DOE/EH-0477

HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Radiobiologist Chet Richmond, Ph.D.

Conducted January 24, 1995

United States Department of Energy
Office of Human Radiation Experiments
August 1995

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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 decision-making 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 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 RADIOBIOLOGIST CHET RICHMOND, Ph.D.

Dr. Chester Richmond was interviewed on January 24, 1995 by David Harrell of COMPA Industries and Dr. Darrell Fisher of Pacific Northwest Laboratories, on behalf of the U.S. Department of Energy (DOE)—Office of Human Radiation Experiments (OHRE).

Dr. Richmond was selected for the Oral History Project because of his distinguished career at Los Alamos Scientific Laboratory (LASL) and at Oak Ridge National Laboratory (ORNL), and his pioneering work in discovery of the metabolism of various radionuclides. The oral history covers his education, his career at Los Alamos, and his 16-year tenure as Associate Director of ORNL.

Short Biography

Chester ("Chet") Richmond was born on May 29, 1929 in South Amboy, New Jersey. He is married and has four children. He received his B.A. from New Jersey State College in Montclair, New Jersey, in 1952. In 1954, he received his M.S. from the University of New Mexico. He received his Ph.D. in Biology from the University of New Mexico in 1958. While at the University of New Mexico, Richmond became employed at Los Alamos Scientific Laboratory (LASL) in Albuquerque. From 1955 to 1958, he was an assistant staff member, and then a staff member, at LASL. In 1958, Dr. Richmond was appointed to the staff of the Atomic Energy Commission (AEC)—Division of Biology and Medicine (DBM) in Washington, D.C., where he remained until 1971. From 1971 to 1973, Dr. Richmond was the leader of the LASL Biomedical Research Group. He served as Alternate Health Division Leader at LASL until 1974, when he was appointed Associate Laboratory Director for Biomedical and Environmental Sciences at the Oak Ridge National Laboratory. In 1990, Richmond was named Director of Science Education and External Relations for ORNL. He held this position concurrently with that of ORNL Associate Director Emeritus, until his retirement on January 1, 1995.

Dr. Richmond has published extensively on the biological effects of internal emitters, the health and environmental effects of energy production, and the radiobiology and metabolism of the alkaline metals and actinide elements in man.

Dr. Richmond is a member of five professional societies:

- Health Physics Society,
- Sigma Xi,
- Society of Risk Analysis,
- Radiation Research Society, and
- New York Academy of Science.

He has received several awards and recognitions:

- fellowship, American Association for the Advancement of Science;
- the E.O. Lawrence Award, U.S. Atomic Energy Commission (1974);
- the G. Failla Award and Lecture, Radiation Research Society (1976); and
- W.H. Langham Memorial Lecturer, University of Kentucky (1987).

Graduate Studies at the University of New Mexico and Los Alamos Scientific Laboratory (1955–58)

FISHER: [My name is Darrell Fisher from Battelle, Pacific Northwest Laboratories[1] and I am here with David Harrell from the Department of Energy's Office of Human Radiation Experiments. We are conducting an oral history of] Dr. Chester Richmond on January 24, 1995, in the Oak Ridge[, Tennessee,][2] Federal Building. Dr. Richmond, we'd like to start out with a discussion of your education and perhaps your undergraduate class and how you got interested in medicine and radionuclides.[3]

RICHMOND: Thank you. First of all, I would like to thank you for the opportunity to spend a little time with you talking about the past. I went to school in Montclair, originally an undergraduate school Montclair, New Jersey, which is now Montclair State University. That started in 19[48], just several years after I was discharged from the Army. My interest in Montclair at the time was that Montclair was a state teachers college and I thought I wanted to go into an education career. However, Montclair had some outstanding teachers in the sciences; they were well-known. That was the driver, I think, and that's very typical. People who go into sciences very often state the reason as being someone—a teacher—that provided that very strong stimulus to go into science.

I graduated from Montclair in 1952, and went to New Mexico, because I had [a] relative who was living in New Mexico, and learned about a program whereby a person could attend the University of New Mexico and do a dissertation at the Los Alamos Scientific Laboratory. My degree was in Biology from Montclair, so I pursued a biological career at the University of New Mexico. As I mentioned, that provided me the opportunity to go to Los Alamos and finish my dissertation.

Interestingly, the agreement was [that] I would spend at least three [years] working on a research problem. I highly recommend that kind of training in education because I had the opportunity to work three or four research groups for [about] a week at a time to choose the area in which I wanted to do my dissertation.

I was extremely interested at the time in how things behaved in the body because my submajor was Physiology,[4] especially comparative physiology. Actually, there was an individual at the University of Illinois, C. Lad Prosser, who was well-known in the field of comparative physiology, who actually had done some work related to the Atomic Energy

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1 Since 1965, Battelle Memorial Institute, headquartered in Columbus, Ohio, has operated the Pacific Northwest Laboratories in Richland, Washington, for the U.S. Department of Energy.

2 During World War II, the Manhattan Project had built a vast complex of highly classified facilities in and near Oak Ridge, Tennessee, to process uranium for use in atomic bombs. The Atomic Energy Commission took control of these facilities upon its creation. Today, they belong to the Department of Energy.

3 atomic species in which the atoms all have the same atomic number but different mass numbers according to the number of neutrons in the nucleus

4 the branch of biology dealing with the functions and activities of living organisms and their parts

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Commission (AEC)[5] in the early days. Of course, I wasn't aware of that because I was a young student. The opportunity to use radioactive materials as tracers[6] was just a fantastic opportunity since I was in the field of comparative physiology. [I was] interested primarily in [the sense] that I was aware of the history of how radioactive materials, even in 1950, were so replete and so comprehensive.

I always try to remind people that there's a continuum of how radioactive materials have been used in medicine or in biology. That starts with the naturally occurring radioactive materials, and there's a wealth of history on the use of radium, then thorium, uranium, and materials such as those [in] nature. Then, with the invention of the cyclotron[7] and other machines, a whole new group of radioactive materials were made available for study. Actually, Nobel prizes were awarded for these studies; Hevesy, in particular, who developed the terminology for tracers, etc.[8] That led to a large number of studies, again tracing the dynamics of individual elements in the body.

In 1938 [and] 1939, with the advent of fission, there was again an explosion (in essence), of the numbers and kinds of radioactive isotopes that could be used for tracing [a] particular element in the body. So that was a fantastic opportunity for me, and I guess at the time I really didn't appreciate the uniqueness of the situation, being a young student. Well anyway, I did get [both] my master's and Ph.D. from the University of New Mexico.

FISHER: In which year?

RICHMOND: I got my master's in 1954, and a Ph.D. in 1958. But through the working arrangement between University of California, which was the prime contractor for Los Alamos, and the University of New Mexico, I actually was able to start employment at Los Alamos in August 1955. So [the] last couple of years when I was working full-time on my dissertation and taking classes at the same time, both in Albuquerque and then Los Alamos, I was working on my research program.

HARRELL: So, you were getting paid while you were doing your dissertation?

RICHMOND: As a student, right. It was fantastic.

HARRELL: Who were some of the people you worked with when you were a student?

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5 predecessor agency to the U.S. Department of Energy and Nuclear Regulatory Commission (NRC); established January 1, 1947

6 radioactive tags on biomolecules, used to study a biological, chemical, or physical system

7 an accelerator in which particles move in spiral paths in a constant magnetic field. The resulting beam of high-speed particles can disintegrate atomic nuclei and produce radioactive isotopes.

8 George Charles von Hevesy (1885–1966), Hungarian-born chemist who won the 1943 Nobel Prize in Chemistry for his discovery of hafnium and his work on the use of isotopes as tracer elements

RICHMOND: As I mentioned, it was a fantastic opportunity. I worked with people like Ernie Anderson,[9] and Ernie worked with Willard Libby,[10] who won a Nobel prize working with potassium-40 and other materials. Libby himself use to say that Ernie Anderson did most of the work. That's [the] kind of third-party stories we used to hear.

Ernie was a fantastic man. He had several careers. He was involved in anthropometry,[11] body composition, and the cell cycle. To me, that's always the mark of a good scientist, where they can leave their mark in several areas, not just one.

I worked with Newton Hayes, an organic chemist, who, with the help of a handful of people, [comprised] the whole starting crew working with liquid scintillators.[12] Also, Ernie Anderson and these people developed the liquid scintillators that were used in whole-body counters,[13] and small counters that grew into many large industries for counting[14] samples with scintillators and radioactive materials.

I worked with Kent Woodward, who was an Army physician assigned to Los Alamos. I worked with people like John Storer, who was, at that time, well-known in radiation biology and went on to an even more distinguished career. People like Ernie Anderson and Newton Hayes; those individuals all ended up getting widespread recognition; I believe Ernie Anderson was an E.O. Lawrence Award winner; I'm not certain of that, but I think so.[15] We had a lot of people working on the cell cycle like Don Petersen,[16] for example, who [is] now retired.

Wright Langham is one I shouldn't forget because he was the group leader in the group where I worked.[17] Wright was sort of an understanding workaholic. By that, I mean that he was a workaholic, but he didn't expect other people to work full-time, like Wright did.

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9 Ernest Carl Anderson was a physical chemist who worked at the University of Chicago Metallurgical Laboratory during the Manhattan Project, 1942–44, and then at Los Alamos Scientific Laboratory. Dr. Anderson received the AEC's E.O. Lawrence Award in 1966. He conducted research in natural radiocarbon, liquid scintillation counters, low-level radioactivity measurements, and cellular biochemistry. He also designed the HUMCO II, an improved version of the first whole-body counter, HUMCO I.

10 an American chemist who researched physical, inorganic, and nuclear chemistry and radiochemistry. He worked at Los Alamos in the late '40s and early '50s and received the Nobel Prize in Chemistry in 1960 for the discovery of radiocarbon dating—the practice of measuring the ratio of radiocarbon to stable carbon to establish the approximate age of an artifact, region, etc.

11 the measurement of the size and proportions of the human body

12 chemical solutions that contain phosphors which ionize when struck by an energetic photon or particle, producing flashes of light

13 apparatuses that measure radionuclides in man using shielded detectors and multichannel energy analyzers

14 counting the rate of radiation emissions, using radiation detection instruments

15 Dr. Anderson received the E.O. Lawrence Award from the Atomic energy Commission in 1966.

16 For the transcript of the November 29, 1994 interview with Petersen, see DOE/EH-0460. *Human Radiation Studies: Remembering the Early Years; Oral History of Cell Biologist Don Francis Petersen, Ph.D.* (August 1995).

17 At Los Alamos National Laboratory in New Mexico, Langham led the Health Division's Radiobiology Group from 1947 until his death in 1972. The group was renamed the Bio-Medical Group.

It was an interesting group; there were physicians, biologists, and there were chemists, mathematicians. Another individual is Payne Harris, who was a physician with a degree in Physics—very sharp, in terms of mathematics—and he was a rare commodity. Payne Harris did a lot of support work for studies at the Nevada Test Site.[18] I worked in the Bio-Medical Research Group. There were other groups who worked in support of field tests in Nevada—nuclear explosions and that sort of thing. That was quite an experience.

HARRELL: Were there tracer studies that you worked on while you were a student at Los Alamos?

RICHMOND: Yes, absolutely. My thesis was on the comparative metabolism of alkaline metals. Alkaline metals are, as you probably know, a group of elements which have similar characteristics in the periodic table.

Let me explain a little, if I may, why these studies were important. For example, sodium. If you think of the element sodium, it's very common in the body, and you have 60 or 70 grams [(2.1 to 2.5 troy ounces)] in the typical person, and 4 or 5 or 6 grams per day come in, and that many go out. That doesn't tell you anything about how an individual population of sodium atoms behave. So when you take and label some of those with a radioactive or other material that will distinguish those atoms from the general population [of sodium atoms], then you can trace that particular cohort. So, in the case of sodium, you know the material will leave the body at different rates, not just one rate, and it will go to different locations [in the body].

The main interest was in the alkaline metals in particular, and that was a thrust of my thesis. That was reported in a Los Alamos document. But then, many, many years later, it was selected to appear in a *Health Physics Journal* as one of the 20 articles or so for a 100-year period that were considered to be, the word isn't "classic," but "particularly interesting"—something like that.

FISHER: That was on the 25th anniversary of the [Health Physics] Society's special publication of historical papers.

RICHMOND: You're right, and I listed it in the materials I sent you.

FISHER: That was your thesis?

RICHMOND: That was my thesis, right. So, it got published immediately as a Los Alamos document, and then, 25 years later, as sort of a classic.

FISHER: This thesis described the metabolism of which radionuclides?

RICHMOND: The main intent was in cesium because of fallout [from nuclear weapons tests in the atmosphere]. Cesium was detected [in people]. You see, as I mentioned, Los Alamos was at the forefront of detecting materials for use in detectors, for developing materials that are used in detectors. At

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[18] the location where most nuclear weapon tests within the Continental United States were conducted

that time, the major interest was in liquid scintillators, and later in solid crystals [such as iodine].

Around the early 1950s or so, or late '50s, in that period, it was determined that you could actually measure cesium within the body by placing a person in a liquid scintillation counter. Of course, separated from [the] liquid that the gamma rays would interact as they leave the body, would go in and interact with the [scintillation] fluid, cause a flash of light, and that light could be detected by a photo[multiplier] tube.

Cesium was tops on our priority, and sodium of course, because it was in the alkaline metals. We worked with sodium-22, mainly; sodium-24; [and] rubidium. Francium is in the alkaline metals but there's no usable radioactive tracer of it. The purpose of those studies was to try to predict what would happen in people, because protection was really a major goal of all that work at Los Alamos at the Bio-Med group.[19]

What [do] I feel particularly good about? Two things. A lot of that work was published. Some of it is still cited in physiological literature; comparative work done on tritium, for example. So even though protection was the main goal, radiation protection, and protection of the people in the business and in the laboratories, a lot of that information turned out to be extremely useful in terms of the literature on the biology and physiology of tracers.

Again, I was pretty young at that time, and I didn't realize [the importance of the work]. In my mind, the major goal was to do things that would help develop a better understanding and better guides for radiation protection.

What we were really interested in was doing work with small animals, mice, rats in particular, dogs, and monkeys in some cases, and understanding the metabolic kinetics[20] so that we could predict, on the basis of some model, what would happen in people. By understanding the kinetics of how the materials were turned over,[21] you can predict equilibrium levels.

