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HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Biochemist
William D. Moss

Conducted November 30, 1994

United States Department of Energy
Office of Human Radiation Experiments
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FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

The Department has produced a roadmap to the large universe of pertinent information: *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records* (DOE/EH-0445, February 1995). The collected documents are also accessible through the Internet World Wide Web under `http://www.ohre.doe.gov`. The passage of time, the state of existing records, and the fact that some 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 BIOCHEMIST WILLIAM D. MOSS

Conducted on November 30, 1994, in Los Alamos, New Mexico, by Dr. Darrell Fisher, a health physicist from Pacific Northwest Laboratory, and Marisa Caputo, Oral History Team Leader and Special Assistant from the Office of Human Radiation Experiments, U.S. Department of Energy.

William D. Moss was selected for the Oral History Project because of his plutonium chemistry research at Los Alamos National Laboratory (LANL). Specifically, Mr. Moss has been one of the primary researchers in the yearly studies of the Los Alamos plutonium workers. In addition, Mr. Moss studied bioassay, the metabolism of plutonium in the human body, and updated the Langham excretion functions for plutonium.

Short Biography

Mr. William D. Moss was [redacted] He received his B.S. (Chemistry and Biology, 1950) from Sterling College in Sterling, Kansas. Mr. Moss has also taken advanced courses at the University of New Mexico, Los Alamos Center. Mr. Moss joined the staff of the Los Alamos National Laboratory in 1953 and is still employed at the Laboratory. From 1953 to 1957, Mr. Moss was a Chemical Technician in the Health Division's Industrial Hygiene Division (H-5). He became a staff member in H-5 in 1958. From 1964 to the present, Mr. Moss has held the following positions within H-5: Alternate Section Leader, Bioanalytical and Chemistry Section; Acting Section Leader, Bioanalytical and Chemistry Section; and Section Leader, Bioanalytical and Chemistry Section.

Mr. Moss has focused his work on analytical chemistry related to industrial hygiene and health physics. Moreover, he has set up sampling methods for toxic materials and evaluation of occupational health hazards. Specifically, Mr. Moss has researched and published numerous articles on the medical follow-up studies of the Los Alamos plutonium workers and on the effect of plutonium on the human body.

Early Career at Los Alamos

CAPUTO: Today is November 30, 1994. My name is Marisa Caputo. I'm with the Office of Human Radiation Experiments of the Department of Energy [(DOE)]. With me is Dr. Darrell Fisher from Pacific Northwest Laboratory. We're here today at Los Alamos National Laboratory[1] to interview William Moss about his experiences at Los Alamos, and his knowledge of human radiation experimentation that occurred here.

Mr. Moss, I'd like you to begin with a quick sketch of your educational background and what brought you to Los Alamos to begin with.

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[1] Los Alamos was a key research and development center for the secret effort during World War II to create the first atomic bomb, the Manhattan Project. Subsequently, as a National Laboratory of the Department of Energy and its predecessors, Los Alamos has been a research and development center for nuclear weapon designs, high-energy physics research, and other scientific endeavors.

MOSS: My educational background: I have a bachelor's degree in Chemistry and Biology from Sterling College, Sterling, Kansas. My interest in Los Alamos stems from the use of the atomic bomb [against] Japan in August 1945. I read about that, and I read an article in *Life* Magazine about Los Alamos. [As a result,] I had an interest in Los Alamos and what was going on [there]. After graduating from college in 1950, I taught chemistry at a high school in Arizona. I met a young lady out there who, eventually, I married. Her sister lived in Los Alamos, and we [went] to visit [her]. I [applied to work at Los Alamos] in November of 1952, and I was employed in March of 1953. I took the job as a chemist in the Health Division[2] [and] was involved in bioassay[3] of samples for radioactivity. I've essentially been in that profession since then.

CAPUTO: So, for over forty years, you've been at the Lab?

MOSS: I've had diversions within that [time], but I've stayed within that same interest. I became rapidly interested in perfection of analytical procedures and the interpretation of the data, and that's where we are today.

FISHER: Who did you work for initially? And who were your professional colleagues?

MOSS: My first contact was the fellow who hired me: Morris Milligan. Harry Schute was the group leader [(H-5, Industrial Hygiene)] and Ed Hyatt was the deputy group leader. The assignment I had was to learn the analytical procedures involved in the analysis of not only radioactive materials, but organic and inorganic [materials]. It was a combined function of that chemistry group to do industrial hygiene and [radio]bioassay[4] samples. Morris Milligan was my immediate supervisor.

Early Development of Bioassay Tests to Measure Plutonium Exposure

FISHER: Can you describe some of the capabilities of your group in H-5 for plutonium[5] (Pu) bioassay at that time?

MOSS: When I came into the group they were using a procedure that had been developed by [Edwin] Russell at the Met Lab[6] in Chicago. It was called

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2 Formed in a May 1947 reorganization, the "H" or Health Division had responsibility for a much broader range of health activities than its predecessor, the Health Group (Group A-10). These responsibilities included radiological safety, health physics, and industrial health. The H Division also monitored exposures and was responsible for safety for all weapons tests conducted by the Laboratory.

3 analysis of urine or feces

4 analysis of radionuclides in the urine or feces

5 a fissionable transuranium (synthetic) element. Pure plutonium is a silvery metal that is heavier than lead and is used as a critical component in nuclear weapons. The first atomic bomb, detonated at Alamogordo, New Mexico, July 16, 1945, and the atomic bomb detonated over Nagasaki, Japan, August 9, 1945, were plutonium weapons. Because plutonium is highly toxic, concern developed early in the Manhattan Project about its potential health effects on workers involved in machining, chemically processing, and handling the material.

6 Metallurgical Laboratory, the laboratory set up at the University of Chicago during World War II to lead the secret research and development of controlled nuclear fission under the Manhattan Project. In particular, the Met Lab was responsible for the design of reactor facilities to make plutonium and development of methods

(continued...)

the bismuth phosphate procedure [for extracting plutonium]. The laboratory at Los Alamos started using that procedure in October 1949. It's interesting, the background on that. They [(the lab's researchers)] previously had used what they called the cupferron procedure.[7] That procedure was developed here at Los Alamos in the latter part of 1944 to analyze low concentrations of plutonium in urine, fecal, and tissue samples. That procedure was used from [December 1944 to October 1949]. [Wright Langham][8] developed its scientific use, including quality control and the use of spike samples,[9] and so on. The procedure had been developed [in 1944] and Langham took on the responsibility of supervising the people [using the procedure in February 1945].

FISHER: Who developed the procedure?

MOSS: It was developed by a team of people, headed by a fellow named Don Mastick. It's an interesting story about Don. He was involved in a serious plutonium exposure in August 1944, in which 10 milligrams[10] of plutonium blew up in his face. He ingested some unknown quantity of plutonium. He was a young scientist, who had just come to the project. The division leader for chemistry, a fellow by the name of Joe Kennedy,[11] had previously worked with [Glenn T.] Seaborg [12] out of Berkeley[13] and was a codiscoverer of plutonium in 1941 along with Art Wahl.[14] Art Wahl and Kennedy had come to Los Alamos and Seaborg, of course, was working back in Chicago [at the Metallurgical Laboratory], when this accident happened in August [1944].

Kennedy went to Hempelmann,[15] who was leader for the Health Group, and said they were quite concerned about this person's exposure [be-

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(...continued)

for chemical separation of uranium, plutonium, and fission products from irradiated nuclear fuels.

7 a chemical extraction procedure using organic solvents to extract plutonium from a dissolved ash solution

8 Langham, regarded at the time as "Mr. Plutonium," led the Health Division's Radiobiology group from 1947 until his death in 1972.

9 samples of urine or feces to which is added a known amount of plutonium to test the efficiency of plutonium recovery during the bioassay analysis process

10 A milligram is one-thousandth of a gram; there are about 28.35 grams in one troy ounce.

11 Dr. Joseph Kennedy, Ph.D., U.S. chemist. After working with Dr. Glenn Seaborg at the University of California, Berkeley, prior to World War II, Kennedy was chosen by J. Robert Oppenheimer to lead the Chemistry Division of the Manhattan Project. After the war, Kennedy taught at Washington University in St. Louis.

