do. Right now we're trying to figure that out. That whole series of the 26 workers that we've been following—that has all been done as back-burner work. The only help we ever got on that was transportation to bring the guys back. It wasn't funded for the first 20-some years. Right now it doesn't look like it's going to be funded for the next five-year follow-up. The Los Alamos cohort is not being funded now. Our epidemiology is gone. The same is true for Ethyl Gilbert's Hanford workers study, up in Richland [Washington]. Through the DOE agreement, NIOSH[38] is doing the management of epidemiology studies. But they just have selected other things to do. They want to get out of the credibility issue, because anyone connected with these former projects is not credible. It's easier to avoid that issue by getting other studies going at universities in other places. In the meantime, the kind of things that were done to support worker protection has shifted. I don't know what's going to happen to that. If I were younger, I would be more vociferous about this.

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38 National Institute of Occupational Safety and Health

FISHER: Some things have to be done while there's an opportunity. There may never be the same opportunity once again.

VOELZ: In our paper we have the plutonium curve, showing when we expect the major portion of the plutonium workers to pass on. It's a pretty steep curve. The paper that we just wrote, if I remember correctly, indicates that 15 percent of the Los Alamos cohorts are deceased. In the plutonium cohorts, it's somewhat higher. Because they were older and were exposed at an earlier age, it may be around 21 percent or 22 percent that are deceased. Most of the plutonium exposures came early. By the year 2003, that's less than 10 years. Using standard U.S. mortality rates, over 75 percent of their group is going to be dead by then. There's no effort being made to follow those people, or will not be unless we find ways of getting some help.

Putting together this cohort, between the females and males, there are over 25,000 people. Some of these records took over 10 years to pull together. It's together now. It just seems bad business to not keep it up, because it will be much easier, and much less costly now because the early people were very hard to put together.

Records were never intended to do studies like this. Like dosimetry records, for example. I don't know how many "Jose Martinezes" there were, but in the early days when they got a badge, the employee would write down their name. The first time they would write "J. Martinez" and the next time they would write "G. Martinez." They didn't remember from one time to the next what they wrote down. Then you had a bunch of people like us saying, "Look what's in these records" when you see 30 Martinezes you can't match up, because of the way it was entered into the records. We couldn't straighten out all of it. It was really a difficult job.

FISHER: We'll conclude this interview, then.

CAPUTO: Thank you very much for your time today. We appreciate it.

VOELZ: Thank you. ❑