But that in itself led to a whole group of studies. Karl Morgan[22] in particular was interested in that, and Walter Snyder in Oak Ridge [was interested for purposes of better internal dose calculations]. They did not have an experimental setup. There was a group in England, as I recall, and our small group of people, [who] were very much interested in trying to show experimentally that you could, indeed, predict the equilibrium level that [a] radioisotope would reach in the body based on a single administration. You can administer the radioactive material once, do the long-term study, and construct from that data what the buildup

[19] The Health Division's Group H-4, Radiobiology; renamed Bio-Medical Group in 1974

[20] the rates of metabolism of elements, and their metabolic pathways

[21] cleared from the body and were replaced by new intakes of the same materials

[22] For the transcript of the interview with Morgan, see DOE/EH-0475, *Human Radiation Studies: Remembering the Early Years: Oral History of Health Physicist Karl Z. Morgan, Ph.D.* (June 1995).

would be. So, we did experiments primarily on cesium in small animals to do that sort of work, as well.

The Los Alamos report, then, was my thesis at the time, and it had the usual committee [reviewing it]: academic[s] at the University of New Mexico and some people from the Laboratory. It was a fantastic time, actually.

HARRELL: Were you part of an AEC scholarship program?

RICHMOND: Yes, it was—I don't remember the name of it—but it was done throughout the Atomic Energy Commission. I was about the third or fourth person to be involved in the one in Los Alamos. Turns out, in later years, when I moved here to Oak Ridge—Herman Postman was the Director at the time and I was an Associate Director—several of us used to tell students that we started our careers as students through the Atomic Energy Commission Programs. Work at Los Alamos was fantastic, because there were all kinds of people around [as] human resources [and] equipment that was not really readily available in most schools [and] universities. A lot of that work, not just my work but [that of] others, was done collaboratively with people at the University of New Mexico in Albuquerque. They had very good interactions, even back in those days.

HARRELL: Not much interaction with people back in California, with the [future National] Labs [at Berkeley, San Francisco, and Livermore]?

RICHMOND: No, very little. Los Alamos has been very independent in terms of the contractor relationship over the years.

HARRELL: So, when did you become employed full-time?

RICHMOND: When I got my [Ph.D.] degree in 1958, I had several job interviews; one at Purdue [University in Lafayette, Indiana], one at Walter Reed [Medical Center in Washington, D.C.]. Anyway, I was making a circuit, [interviewing around the country], and Wright Langham called me [from Los Alamos] and said, "What are you doing?" I said, "I'm looking for a job." He said, "Don't [you] want to work for me?" I said, "You never asked me." So he said, "Oh, I just assumed you knew you were going to work with me." That's how I got to work there.

Oak Ridge was on the list, too. Walter Snyder and Karl Morgan were trying to get me to come to Oak Ridge, then and later. So, I started my full-time formal employment after I got my [doctoral] degree in 1958.

Research on Metabolic Kinetics of Radionuclides

HARRELL: Did you move into the right into the same section [(Metabolism[23] and Dosimetry[24])] you had been in as a student?

RICHMOND: Yes, I [was] working with other people in that general area. Other people were heavily involved in studies with tritium,[25] for example. A fellow named Ted Trujillo, who was a chemist who did a lot of work with tritium, and John Storer were working with tritium. It was very common for people in the Laboratory to volunteer to help someone else in an experiment. I know I, myself, have ingested one or two millicuries,[26] not microcuries,[27] of tritium. Tritium is turned over very rapidly in the body, and there was that camaraderie of researchers who helped each other in these studies.

HARRELL: Did you do much of that when you were a student, or was that after, when you became an employee?

RICHMOND: Most of it was after I became an employee.

HARRELL: There were some students who were volunteers in experiments?

RICHMOND: As a student, I was involved. Cesium in particular was very important, in terms of gaining knowledge about it. It was my major interest also, so I ingested cesium [on] several occasions. Part of that was for my thesis, actually. We had medical students [from the University of New Mexico in Albuquerque] who would come and work in the laboratory occasionally. Most of those students were sort of too busy to become involved. We never coerced them; we offered them the opportunity. Medical students in particular have a history of wanting to get involved in experiment situations, strictly volunteering. There were several dozen, I don't remember exactly, three dozen or so people in that organization, maybe even more. It was called H-4 at the time, and Wright Langham was the leader. I worked in the Metabolism and Dosimetry [section]. Many years later I ended up running that organization, but it was many years later.

HARRELL: So the attitude back then was not to think much [of] the dangers of ingestions of these elements?

RICHMOND: Well, we obviously did give it enough thought. For example, in cesium— well, from almost any radioactive material—there were guides for workers. The uncertainty of those were, in some cases, very large. In many instances, we would end up, on the basis of small-animal experiments, having a pretty good idea of what would happen in people. To

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[23] the rate at which chemical processes take place in the body

[24] the process or method of measuring or calculating the dose of ionizing radiation, or energy absorbed per unit mass, using data from bioassay and other radiation measurements

[25] a radioactive isotope of hydrogen having an atomic weight of three; the heaviest isotope of the element hydrogen, tritium gas is used in modern nuclear weapons.

[26] thousandths of a curie; one thousand microcuries. A curie represents 37 billion radioactive decays per second.

[27] a millionth of a curie

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verify this in some cases, we would, ourselves—two or three people—ingest the material and do the experiment actually on ourselves. We felt very comfortable, because of the interspecies correlations we were developing; but at the same time, there were many people out in the field, in the Laboratories, who were using these materials, and there were guidelines—something called "maximum permissible concentrations." The irony was, in some cases, the amounts of materials that we took were well [below] the guidelines that were being applied at the time, in terms of the allowable amount to be taken in.

FISHER: To workers?

RICHMOND: Yes. So that was sort of the good news, that we felt very comfortable about it. You know, when you study a material thoroughly in mammals of different sizes and metabolic rates, generally you can get a pretty good feel for what's going to happen in a person.

Cesium, it turns out, is probably the single most important radionuclide, in terms of potential damage to man. That surprises a lot of people, because they think of plutonium right away. If you look at Chernobyl,[28] and look at the accidents in many other countries, like Mexico, through Central and South America, and Europe, there were many cases where cesium was used for medical purposes—literally, it ended up in a dump and [subsequently] the people got exposed;[29] nuclear fallout [from atmospheric weapons tests]: the prime bad actor in [all those cases,] has been cesium. In terms of the number of people who have been touched by radiocesium, and things of that nature, it is probably the most important [of radionuclides].

FISHER: Cesium is a problem for those who ingest meat of animals that have grazed off grasses after fallout. What were some of things that you learned in you dissertation work on metabolism of cesium?

RICHMOND: While we were trying to understand the metabolic kinetics, we were doing other experiments, because we had to concentrate on a thesis problem, and that was the kinetics [issue], for my dissertation. What we learned, number one, [was] that you can, indeed, predict the equilibrium level in people from a single administration, by injection or oral [route], because cesium is almost 100 percent absorbed in the GI [(gastrointestinal)] tract. So, we did confirm our beliefs that you could predict equilibrium for chronic administration without doing the experiment, by using a single administration and looking at the mathematics.

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[28] a Ukrainian city in which a Soviet-designed graphite-moderated nuclear reactor in April 1986 sustained the world's worst radiation accident to date. At the reactor site, 31 workers and firefighters were killed during the accident and immediately following. According to contemporary Soviet assessments, 1,000 square kilometers (370 square miles) of land were contaminated, 135,000 people and 86,000 head of cattle had to be evacuated, and fallout spread to 20 countries. An international effort to aid the victims and contain radioactivity at the site ensued, including sharing of technology and research.

[29] A mass-exposure accident involving cesium waste in Brazil is discussed later, under "Career Accomplishments."

HARRELL: Through animal experiments?

RICHMOND: Through animal experiments. That was very important, because it turns out you could do that from many materials—not all, but many. It was very exciting to be able to measure the so-called biological half-[time][30] of cesium in people, although a small sample. [The half-time] was uncertain, and it turned out, in the first ones we did—I think, four people—[turned out to be] between 80 and 120 days.

After that, a lot of people in England and the U.S. and other countries made the same measurements. Over the years we became pretty comfortable knowing, and having a pretty good accuracy [in establishing], [a] range of values for [cesium's half-time in] people. But we knew it's different with children. We know it's different under many conditions. Verification of the equilibrium level for chronic administration [is important], and one of our papers has all the mathematics in it. It gets very detail[ed] if you try do that.

HARRELL: Did you ever have an example of a human who had reached the equilibrium level through a massive dose of some accidental kind?

RICHMOND: It turns out we had a lot, but it was through nuclear fallout, because [for] fallout you get it incrementally. So we were able, actually, to get a relationship between—and it's very involved, mathematically, and again, it's in one of our papers—how the material would build up in people through a chronic exposure but [specifically under conditions,] where that level was not the same everyday, but variable. And then, [the buildup] stopped, which was done twice, mainly in 1964, during the Limited Nuclear Test Ban [Treaty]:[31] there was no more cesium being added [to the atmosphere]. But it was still turning over in the body. So, we were able to predict what the turnover would be in people, after they had a continued variable source of intake. And that we've also published.

Accelerating the Turnover of Cesium With Prussian Blue (Circa 1960)

FISHER: What did you learn about the excretion of cesium, its excretion rates and [clearance] pathways?

RICHMOND: That's an interesting question. We were able to determine [that] there were two primary excretion rates, rates of change in the body. [One was a] very short one, which accounted for maybe 10 or 15 percent of the

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[30] biological half-time—the time required for half the atoms to clear the organ or tissue as the result of normal physiological and metabolic processes

[31] Signed in 1963, ratified in 1964, and still in effect, the Limited Test Ban Treaty (LTBT) commits the United States and Russia (formerly the Soviet Union) to refrain from testing nuclear weapons in the atmosphere, under water, or in space, thus moving nuclear testing underground. The United Kingdom also acceded to the LTBT. The LTBT put an end to additions to nuclear fallout from U.S., Soviet and Russian, and British nuclear tests except in those rare cases when an underground nuclear test accidentally vents to the atmosphere. Prior to negotiation of the LTBT, an atmospheric testing moratorium was observed by the U.S. and the Soviet Union until it was broken by the Soviets. This moratorium may be the first of the two periods to which Dr. Richmond refers when the buildup of fallout-borne cesium was halted.

population of atoms that had a very short, several-day half-[time]. But then, most of it had a very long half-[time]. For some elements, that's extremely important: sodium, for example. Traditionally, in the literature, even before isotopes were used, everyone thought that sodium turned over with a 10- or 11-day half[-time], which is true. But, if you measure long enough in time, we found (and this is corroborated by others) a very long half-[time]. A small amount, but a very long half-[time]. But knowing the amount and the turnover time, you can calculate the pool size.[32]

So it turns out we were among the—maybe *the* first; I'm not certain—*among* the first, at least, to show that there was a long-term, slow-turnover component of sodium in bone. That was published either in nutritional literature or the physiological literature.

Cesium is a very exciting element to work with. Of course, at the time I didn't have any idea. The interest was in fallout. This was before the widespread application when we got involved in the accidents, having these [cesium] sources end up [in] local dumps, being burned. Places like Chernobyl.

You mentioned the animals that were fed materials to get rid of cesium. There was a group in Norway and our group in Los Alamos who worked with a compound called ferric ferrocyanide. [The] common name was Prussian blue.[33] It was used as a dye early on, in histology[34] and other [applications].

Well, it turns out that we did some experiments, again with small animals. We fed them this dye after they [had] been given cesium for [a] while. They were given cesium—it was chronic—to see if that material would bind cesium. The use earlier had been in the chemical process [that] said, "Yes, this stuff binds cesium." Our argument was [that] if you ingest cesium, it's almost 100 percent absorbed; there's a flux[35] where it goes back and forth through the gut wall, also. So, if this insoluble dye was going through the GI tract and bound up the cesium atoms, well, that would take them out of the body. Well, it turns out, that did work.

I actually did work on myself; it's a blue dye, so [the] stools turned blue. We found that you can very, very effectively change the turnover rate from maybe 100 days for [a] long compartment [(body organ)] to about 30 or 40 days. In that experiment, after I ingested the cesium and waited a while until we were sure we were on a long-turnover component, I would [then] ingest the ferric ferrocyanide; eat it for a couple of days. [Next,] by measuring myself in a whole-body counter, [I could observe the] turnover rate would get down to about 40 days. I would stop the ingestion of the dye. Then I'd go back immediately to [something] like

32 the original quantity of cesium atoms in the subject's body

33 a chemical material used to enhance the excretion of cesium

34 the study of the structure of tissue

35 a quantity expressing the strength of a field of force in a given area

100 to 120 days [half-time].[36] So, we did the this seesaw experiment with dogs, primarily, before we did it with ourselves. And then, the group in Norway was doing work with that, also.

But it turns out that [for] ferric ferrocyanide, there's a pharmaceutical[37] material in Germany called Radiogarde. I've seen it in reactors, for example in Brazil: in the reactor in case of an accident [involving radiation], workers can ingest it. It's never been approved for use in the United States. After the Chernobyl accident, as you mentioned, it was given to people and animals, [because of the] commercial aspect for the animals to rid the cesium faster.[38] So that work was done at Los Alamos. At the same time, there were people interested in working with compounds to accelerate the turnover of other radioactive materials, including plutonium. DTPA[39] [and] materials like that were being studied.

FISHER: Is Prussian blue still the most effective antidote for ingestion of cesium?

RICHMOND: Yes, for accelerating the turnover of cesium, Prussian blue is the most effective.

FISHER: What about for cesium which is distributed throughout the whole body? Is there any mechanism for enhancing the clearance of cesium [deposited in tissues]?

RICHMOND: Yes, actually, ferrocyanide [(Prussian blue)] will do that. Because even though the long-term retention component is 100 to 120 days, that doesn't mean that it's sitting in muscle, for example, all that time and periodically losing some of the atoms to give you that 120-day turnover time. The flux is very complicated. So those [cesium] atoms are getting out into the gut and back in, during that time when you have this real nice single exponential.[40] But, every time one of the atoms goes out into the gut—and think of the gut as a hole in a donut—if you entrap it there, it can't get back in. So, that's one of [the] nice things [about] ferrocyanide: the efficiency of treatment is proportional to the [length of] time [over which] you administer it.

HARRELL: So, you have to keep taking this antidote to keep clear of everything that comes into the gut?

RICHMOND: Exactly. Your treatment—think of it this way: You have a single pill that you take, and it's affected by materials that it sees in the gut, coming from the body. Well, if you can take that pill and subdivide it into smaller and smaller pieces and take that during the day many times, there are many more opportunities for that [binding] to happen.