12 U.S. chemist, born 1912, professor of chemistry at the University of California, Berkeley, discoverer of several heavy elements and Nobel Prize recipient in 1952. Served as Chairman of the U.S. Atomic Energy Commission.

13 University of California, Berkeley, site of groundbreaking early research in nuclear science and location of Lawrence Berkeley Laboratory

14 Dr. Arthur Charles Wahl, Ph.D., U.S. chemist. Codiscoverer of plutonium with Glenn Seaborg. Worked in the Manhattan Project in World War II, then taught at Washington University in St. Louis.

15 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.

cause] there was no method of measuring the concentration that might be in his body after ingestion of an unknown amount of plutonium. Ten milligrams, at that time, was a lot of plutonium. By August 1944, Los Alamos had received only 51 grams of plutonium. This accident was serious enough that they wrote a memo stating they wanted some [measure of] protection for their people.

Developing Occupational Exposure Limits

FISHER: Wasn't the early permissible intake limit for plutonium about 5.0 micrograms [millionths of a gram]?

MOSS: That was the tentative level established by the Met Lab in Chicago. There is some suggestion, from reading the notebooks of the people who were employed in the [Los Alamos] Health Group in July and August 1944, that Hempelmann was quite concerned about [the] 1.0-microgram burden. The absolute certainty of that is not documented. As a result of the accident, Oppenheimer[16] authorized Hempelmann to assign people to develop a more sensitive method for measuring plutonium [in excreta]. The Met Lab had suggested a method for measuring 5-microgram body burdens. In looking at [researcher] Ann Perley's[17] notebook ([she was] the person that was assigned to look at the Chicago procedure) she noted that Hempelmann wasn't satisfied with 5.0-microgram [detection]. Hempelmann was interested in measuring 1.0-microgram [or less plutonium burden].

FISHER: This was in the fall of 1944?

MOSS: This is August '44. [The first reference to a 1.0-microgram Pu burden is contained in a March 26, 1945 memo, Hempelmann to Oppenheimer.]

FISHER: Hempelmann indicates in other memos that a procedure is needed to accurately measure plutonium [in excreta] for purposes of occupational radiation protection.

MOSS: I'll give you the background on that. I got this from Louis Hempelmann himself. He was the M.D. who had come to Los Alamos in April 1943. You probably know the background of his selection, so it's not important here.

FISHER: Go ahead.

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[16] J. Robert Oppenheimer, U.S. nuclear physicist (1904–67) who was chosen by General Leslie Groves to direct the development and construction of the atomic bombs at Los Alamos

[17] Perley, a biochemist, had worked for Hempelmann at Washington University in St. Louis, before Hempelmann went to Los Alamos; she died in 1995.

MOSS: Hempelmann had previously worked at the Berkeley [(Lawrence Radiation)] Lab in association with Joe Hamilton[18] on neutron therapy.[19] He [(Hempelmann)] went from Berkeley to Washington University in St. Louis, where he was working on neutron therapy with their cyclotron. [In 1942], they were using the cyclotron[20] at St. Louis to make plutonium by bombardment of [uranium compounds].

FISHER: Before 19...?

MOSS: This is 1942. So they're making plutonium, and the bombarded uranium was shipped to Chicago and Seaborg extracted the plutonium.[21] Hempelmann had contact with Hamilton and Robert Stone[22] at Berkeley, [and] he probably met Seaborg when he was out there. He [also] knew Oppenheimer. He knew these people quite well. He [(Hempelmann)] was recommended for the [Los Alamos] Health group by John Lawrence,[23] the brother of Ernest.[24]

He came here in April of 1943 and was given the assignment then to provide [health] protection for Los Alamos people. The matter of plutonium never really came up in any of the records of the health reports written by Hempelmann until 1944, [when] plutonium appeared on the

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18 an M.D. who worked at Crocker Laboratory, then the site of a 60-inch cyclotron that he operated to produce radioisotopes in support of research and some medical diagnosis and treatment. Crocker was part of the Lawrence Radiation Laboratory, later renamed Lawrence Berkeley Laboratory.

19 therapy of cancer using an accelerator to produce a neutron beam of radiation

20 an accelerator in which particles move in spiral paths in a constant magnetic field

21 Seaborg was a meticulous diarist whose detailed records have been edited and supplied with accompanying notes by a professional scientist and two professional historians of science. See Ronald L. Kathren, Jerry B. Gough, and Gary T. Benefiel, eds.; The Plutonium Story: The Journals of Professor Glenn T. Seaborg, 1939–1946; Columbus, Ohio: Battelle Press; 1994; ISBN 0-935470-75-1; 920 pages. The book presents Seaborg's account of the discovery of plutonium and the urgent activities to unlock its secrets and enhance its productivity to the levels needed to build an atomic bomb. It provides a step-by-step description of the scientific activities and the thought processes of Seaborg and his team throughout the war years and provides insight into the operation of the Manhattan District and of the scientists who played an important role in its functions.

22 A pioneer in radiation therapy, Robert Stone, M.D., had conducted human radiation studies before World War II. He was an early researcher at the Lawrence Radiation Laboratory and became a major figure in radiobiology research. In 1942, while chairing the Department of Radiology at UC San Francisco's medical school, Stone was recruited to lead the Medical Division of the Manhattan Project, overseeing all biological, medical, and radiological protection research. Accordingly, he moved to the University of Chicago, where he served as Associate Director for Health under Arthur Compton. In the 1950's, after serving in the Atomic Energy Commission, Stone returned to his post at UCSF as head of the Department of Radiology. Under Stone, UCSF acquired a 70-MeV synchrotron for conducting therapeutic research.

23 Dr. John Lawrence was Director of the Division of Medical Physics at the University of California, Berkeley. He operated a clinic at Donner Laboratory, where he treated leukemia and polycythemia vera patients with radioactive phosphorus.

24 U.S. physicist, 1901–58; a pioneer in nuclear physics who built and operated (with M. Stanley Livingston and Milton White) the first cyclotron in 1930 on the Berkeley campus of the University of California; established the University of California Radiation Laboratory in 1936 and served as its director until his death. His ingenuity and drive made the Berkeley-based Radiation Laboratory a center of nuclear physics in the United States.

scene.[25] At that time [1944], from the memos that Hempelmann wrote, the philosophy of the Met Lab in Chicago was to use blood profiles as a means of determining radiation exposures. It became obvious, after Hempelmann visited Chicago [in April 1944], that [use of blood profiles] was not a satisfactory method for measuring low-level radiation exposure. He came back [to Los Alamos] and wrote a memo to Kennedy recommending that they take nose swipes [for evidence of Pu inhalation exposures].

FISHER: Nose swipes were probably totally unacceptable [(lacking in sensitivity)] for assessing internal [low-level] burdens of radionuclides.

MOSS: It slowly evolved that [nose swipes] weren't adequate for that. There wasn't much plutonium here. The first quantity of plutonium, in milligram quantity, was delivered to Los Alamos in January [1944]; it was 6.0 milligrams.

Exposure Accident Creates Need for More Sensitive Testing

MOSS: By the end of May, there [were] perhaps 10 grams [of Pu] here. The problem [of controlling Pu exposures] was developing rapidly. Hempelmann was using nose swipes.

There was a serious accident that occurred on May 26, 1944, in which two people disregarded safety rules, opened up a container of plutonium, and dispersed plutonium dust within the room. They took nose counts on those people, and they decided that one of them had had a [serious Pu inhalation exposure]. They removed him from further work with plutonium. That was the beginning of really cracking down, if you will, and following [safety] rules. The explosion occurring in August of 1944 sort of climaxed the whole situation. Los Alamos was then given permission [by Oppenheimer] to develop a more sensitive [urine] method than the method that had been suggested for use by the Met Lab in Chicago [for detection of less than 1-microgram burdens].

FISHER: For historical interest, can you explain the cause of the explosion in August of 1944, the chemical process?

MOSS: [I don't know the cause of the August 1944 accident]. The two people [who were involved in the May 1946 exposure is documented]. I talked to the chemist, the metallurgist. He's part of this group of 27 men that Langham, [Payne] Harris, and Hempelmann put together [and would] follow [in future years].