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36 the time required for half of the atoms present in a compartment of the body to leave that compartment by normal biological processes, such as metabolism or excretion

37 relating to drugs approved for human use

38 If a livestock animal could rid itself of the cesium quickly, the odds improved that it would not have to be killed to prevent humans from ingesting contaminated meat, eggs, or milk.

39 diethylene triamine pentaacetic acid

40 The behavior can be explained by a simple, first-order mathematical formula

FISHER: What year was this when you did this studies on Prussian blue, approximately?

RICHMOND: Around 1960.

Use of the Whole-Body Counter for Fallout Studies

FISHER: How much time did you spend in the whole-body counter?

RICHMOND: A lot. One: the counters were being developed at Los Alamos. Two: we were very much interested in cesium from fallout in people. So one of the studies that Ernie Anderson and I were responsible for was to measure the cesium-137 in people from fallout. So we had a select population of people from Los Alamos, primarily people in the Lab—not just our H-4 group, but from other parts of the Lab. We would count those people weekly or monthly and plot the amount of cesium in the body as a function of time. It went way up, as you probably know, prior to the 1964 test ban. So that involved a lot [of] materials that we took ourselves: zinc-65; some sodium[-24]; or cesium[-137], for example, we used. Fortunately, for the alkaline metals we were able to use gamma emitters. Gamma emitters are easy to measure with the liquid [scintillation] counter. The liquid counters were large tubes, actually. But you had [a] sling and pulled the person into the center of this long tube.

FISHER: Did you do your own dose calculations on these radionuclides before the experiments?

RICHMOND: Yes.

HARRELL: Or did you have another physicist to help with the dosimetry?

RICHMOND: Well, that was part of my education, to learn how to do that, but obviously I got a lot of help.

HARRELL: Who taught you this?

RICHMOND: Ernie Anderson was a key guy. Langham was involved. And a man [named] Joe Sayeg, who was sort of the key dosimetrist for that group. He's now at the University of Kentucky.

HARRELL: Could you spell that name?

RICHMOND: S-A-Y-E-G. Joseph Sayeg.

FISHER: He was the photographer then for the Health Physics Society.

RICHMOND: Yes, right. He used to tutor me and help me a lot with on the mathematics.

FISHER: I know him very well.

RICHMOND: There were a lot of questions about whether materials would follow exponential rules, or something called the power function [$(y=ax^n)$]. So we did a lot of the mathematics.

FISHER: Even though the doses were considered to be low, what were the doses that [you] allowed yourself to ingest? In terms of absorbed dose, did you set for yourself any maximums?

RICHMOND: No, these were all within the allowable guides at the times, so we didn't worry about that. That was done, and there was obvious interest in the material itself: "How [should] you put the radioactive material in the thing you were going to drink?" George Voelz was [the] physician at the time, and he used to be involved in helping us with the protocol.[41]

Physicians Customarily Attended During Intake of Radionuclides

FISHER: Was there any time when you took a radioactive material not administered by a physician, or was there always a physician [present] for these intakes?

RICHMOND: As far as I remember, there was always one [present]. [Such as] Tom Shipman, who head the Health Division [and served as the Lab's Medical Director], or Dr. Lushbaugh. Now, Lushbaugh was in that group, also H-4, and he was doing work related to patients in the hospital, leukemia-related studies. So Lush was sort of the resident physician. There were several others involved in H-4 who were physicians: Irene Boone, and Harry Foreman. In fact, there [were] quite a few physicians. Harry Foreman was the man who did most of the work with chelating agents[42] to try to accelerate the loss of plutonium.

FISHER: So in other words, was there a Lab policy, or just an understood policy, that you wouldn't ingest or receive an injection, of course, without this being administered by a physician?

RICHMOND: I don't think there was any written policy.

FISHER: But it was understood?

RICHMOND: Yes, it was understood.

FISHER: There had to be a physician.

RICHMOND: In fact, the technicians who worked with me, they were also medical technicians, for example. Since they were not physicians, they couldn't inject people. All [injections] had to be done by licensed physicians.

HARRELL: In your experience, do you think there was more experimentation on the scientists themselves done at Los Alamos than at the other Labs?

RICHMOND: Probably true, probably true.

HARRELL: Why do you think that was: just the people that were there?

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41 For the transcript of the interview with Voelz, see DOE/EH-0454, *Human Radiation Studies: Remembering the Early Years: Oral History of Dr. George Voelz, M.D.* (May 1995).

42 chemical agents that remove heavy metals from the bloodstream and soft tissues

RICHMOND: Well, I think several things. One, there was an interesting grouping of physicians, mathematicians, physicists, biologists. Two, the building was right next to a hospital, so that naturally you had more physicians around. Three, that's where the development was taking place, in terms of detectors, scintillators, [and] whole-body counters. They graduated from liquid to crystals to even more sophisticated [binds] into the modern counters [used] in medicine today, actually. Later on, I think, some of the Government Laboratories had hands-on, physician–patient interactions. ORINS[43] here in Oak Ridge, Brookhaven,[44] for example. There were at least those two, maybe one in the Chicago area,[45] but they are all gone now.

FISHER: It would be interesting for you to describe some of the work done by these people: Ernie Anderson, Wright Langham, maybe going back to Harry Foreman. He was, as you say, interested in EDTA[46] as a chelating agent for plutonium. And he also did some studies of carbon-14–labeled EDTA to understand the metabolism of just the salt itself. Can you remember some of those studies?

RICHMOND: Yes. In fact, there were several people involved in that. Newt Hayes, as I mentioned earlier, was an organic chemist. He and Harry had some common interests.

Newt would get some materials called terpenes; that's a chemical that you can extract from plants. Newt was looking at the ^{14}C-labeled terpenes, using ^{14}C [(carbon-14)][47] to measure certain things about terpenes as an indicator of carbon-14 in nuclear fallout—in terms of how it built up in different [carbon] reservoirs in different plants. ^{14}C was used by quite a few people, in dating for example.

Ernie Anderson had an interest in using different radionuclides for dating. Harry, as you mentioned, was interested in using ^{14}C and other labels for tracing things like DTPA and EDTA. Irene Boone was a physician using ^{14}C labels with isoniazid[48] [and] some other compounds used in medicine.

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43 Oak Ridge Institute of Nuclear Studies, established in 1946 by the Manhattan Engineer District and operated under a Manhattan Project (and later Atomic Energy Commission) contract. ORINS was responsible for training physicians and researchers in the safe handling of radioisotopes and in the development of isotope applications in medicine. In addition, ORINS was responsible for selecting both students and established scientists for fellowships and other temporary research assignments. Today, the educational and training functions of ORINS are carried out by its successor, Oak Ridge Institute for Science and Education (ORISE).

44 Brookhaven National Laboratory (Long Island, New York)

45 Argonne Cancer Research Hospital, one of three clinical facilities created by the AEC in 1948. While the AEC owned the 58-bed Chicago hospital, the University of Chicago medical school administered and staffed the facility. The hospital admitted its first patient in January 1953.The Energy Research and Development Administration terminated Government support for Argonne and the other AEC-created research hospitals in 1974, three years after the hospital's name was changed to the Franklin McLean Institute. The facilities are now used by the university's medical school for studies in radiology and hematology.

46 ethylene diamine tetraacetic acid

47 a radioactive isotope of carbon having a half-life of about 5,730 years: widely used in the dating of organic materials; also called *radiocarbon*

48 a water-soluble solid compound used to treat tuberculosis

Some of the military folks th[illegible] ere there—there were usually one or two physicians from the milita[illegible] Kent Woodward was one—they were also using ^{14}C and other tags, for different reasons. And again, the reason for that is that the technology for measuring small amounts [of radionuclides] was being developed and evolving about the same time in that Laboratory, in that complex.

Follow-Up With GIs Exposed to Radiation in the Manhattan Project (Ongoing)

FISHER: You haven't mentioned [that] you worked a lot with Louis Hempelmann.[49]

RICHMOND: Yes, that was later on, toward the last of my service at Los Alamos. Louis had been at Rochester, and Louis was very much interested in the study that Wright and Louis started on following up GIs,[50] a couple of dozen military folks, who worked on the first bomb in Los Alamos.

Hempelmann was a very interesting guy. Early on, he was involved in the Manhattan District,[51] in the medical parts, and he did a lot of the tromping around down at the Trinity[52] site, trying to find people who might have been exposed inadvertently in the fallout. Anyway, my main involvement was as a coauthor on papers to follow the health of a group of a couple dozen, roughly, GIs who [had] worked with very high levels of plutonium, under pretty primitive conditions, when they were racing to build a bomb. You probably know that history. They wanted to get this thing done and tested before the Potsdam meeting[53] and things like that.

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[49] Hempelmann was a group leader in the Health Division at Los Alamos Scientific Laboratory from 1943 to 1947, and led the division from 1946 to 1948. An expert in radiology and radiobiology, Hempelmann served in the Atomic Energy Commission from 1948 to 1950, then joined the faculty of the University of Rochester before coming to Los Alamos.

[50] a contraction of Government Issue that emerged during World War II as an American colloquial term for any member of the U.S. armed forces, but particularly enlisted persons

[51] Manhattan Engineer District, an organization created by the U.S. Army Corps of Engineers to administer the development of the atomic bomb under the top-secret Manhattan Project

[52] the operation name for the first test detonation of a nuclear weapon conducted at Alamogordo Bombing Range in New Mexico, July 16, 1945

[53] a "Big Three" meeting of the United States, the United Kingdom, and the Soviet Union, represented respectively by President Harry Truman, Prime Minister Winston Churchill, and Generalissimo Joseph Stalin, in a suburb of Berlin, Germany, July 17–31, 1945. With the western allies increasingly concerned over Soviet actions, intentions, and military position in Europe, the Potsdam Conference was a critical forum for discussing many issues facing postwar Europe. But another important objective of Truman and Churchill was to persuade the Soviets to enter the war with Japan. Out of respect, Stalin needed to be informed that the United States had the atomic bomb before the weapon's existence became known to the world through its actual use in the war, and Truman did so on July 24, after the successful Trinity test. Dr. Richmond's comment implies that a successful test of the atomic bomb in advance of the Potsdam Conference was an objective of the Manhattan Project. After the conference, Truman told Ambassador Joseph Davies that an unstated reason for telling Churchill that he could not leave Washington for a heads-of-state conference until after June 30 was that July would be the month in which the atomic bomb would be tested. Truman biographer Robert Donovan concluded from this, "Evidently, Truman and his advisers believed that a successful test would strengthen the United States position in Potsdam" (Donovan, p. 72). Source: Robert J. Donovan; *Conflict*

(continued...)

Langham was key in getting that started. Louie Hempelmann was the physician, and [later] George Voelz kind of inherited the responsibility [for the follow-up studies]. He still has it, I guess, even though he has retired. Hempelmann had moved to Los Alamos—I'm not sure when—from Rochester. When I was there, I got involved in a 25-year follow-up or something like that. Periodically, these survivors are seen by their own physicians, and there's a protocol for their health studies. And then, every once in a while, [every] three [or] five years, they all congregate at Los Alamos for very thorough measurements. The idea here is to try to find out as much as you can about the potential impact, adverse effects of plutonium on those folks who were known to get high exposures.

HARRELL: So, these 25 or 26 men were happy to participate in this program?

RICHMOND: Yes, in fact I understand, I don't know how many are left, but they still get together periodically.

FISHER: There was a report in the December [1994] issue of *Health Physics*, an even later follow-up of this same worker population.

RICHMOND: It has been published five or six times, I guess, over the years. The risk is [that] if that happen[s], in today's world, if we had a couple of dozen people exposed to an almost unknown agent, except [that] you knew it could cause problems based on animals or similar materials in man, that kind of study would never be done because people would say, "You cannot prove epidemiologically[54] whether or not, if you see a bone or lung cancer later on, that was due to chance or whatever."

Now that's true. But, what is also true is the uniqueness at that time. Here was a handful of people who were exposed, and you knew they were exposed to fairly high levels compared to what you think they should be exposed to, based on animal experiments. The question at the time was, "What do you do? Do you study them or don't you?"

Retrospectively, after all these years, all those fuzzy things are going to come out, because there is at least one bone cancer and lung cancer [among the study population]. And the statistics say, "All right, that's what you'd expect." I'm not up on that last study, so I don't know where they are exactly. But, on the other hand, I think today the pressures would be so strong [that the prevailing sentiment would be, "A study] may or may not show something, so why bother?" That's the wrong attitude. [When] you have unique agents [(factors contributing to exposure)], you have to take advantage of those human populations [who were exposed].

FISHER: There may never be the same opportunity again.

RICHMOND: Exactly.

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53 (...continued)
and Crisis: The Presidency of Harry S. Truman 1945–1948; W.W. Norton; New York; 1977.

54 involving the statistics of incidence and prevalence of disease in large populations; *also:* the factors contributing to the presence of absence of a disease

Declassifying the Los Alamos Report on Plutonium Injections (1971)

FISHER: In conjunction with the original plutonium-injection cases, it's been said that Wright Langham was very reluctant to discuss this after the 1940s. You came to Los Alamos sometime in the 1950s. Did you ever have discussions with your group leader, Wright Langham, about the 18 people injected with plutonium in the 1945-to-1946 era?

RICHMOND: No, that was never really [a] point of discussion. What was interesting—it involved *LA-1151*[55]—was the document that was classified for many years. We would continually take that data and feed in all sorts of additional data and try to refine the excretion curve.[56] A lot of us were interested in that.

HARRELL: *-1151* was the Rochester report?

RICHMOND: No, Los Alamos, by Langham et al. That's the one that was classified. But in terms of the early history of "who did what" and "who authorized what" and "where were they done," that was never really kicked around, and I don't understand why. I guess my feeling was, "Here is a set of data."

In fact, right now, one of the key issues is whether or not these injectees were or were not expected to have long or short survival times. What everyone said and wrote at the time—Wright and others—was that they were expected to have short lifetimes. *Now*, that's under contention. And the argument is—this *New York Times* argument and all—that not only were they *not* told what was happening, but they, indeed, were more healthy than Langham [had indicated]. And others said, "If you just look at the literature, it's clear that everyone who wrote on it would refer back to the early statements and documents and say they were expected to have short survival times."

FISHER: Less than 10 years. You've written extensively on the plutonium injection cases; I've read some of your articles on this.

RICHMOND: The reason for that is I was on loan to the Atomic Energy Commission in 1968 for about three years. During that time, Pat Durbin[57] got very much interested in the plutonium data and revisiting it. And while I was on loan, she asked me to try to get some help in getting *[LA-]1151* declassified. It took a while, but we did. So, the last declassification step [was] from *LA-1151* OUO, [meaning] Official Use Only, to no classification. That was really done through Pat Durbin's pushing, with my help when I was on loan from Los Alamos to the Atomic Energy Commission. And that happened in 1971, I believe, the final declassification.

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[55] the document number of the Los Alamos report on results of research involving injection of plutonium into human subjects

[56] the mathematical formula that, when plotted, would graphically show the rate, over time, at which plutonium was being excreted by the subjects

[57] For the transcript of the November, 11, 1994 interview with Durbin, see DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years: Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (June 1995).

The reason for that is, Pat was digging into that literature, and people had been referring to bits and pieces of that for years. Wright was referring to *-1151* in the literature and the English were refining the [plutonium turnover] curves. Pat's argument was, "Why don't we get that key document out, where everybody can see all the details, instead of writing to Wright Langham?" and things like that. That declassification was, as I recall, in May 1971. I'd have to go back and confirm that, but it happened just after I spent my couple of years there. I actually ended up working at AEC for two of those three years.

On Assignment to AEC Headquarters in Washington, D.C. (1969–71)

FISHER: Who did you work with and for at the AEC during this period?

RICHMOND: Mainly Dave [(H.D.)] Bruner, who was a physician. [Also] a man named Joe Goldstein, who was a physician. He was mainly involved in nuclear-medicine–type activities.

He was involved with the external radiation studies of the prisoners up in the northwest; [Carl G.] Heller, et al.[58]

FISHER: Which one, Goldstein?

RICHMOND: Joe Goldstein, when I was there, he was at...?

HARRELL: He oversaw those programs?

RICHMOND: At that time. He was the contact in Washington in the AEC.

FISHER: You worked under John Totter,[59] also?

RICHMOND: Totter toward the end. Earlier on, Chuck Dunham[, his immediate predecessor as head of the AEC's Division of Biology and Medicine]. In fact, Dunham asked me, when I was at Los Alamos, to come back to Washington. I remember because Wright was still alive and said, "Oh, you want to be a bureaucrat."

FISHER: *(smiling)* The end of a good scientific career.

RICHMOND: There were three or four vets [(veterinarians)] involved with that group I was in. Ed Still, Frank Brooks, Roger McClellan had been up there earlier. So there was a mix of mainly physicians and veterinarians.

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[58] From August 1963 to May 1971, the Pacific Northwest Research Foundation in Seattle, Washington, used inmates at the Oregon State Prison in Salem to determine the effects of ionizing radiation on sperm production and to determine minimum dose levels for initial effect and permanent damage. Sixty-seven healthy volunteers ranging in age from 24 to 52 years were irradiated by x rays one or more times. Testicular absorbed doses ranged from 8 to 640 rads. Subjects were compensated for their participation and for each biopsy. All subjects who had not been previously vasectomized agreed to undergo a vasectomy at the conclusion of the study. All did so, receiving additional compensation. For details and a list of references, see OT-21, "Testicular Irradiation of Oregon State Prison Inmates," in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (213 pages), DOE/EH-0491, July 1995.

[59] Totter headed the AEC's Division of Biology and Medicine from 1967 to 1972. For the transcript of the January 23, 1995 interview with Totter, see DOE/EH-0481, *Human Radiation Studies: Remembering the Early Years; Oral History of Biochemist John Randolph Totter, Ph.D.* (September 1995).

HARRELL: What did you do when you got to Washington?

RICHMOND: That's interesting. I was kind of in awe. I went to work for this big Federal agency—John Storer was there, by the way, at the time. The first thing they told me was, "Here is what we want you to do." I saw this list of Hanford,[60] the Albuquerque laboratory [named] ITRI,[61] Salt Lake City's University of Utah, U[niversity of] C[alifornia at] Davis. I guess they're the main ones, and Argonne [National Laboratory]. [To] kind of worry about these animal studies. And my first reaction was, "What are all you guys going to be doing?" And [John Storer] said [basically], "Obviously there has to be a second round of large-animal studies for plutonium, and I want you to go to these laboratories and do an assessment, and come back and tell us [which lab] we ought to put them [in]."

I remember, because I went first to [the University of] Rochester [in Rochester, New York]. They were an interesting group. They had gotten off into more basic cellular biology. They basically said, "We don't want to do that scutt work." To make a long story short, we focused on two, finally; the Albuquerque lab [(ITRI)] and Battelle [Pacific Northwest Laboratory in Richland]. Our assessment, the one I gave back [to AEC], was that the Albuquerque lab was so involved with their original mission on fission product [toxicology], a combination of fission products,[62] which I don't think they ever got around to really, except indirectly. We decided that Battelle should be the site for a follow-on study, so-called "low-dose plutonium." That was my first assignment: to make th[at] recommendation.

FISHER: Part of the reason for that second study was that the first studies were conducted at high levels, and a lot of information was missing at the very-low-intake levels. The work at Battelle was primarily inhalation [toxicology], and it was felt that there was too little information on the metabolism of inhaled plutonium.

RICHMOND: These are, like, 16- to 18-year commitments.

FISHER: [Yes, because the studies would have to consider] different chemical forms of plutonium: the insoluble plutonium-238, -239; and then the soluble plutonium nitrate. It's interesting that you were responsible for getting that work started at Hanford. For me, that's interesting because I've had a chance to do some analyses on those dogs who were involved in that study. That's very, very, interesting.

HARRELL: Did you design the protocols or the types of materials to be studied?

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[60] the DOE's 570-square-mile former site for plutonium production, located near Richland, Washington

[61] Lovelace Inhalation Toxicology Research Institute outside of Albuquerque, New Mexico (along one part of the Kirtland Air Force Base)

[62] products such as the elements strontium and cesium that are formed during the splitting of uranium atoms in a nuclear reactor

RICHMOND: No, that was basically the responsibility of the Hanford people. We were involved in reviewing it, and all. But, we didn't design it and say, "Here, go do it."

The other major activity I had was that before I went there to AEC, there was a lot of uncertainly about the so-called nonuniform dose distribution, "hot particles." We were trying to get a better hold on that.

Study of Plutonium-238 Exposure With Animals (Early '70s)

FISHER: You might give some introduction as to why the controversy developed, where it came from.

RICHMOND: I guess one of the big drivers was the use of plutonium-238 in radioisotope thermoelectric generators for space applications. As you may know, there's a bunch [of plutonium-238] on the moon that was left there. There's some in space vehicles and there's one in the Tonga Trench [in the Tonga Islands of the South Pacific] that came back with Apollo 13, plus the [plutonium left on the moon by] astronauts on the lunar module, the landing module. Half-life is a lot shorter—it's about 87 years, as, I remember, compared to the 24,000-plus years for plutonium-239.[63] What happens is that it disintegrates relatively rapidly, and the concern was the spallation[64] [from] the surface of other materials; alpha emitters [65] and plutonium-238. Plus the fact—and this [concern] spreads over to plutonium-239—if you have any applications where you might have small particles, even very, very, small particles, any exposure to people would have a different pattern compared to where you have [only] individual atoms.

So the question was, is the radiation dose more effective or less effective for a dose, a given quantity, a given weight, whether you have it evenly distributed in many, many, tiny pieces or the other [case], if you imagine it coming, aggregating, now you have one piece. So we did at lot of the original work in Los Alamos using rodents, where we would [inject] extremely small particles, which were still horrendously large for radiation dose. These were very small, ten microns,[66] in some cases. We would inject them into the tail vein, and these would circulate. [We'd] inject them in different places, where they could lodge in the lungs, and then [we would] do the [effects] studies.

There [were] other reasons for that, too. That was the point in time where the U.S. thought we would have a lot more involvement with the use of reactors—not just energy generators, but full reactors in space.

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63 Plutonium-238 has a half-life of 89.6 years; plutonium-239, 24,400 years.

64 breaking or splitting off in chips or bits

65 emitters of alpha particles—positively charged particles, each consisting of two protons and two neutrons, emitted in radioactive decay or nuclear fission; an alpha particle is the nucleus of a helium atom.

66 A micron is a millionth of a meter or about one twenty-five-thousandth of an inch.

Kiwi[67] was tested in Nevada for that. As you probably know, at least three of those Russian satellite [reactors] have reentered the atmosphere, one over Canada.[68] Question there was, "What happens to particulates if you ingest them?"

Number one question was, "Do small particles get hung up in the GI [gastrointestinal] tract in the diverticula[69] and the villi[70] and all?" So in that case—getting away from plutonium now—we used simulated particles of different densities. At Los Alamos, the interest was on the transit of these materials, very small particles. Up at Hanford, I'm trying to think of the name—Vic Smith, I think—was interested in using highly radioactive particulates. They were using pigs, as I remember. There were several people around the country, who were looking at several parts of the same problem, namely "Do small particles get trapped in [the] GI tract by virtue of their small size?"

The second part of the question is, "If [the particles] are highly radioactive, do they behave differently?" Plutonium-238 pilot studies that we did in Los Alamos, I led that effort. We actually used more plutonium-238 in the pilot studies and the design of the experiment and getting the new facility (which was only about four or five rooms, actually) up and running, than was used in the entire experiment that went on for a couple years after that. Working with plutonium-238 was a real challenge because it's so highly radioactive. Even the tiny particles, materials will flake off the surface, so it's a very difficult thing to work with.

HARRELL: Was it even dangerous to have in your hands the syringe to inject into the animal?

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67 a series of experimental reactors related to development of a direct-cycle nuclear rocket engine for propulsion of space vehicles. A Kiwi reactor went critical in 1965 and thereafter was shut down. Source: *Directory of Nuclear Research Reactors*, STI/PUB/853; International Atomic Energy Agency; 1989; Vienna; p. 788. While the program struggled on with development problems, most of its funding was cut in 1963. (Paul's note: Not clear: 1963 predates the accident.) For a detailed history see Linda Neuman Ezell; *NASA Historical Data Book Vol. II: Programs and Projects 1958–1988*; National Aeronautics and Space Administration; Washington, D.C.; 1988; pp. 476–88.

68 In January 1978, the Soviet military space satellite, *Cosmos 954*, broke up during an uncontrolled reentry and scattered radioactive parts and fuel from its on-board nuclear power plant over a 483-mile-wide swath in the vicinity of Great Slave Lake in the Northwest Territories of Canada. *Cosmos 954* was a Soviet radar ocean reconnaissance satellite (RORSAT) that had been sent into orbit to detect and track U.S. Navy aircraft carriers worldwide. Because the power demands of the satellite's radar exceeded the capability of solar power systems of the day, the Soviet low-earth-orbit RORSATs were powered by a small nuclear generator. The U.S.–Canadian North American Air Defense Command (NORAD) detected the fact that *Cosmos 954*'s orbit had experienced unplanned decay, leaving the time of reentry predictable to within a day, but the point of reentry impossible to foretell. Civilian emergency service organizations in many parts of the world were placed on secret alert (without being told why) until after the reentry. The crash of *Cosmos 954* in Canada resulted in no reported human injuries. Under an existing treaty, the Soviet Union was liable for all costs associated with cleanup. The event led to further international negotiation to limit the use of nuclear power in space. Later generations of Soviet RORSATs were redesigned to separate and boost their nuclear power plant into a higher parking orbit at the end of their mission life.

69 the pouches or sacs opening from the intestinal wall

70 fingerlike projections on the surface of the mucous membrane of the small intestine, that function to increase the area for the absorption, secretion, or exchange of materials; *singular:* villus

RICHMOND: No, because [for] alpha particles, the energy isn't high enough to get through the glass or anything. However, the particles we worked with were so small that it was difficult to know where they were, at times. One of the interesting parts of that is that we had detection equipment; we used the alpha meters [that] were in a web-type canvas carrying material. Well, the instruments themselves were sent to Santa Fe periodically to get recalibrated. Then they would come back into the Laboratory somewhere else, and they would be assigned to different research groups. At one point in time, we had one come back from Santa Fe, and in the process of checking it in or something, they detected one of these small particles in the webbing of the carrying case. It was traced back to our laboratory, actually, where one day one of them got loose and we couldn't find it. But it ended up [being found] in the webbing of the alpha meter!

Anyway, the conclusions of many studies [at] many places were that generally, for alpha emitters, the more the material diffused for a given [amount of] material, the more potential hazard than if you have it in one piece. With the one piece, the damage is so intense that you have cell killing and walling off, as opposed to a lot of events, where one of them, statistically, can initiate a tumor.[71]

FISHER: You published a nice report, an AEC report in 1974 on the hot particle problem.

RICHMOND: With Dr. Bair?[72]

FISHER: With Dr. Bair and Bruce Wachholz.

RICHMOND: He[, Wachholz,] is at a National Cancer Institute now. Bill Bair, myself, and Bruce did that.

FISHER: And, just as a side-light, I think it's interesting that I ended up using some of these plutonium microspheres in my own work in the early 1980s. I think I got Dr. [Ernie] Anderson to send it to me. Similar kinds of work on cells in vitro,[73] which reconfirmed your report in 1974, using the same plutonium–zirconium microspheres.

RICHMOND: This was a real challenge because of the nature of the material and the fact that it had not been used, in a large sense, in biological settings. But no one on that project received any measurable internal contamination.

FISHER: Even though it was a fairly difficult thing to make those zirconium oxide microspheres of uniform particle size. I remember there was a spinning-disk technology that had been developed to make them.

RICHMOND: Sol gel, actually? But, earlier on, when we got the plutonium-238 spheres from Mound Laboratories [Miamisburg, Ohio] they were, allegedly, intact. There would be no spallation. These were the bigger ones,

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[71] an uncontrolled, abnormal, circumscribed growth of cells in any tissue; neoplasm

[72] For the transcript of the October 14, 1994 interview with Bair, see DOE/EH-0463, *Human Radiation Studies: Remembering the Early Years; Oral History of Health Physicist William J. Bair, Ph.D.* (June 1995).

[73] developed or maintained in a controlled, nonliving environment, such as a test tube

a couple hundred microns. Boy, we got shocked when we first opened some of those shipping containers. The material had crept all over. People denied it and they [basically] said, "No, no, no. The way these are made with the plasma torch, no way that this could be happening." After a while everybody agreed, "Yes, it [is] happening."

It's a common phenomenon, it turns out, for very intense, relatively short-lived alpha emitters. You get the spallation from the surface. We were able show that experimentally. We would embed some of these spheres in an organ somewhere [or] in muscle very carefully, and, then we would take tissues from the animals at different times and do autoradiographic[74] studies. You could actually see [on the photographic negative] single tracks [of individual alpha particles at] long distances. [It] meant that the material was coming off, not in flakes, but ultimately in individual atoms. You could see this with the autoradiographs, [at] long distances from the spheres. That information got back into people who were making them and working with them at Mound Laboratories.