FISHER: Twenty-seven?

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[25] Plutonium first became available at Los Alamos in small quantities for biological testing and chemistry studies in January 1944; large amounts were available in 1945 from Hanford for the first atomic weapons.

Long-Term Follow-Up Studies and the UPPU Club

MOSS: Twenty-seven men originally selected for the UPPU Club. Are you familiar with that? You urinate, PU [(you pee Pu)]. That was why they called that [the UPPU club]. Those 27 men were selected on the basis of their occupational exposure [to plutonium], mostly by inhalation. They have been followed regularly for some 42 years. I guess the last [DOE] funding didn't include money [for additional studies].

FISHER: Those are the follow-up people Dr. Voelz[26] has mentioned and reported on.

MOSS: Yes. The number-one person on that list of 27 or 26 names, was the fellow exposed on May 26, 1944. The fellow that was exposed in August of 1944 wasn't included in those 27 because he had an ingestion exposure, not an inhalation [exposure]. The 27 men that were picked were predominately inhalation exposure people.

Let me go back to the May exposure of 1944. These two people were working at midnight without authorization from their supervisor. The chief chemist was a metallurgist who previously worked at Berkeley [(at the University of California Radiation Lab)], got his Ph.D. [from University of California at] Berkeley, moved to the Met Lab with Seaborg, and then was transferred to Los Alamos. He knew what he was doing.

The conversation I had with him was that VIPs[27] were coming to Los Alamos the next morning. He thought he had an idea for making [Pu] metal reduction.[28] Previous to this they hadn't been successfully making a metal reduction of plutonium.

He and his technician began working at midnight, and they were able to reduce plutonium salt to a metal. They got a metal button [(disk)] using what they call [a reduction] bomb. A bomb is a sealed steel tube, and you put the [Pu salt] inside and weld it shut, put it in the furnace, and heat it up. They proceeded. When the reduction was done, they opened up this metal container and plutonium dust was dispersed into the air. But they did have a button of plutonium metal. They put that on their supervisor's desk, Cyril Smith. He went to Battelle[29] [after the war].

FISHER: Can you repeat that again?

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[26] Dr. George M. Voelz, M.D., conducted health studies of the Manhattan Engineer District plutonium workers, inhalation studies of radiation workers, and environmental surveys of various Department of Energy sites. He was at Los Alamos from 1951 to 1957 and served as Idaho Operations Officer for the AEC from 1957 to 1963. See DOE/EH-0454, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. George Voelz, M.D.* (May 1995).

[27] abbreviation meaning "very important person"

[28] conversion of plutonium ore to a metallic state by driving off nonmetallic elements; smelting

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

MOSS: Cyril Smith. He went to Battelle and was involved in the operation of Hanford[30] research [on Pu]. He was the metallurgist in the [Los Alamos] Chemistry Division. [Dr.] Ted Magel, the fellow I'm talking about, opened the bomb and was exposed to very high concentrations [of Pu dust].

FISHER: This means that some fraction of the plutonium went to metal, but other fractions stayed [as oxide powder].

MOSS: You call it crud [(oxidized Pu)].

FISHER: But other fractions were oxidized?

MOSS: [Yes.] Oxidized [Pu]; you also had residual material. They did actually make the first [Pu] metal reduction from that. That's pretty well-described in a book by [Cyril] Smith on plutonium metallurgy. Nevertheless, they [(Smith and Magel)] thought they were [not] seriously exposed.

Hempelmann thought they were. They [(Hempelmann and Smith)] tracked them down and found them in a bar in Santa Fe celebrating their success. When they took the nose swipe the next day, the one fellow, Magel, had a high concentration of plutonium in his nose. They removed both of these people from further plutonium work.

Then the story goes like this: Nagel, I talked to him directly. I said, "According to the records, you left Los Alamos in August of 1944." He said, "That's correct. After I was exposed to plutonium, they wouldn't let me work with it anymore, so they assigned me to another job, which was mundane as far as I was concerned. So, I went directly to Oppenheimer." (He knew Oppenheimer.)

He said [to Oppenheimer], "I'd like to leave the project. I want to go back to Massachusetts." Oppenheimer gave him permission to leave. So he left in August of 1944, and he took his technician with him. That was really the first serious exposure.

The second was in August 1944. The fellow was operating equipment; it blew up in his face and he ingested plutonium. So they took him off plutonium work and assigned him to Hempelmann. He was put in charge of developing a more sensitive plutonium urine-analysis method.

That puts you right up to January of 1945. They had the cupferron procedure developed [by January 1945], and they were receiving Hanford plutonium. The question came up as to what can we do about measuring the concentration in people's urine based on the high nose concentrations.

Wright Langham Joins the Los Alamos Health and Safety Group

The story goes like this now: Hempelmann was giving lectures within the Laboratory about the risk of working with plutonium and the safety procedures to be followed in handling plutonium. During the course of

[30] the Department of Energy's 520-square-mile former site for plutonium production, located near Richland, Washington

one of these meetings, Hempelmann was giving the lecture, and Wright Langham, who had transferred to Los Alamos in August 1944, got up and made some comments to the effect that he didn't appreciate the biomedical research being done at Chicago. He thought it was misdirected. Hempelmann told me this story. He said [to Langham], "Would you mind talking to me afterwards?"

At this time, they were looking for somebody to head up the biomedical research program at Los Alamos. Hempelmann offered that job to Wright Langham at that time. Louis said [later], "We had some heated discussion about which direction this should go, but we always kept ourselves on an even level of friendliness." He persuaded Langham to join the medical group and to supervise the use of the plutonium [cupferron] procedure, including recognizing contamination problems, quality control measures, overspikes,[31] blanks, and so on.

Langham started his job in February of 1945. I have a copy of his notebook, and it says in there, the date is February 1945, "The first thing we're going to do is to repeat Hamilton's plutonium injection studies in rats." The purpose of that was to study the metabolism of plutonium as a function of dose in groups of rats [and to obtain excretion rates as a function of time in days past injection].

Langham Analyzes Results of Previous Plutonium and Polonium Injection Studies

FISHER: By dose, you mean in terms of injected dose?

MOSS: Injected dose, yes. They wanted to be sure that the metabolic behavior was independent of the dose. The Chicago people, as you probably know, their function was to study the acute effects of plutonium [in animals]. [University of] Rochester [(Rochester, New York)] people were studying the acute effects of radiation damage from polonium. At Los Alamos, Wright Langham said, "We need to know whether the [Pu] metabolic process is going to be identical for different doses."

FISHER: In other words, "Is a low-level exposure metabolized in the same way as the high-dose exposure?"

MOSS: [Yes.] I don't have in front of me the doses that they administered, but [they ranged from a low dose] up to a very high-level dose in rats [(0.032 microgram of ^{239}Pu to 52 micrograms of ^{239}Pu)]. The data they got from using the cupferron procedure demonstrated that the metabolic behavior was independent of the dose. They were satisfied. By this time, they had people working with the kilogram [(2.2 pounds) quantities] of plutonium and workers were being exposed, as evidenced by [high] nose counts and urine analyses [(February 1945)].

FISHER: [Exposed] at the Met Lab?

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[31] the addition of too much plutonium to urine and fecal samples, leading to low-level contamination of laboratory equipment

MOSS: Right here at Los Alamos. The notebook record of our [urine] bioassay records begins in February of 1945. The first sample they got on people who were highly exposed, was in connection with the use of the 0.01-percent-per-day [Pu excretion rate] suggested by the animal studies out of Chicago and Berkeley. [These data] suggested there were people who were exceeding 1.0-microgram body burdens.

Langham took a look at this [data]. He was a plutonium chemist. He recognized there was a great potential for sample contamination. He got authorization then to [place employees] in a hospital ward [for collection of urine samples under controlled conditions].

Langham Enacts Stringent Research Controls

FISHER: In Los Alamos?

MOSS: In Los Alamos. Scrub down, wear surgical clothing, and spend a twenty-four hour period in the hospital collecting urine samples under sanitary conditions. The previous [urine] samples collected at home showed a comparison; a before-and-after [effect]. There was a dramatic drop in the plutonium concentration in a selected group of about six or seven people [used for this study]. This had quite important implications later on. I'll touch on that right now.