HARRELL: So you would end up having several [smaller] particles after you injected [one larger] one?

RICHMOND: For those big ones, you really didn't know what was happening. One of the other things that we worked with [was] iodine, for example. Folks were doing a lot at Los Alamos working with iodine-131, primarily. We were interested in isotopes that had longer half-lives, so we did a lot of work with iodine-125, which had a 60-day half-life. Several things came out. I'm a biologist, but I have at least two publications on the physical half-life of radionuclides.

FISHER: Cesium-132?

RICHMOND: Right, and iodine-125. It turns out that iodine-125 is the most often used radionuclide today, by [some] of the protein chemists and folks like that. Enormous applications. And, at that time, I thought everybody kn[e]w the half-life of iodine-125, and it wasn't so. The argument is that even people doing these tracer studies were contributing to the literature, which traditionally physicists generated; half-lives and physical characteristics.

Animal Studies of Strontium Retention; Post-1956 Human Studies of the Effect of Particle Density on Clearance Rate

HARRELL: You also did some work on strontium-85 retention?

RICHMOND: Yes.

HARRELL: Was that part of overall fallout-studies program?

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[74] pertaining to autoradiography, a technique whereby photographic film is placed over thinly sliced tissue to record, in image form, the radiation tracks from the tissue that pass through the film's emulsion

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RICHMOND: Yeah, the rationale there again was that strontium-90 from fallout was a beta emitter[75] and you couldn't put a person into a counter and get an accurate estimate of body burden. What we were trying to do was get a feel for retention of strontium by using external counters, i.e., some isotope that had a gamma emission.[76] We found that strontium-85 was the isotope that met those characteristics, so we did some work with strontium-85 in animals, not in people.

HARRELL: Why did you use skin absorption?

RICHMOND: Back in Los Alamos, the questions that arose were, "How do you get material in your body through wounds, ingestion, and also skin absorptions?" The [person] at Los Alamos, as I recall, who did most of that work was [physicist] Marv[in A.] Van Dilla, again using the advanced counting equipment.[77] I don't remember him doing strontium-85. I'm not surprised; it's probably something we did jointly.

Iodine was of interest. The ritual was to take a person and set them next to a very sensitive detector; put some material on the hand in a controlled way; put the hand in a shield that would shield the radioactive material from the [detector]; and the body is looking at a detector. So anything that's absorbed through the skin gets into the circulation [and] will leave [the hand in] that shielded cave and get out into the body. Once it's in the body, it can be detected. So, that was all part of this general interest of the skin absorption; how the materials moved through the skin. That never turned into a large study, actually. Van Dilla was the main PI [(principal investigator)] on that, Marvin Van Dilla.

FISHER: So, you never really did work with any strontium-85 or ingestion or injections in human subjects?

RICHMOND: No. I think we did some animal work.

FISHER: You did some animal work, but did you do some human studies?

RICHMOND: I don't think so, but I'm not positive.[78]

FISHER: At least none come to mind?

RICHMOND: No.

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75 a radioactive substance that emits electrons or positrons during radioactive decay

76 emission of gamma particles, highly penetrating photons of high frequency, usually 10^{19} Hz or more, by an atomic nucleus

77 a reference to HUMCO I, 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.

78 From 1961 to 1962, Los Alamos conducted studies on the whole-body retention of strontium-85 in humans. Three male laboratory employees ingested 1.07 microcuries of strontium-85 in 100 milliliters of tap water. The studies showed that strontium-85, with its 65-day half-life, is suitable for studying short-term retention of fallout but not appropriate for long-term retention studies. The work was supported by the AEC. In LANL-18, "Retention of Strontium-85," in *Human Radiation Experiments (DOE/EH-0491)*. The sole reference was authored by Furchner, Van Dilla, Rowe, and Richmond.

HARRELL: For these body counter experiments, were the subjects Lab employees?

RICHMOND: Yes, all except one. —No, that's true[, what you suggested:] for the whole-body experiments, they were all employees.

HARRELL: Were most of the human subjects Lab employees in the studies that you did?

RICHMOND: Any of the work that I did, they were all Lab employees, except one. You probably heard this story already, when we were interested in the movement of small, discrete particles. We did a study, after we did studies with dogs, where we would feed them the particle once a day and follow that through [their digestive tracts]. We needed some information on people (to confirm that). And that was a study that's been reported in several of these reviews. We took small spheres, very small, 100 to 200 microns. ([A] hair is about 50 microns across, to give you some feeling of that [size].) We had two kinds of particles. One was a very low density. Essentially a plastic kind of material, ceramic with manganese-54 in it, that could be easily measured from [its] gamma rays. Then, we had some unfissioned uranium spheres, very high-density. So, what we wanted to see was, "When you have a very tiny particle go through the GI tract, how important is density?" People were arguing that plutonium, uranium, and things like this, or fuel from space reactors or accidents, [of] high density would lodge in the villi in the small [linings] of the gut. So we did this and we reported it.[79]

Since then, I've been asked several times about details of that experiment. It was published, [in the] open literature, [complete with] dosimetry [information]; everything is in there. [As] it turns out, one of the people was not an employee [but] was my wife. Well, I just used to talk to her about what we did, and she was interested, so that was the one exception.

FISHER: What was the outcome of the project? Did these small particles pass entirely through GI tract?

RICHMOND: There was no difference in [the excretion rates of particles of different densities]. The reason we put the tag on them was so that would measure that, but it was a very difficult measurement problem. We would take stools, feces, and put them in— press them, actually—in plastic bags between two very large detectors [made of] sodium-iodide [crystals]. And these would be counted all night, as I remember. So it was a very challenging process to detect them. The doses were extremely small, and that was all documented.

FISHER: The doses were very small, but how did you get Mrs. Richmond to participate in the fecal collection part of the study?

RICHMOND: *(laughter)* Devotion.

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[79] For more on the concern about particles from nuclear-powered spacecraft and the Los Alamos human-radiation experiments that ensued, see "Dr. Wright Langham's Postwar Studies of Plutonium" in the Don Petersen transcript (DOE/EH-0460), August 1995.

FISHER: *(laughter)* Devotion to the scientist or the husband?

RICHMOND: Both, I guess. She just got interested in it, I guess.

HARRELL: Did you do studies similar to those for nonradioactive particles and other kinds of pollutants?

RICHMOND: No.

Studies of Other Radionuclides

FISHER: Weren't there also some uranium microsphere studies done with 100- to 200-micron ceramic particles with ^{235}U?

RICHMOND: Yeah, that's what I just mentioned. One was manganese-54, low-density, and the other was uranium, nonfissioned, but extremely small amounts [of] uranium in those 100-micron spheres.

FISHER: Were they high-density or low-?

RICHMOND: High-density. The manganese were in ceramic, low-density. The uranium fuel pellets, spheres, had traces of uranium-235 in them; that's what we were measuring, actually. So again, we had low-density and high-density; same size, different tags. One [tag was] built into the uranium-235, and the other [(manganese-54)] was commercially available. The medical profession used it for lodging in different tissues. That material, the only way we could detect it, since they were so small, was to put a tag it. Our conclusion was that it didn't make any difference. That does not say that if th[ere] was a fission particle with a high radiation field it would stay in.

FISHER: I've looked through your list of publications, and also we've done some searches on the annual reports at Los Alamos, and I was going to ask you if the following radionuclides were studied in man, or just in animals. In some cases its difficult to find specific mention of radionuclides studies in man where there are many animal studies. Some examples would be ruthenium-106.

RICHMOND: All [exclusively in] animals.

FISHER: All animals. Silver-110, manganese-54 as a metabolite.[80] Do you recall any human studies with manganese-54?

RICHMOND: Not offhand, other than the spheres that I just mentioned.

FISHER: Any of the plutonium isotopes in man?

RICHMOND: No.

FISHER: Beryllium.

RICHMOND: Beryllium, as I recall, was all [studied in] animals.

FISHER: I'm sure I haven't covered them all.

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80 a product of metabolism

RICHMOND: Many of these, after we worked on cesium and a few others, we felt pretty comfortabl. about the doing the work in animals and predicting what would happen in man.

FISHER: You've already mentioned strontium.

RICHMOND: I'd have to recheck that but I don't recall work with strontium-85 in people. But I'd need to look carefully.

FISHER: One of the most worked-with radioactive materials at Los Alamos was radiolanthanum—barium-140, lanthanum-140. Did you do any biological studies on this radionuclide whatsoever?

RICHMOND: Not at all, and I don't think anybody in that group did, at all.

FISHER: Why was this one not studied in your group?

RICHMOND: I have no idea. They were used in a testing—"RaLa" I guess it was called.[81] So, that was happening in some of the canyons [near Los Alamos]. I knew that, at Los Alamos. I'm not sure why it wasn't.

FISHER: Was it perhaps because the use of radiolanthanum was classified?

RICHMOND: I have no idea.

HARRELL: So you had no connection with that program or the warfare program?

FISHER: That's interesting to me, I think, in an historical sense, why barium lanthanum was not given intense biological investigation. Maybe it had been but we just don't know about it.

RICHMOND: Or it might have been done somewhere else.

FISHER: Done somewhere else previously.

Need for Long-Term Follow-Up of Exposure

RICHMOND: My extent of that was hearing bangs once in a while when they had explosions down in the canyons.

Incidentally, one thing I did want to mention, in that '74 publication that you mentioned on plutonium[–zirconium microspheres]. I think I brought a copy with me. *(picks up his copy and begins to flip through the front pages as he talks)* We were very careful to point out that even though 30 years had elapsed from those original exposures of the GIs and other populations, we shouldn't be complacent; we needed to wait and see what the outcomes would be.—Here it is, it's a front-end summary:

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[81] From 1944 to 1962, Los Alamos conducted 254 open-air implosion physics tests in nearby Bayo Canyon. The purpose of the program was to test weapons designs using conventional high explosives and radioactive lanthanum (RaLa), a short-lived but intense radiation source. Tests were performed specifically to diagnose material motion and compression through high-speed x-ray photographs of the earliest moments of the implosion. The sources involved contained quantities ranging from around one hundred to several thousand curies of lanthanum-140. Source: "Environmental Releases of Radiation" in DOE/EH-0445, *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records*, February 1995.

> The message I would like leave with you is that we have not observed any serious biological consequences of man from occupational exposure to plutonium. The next logical question is, "How do you know or have you looked?" We have indeed looked and the record—

This was a summary—

> —the record today is quite reassuring. However, I would like to point that past is not always prologue, cheerful as that thought might be. Although the final answers are not in, I would hasten to add that we now have information on people which has spanned almost three decades, so that the argument of latency for certain late effects (how long does it take to produce a tumor) is losing impact in at least some of the cases I will discuss.

FISHER: You have just read one [of] the paragraphs from a paper entitled "Current Status of Information Obtained from Plutonium-Contaminated People," by yourself, Chet Richmond, reprinted from *Radiation Research— Biomedical, Chemical and Physical Perspectives 1975*, published by Academic Press.

RICHMOND: The point is, I think serious investigators who are interested in radiation effects [are] well-aware that, in many cases, you have to wait, depending on what you're looking for. And this was specifically—I was pleased to go back and read this [just now]—I was pleased with myself, that we had foresight to state that, "Even 30 years might not be enough time."

FISHER: For follow[-up] studies on these workers. It was interesting that this population was also fairly young when it was exposed.

RICHMOND: Young GIs, just out of college.

FISHER: So, it was a unique population.

HARRELL: Were there animal studies that ever had a span of twenty years?

RICHMOND: Closest ones I know of were some of the beagle studies at Hanford, Albuquerque, Salt Lake [City], and [the University of California at] Davis.

HARRELL: I guess you have to have a relatively long-lived species?

RICHMOND: Right. I think some of the swine studies, the pigs at Hanford, I think some of those extended out for quite a few years. But rodents don't live that long: a couple of years or so for a mouse. Four or five, [in] that range, for rodents.

Radiation Studies Resulting From a 1958 Los Alamos Criticality Accident

HARRELL: You did some work on the effects of a criticality[82] at Los Alamos in 1958, with Lushbaugh. There was an injury to a man.

FISHER: Are you referring to the Kelley case?[83]

HARRELL: Yes.

RICHMOND: I was involved in that, but I wasn't an author or anything. It was sort of a—almost everybody in that group, H-4, got involved.

FISHER: Why don't you give us a little background on the accident?

RICHMOND: It's kind of interesting. It happened [at] the end of the year; I don't quite remember the year.

HARRELL: 1958.

RICHMOND: Right, 1958. That was pretty common. A lot of the criticality deaths were done during inventory activities, which normally were the end of the year.

FISHER: And when you say at the end of year, you probably mean the last day of the year, December 31st.

RICHMOND: The calendar year, toward the end of calendar year [1958]. The inventories were done for the year. Kelley was my neighbor; he lived next-door in Los Alamos. I knew him and his wife very well. Anyway, he was exposed, and died 30-some hours later. New Year's morning [I remember], because several of us were at a New Year's party. There was a snowfall; it must have been a foot and a half of snow. I had a call from Dr. Lushbaugh saying, "We want you to come over to HRL (Health Research Lab). Kelley died."

Now, from the time of the accident until he died, the major interest was in the site of the excursion,[84] where that happened, the criticality. Because there was concern that it was still pulsing, there was concern that it could go through more criticality steps. Kelley was in the hospital. A couple of other people had been involved.

Before Kelley died—Let me go back a little. We had been making measurements on some other people who [had been] involved [in the accident], and it turns out that as we took them down the stairwell to the counting areas where we had the whole-body counter and the crystal counter, the count [rate] went off [the] scale on the liquid [scintillation] counters. So, we knew we couldn't measure Rod. I don't remember his

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

83 December 30, 1958. For a detailed account of the ensuing pathological investigation, see "Investigations of Radiological Accidents" in the Lushbaugh transcript (DOE/EH-0453), April 1995.

84 an unexpected rapid increase in fission rate, resulting in the reactor "going critical"—beginning a nuclear chain reactic

[last] name. He was the person who had the second-highest dose; his first name was Rod. We had been very much concerned about Kelley, and again all [the] physicians, everybody, was sort of [on] standby.

After Kelley died, there were a lot of samples made available—nails, teeth, hair. Lots of people were making measurements, sodium activation [in the victim's hair etc.], trying to get a better estimate on the dose. Several large publications came out on reconstructing the accident, the dosimetry, what was known about blood counts, and things like that.