I was involved in setting up the bioassay lab in Madrid [Spain] in 1966, as a result of the Palomares Incident, where two of the bombs [(*not* nuclear)] exploded [above] the village of Palomares, Spain.[32] The first urine samples collected in Palomares, were analyzed in Madrid; half of the earlier samples were sent to Los Alamos and were analyzed there. [The Pu concentrations] were extremely high. Right away, Langham recognized the problem. He said, "You can't collect samples in the environment in which there's potential [Pu] contamination." And there was [extensive contamination] in the town of Palomares. The people in Palomares were brought to Madrid, and put in a hospital. Collection occurred under sterile conditions and the results were all zero.

Langham's foresight in 1945 paid off in 1966. He recognized that right away. They maintained at Los Alamos what they called the "Health Pan Ward," thereafter from 1945 until 1952. The [Los Alamos] plutonium [accident victims] were put in the hospital. They spent 24 hours there, and

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[32] On January 17, 1966, a U.S. Air Force B-52 bomber and a KC-135 tanker plane collided while attempting an aerial refueling at 30,000 feet over the Mediterranean Coast of Spain. The B-52 was carrying four thermonuclear weapons on an "airborne alert" mission (then a routine practice). The four bombs fell to earth. One landed in a dry riverbed, from which it was recovered intact; another landed in the sea. The high-explosive trigger on each of the other two bombs detonated. Their safety devices performed as designed and prevented a nuclear explosion; however, the nuclear materials package in each bomb was blown apart by the chemical explosion. Plutonium was scattered over the village of Palomares and surrounding farmland. A massive joint effort by the United States and Spain was initiated to decontaminate the land area affected, and the U.S. Navy successfully recovered the bomb lost at sea after an intensive search. The decontamination effort included monitoring the population affected for exposure to radiation.

[the doctors] collected their [24-hour] sample under controlled conditions. [That way, they] minimized the possibility of sample contamination.

I'll get back to where I was now; the procedures used for the human injection studies. The study was set up and administered by the Medical Corps of the U.S. Army at Oak Ridge under [Colonel] Stafford Warren.[33] Los Alamos had pushed for some human studies. [(Memo of March 29, 1945, Oppenheimer to S. Warren)].

December 2, 1944 Memo Proposes Human Injection Study

FISHER: By Los Alamos, are you talking Hempelmann or Oppenheimer or Wright Langham?

MOSS: There's nothing really direct that can pin down that kind of an answer, that kind of a statement. There was a committee of people that was formed, and the only record I have is a memo dated December 2, 1944, in which the members of this committee were [listed]. Stafford Warren was, of course, the chairman of the committee. Hempelmann was a committee member. [Joe] Hamilton was a committee member. A fellow, William [F.] Bale, [a biophysicist] from [the University of] Rochester, was a committee member. They met and discussed a human study in which they would inject plutonium into humans and compare the data with the animal experiments. I don't know if you have that memo or not.

FISHER: I think we've seen it.

MOSS: You have a copy of that; we may have gotten it to you. That's the only record in which they had a committee.

There is a memo written on August 16, 1944, by Oppenheimer to Hempelmann, in which he says he's authorizing Hempelmann to set up procedures for a more sensitive urine method. Oppenheimer said, "As to biological studies, which may involve human experiments, we'll discuss that on a later date when you've developed an analytical method." You've seen that memo, I'm sure. That was the clue that was discussed in August of 1944. But the memo of December 2, 1944, is the only evidence that a committee was formed to [plan the plutonium injection studies].

The other documents inform us that Hamilton was involved, and it would be his responsibility for the study involving injection of plutonium-238 [in animals], to compare the toxicity of plutonium radiation and the toxicity of plutonium metal. There were those two concerns. It was the toxicity of the two things: were they independent or not? We do not have evidence that this committee met in December in 1944 and discussed setting up human experiments [but events suggest that they did meet and plan Pu experiments].

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[33] a professor of Radiology at the University of Rochester (Rochester, New York), site of research involving plutonium and human subjects. Dr. Warren worked on the Manhattan Project in Oak Ridge, Tennessee, as head of the medical section and headed an Intramedical Advisory Committee. After World War II, Dr. Warren became dean of the University of California, Los Angeles, Medical School.

Polonium Studies at Rochester

MOSS: The other thing that's quite important in this, is that the matter of polonium[34] [(Po)] had come up before plutonium appeared on the scene. You may be familiar with that, the studies back at Rochester. They did some five or six people; injected four people and gave one person a liquid [containing Po]. The procedure of doing human studies involving radioactive materials within the Manhattan Project had already been established, apparently under Stafford Warren, and these experiments [using polonium-210] were conducted at Rochester [starting in late 1944].

I don't have any evidence that these people sat down and Stafford Warren said "We'll do the plutonium at Rochester also." But there is some suggestion that Robert Stone might have entered into the picture and said, "Okay, Dr. Warren, I'll authorize the Chicago plutonium injections in humans, and Hamilton will take care of the plutonium-238 experiment at Berkeley." I don't have any evidence of that, but that line seems to be written in between the official memos [we have].

Los Alamos Analyzes Urine and Fecal Samples for 1945 Oak Ridge Injection Study

MOSS: Los Alamos, in March of '45, pushed to have human data for comparison with the animal data. Under Stafford Warren's authorization, personnel at Oak Ridge [injected] the first human with plutonium-239 on April 10, 1945.[35] Los Alamos offered to do the chemistry on urine and fecal samples and had them shipped [to Los Alamos] for analysis. It's apparent that because Los Alamos had developed a sensitive analytical method, which was capable of measuring less-than-1.0-microgram body burdens, Los Alamos was the ideal place to do the plutonium chemistry.

That seems to be the story of how Los Alamos got involved, and how they pushed to have it done so Los Alamos could have some comparative [animal and human] data. Los Alamos people were definitely being exposed, and there was no way of assessing the concentration [of Pu in the body other than against the animal data excretion rate suggested by the Met Lab, namely 0.01 percent per day in urine.]

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[34] a radioactive metallic element, chemically similar to tellurium and bismuth, that emits an alpha particle (a helium atom) to form an isotope of lead

[35] The event and the circumstances leading to it are described by Karl Morgan under "Plutonium Injection Studies at an Oak Ridge Military Hospital (1945)" in his oral history transcript (DOE/EH-0475, June 1995).

Decisionmakers Involved in Authorizing Human Injection Studies

FISHER: So, it could have involved Oppenheimer?

MOSS: It couldn't have been done without Oppenheimer knowing.

FISHER: So, that leaves Hempelmann and Wright Langham at Los Alamos as perhaps working with Stafford Warren, and maybe even Dr. Friedell?[36]

MOSS: There's no question about it. Yes.

FISHER: Because we know that Dr. Friedell came to Los Alamos in March of '45, two weeks prior to the first injection in Oak Ridge. Apparently that meeting at the end of March had something to do with coordinating the plutonium injections.

MOSS: And the urgency assigned to it? Yes. Correct.

FISHER: Do you remember talking with Wright Langham about the plutonium injection studies?

MOSS: I never did. Like you say, it's quite apparent that Hempelmann was on the committee that discussed these experiments. He was feeding information to Los Alamos people, saying that the committee, under Stafford Warren, was considering human studies. Hempelmann knew this and went to Oppenheimer [in March 1945]. Oppenheimer wrote the letter to Stafford Warren encouraging human studies. Of course, you know the background; that the animal data was variable. Higher concentrations [of Pu] came out [in urine] in different species.

Animal Injection Studies Prove Inconsistent Across Species

FISHER: You mean there was an inconsistency among species?

MOSS: [Yes, there was an] inconsistency among species. The other thing that's quite important to remember here [is this]. The experiments started on the animals in, probably, March of 1944. Hamilton and the people in Chicago picked up on it right away, and they did some rabbit experiments [in Chicago]. The rabbit experiments were high-level: milligram quantities were given to the rabbits. The Berkeley [studies], under Hamilton, were 15-microgram doses. There was a question about whether [the toxicity of Pu was due to the chemistry of the metal or to its radiation]. According to Ann Perley's notebook, the chemist that was assigned to investigate the Chicago procedures in August 1944, the Chicago people said that the [amount] 0.01 percent per day [excreted in urine] appeared to be a reliable lower level for excretion of plutonium in animals, in rabbits. It specified rabbits.

It's important to recognize, this is considered to be the minimum amount [excreted per day]. The animal [data] may have actually been a little bit higher than that, at 0.03 [percent], maybe even 0.06. [There

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[36] For the transcript of the January 28, 1995 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).

were] only two or three rabbits, so they didn't have a lot of data. They took the minimum level, extrapolating the 0.01 percent per day to the 5.0-microgram level and achieved detection for 70 DPM [(decays per minute)] of plutonium per day if the 0.01 percent was the true value of the lower level. They offered what they called [the] anion exchange procedure, which analyzed a 50-milliliter volume of urine directly. The logic was that, with the 50-milliliter samples, you could measure maybe two or three counts of plutonium and you could extrapolate that back to where that [measure] represented a 5.0-microgram body burden.

Los Alamos apparently took the position that they were interested in measuring less than the 5.0-microgram that I mentioned earlier. Hempelmann at this time told his people, "Let's look at another procedure, to see if we can improve on that." His people came back and said, "With this procedure, we'd have to analyze two or three days' worth of urine, with our instrument backgrounds being what they are."

Langham and the Development of the Cupferron Procedure

MOSS: That leads up to the decision made to develop a more sensitive method. Los Alamos developed the cupferron procedure, which was ready for use by January 1945. Langham was always given credit for developing that procedure. He was a consultant to the people that were developing the procedure [in 1944]. I've looked in several documents that Langham had written prior to his involvement in the Health Division, and he had used the cupferron procedure for analytical procedures [in connection to research on Pu purification in 1944].

FISHER: It does appear that he took some credit for that new procedure.

MOSS: He wrote the procedure up [and listed the people involved]. There were a number of people that worked on the procedure. He published that document in 1947 in [LAMS-603, *Determination of Pu in Human Urine*]. He was the author.

FISHER: Earlier I asked you this question: Do you remember talking with Wright Langham about the plutonium injection studies?

MOSS: No, I never talked to Wright directly [about the human Pu studies]. You realize I came in 1953. We never talked directly about the injection cases. I had records on key people who were excreting quantities of plutonium that were easily detectable. I had graphed these [data], and I had showed them to Dean Meyer, who was the Health Physics group leader. He said, "That's very interesting."

About this time in 1957, we adopted the Hanford NTA[37] method. Langham went to Mo [(Morris)] Milligan, who was my supervisor, and said, "We want to collect some more samples from the 27 UPPU people." They got the containers together, and shipped them. We got the urine samples back, and analyzed them by the NTA method. It became

[37] Nuclear Track Alpha, a method developed by Jack Healy for plutonium measurements

quite apparent that the excretion rates on those people were not following the so-called Langham power function.[38] They recognized early in the game that they weren't following the function that had been described [by a single-term power function model].

Use of the Bismuth Phosphate Test

FISHER: That a new mathematical function needed to be developed?

MOSS: They talked about that. In 1949, there was a group of people working in the bioassay group under [Mo] Milligan. There were four people analyzing the urine samples at that time. They took a turn each week to analyze groups of samples. The cupferron procedure used a color end-point indicator. That end point was critical for extracting plutonium [from the ashed urine sample solution]. Two of those four people were color-blind. I talked to the fellow that was running this procedure at that time. You know Ken Shillern; he's in Albuquerque. I called him up and I said, "Ken, why did you change [the Pu] procedure in 1949 from the cupferron to the bismuth phosphate procedure?" He said, "Very simple: two of us couldn't see the color end point!" So, they talked them into switching over to the bismuth phosphate. I called Mo up—he lives in California—I said, "Why did you change to the bismuth phosphate procedure in 1949?" He said, "We could get more samples analyzed." I said, "What about the color-blind people?" He said, "I never heard of that."

Once they got this bismuth phosphate procedure [in 1949], they also got better instrumentation for measuring lower concentrations [of alpha activities].

Long-Term Follow-Up of Two Rochester Plutonium Injectees

MOSS: At that time—this is now 1950—Langham is interested in getting additional samples from the injected people. He knows that two of those people are still living. So, they had a series of correspondence between Langham and the people at Rochester ([Samuel] Bassett was there), saying, "Can you locate these people to get some more urine from them? We've got a mathematical model that has been developed, the power function model, that describes the excretion over the first 138 days. We want to get some additional points."

He's saying that Mo Milligan, who was in charge of the analytical procedure, had a more sensitive method, and we think we'll be able to measure [Pu alpha activity]. He does not say this, but it is obvious that, based on the power function model, the concentration will be quite low. So you need a more sensitive method. They were able to collect a series of four 24-hour samples from two of the previously injected people from the Rochester 1945-to-1946 series. Those urine samples were shipped to Los Alamos and analyzed here [in the summer of 1950].

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[38] Langham power function curve: $Y_u=0.23X^{-0.77}$, where Y_u = percent of injected amount per day, X = time post-injection (days)

FISHER: Did you do those analyses?

MOSS: No, I wasn't involved. This was 1950. [I wasn't] here [then]. The results were again analyzed by three of the four analysts [responsible for using the bismuth phosphate procedure in 1950].

FISHER: Since 1946.

MOSS: This is 1950. The [humans were] injected in 1945 to '46. Five years later, they [obtained] some more samples from the same people. The results [obtained in 1950] were consistent with the [138-day] power function [model] prediction. They modified the parameters of the power function [model] and came up with a five-year power function model. That essentially puts you right to 1950. The people at Los Alamos were anxious to publish the human injection studies and they had [already] written the text [based on the 1945-to-'46 data but] decided they needed some more data points. Do you know Payne Harris?

FISHER: No.

MOSS: Payne is one of the coauthors of the famous plutonium injection studies publication from 1950 [(*LA-1151*)].[39] Payne comes into the picture in 1949. Wright [Langham] had asked him to work on the mathematical development of a model to describe plutonium excretion.

Payne visited here [in 1994]. I talked to him twice. I asked him about the circumstances of how they used the power function. He said, "We tried everything, including other mathematical models, and it looked like the power function was the most logical." I said, "Were you aware at that time that [William] Norris[40] at Chicago had applied a power function to describe radium excretion?" It was published in 1948 and was given in a Cold Spring Harbor meeting. I have a copy of the publication. All the attendees at the meeting [are listed], and nobody was there from Los Alamos. Payne said, "No, we never realized that anybody had used the power function to describe this sort of thing previously." He was quite gratified that someone else had done it. I explained what we were doing and he said, "Great! Good that you're doing this. It helps clarify the problem."

Essentially, this brings us to where we are at this moment. We recognized, first of all, that the power function wasn't adequate to describe long-term [Pu] excretion. When the Rundo article came out [in 1976–1981]—

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[39] the short title for a Los Alamos report on results of research involving injection of plutonium into human subjects: W.H. Langham, S.H. Bassett, P.S. Harris and R.E. Carter, "Distribution and Excretion of Plutonium Administered Intravenously to Man." Los Alamos: Los Alamos Scientific Laboratory, LA-1151, 1950; reprinted in *Health Physics*. Vol. 38, No. 6, 1980, pp. 1031–60.

[40] William P. Norris, division of Biological and Medical Research, Argonne National Laboratory. He reviewed measurements made on Elgin State Hospital patients, and also analyzed some original radium injection solutions to determine the amount of radium administered to patients.

FISHER: John Rundo's paper?[41]

Moss Reevaluates Langham's Results

MOSS: —they obtained [urine] samples in 1972 from the same two Rochester [Pu injection study] people that were originally sampled in 1950, and got a completely different picture [from the data]. Do you remember the paper that was given at the Snowbird, Utah, meeting [in 1979]?[42] [In it,] there was some question about whether more plutonium was coming out [than predicted by the Langham model]. That's essentially when I got back into the picture. I said, "Let's start looking at the records." Langham died at the same time in 1972.