My major involvement was, when Kelley died, they said, "We really want to try to get a better estimate on the things that were activated by neutrons in the body—[stable] sodium[-23] for example." That's why we couldn't measure Rod, one of the survivors. He was so intensively radioactive, because the sodium[-23] in the body, a lot of it, got activated to make sodium-24.

Anyway, they took Kelly's body—In fact, since I knew Lushbaugh, I guess it was just coincidence that I was one of the first people there after he died, early on the first [of January]. Lush and I took the body over on the gurney[85] from the hospital to the HRL building, which is just a couple hundred yards. Last time I talked to Lush, [we] couldn't remember if it was a gurney or a stretcher.

We placed Kelley's corpse in one corner of a large cubical room with about eight- to twelve-inch steel [walls], inside of which was a sodium iodine [crystalline] detector. [The corpse] was still so intensely radioactive that we just had no way of calibrating. So, we left the door [of the room] open, then we closed the door, as I remember. We made some measurements and said, "Well, now we have to try somehow to calibrate this."

So Marvin Van Dilla and I think Ernie Anderson were in charge of that, and they said to me, "You've got some cesium in you, don't you?" I said, "Yes." So anyway, I lay down exactly where Kelley [had been], and this was early in the morning, very cold, [on] eight or so inches [of] steel, and I remember because they wanted—I was taking [his] place—the [sodium iodide] crystal in the same position [in relation to Kelley's corpse]. And, from my naturally occurring potassium and cesium and maybe even zinc at the time, we knew we could get a calibration match.[86] So it turned out, I think I was there for an hour or so in that position, trying to get enough data collected, because the crystal was so far away.

FISHER: Did you get enough counts?

RICHMOND: Yes.

FISHER: To do the calibration?

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85 a flat, padded table or stretcher with legs and wheels, for transporting patients or bodies

86 The sodium iodide crystal would measure the trace radioemissions from the radioisotopes in Richmond's body. By knowing three values—the amount of radioisotopes in his body; his distance from the crystal; and the crystal's response to his radioactivity, the researchers could determine the crystal's response to radiation and roughly calculate, at a level many orders of magnitude higher, the amount of activity in Kelley's body.

RICHMOND: They took that data and somehow used it.

FISHER: That's rather remarkable, in and of itself.

RICHMOND: Of course, I was still fairly young then and I was thinking, lying there in the cold, that, "Gee, this guy just died, a radiation accident criticality!" I had read about the earlier deaths, criticality in the early [radiation] pioneers, Dahlgren and some others. There was a lot going through my mind at the time, plus [the discomfort of] being real cold.

Anyway, that was a full-bore thing. Payne Harris was doing calculations, and they were resimulating it, and they were taking samples, as I mentioned—hair and nails—and looking at things [in the body] that were activated, [such as] sulfur. So, that was my only involvement with that criticality.

HARRELL: Did you have any involvement with the SL-1[87] autopsy?

RICHMOND: No. Some of the people did at the Lab. Lush was involved, and Payne Harris.

FISHER: Don Petersen?

RICHMOND: Petersen probably.[88]

HARRELL: Was that because of the experience at Los Alamos with other accidents of that type that they were called in?

Declassification of a Key Los Alamos Report

RICHMOND: I think *that,* plus the closeness. Well, one thing I did want to mention to you. There was, back in that 1970, 1971 period, around that time when Pat Durbin was really exercising a lot of people to sort of get more information out on the plutonium, she had been contacting me because I was in Washington. Then, after I went back to Los Alamos [from my two-year stint at the AEC] in 1971, [in] the AEC there was intense interest in all this business on plutonium and injectees. I sent this to Don Petersen in case they didn't have it at Los Alamos. I sent this letter to Sid Marks, who was a physician in the Division of Biomedical Environmental Research. This is March 22nd, and that was in response to a request from him, sort of [asking me] "What do you know about plutonium in people, reports?"

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[87] The SL-1 (Stationary Low-Power Reactor) was a 3-megawatt prototype military reactor that was being developed at the National Reactor Test Site in Idaho Falls, Idaho, as a power source for remote bases. On January 3, 1961, while a military crew of three was reconnecting control rods for a scheduled restart of the reactor, a steam explosion occurred that killed all three crew members. These were the first deaths caused by such a reactor accident in the United States. For an extended discussion of the SL-1 reactor accident, see "Fatal Worker Accident at Idaho's SL-1 Reactor (1961)" in DOE/EH-0454, *Remembering the Early Years: Interview With Dr. George Voelz, M.D.* (May 1995). For a discussion of the recovery of the bodies, see "Investigations of Radiological Accidents" in the Lushbaugh transcript (DOE/EH-0453).

[88] Petersen's chief responsibility was to determine the dose that the victims had received. For his account of Los Alamos's involvement, see "Postmortem Assistance Following the SL-1 Reactor Accident (1961)" in the Petersen transcript (DOE/EH-0460), August 1995.

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I just noticed, I don't have the list of references attached, but I think there were like 24, including some of the early Manhattan District. Some of these reports that I mentioned in my letter to Sid Marks of March 22, 1974, mentioned that these reports are available, were available.

HARRELL: That was after the declassification of *[LA-]1151*?

RICHMOND: Yes, [the declassification] was 1971, as I recall. This was an attempt on my part to respond on anything I knew that was available. As I mentioned, open-literature publications and I did, yes, you're right. I said, "*Los Alamos Scientific Lab Report 1151* is one of the most valuable prime sources of data on the subject. The OUO classification was removed in May 1971." So that was an attempt, in 1974, to provide information back to AEC, reports and things. I guess that must have coincided with a previous time when people were interested in gathering information on the [human] subjects.

FISHER: Well, there were three. There have been several reviews of this: one in 1974 by the AEC, another in 1985 by DOE, and then the present reconsideration.

Participation in AEC Assessment of Programs (1969–71)

HARRELL: You mentioned your work with the inhalation program at PNL[89] as part of your time with the AEC. What else did you work on when you were in Washington?

RICHMOND: The University of Utah; we were very much interested in sort of the assessing where they were and where they were probably going. A lot of effort with the Albuquerque Laboratory, ITRI, [namely the] Inhalation Toxicology Research Institute. That lab had been created many years earlier. Their mission was sort of, I'm simplifying this, "What would happen if a reactor exploded and you had a lot of mixed fission products in various forms, gaseous and particles, and so on?" They had been studying a typical beta emitter, a typical alpha emitter, etc. Ultimately [they were] going to combine these [research areas]. There was a fair amount of concern at the time about, "Hey, is this huge project on track? Where is it going?" Because they were diversifying into cellular biology and all kinds of things.

HARRELL: Were they interested in short-term effects, or all kinds of effects?

RICHMOND: Their mission, as I recall, was, "What would be the biological effects, both short-term and long-term?" So, it was whoever designed that [who could answer your question]. Retrospectively, I think it was kind of not well-thought-out, really. I think the pressures at the time were concerns about large reactor accidents.

[89] Pacific Northwest Laboratory in Richland, Washington, operated for the Department of Energy by Battelle Memorial Institute, Columbus, Ohio

HARRELL: Was some of that information used in Chernobyl to help clean up or help the victims?

RICHMOND: I doubt it very much. I don't think so. Anyway, that was mainly the thing when I was back in the AEC, on loan, or as an employee reviewing some of those programs.

In fact, what I just told you was part of the reason for the decision to have that new low-dose experiment with dogs take place at Battelle. Because we were kind of concerned about exactly what would be happening next at ITRI in Albuquerque, and what their capabilities were to take on something new before we fully understood where they were going.

Interestingly, I didn't have much to do—nothing to do actually—with Rochester, except on that first tour. Your letter asked me to mention that. [During] the first tour, I mentioned earlier about looking at four or five sites to see where we would place—where AEC would place—that low-dose plutonium dog experiment. I think the reason for that is that there were other people in the division of AEC, Division of Biology and Medicine, who were assigned to those labs, and I'm fuzzy on this, but I think Frank Brooks or Ed Still probably had that assignment.

HARRELL: Did you get involved much with what was going on here in Oak Ridge?

RICHMOND: No there were no animal experiments going on down here. There was no comparable either small-animal or large-animal facility.[90] I guess I visited Oak Ridge a couple times, probably just as part of program reviews that people would have ongoing because there were other kinds of funding going on. That time, funding was for primarily mammalian genetics; Bill Russell and [his wife], Liane Russell.

HARRELL: Was there interest in the low-dose information coming out of the LETBI[91] facility and cancer therapy program here?

RICHMOND: At the ORINS, at ORAU?[92] That, I had nothing do with at all. I'm trying to think, again that would be [physician David] Bruner and Joe Goldstein and folks like that. I'm not sure Storer was involved—probably, since he was a physician. I think Ed Still, I'm pretty sure Ed Still was one of the staff up there who was assigned to labs, like I was, [to] some of the beagle labs.

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90 See the next section. "To the ORNL Division of Biomedicine and Environmental Sciences (1974)."

91 Low-Exposure-Rate Total Body Irradiator. Clarence Lushbaugh directed the LETBI facility.

92 Oak Ridge Associated Universities, the managing and operating contractor of the Oak Ridge Institute for Science and Education, formerly known as Oak Ridge Institute of Nuclear Studies (ORINS)

Adoption of Guidelines for Protection of Humans as Research Subjects (1970)

HARRELL: In 1970, the AEC adopted the NIH guidelines for the protection of humans as research subjects.[93] Were you involved with the process of approving that for adoption and discussing how that would be implemented?

RICHMOND: Not directly, no. Again, that was mostly the physicians. I do remember discussions about, "AEC, let's try to make to make this pretty much as HHS[94] has it: not develop all-new ground rules, but sort of adopt it."

Now, when I was here at Oak Ridge as Associate Director, I was involved in human-use committees, from the standpoint of making sure we had them and [ensuring] proper representation [from the various Oak Ridge divisions]. In fact, the study in the northwest on the irradiated prisoners,[95] the gonadal irradiations, some of the biologists were going to be involved in that study here at Oak Ridge just by getting some of the tissues and doing some analyses. That went through a very rigorous review.

In fact, I brought that along; it's kind of interesting. *(pulls out an old document and begins to scan its pages as he talks)* Actually this is September 1969, so it was before I came to Oak Ridge. But what impressed me is that Tom Lincoln—let me back off. When I came here, I was involved [in human-use guidelines] because I was the Associate Director for Biomedical and Environmental Research, [working] with human-use committees. They already had one [such committee] before I came here, but this is Tom Lincoln's statement for the minutes of the review committee for the Pacific Northwest Research Foundation[, which was conducting the prisoner studies]. Tom just points out here that,

> By participating in this study, we (ORNL) are necessarily identified with it, regardless of any agreement, written or otherwise, with Dr. Heller. If there's any eventual ethical criticisms of this program, ORNL will inevitably share the criticism. I, therefore, believe a careful review should be conducted so that we approve of the basic ethics of the program.

That was in 1971.

FISHER: This indicates that ethical considerations for the prisoner study you were very high on [the] list?

RICHMOND: Yes.

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93 In 1966, the National Institutes of Health (NIH) submitted 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.

94 Department of Health and Human Services (then called Department of Health, Education, and Welfare), a cabinet-level Federal agency

95 See earlier footnote under "On Assignment to AEC Headquarters in Washington (1969–71)."

FISHER: And, it also indicates there must be some opinion that there were unethical aspects of the work in the State of Oregon, also, [and] up in the State of Washington?[96]

RICHMOND: I want to make it clear, I had nothing to do with this, but [in the] process of what's been going on here at Oak Ridge, in terms of releasing documents, this one struck me as being kind of interesting. There are comments from all the reviewers here. Now, I'm on record though, in something here that was published in Oak Ridge when I got interviewed, as saying—[and] I'm kind of restructuring this, it's not verbatim—that people have their own opinions about what is ethical and what is not. I personally, and it's just a personal prejudice, do not think that we should be studying, using prisoners for experimentation. That is just a reaction I have. There's no logic to it, really, but it's a gut reaction. I [have] that feel[ing], even if those people sign something.

FISHER: Even if they volunteer?

RICHMOND: Even if they volunteer. To me, ethically, it doesn't make sense. I'm on record for saying that. At the same time, it's interesting. They did a pretty good job [of adhering to ethical standards] back then.

FISHER: And the outcome of this review is that ORNL would not participate or would participate in the analysis of tissues, do you remember?

RICHMOND: No, I sure don't. I didn't get here until 1974. I'm not sure. It would be interesting to chase that down.[97] Grant Brewer was the person here who would have been involved or was involved.

HARRELL: Yesterday, Dr. Totter told us that the University of California[, which managed the Los Alamos Scientific Laboratory,] declined to get involved with analysis of tissues for similar reasons at about that time.

RICHMOND: Interesting.

HARRELL: Were there ever any such human-use committees at Los Alamos back in the 1950s? Did the University of California have anything to say?

RICHMOND: I don't think there was at all.

HARRELL: Did California have anything to say about the policies?

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[96] From 1963 to 1973, the University of Washington, Seattle conducted studies on the effects of radiation on human testicular function, using inmates at the Washington State Prison in Walla Walla as subjects. Initially, 232 healthy volunteers were accepted into the study program. Sixty were subsequently irradiated with acute doses of x rays, ranging from 7.5 to 400 rads to the testes. Each selected inmate had expressed a desire to undergo a vasectomy at the conclusion of the study; 53 did so. All subjects eventually recovered to their normal preirradiation condition prior to vasectomy. The work was supported by the U.S. Atomic Energy Commission. See OT-14, "Testicular Irradiation of Washington State Prison Inmates," in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (213 pages), DOE/EH-0491, July 1995.

[97] According to OT-14 in *Human Radiation Experiments* (ibid.), tissue samples from the Washington State prisoner experiments (described in the preceding footnote) were analyzed at the Biology Division of Oak Ridge National Laboratory. OT-21 (Carl Heller's testicular studies of Oregon prisoners) does not state where tissue samples were analyzed.

RICHMOND: Not in the 1950s, not that I remember. Of course, I didn't get there until late, and I was pretty young and innocent. We did while I was there, had people have things explained to them, and there were documents on file about the experiments and things. In terms of formal policy, I doubt if there was anything in the 1950s.

FISHER: When did the concept of "informed consent "or "signed informed consent" first come up in your professional career? When were you first faced with this issue?

RICHMOND: I don't really remember.

FISHER: Did it evolve slowly?

RICHMOND: Yes it evolved slowly but, as I say, [for] some of the work we did with even our colleagues at Los Alamos, we would have [printed] materials explaining what was going to be done. I don't know if they were signed or not, but even though we were researchers, that got discussed and written down.