I went to the Los Alamos Military Archives Records [Center], pulled out the Langham notebooks, and looked at the analytical procedures [used]. As we reexamined this project, I went back and looked at the bioassay records for one man in particular. This fellow had serious [acute Pu] exposure in July of 1945 and has been here [(Los Alamos)] for fifty years. Looking at his [plutonium bioassay] profile, you can track the variation in his urinary excretion rate as a function of the analytical method [used]. [These fluctuations are] not so much [due to] the physiological function, but the analytical methods.

FISHER: Which has been evolving through the years.

MOSS: Which involves four or five different analytical procedures, including the first sample [analyzed] by the cupferron procedure [in 1945], the bismuth phosphate procedure [in the 1950s], the NTA method [1957 to '62], the method we used later where we do direct [Pu] precipitation [and] ashing, and now the procedure where we add ammonia hydroxide to the sample and precipitate [anion exchange, electroplate and alpha spectrometry counting with plutonium-242 tracer].

FISHER: That makes the analysis fairly complex.

MOSS: Simple. It's very simple now. As you look at his records, you can see a scatter, but you will also see a trend as a function of changing analytical methods [used since 1945].

Then of course the tracer methodology came into play about 1966. About 1969, we obtained NBS alpha standards,[43] [plutonium-]238 standards. You get a little more jump [in the data in 1969]. We can apply corrections as a function of the analytical method. Of course, you've got the cupferron procedure for the '45-to-'46 period, you've got the bis-

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41 John Rundo, "The Late Excretion of Plutonium Following Acquisition of the Known Amounts," *Actinides in Man and Animals*, 1979.

42 the Snowbird (Utah) conference on Actinides in Man and Animals, October 15–17, 1979. Proceedings edited by M.E. Wrenn, published by the RD Press, Salt Lake City, Utah. Rundo's aforementioned paper (*ibid.*) appears in the proceedings.

43 plutonium-238 alpha-emitter standards (precisely known amounts) from the National Bureau of Standards, since 1988 called the National Institute of Standards and Technology (NIST), in Gaithersburg, Maryland

muth phosphate for the 1950 period, and you've got the tracer method for the [1966-to-1994 period] At the time, the Chicago people collected the additional [urine] samples from two of the [Rochester Pu] injected people in 1972. They split the samples and sent half [of them] to Los Alamos. So, we've got two laboratories analyzing the samples, and they are in absolute, complete agreement.

The information I'm writing up now is the correction of the [Rochester] injection data as a function of evolving analytical methods. It helps clarify the record. It also tells us that the data collected in 1950 [from both the Pu injected subjects and occupational exposed personnel is in error].

FISHER: For the reasons that?

MOSS: The analytical method that was applied to those samples had very poor quality control measures. There were four analysts and a new procedure that hadn't been thoroughly checked out. [Also,] this is the answer they wanted to get, when they [(Langham, etc.,)] really got the answer that was close to what they might have expected to get [based on the power function model]. The results they got in 1950 suggested that somewhere around 0.001 percent of the injected [plutonium] was the rate [eliminated per day] [after 160 days]. The [actual] answers are really close to 0.004 or 0.005 percent per day. You've got quite a scattering there as a function of the analysts' [method used]. [The answers they got in 1950 were] close to the power function fit that was developed from the short-term [(138-day)] data.

We went back and looked at the original injection data, and we also realized that the urine data was not corrected for chemical yield. It had about a 12-percent [analytical] error. Fecal samples were corrected for chemical yield by overspiking the fecal sample. The fecal data are corrected; the urine [data] are not corrected.

I think we can straighten out some of the misunderstanding about the power function modeling and why it doesn't agree with the trend that we see [in the data after 138 days]. I had a statistician here and he helped me work this up [using] sum of the exponential models, also the sum of two exponential models.

Reanalysis of Excretion Studies to be Published

FISHER: Are you close to having that reanalysis published?

MOSS: Yes, that's the goal of this whole thing. I retired five years ago, [went back to work soon thereafter,] and I was planning to retire again last January. Then this subject came up, so they got me into this project.

By the way, as soon as this group was formed, I went to the project leaders, who at that time were Alan McMillan and Gary [Sanders].[44] I took the Langham notebook and said, "Look, the first thing you've got

[44] Moss is referring to the group assigned at LANL to respond to Secretary O'Leary's Openness Initiative on human radiation experimentation and conduct a comprehensive review of documents and records at LANL.

to know is that there are some errors in the Langham data. There are some unused data that were never recognized. And, there were some mistakes in the analytical method that contributed to this."

They said, "Fine, you're onboard. Let's get everything together. Let's get the notebooks out. Let's get them available." So they've been released. I talked to Pat Durbin,[45] I explained to her what I'm doing. She and I both agreed she wanted the tissue data. [I would be responsible for the excreta data.]

FISHER: Have you been able to locate all the data from the urine samples that were originally collected?

MOSS: Yes, we got all the notebook records. [Two notebooks detail the Oak Ridge data.]

Then in October, they started a second series [of Pu injection studies], and there were 11 other people injected. They got urine samples from 10. [Including the Oak Ridge case], there were 11 sets of urine samples analyzed at Los Alamos. There were other samples collected in 1946, '47, and '48, from three of the people [from the Rochester series]. This isn't Langham's notebook [*per se*]. They put quotations on Langham's notebook. These are [entries from] people that worked under Langham's supervision. When they entered these results in the notebook, they would write down a code number for [each sample]. They wrote the date of collection, [wrote] the volume [of the sample], and [recorded the Pu measurement data].

Finding and Correcting Errors in the Data

FISHER: The volume of the sample?

MOSS: Yes, the volume of the sample [per 24-hour collection]. Using the cupferron method, they would ash the sample, dissolve it in acid, and then they would take two aliquots[46] for analysis. These got paired sample [results] from each urine sample collected from the [Rochester] people from the initial exposure date starting in October 1945.[47] Then, when they went to tabulate that data—this is the story I got from Payne Harris—when they tabulated the data, someone went into the notebook and would write down "day one," the result; "day two," the result; "day three," the result; all the way down [to the last sample].

But there was a break in three or four of those people's [data]. Samples were collected to twenty-some days, and [then] there was a break in the collective sampling [and sampling did not resume until additional sam-

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[45] Dr. Patricia Wallace Durbin worked as a chemist and radiobiologist at the Crocker Laboratory of the Lawrence Radiation Laboratory (now Lawrence Berkeley Laboratory) from 1951 to 1977. See DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (June 1995)

[46] a part that forms a known fraction of a whole and constitutes a sample for chemical analysis

[47] They composited two daily urine samples (into one).

ples were collected at] day 80, 81, 82, 83, etc. But when the person tabulated the data, it was listed as day 21, 22, 23, 24, etc. Now, when you reconstruct this data, in four of the cases, it doesn't fit a [simple, single-term] power function.

FISHER: Because the wrong days were assigned?

MOSS: [They were] incorrectly tabulated. I asked Payne Harris, "Did you ever look at the notebook?" Payne Harris said, "No, we just had a yellow tablet, which they had to tabulate the data." There is no question that there was a clearing out of plutonium from the blood [following Pu injection]. Then you get metabolized plutonium coming out [in the urine some 20 days post-injection. The two excretion rates are different and are not described by a single-term power function model.]

FISHER: The clearing from the blood is either to the liver or the skeleton?

MOSS: Yes, a bit of phenomenon appears to be occurring. The injected plutonium pretty well concentrates in the bone marrow and then clears out of there. Then, you eventually get deposits in the bone and then you get metabolized plutonium coming out [in the urine]. The initial plutonium is not really metabolized plutonium. It's simply filtered out [by the kidneys], and then eventually you've got [metabolized Pu] coming out [in the urine]. We've got a nice model. We've got an error bar[48] associated with using all the data. We did exclude the Oak Ridge case.[49]

FISHER: For what reason?

MOSS: The Oak Ridge data have several errors associated with them. One is that the Langham record indicates that 65 consecutive samples were collected. But that's not really true. There were something like 42 consecutive samples and then there's a gap. [(Samples were collected out to 89 days.)] The last series of samples collected from the Oak Ridge case were 48-hour samples, not 24-hour samples. They were never acknowledged that way. The notebook records indicate that they were 48-hour samples. So there's a big jump in the quantity of plutonium coming out [at that time, day 71 to day 89].

FISHER: By a factor of two?

MOSS: Yes. There's no question about it. So, we had justification for excluding the HP-12 case, which is the Oak Ridge case, from our modeling.

FISHER: Did you consider some of the bone sample analysis from HP-12?

MOSS: No, the person that was injected at Oak Ridge was involved in a serious automobile accident. They did the injection and, then, they did a repair of his bone fracture and obtained small sample fragments of the femur

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[48] one standard deviation (plus and minus values) of the mean when results for several subjects are plotted together, from which a model was or is developed

[49] See "Plutonium Injection Studies at an Oak Ridge Military Hospital (1945)" in DOE/EH-0475, *Human Radiation Studies: Remembering the Early Years; Oral History of Health Physicist Karl Z. Morgan, Ph.D.* (June 1995).

and then some teeth. They weren't really adequate samples. There were bone samples. However, the [urine] data for the first five or six days were not very good. It doesn't fit the model.

FISHER: The urine data?

MOSS: Yes, the urine data. They analyzed each voiding independently. So they compounded chemical errors. We've made a justification for excluding HP-12 [(the Oak Ridge victim of a vehicle accident)] from our selection of modeling data.

FISHER: And you didn't really use the bone sample data?

MOSS: I'm not really talking about that. Pat Durbin will write that up independently. Then we'll get together and compare our notes. We also made a decision that California-1[50] is probably not adequately monitored for chemical yields. Pat seems to feel the same way, that's a bit of funny data [beyond 100 days post-injection].

Other Information Contained in the Langham Research Notebooks

FISHER: Is there anything else, with regard to the plutonium injection study, that you'd like to add? Any personal recollections of that study?

MOSS: I don't find anything written other than what I mentioned.

FISHER: Was there anything in the Langham notebook with regard to selection of subjects?

MOSS: No, nothing.

FISHER: Consent of subjects?

MOSS: Nothing.

FISHER: Their names?

MOSS: The names for the first four or five, maybe six people were written in one of the laboratory notebooks. Then, I think, starting with HP [code after subject # 5], there were no names included thereafter.

FISHER: How were the subjects identified, by number?

MOSS: By number, the HP number. I'm not sure what that stands for. It may be for "Hospital Patient"; I don't know.

FISHER: Is there anything in the notebooks that discusses the physical condition of the subjects, their health status?

MOSS: Nothing.

FISHER: Their life expectancy?

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[50] also referred to as "Cal-1," one of the original 18 people injected with plutonium during the Manhattan Project. Although assessed to be terminally ill at the time, several of the people injected lived for an extended period. Cal-1 lived for 20 years after the plutonium injection. See "Reanalyzing the Human Plutonium Injection Studies" in DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (July 1995).

MOSS: Nothing.

FISHER: Or their death when that occurred?

MOSS: Nothing. Nothing at all in the notebooks. It's all written up in *LA-1151.* That's the only source of information.

FISHER: Have you identified any other laboratory notebooks or records with data, or do you only have the notebooks to go on?

MOSS: We only have the notebooks. When they first started, they made parallel notebooks. One notebook contained the HP number and the other contained the names. They [contained the] same analytical data, though. At the end, when they got to HP-5, they quit putting the names down, and they exclusively used one notebook with the HP number only.

FISHER: There's no indication as to why they changed that procedure?

MOSS: No, nothing. [(Rochester didn't include names after HP-5.)]

Other Bioassay Studies

FISHER: Do you recall doing bioassay chemical analysis of either urine, feces, tissue, or other bioassay for other experiments conducted at Los Alamos?

MOSS: No, not in the sense that you'd call them [that]. I think you have to differentiate from the tissues that we got from deceased people.[51]

FISHER: There were selective administrations of radioisotopes after the whole-body counter[52] was developed.

MOSS: I wasn't involved in that.

FISHER: Certain subjects were counted in the whole-body counter. I wondered if any of the urine had been sent to your lab for analysis?

MOSS: Not that I'm aware of.

FISHER: To develop urine excretion functions?

MOSS: We never did that here.

FISHER: Do you remember developing any urinary or fecal excretion functions for any radionuclides other than plutonium?

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[51] workers with occupational exposures at LANL; sometimes tissues were obtained from recently deceased workers for radioanalysis to determine residual radionuclide content in selected organs.

[52] an apparatus that measures radionuclides in man using shielded detectors and multichannel energy analyzers. HUMCO-I was the first whole-body radiation counter that became operational at Los Alamos National Laboratory in 1956. The sensitivity and non-invasive nature of this instrument permitted studies at levels 10 to 100 times below established limits of exposure. It opened an entire area of clinical diagnosis and the development of new diagnostic methods. See DOE/EH-0452, *Human Radiation Studies: Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

MOSS: None whatsoever. You should put it in context here. You've heard the name Harry Foreman,[53] of course. Harry was brought to the lab out of Berkeley to work on chelation[54] therapy technique. Harry had a lot of other interests. He was looking at fallout in antlers of deer from this area. He also had the idea that it would be interesting to study the concentration of plutonium in the lungs of people. He made arrangements [in 1956] with Clarence Lushbaugh,[55] who was an M.D. and also a local pathologist[56] for the hospital. He collected some lung samples and he asked them—

FISHER: Lung samples from—

MOSS: —Los Alamos people, who had died [in the Los Alamos Hospital]. Then he asked for the samples and brought them to me. Harry asked for a chemical analysis for plutonium. We did about five or six lung samples [in 1956], and the numbers are recorded in a notebook, but there were no names associated with [the samples].

FISHER: What did you do with this information?

MOSS: It's in the notebook.

FISHER: It was never used for any other purpose?

MOSS: No other purpose. There was a case called the [Oral] Epley Case[57] in Nevada [at the Nevada Test Site],[58] where there was a worker that was exposed out there. He died later. They got tissues from that person. Harry Foreman gave me the residue and asked me to analyze it. I analyzed it for plutonium. That was in '56. Kelley died in 1959.

FISHER: Cecil Kelley?

MOSS: [Yes,] Cecil Kelley.[59] Since he had a plutonium body burdened by his urine analysis record, we got permission from the wife to get tissues. Of course, I was involved in the analysis of those tissues.

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[53] Harry Foreman, M.D., came to the Health Division at Los Alamos from Dr. Joseph Hamilton's group at Crocker Laboratory in Berkeley, California. In the Health Division, Foreman conducted most of the research in use of chelating agents to try to accelerate removal of heavy metals, such as plutonium, from the bloodstream and soft tissue.

[54] the use of a substance that removes heavy metals from the body fluids and carries them to excretion (urine)

[55] Dr. Clarence C. Lushbaugh, M.D., Ph.D., was a staff member of the Biomedical Research Group at Los Alamos National Laboratory from 1949 to 1963. Chief Scientist of the Medical and Health Sciences Division at Oak Ridge National Laboratory, 1963 to 1975, and Chairman of the Medical and Health Sciences Division at Oak Ridge, 1975 to 1984. For the transcript of the interview with Dr. Lushbaugh, See DOE/EH-0453, *Human Radiation Studies: Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

[56] a physician who studies the study of the origin, nature, and course of diseases

[57] Oral Epley was one of several workers exposed to radiation in an accident at the Nevada Test Site in 1956. He died three days after the accident.

[58] the site where most nuclear weapon tests within the Continental United States were conducted

[59] a worker at Los Alamos who died in a radiation accident, December 30, 1958. For details, see "Investigation of Radiological Accidents" in the Lushbaugh transcript (DOE/EH-0453, April 1995) and "Radiation Studies Resulting from a 1958 Los Alamos Criticality Accident" in DOE/EH-0477, *Human Radiation Studies, Remembering the Early Years: Oral History of Radiologist Chet Richmond, Ph.D.* (August 1995).

FISHER: You were involved?

MOSS: Yes, I was involved in the analysis of those tissues. That was the first case in which we had [both] a fairly good [Pu] bioassay record that showed evidence of positive [Pu] urine results and tissue [samples]. That was going to be the first test to see how the Langham power function model [worked].

FISHER: How long after intake of plutonium did Mr. Kelley die?

MOSS: I'd have to look it up in the record. That was '59 when he died. He probably had a 15-year [Pu bioassay] record.

FISHER: Of plutonium bioassay?