FISHER: Do you have children?

RICHMOND: Yes.

FISHER: Did they participate in any of the studies? There are some children of Los Alamos workers who did, but yours were not included?

RICHMOND: I would imagine they were [in] some of the skin absorption studies, I guess.[98]

HARRELL: I think some iodine studies, also, [were conducted] on some of the children.

RICHMOND: Now, some of that work, I know was done through patients in the hospital. In other words, some of the patients that would show up in the Bio-Medical Research Group, and especially the [radiation] counting area, were patients in the hospital.

HARRELL: Which hospital was that?

RICHMOND: The Los Alamos Memorial, whatever.

HARRELL: People in the immediate area?

FISHER: Next to the HRL [(Health Research Laboratory)].

RICHMOND: Right. See, the HRL building, Health Research, [is] on the town side of the bridge, which separates most of the Laboratory, and it's right adja-

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[98] Don Petersen and other Los Alamos researchers allowed their own children to be used as subjects in experiments involving radioiodine. See "Measuring Iodine-131 Uptake in Children (Circa 1963)" in the Petersen transcript (DOE/EH-0460).

cent to the hospital. There were kids with leukemias[99] who were in and out of there quite often.

To the ORNL Division of Biomedical and Environmental Sciences (1974)

HARRELL: When did you come to Oak Ridge, then, after?

RICHMOND: 1974, twenty-some years ago.

FISHER: You came to Oak Ridge as Associate Laboratory Director for Biomedical and Environmental Sciences. It's a fairly distinguished position.

RICHMOND: Herman Postman[, whom I was replacing, had just been] selected to replace Alvin Weinberg [as Director of Oak Ridge National Laboratory].

FISHER: Had you received the [AEC's] E.O. Lawrence Award before that time, or did that come afterwards?

RICHMOND: It was awarded before I came here as part of what I had done [in] research on at Los Alamos. Herman Postman had just been named Director here, and that was about the time that ERDA[100] was being born. Herman knew that the job at Los Alamos involved environmental [radiation studies]. The Health Division had a lot of pieces to it that got involved with medicine, environmental research, and biological research and he thought I'd be a good fit for the biomedical and environmental program here. So that's what attracted me.

Also, [I] had waited for years. That was the third job offer, actually, to come to Oak Ridge. One when I got my [doctorate] degree [in 1958], and the second time, ten years or so later from Weinberg, and then the third time in 1974. But part of the rationale was that there were still people here who thought that Oak Ridge could build more animal kinds of research activities because—long story, I'm trying to think of the name of it now—the University of Tennessee had the facility here onsite, the CARL laboratory,[101] [an] animal research lab. They had a lot of nice facilities here, large rooms for working on animals.

HARRELL: And they used that for some of the bone marrow work.

RICHMOND: Some of those folks thought I would be a good addition here to help them improve, or whatever, that program. But that never materialized

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99 any of several cancers of the bone marrow characterized by an abnormal increase of white blood cells in the tissues, resulting in anemia, increased susceptibility to infection, and impaired blood clotting

100 The U.S. Energy Research and Development Administration succeeded the AEC in the early '70s, and in turn was replaced by the DOE in 1977.

101 an animal research laboratory, operated by the University of Tennessee's School of Agriculture. Situated five miles from ORINS (ORAU), CARL housed a total body irradiation (TBI) machine that was used to deliver massive doses of radiation, within minutes, to plants, seeds, and animals as large as cows placed in a large room. Oak Ridge Associated Universities used this facility when it began administering bone marrow transplants. CARL was in operation by 1970.

because those programs were phasing out, really, and I just didn't get interested in that, personally.

HARRELL: And they phased out the ORAU hospital at that time, too?

RICHMOND: Right.

HARRELL: Are you aware of the decision-making process that went into that?

RICHMOND: No, just that I know it was part of a planned decision on the part of the Government to do away with hands-on patient–researcher activity at places like Brookhaven, here at Oak Ridge. and I think Chicago. I think there were some involvements with Argonne, and one of the hospitals there [(Argonne Cancer Research Hospital, Chicago)].

Radionuclide Research at Los Alamos

HARRELL: You did some work back in Los Alamos with the Damon Runyon [Memorial Fund]. Was there some collaboration there?

RICHMOND: I think that was University of New Mexico.

HARRELL: That was back when you were a graduate student?

RICHMOND: Yes that was with Tim Eversole, one of my first publications. I don't think that was at the Lab; I think that was strictly University of New Mexico. In fact, that probably [was] what I [was] funded by to do my research. I did a master's degree completely onsite in Albuquerque, working with hormones, adrenals,[102] adrenalectomy,[103] etc.

FISHER: There are some other radionuclides that were studied at Los Alamos, and I wondered if you had any further involvement in them. We've covered almost all of them, I think. A couple I wanted to ask you about. Did you do any work on chromium-51 metabolism, or was that primarily the occupation of the physicians in your group?

RICHMOND: I didn't. I think most of all that was done by Dr. Lushbaugh on red blood cells, chromium tags.

FISHER: Iron-59 metabolism studies, same situation?

RICHMOND: And he, again, was doing that jointly, as I recall, with some physicians in the hospital.

FISHER: Was potassium-42 a part of your doctoral research?

RICHMOND: Very short-lived.

FISHER: Did you do any metabolic studies with barium-133?

RICHMOND: Yes. With animals, but not man.

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[102] a pair of ductless glands, located above the kidneys, that produces steroidal hormones, epinephrine, and norepinephrine

[103] surgical removal of the adrenal glands

FISHER: Did you work with both cesium-134 and cesium-137?

RICHMOND: Yes, and -132.

FISHER: In man?

RICHMOND: Cesium-137 and -134 in man—[in] myself, [so] at least one. Mostly cesium-137, some -134, and the reason is [that] -134 has a much shorter half-life and -132, well, that was dosimetric [(taken to calibrate the doses of the -134 and -137)].

FISHER: You needed additional subjects. One subject would not give you statistically significant results. Did you recruit some help? You mentioned three or four other subjects who were participants in your doctoral research. Do you remember who they were, or what they were?

RICHMOND: They were employees of the Lab, and they were in the same group: Health Research. They were whoever was interested and available at the time who was probably not involved with tritium studies. I would say it was probably on the order of six people or so. Wright Langham volunteered. Kent Woodward, who was a physician; myself; Jeff [E.] Furchner, who was my closest colleague in research, coauthor on many of the papers.

HARRELL: Is it "Jeff" or "John"?

RICHMOND: John. He was known as Jeff.

FISHER: Tell us a little about Furchner; you haven't said much about him.

RICHMOND: He was at Los Alamos when I joined the group. Originally, when I first got there, he was mainly involved in external-radiation studies. There had been a lot of those ongoing with rodents, primarily mice. There were some monkey experiments, high-dose monkey experiments. Jeff's main interest had been in biological effects from external radiation. Somewhere along the line, [he] moved over into the internal emitter area, and he and I worked together very closely. Paul McWilliams also, was a theoretician, mathematician-type.

HARRELL: What became of these people later on? Did they stay with this Lab? Did they move on?

RICHMOND: Jeff is retired; I think he's still alive. He'd been there a long time.

Obtaining Authorization and Funding for Research

FISHER: We've discussed a lot of radionuclide metabolism studies in animals and in man. Were these studies, the ideas for them—generated at the scientific level, proposed to the Laboratory, and [then] authorized by the Atomic Energy Commission in Washington? Do you remember how that whole process took place? You got a good idea, you took it to your group leader. What was the approval process for a project or new experiment?

RICHMOND: I'm trying to think what the title of the request was for funding. But there were, back in those days, something we had to write that ultimately went through the process to get funding. At one point, they were called FWPs—Field Work Proposals. The[re] were [Form] 189s; I'm not sure what they [(the form that served that function)] were back then.[104] We had to a make a request, and ultimately that had to be approved somewhere along the line.

In fact, when I went back to Washington [on loan to the AEC] in the late 1960s for a couple of years, that's one thing I did that I didn't mention before. We would review the requests that came in from laboratories for continuation of funds, or new funds, or whatever. So there was a budgetary process; no one just came and gave you money.

Then, in-house, the work that I did had to be reviewed and approved [by] Wright Langham, for example. His superior was Tom Shipman for most of the years that I was there. Tom was the head of something called the Health Division, and he had to approve things. I don't know how formal that was, but Wright had to go and interact with Tom Shipman. So that was at the division level at Los Alamos. And then, on the reverse end, we would get information on our budgets and we had to decide what we could do within those budgetary limits.

FISHER: Was there a lot of scientific discretion with the use of those budgets?

RICHMOND: A lot more than there would be today, absolutely. For example, the allowance for me to do a lot [of the] work I did that ended up in the open literature on physiology[105] and metabolism. That was all considered to be part of the whole project. No one said that you could only work with fallout materials and that they had to be directly related to public safety. Rather, the approach was: "Here's the alkaline metals, work on them." The target being health, radiation protection. But the spinoffs of that were actually a lot of the most interesting things, and a lot of fun, really.

FISHER: Was there ever a proposal turned down in those early days at Los Alamos? Did you ever have work that was proposed, that was rejected at [AEC] Headquarters?

RICHMOND: I don't recall, but I wouldn't be surprised [if] somewhere along the line someone [at AEC] wouldn't be interested. We had periodic program reviews, too. I remember one time [that] Cyril [L.] Comar, I remember he questioned whether or not there was enough information on how to predict equilibrium levels and things. I don't know when and what impact that had, but I remember that, because here was an outside reviewer saying, "When do you have enough information?"

FISHER: Who would lead the teams from AEC Headquarters when these reviews took place?

RICHMOND: Charles Dunham—he's the one who asked me to go back to Washington. [John] Totter, I guess in his job. Dave Bruner was very much in-

[104] Form 189 (Research Proposal), a funding document used by the National Laboratories for preparation of short-form scientific proposals to the Atomic Energy Commission, and later the Energy Research and Development Administration, the Nuclear Regulatory Commission, and the Department of Energy

[105] the branch of biology dealing with the functions and activities of living organisms and their parts

volved in program reviews. He was a physician, I mentioned earlier. John Storer was involved as a key person.

Now when I was there, there were some reviews that I led. We would get a team of people to go out, and very often we would tie these on with other reviews that were taking place. Before the institution would have a review group in, we would tie into that, for example. A lot more, and you could almost plot, I think, the amount of direct involvement, technically, in Washington staff as a function of time—how that's decreased. Very little anymore.

HARRELL: Were there regular progress reports that you had to send in?

RICHMOND: Yes. We had a lot of the LA [(Los Alamos)] documents that often served as progress reports. That was the main one. Much of what we did had to [be] written as an internal report. These were not; they were available to people, but it was just a designation—"the LA- series," for example. LASL- was another series. That was the primary way of documenting what we did. Except in the budget proposals, you had to show progress. Most of these budget documents are really three years [in scope]. What I don't remember is if that process of writing 189s went back into the mid 1950s. But I'm pretty sure there was something similar [to 189s] the first time I went there.

Cobalt Metabolism Studies With Animals

FISHER: There was a funding mechanism. It started with an abstract of work to be done.

One of the radionuclides I didn't ask you about, which is important, is cobalt-57, -58, and -60. Did you ever do any cobalt metabolism studies?

RICHMOND: I think we did some rodent work with cobalt, but not people. I'm pretty sure we did some rat work with cobalt-60. I don't think we did any with monkeys, or dogs, or people.[106]

FISHER: That's interesting.

RICHMOND: Probably because there were a lot of people [already] working with cobalt.

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[106] Studies were conducted in 1961 by Lushbaugh and D.B. Hale at the Los Alamos to determine the absorption and retention of vitamin B_{12} labeled with 0.5 microcurie of cobalt-60 in humans, using the whole-body counter. See LANL-25, "Clinical Applications of Whole-Body Scintillometry for Determining Cobalt-60–Labeled Vitamin B_{12} Absorption and Retention," in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (213 pages), DOE/EH-0491, July 1995. At Oak Ridge in the 1950s and '60s—years before Richmond's arrival—radiocobalt therapy was administered by the LETBI facility to male and female patients, all of whom had been diagnosed with hematologic malignancy. They were exposed to 50 roentgens or more per treatment series, using cobalt-60. See OR-8, *ibid.* In an ORINS study reported in 1961, researchers in the Medical Division investigated combined total-body irradiation and bone marrow transplants as a possible treatment of leukemia. Seven of the 11 subjects were children with acute leukemia of the primitive cell type. Nine of the subjects received various doses of radiation from cobalt-60 ranging from 210 to 940 rads in single administrations. See OR-27, *ibid.*

Study of Long-Term Retention of Cesium From Fallout

HARRELL: Can you describe the long-term cesium studies that you did? There was a 20-year study, I believe.

RICHMOND: Not 20. —Oh, I'm sorry: we might have followed the fallout levels for 20 years. If that's the one you mean, you're correct.

HARRELL: It wasn't [studied] in people for 20 years?

RICHMOND: This wasn't *administered* cesium: this was cesium from fallout. So everybody had cesium in them. And I think you're right: we did follow that group for 20 years.

HARRELL: Were these groups of people that worked at the Lab for 20 years?

RICHMOND: Right. These were people from the city [of Albuquerque] or the Laboratory. I'm pretty sure they were mixed.

FISHER: Did you find that long-term retention of cesium in the general public from fallout matched pretty well with the expectations from the injection studies or the ingestion studies, really?

RICHMOND: Yes, absolutely. This has been documented in one of our publications. The math gets pretty involved. But even though the cesium was [received] over long times and at different levels, once that was cut off, once you stopped testing, there's still cesium coming down [from the sky]. We were able to predict, pretty much, what that slope of that curve [(the dose per year, plotted over a 20-year span)] should be for a population. Levels from fallout were variable, depending on where you lived in the world. And in far northern latitudes, the cesium levels got up to several microcuries in select small populations. But generally, they were lower by one or two orders of magnitude.

HARRELL: So they were much lower than the highest you ever encountered, much lower than what you personally have ingested?

RICHMOND: Pretty much, except those select populations I mentioned in the far north. Those microcurie levels, around one microcurie, they were about the levels that we were using experimentally.

FISHER: You're thinking of the Laplanders and the Alaskan Eskimos?

RICHMOND: I stand corrected: they were higher than the levels we were using experimentally. I'd have to go back and check, but I think we were in the range of one-half to one microcurie. Probably in that range. But again, those select Laplander populations were considerably higher than what you would find for most populations in the U.S., especially in the northern hemisphere. They were even lower in the southern hemisphere.

Positions Held at Oak Ridge National Laboratory

FISHER: You were jumping around just a little bit. You served as Associate Lab Director for more than fifteen years?

RICHMOND: Yes, 1974 to 1990.

FISHER: Was there a change in Lab Directors during this time or several changes?

RICHMOND: One change. Herman Postman was [the Lab] Director, who was new in his job when he asked me to come here. Alvin Trivelpiece became the Director around 1988 or 1989. He has been the Director since.

FISHER: And after that, you worked as a Director of Educational Programs?

RICHMOND: Right.

FISHER: University and Educational Programs.

RICHMOND: That started in 1984. From 1984 to 1990 I had both responsibilities.

FISHER: You had a dual role?

RICHMOND: Right. Then in 1990, I guess it was 1990, we decided to make that a much more highly visible Laboratory function, the education piece.

FISHER: So you were appointed Director of Science Education?

RICHMOND: Science Education and External Relations. That latter part, External Relations, was in a context of education—state level, national level, and regional level—not public relations.

FISHER: What year did you receive the E.O. Lawrence Award for your work on the radiation biology of internally deposited radionuclides?

RICHMOND: In 1974. I remember because during one of my last interviews here, when I'd learned about it, I mentioned it Herman Postman and he said something like, "Great, now I feel even better [about deciding to hire you]."

FISHER: You've received some other awards in your career?

RICHMOND: [The G.] Failla [Award and Lecture, Radiation Research Society, 1976].

FISHER: You gave a Failla Lecture at the Radiation Research Society?

RICHMOND: In the mid-1970s. I've gotten several [awards] from what was then the Atomic Energy Commission, a couple of those. I got something from the University of Utah for involvement with their programs.

FISHER: What was that called? Do you remember the name of that award?

RICHMOND: I don't recall.

FISHER: That's interesting. You've also served extensively on national and international scientific committees, principally the NCRP,[107] member of ICRP[108] Committee Two [(Internal Dose)]?

RICHMOND: Committee Two.

FISHER: You had some involvement in preparation of ICRP-30 reports?

RICHMOND: Absolutely. More than I want to remember.

FISHER: It took a lot of work, I'm sure.

RICHMOND: That developed over the years here, [for] Karl Morgan originally was heavily involved in the International Commission on Radiological Protection, especially Committee Two[, which he chaired for twenty years]. And he set the pace, I guess, for people here like Keith Eckerman and a few others who have followed that on. In fact, many of the dose calculations [for the] Nuclear Regulatory Commission, Environmental Protection Agency, and DOE have come out of that group here. The good news, I guess, is [that] over the years from the Committee, ICRP activities, and Committee Two, the French and the English and the Spanish and many other groups now have gotten very proficient in terms of their ability to do dose calculations and things like that. Most of them built on what was done here in the U.S., [at] Oak Ridge in particular. I finished all of my board involvements with NCRP, I guess, a year ago. I had two cycles on the board, I think it was.

FISHER: Of the NCRP?

RICHMOND: Yes.

Career Accomplishments

FISHER: As you look back on your career, I know you've retired as of January 1, 1995, I would guess that one of the highlights of your career was your doctoral dissertation work.

RICHMOND: Absolutely. Hard work, but fun.

FISHER: At the very beginning of your career in this field. Could you describe for us some of the other principal scientific contributions of which you're particularly proud?

RICHMOND: Yes, I guess some of the work that we did with cesium, in terms of accelerating loss. Ferric ferrocyanide, for example. In the process of getting very comfortable with that, we did work with a lot of materials.

FISHER: Looking at enhanced excretion?

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[107] National Council on Radiation Protection. Although the words "and Measurements" were later appended to the name, the council's initials remain NCRP.

[108] International Commission on Radiological Protection

RICHMOND: Trying to find out mechanisms, really. How can you take advantage of what happens normally physiologically to accelerate the loss? We looked at diamox and different drugs and things. Of course, as I mentioned earlier, the most effective is Prussian blue, ferric ferrocyanide.

I guess the other things that are intellectually pleasing, when I look back, are the work we did in comparative studies—for example, tritium. Most of that was published in early 1960s, particularly 1962. One was in the journal, I think the *Journal of Comparative Physiology*. That's still being cited today.

I don't know if you know this, but most scientific publications, more than half, are never cited, never, even once. Of those, less than 50 percent, that are cited, most of them are cited within the first year or two. Never after. So papers that are cited 10, 20 years later are very, very rare. So it's nice to pick up something and see a document that you published in 1962, still being referred to.

I didn't mentioned this, but because of the undergraduate and graduate work I had in comparative physiology, we got very interested in studies about anomalies: a camel, for example, being able to store water in fat. When we were working on tritium at Los Alamos, some of the people were going up to [the] Nevada [Test Site] for weapons tests. So we asked them to get us some desert kangaroo rats. So they brought them back to [the] Lab and we did tritium studies on them. If you plot the turnover time for mammals [as a function of species body weight], you get a beautiful straight line, with man being like 10 or 11 days and rodents being a half a day or so. But lo and behold, this little rodent that lives in the desert looks like a person [in terms of its tritium turnover rate]! The turnover time is 10 or 11 days, just like [a] person. That paper gets referred to very often, too, in the literature.

The other thing that is of the interest [is] that hot-particle [research] that we've done on the densities. Most of that [interest] seems to be, "Why did you use people?" Recently, the last couple times that Russian satellites reentered [the earth's atmosphere with onboard nuclear] power reactors, they referred back to the work we did on the transit of those particles [through the body if inhaled].

The other one is the work that we did with ferric ferrocyanide way back many years ago; you had mentioned it has been used even with animals, post-Chernobyl. This is a March 1994 publication from the Brazilian accident, involving Prussian blue treatment. And cited in there is the work that we did back in the '60s, 1968, "Acceleration and Turnover of Internally Deposited Radio Cesium," and the "Diagnosis and Treatment of Deposited Radionuclides," which was a Hanford symposium. It's always nice to see, as a scientist, something that you published decades ago still being referred to.

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FISHER: I was wondering if this was the Goiania accident in Brazil.[109] And for the benefit of the transcriber, you've just referred to the paper in *Health Physics* entitled, "Cesium-137, Internal Contamination Involving a Brazilian Accident and the Efficacy of Prussian Blue Treatment" by Dunstana Melo, Joyce Lipstein, Carlos Olaverio, and Louis Bertelli, published in *Health Physics,* Volume 66, pages 245 to 252, 1994. I know these authors, in particular Louis [Bertelli] and Dunstana Melo. You served as an advisor to the Brazilian Government after this accident?

RICHMOND: No. I had no involvement. The Brazil connection was many years ago. Someone from ERDA or AEC asked me to visit Brazil many years ago; I don't even remember when. The Brazilians were building three reactors at Point Angra, south of Rio. And the U.S. changed its policy about fuel reprocessing[, banning U.S. companies from building any more reactors in Brazil]. One [reactor] had been constructed by a U.S. company, and they were bringing in the German and French to build the other two. And the Brazilians still wanted help on doing analyses of what would happen from reactor accidents. I was asked to go down and see what they had and what they were planning. That was one trip, and I was a consultant on that long before this accident.

FISHER: I was thinking you were involved in the review of the Goiania.

RICHMOND: In fact, I was pleased when I visited the completed reactor of Angra that they had ferric ferrocyanide in vials in case of an accident.

HARRELL: There was extensive Oak Ridge involvement in that follow-up at the REAC/TS[110] center.

RICHMOND: Oak Ridge Associated Universities, right.

FISHER: I wondered if you had been involved because of your expertise in cesium metabolism and removal. So maybe I should ask you, why weren't you involved in the Goiania accident investigation?

RICHMOND: I don't know.

FISHER: You would have been a likely choice, I would think.

RICHMOND: Well, that was a long time ago. I was trying to run a program here. Keep people employed. But I wasn't asked, either.

FISHER: You were involved in program development and working to keep Oak Ridge National Laboratory well-funded between 1974 and 1990. That was your major responsibility, and perhaps a lot of that had to with your

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[109] a community 15 miles southwest of the city of Brasilia. In September 1987, a radiotherapy unit with a cesium-137 source, that a clinic moving to new quarters had left behind without notifying authorities, was found by itinerant salvagers. They removed the radioactive source from the machine, and, through a combination of circumstances, the cesium-137 was spread through the community. The incident contaminated 249 people, four of whom died.

[110] Radiation Emergency Assistance Center/Training Site of the Medical and Health Sciences Division, Oak Ridge; maintains a registry for radiation accidents and trainers to come and learn how to treat people who may have been exposed to radiation

knowledge of the way the Department of Energy operates and the way projects are funded.

RICHMOND: Part, I'm sure.

FISHER: Well, we're coming near the end of this oral history interview. Have we skipped over anything in your professional career that you would like to add to what we've already talked about? Have we left anything out that you think we've missed?

RICHMOND: No, I think it's been quite comprehensive. I don't think of anything outstanding right now that I want to add.

FISHER: David?

Opinion on Investigation Into Human Radiation Experiments

HARRELL: What is you opinion of the recent interest of human radiation experiments and this whole investigation?

RICHMOND: I think you said the key word: "investigation." I have mixed feelings. One, I'm very much in favor of openness. I've indicated earlier that I feel good. I had something to do with getting that *[LA-]1151* [report], thanks to Pat Durbin's prodding, completely declassified. My problem, frankly, I'm on record, is the way this was done.

[And] I think this third cycle now was sort of: "Look what we've found! Everything has been hidden under the rug all of these years and we're bringing this out for the first time!" I think that has done a disservice to a lot of people who ha[d] been very much involved in it and aren't here [(alive)] any more to defend themselves.

I think it was the process. It was frustrating, because here someone would turn up in paper a letter about polonium—one in particular that was signed by [R.M.] Fink to Hymer Friedell[111] back in the early years, about polonium. That letter was made public, and obviously the press looked at it and they said, "[It says] Oak Ridge, you know, on the letterhead, and what was going on here that you haven't told us about polonium?"

Everyone was at a disadvantage. The press got no help. The people who released the information didn't understand what it meant. The network would get wired, and I would be talking with people like Pat Durbin and we would be trying to help people here in Oak Ridge and people all over. The press [is] very much interested. Two things. One is [that] a lot of materials that were supposed to be released for the first time had been released for a long time, not just in technical journals.

But my favorite example is that 1974 press release on the plutonium injectees, six pages. I'm sure you've seen that. Most people weren't aware of that. They'd forgot that, but at the time it had a very wide dis-

[111] For the transcript of the interview with Friedell, see DOE/EH-0466, *Human Radiation Studies: Remembering the Early Years; Oral History of Radiologist Hymer L. Friedell, M.D., Ph.D.* (July 1995).

tribution. So, the process is what I think has been frustrating, flawed perhaps. Materials are just dumped out [to the world at large with no explanation of their context].

By the way, that's why I think this [disclosure] process is going to be very valuable. Because I don't want to have to do it a fourth time sometime later on. To get the information out is one thing, but to do it in a fair manner, I think is important. People like Wright Langham, frankly, have gotten pretty much smeared over this.

The big issue is whether or not those folks [(the injectees)] really [had] short-term [or] long-term prognosis for survival. What has been lost is the foresight the man had, working with others, and many things—not just the plutonium injectees. The fact that here we've had this knowledge nationally about plutonium retention and excretion. Where there have been thousands and thousands of people involved, their safety depends, in part, upon that knowledge. All that has sort of been shelved now, and Wright's reputation, I think, has been badly blemished. I know his wife personally, and she's very upset about this.

HARRELL: She's still alive?

RICHMOND: Yes. Her name is Julie Grilly.[112] But you know, that may be because Wright was my mentor and I was close to him. So I may be responding a little emotionally about it. No one, I don't think anyone, especially scientists, have any problems with getting all this information out.

Research Community Shift in Emphasis From Basic Science to Applications

HARRELL: What would like to say about the scope of biomedical work that's being funded by the Government these days? It seems as if it's gone down quite a bit from its peak.

RICHMOND: The years [that] I was an Associate Director [of ORNL], about the only work that came from [the] kinds of things I did when I was in Washington was a program that had been ongoing for many years before, and that was the Russells' genetic work.

Over that period from 1974 until recently, what I've seen was two things. Biological work has decreased very rapidly. This is true of other Labs. The environmental work has increased. The waste remediation has been overwhelming. In fact, it's a huge part in many Laboratories.[113] That's a risk in itself, I think a long-term risk. The work has gone from more of a basic to an applied kind of activity. Current buzzwords are "technology" rather than "applied." But that trend, we've seen over the years.

[112] For the transcript of the interview with Grilly, see DOE/EH-0469, *Human Radiation Studies: Remembering the Early Years; Oral History of Julie Langham Grilly* (September 1995).

[113] For DOE's perspective on the need for a cleanup, see *Closing the Circle on the Splitting of the Atom: The Environmental Legacy of Nuclear Weapons Production in the United States and What the Department of Energy is Doing About It* (106 pages). DOE Office of Environmental Management, January 1995.

The other trend is from small individual or groups of individuals' research to large teams, long[-term], multidisciplinary [efforts]. Those trends are tough, because when we had the reduction-in-force at the Oak Ridge National Laboratory recently, a lot of people were shocked that a large fraction, third or half, probably more like a half of our senior scientists, decided to leave [(accept an early-retirement buyout)]. Of course, that hurts several ways, in future work and all. The reason is that those folks have been the hardest to keep funded in the recent years because the real basic research has been most vulnerable to being cut.

HARRELL: With all the work that you did on the nuclides and all the data you've collected, are there any areas that need to be studied more that you didn't get to? Any areas that you would like to have more knowledge on?

RICHMOND: I don't think so. In fact one of the problems we see today is that a lot of people are doing [numerical] modeling. That's important for any kind of research, but I think in the radiation business it has been overkilled. More people are now saying, in reports and publicly, that the modelers now are sort of demonstrating their knowledge of a very specific thing rather than incorporating changes that really have a basis biologically. As you may know, that's probably what happened with the lung models—now an ICRP model, an NCRP lung model—and there are more components to those models than is really necessary. I think what's happened is that the research has phased out over the years and the modelers have taken over.

FISHER: Part of the reason for their delay in publication has been their complexity.

RICHMOND: A lot of people, like Paul Morrow and others, have said it: There's no need for that complexity because we really don't understand things that much in the real world, biologically. I think basically we're in good shape, in terms of what we understand about radioactive materials internally.

HARRELL: Well, we're in good shape with this interview. I would like to thank you very much for participating and spending some time with us.

RICHMOND: Thanks for the opportunity. ❑