MOSS: He had several incidents of intake of Pu [while working at the Laboratory].

Tissue Registry Program at Los Alamos

FISHER: Were there multiple exposures?

MOSS: There were several. He had been taken off plutonium for a while and then put back on. This was going to be really the first test of applying a long-term bioassay record with chronic [Pu] exposures, the paper that was written by Foreman, Moss, and Langham. The [Pu body burden] estimated by the power function model was 18 nanocuries.[60] The concentration found in the tissues was 19 nanocuries; something close to that value. It was fortuitous that the two burdens agreed that well. They did say it was the first test of the Langham model. That gave a lot of strength to the acceptance of that [Pu] power function model. It was quite important. That was the beginning of a real tissue program in which we got tissues from people who died in Los Alamos and went to autopsy.

FISHER: Has there been any other comparison of your analysis data and tissue sampling on autopsy or donors to the Registry?[61]

MOSS: Quite a few now with the Registry.

FISHER: To confirm the power function model?

MOSS: Yes, those have been written up by Ron Kathren[62] and Jim McInroy.[63] They were recently published in the last couple of years. They had an extensive paper where they compared all the different [Pu excretion] models. That's a couple of years ago. It's important to emphasize that

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60 one billionth part of a curie. A curie represents 37 billion radioactive decays per second.

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

62 Ron Kathren is the current director of the U.S. Transuranium Registry.

63 Jim McInroy was a chemist (now retired) at LANL.

that particular Kelley case gave us some data to support the argument that the power function model was a good estimate of [Pu] burdens. I wanted to get that in the record.

The other thing I wanted to mention: I'm convinced, and Payne Harris is convinced, that Langham never really went back and looked at his laboratory notebooks and identified the fact that there were tabulation errors in the [1945-to-'46] data. Otherwise, I think he would have recognized it. It is rather interesting to note that when the additional urine samples were collected at 80 days from two of those injected people, the urine results are incorrectly tabulated, but the fecal results are correct in the Langham report [(*LA-1151*)]. That's just something to be aware of.

We're going to correct that. We're going to rewrite, if you will, the Langham paper, remodel the data, and then use the Rundo data. Also [we will] use additional data collected from HP-4, HP-7, HP-9, at 300, 400, and 500 days. Why that was never used, I don't know. Perhaps they didn't think it was good analytical data, or they didn't realize it was in the record.

FISHER: I hope this does happen.

MOSS: I've been working on it a long time.

FISHER: We encouraged you to do this as early as 1986.

MOSS: I realize this. Gary [Sanders] was aware of this. You talked to him about it.

FISHER: I personally think that this is very important work because it makes the most utilization of the urinalysis data that were collected as early as 1945. And it is important for accurate assessment of intakes by workers in the present.

MOSS: As a result of [the revaluation of] this study now, and the [HSPT][64] project study, [we've recognized] that some of the scattering [(apparently random deviation from an expected result)] of data is [analytical in nature,] not physiological.[65] When Pat Durbin was out here, I mentioned that to her. She was very insistent. She said, "Go ahead and correct that data!" I said, "That was our intention: to correct the data for what we now recognize are biases contributed by analytical methods."

FISHER: Will that reduce some of the scatter in the data?

MOSS: That will reduce the scatter. Also, I think we can make a justification for not including in our modeling the HP-12 case, which is the [original] Oak Ridge case, and probably exclusion of the California-1 case.

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[64] Human Subject Project Team, Los Alamos National Laboratory

[65] relating to the functions and activities of living organisms and their parts

Los Alamos Staff Participants in Studies

FISHER: Bill, to change the subject and just tie up a few loose ends before we conclude, were you ever a participant yourself in any radionuclide experiments?

MOSS: No.

FISHER: Or any of your children?

MOSS: No. I was a control person, though, for some LANL human studies when they were doing the metabolic studies in the 1960s. They counted me as a control person with no intake of any kind.

FISHER: Do you know of any experiments that took place at Los Alamos that haven't already been released by the laboratory?

MOSS: None that I know of. To qualify that a little bit, the group I was in, which was called the Industrial Hygiene Group [(H-5)], was in the same building as the Langham research [group H-4, "Bio-Medical Research"]. There was a close relationship between everybody in the building. We were all invited to seminars. We, as a group of people from a different group, [attended] most of the [same] seminars and were aware of what studies were going on [or] had been done.

FISHER: So you had knowledge of all of these studies, but were not a participant?

MOSS: I knew people who were participants, and I talked to them about what they were doing.

FISHER: For example?

MOSS: Evan Campbell[66] was one of the persons. I knew Chet Richmond[67] very well.

FISHER: What did Evan Campbell participate in?

MOSS: I'm not sure. He was one of the persons though that took radionuclides; not plutonium, of course.

FISHER: When this happened, was there a general acceptance by the staff that these were appropriate studies, at sufficiently low doses, and that the information was sufficiently valuable to warrant these studies going forward?

MOSS: Absolutely. I think the quality of the people doing the studies was recognized, and the risk was identified as minimal.

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[66] Evan Campbell, section leader in Industrial Hygiene (H-5), responsible for the bioassay program; Dr. Campbell died in 1989.

[67] From 1955 to 1958, Richmond was an assistant, and then a staff member, at Los Alamos Scientific Laboratory. In 1958, Dr. Richmond was appointed to the staff of the Atomic Energy Commission—Division of Biology and Medicine in Washington, DC, where he remained until returning to Los Alamos in 1971 to head the Biomedical Research Group. In 1974, he was appointed Associate Laboratory Director for Biomedical and Environmental Sciences at Oak Ridge National Laboratory. Dr. Richmond retired in 1995. For the transcript of the January 24, 1995 interview with Richmond, see DOE/EH-0477, *Human Radiation Studies: Remembering the Early Years; Oral History of Radiobiologist Chet Richmond, Ph.D.* (August 1995).

FISHER: Were there any such studies with alpha emitters[68] where bioassay was required to assess the excretion rates?

MOSS: Almost all the work that Richmond was doing involved gamma emitters.[69]

FISHER: Gamma emitters that could be measured in the whole-body counter?

Long-Term Profiles Performed With Radium

MOSS: [Yes,] they were easy to measure. That's another thing that's important. In modeling biological data, at that time period, in the 1940s, there had been no previous studies of long–half-life material that gave a long-term [biological] profile. Even though some radium studies had been done, it was not until Norris [at Argonne] was able to locate the radium people that were injected in 1931 in Elgin, Illinois,[70] that they were able to get the 20-year [excretion] profile.

FISHER: On radium?

MOSS: On radium, yes. Before that, all the studies were limited-term, lacked sensitivity, and had no real understanding of the modeling. Almost everything was plotted on linear paper, [with a] dramatic drop and the leveling off of rate. It wasn't until Norris started looking at the semilog paper and log-log paper in the late '40s, that the power function [model] developed.[71]

FISHER: In working with Wright Langham, in his later years, even as late as 1970, did he ever express any regret for having participated in the plutonium injection study that you recall?

MOSS: He never said anything to me that would indicate any regret.

FISHER: Or was he ever reluctant to talk about it?

MOSS: I never talked to him about it. It wasn't a subject we talked about. Not that we ignored it, it just never came up.

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[68] radioactive substances that emit helium nuclei during decay, possibly causing tissue damage if ingested or inhaled

[69] atomic nuclei that emit gamma rays, a highly penetrating photon of high frequency, usually 10^{19} Hz or more

[70] Patients at the Elgin State Hospital in Elgin, Illinois, were injected with radium as an experimental treatment for mental illness, apparently in the period 1931 to 1933. The experiment predates by nearly a decade the creation of the Manhattan Project, the Atomic Energy Commission, and its successor agencies. According to Dr. Robert Rowland, the radium may have been supplied by an organization called the U.S. Radium Extension Service. In the late 1940s, Dr. William Norris, at Argonne National Laboratory, with the help of colleagues, traced the whereabouts of some patients from this study and extracted information from encoded notes left from the original project. By combining measurements taken from these individuals with the notes, Dr. Norris developed a theory of radium retention by the human body. See ANL-1 in DOE/EH-0491, *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (July 1995), p. 5. See also DOE/EH-0461, *Human Radiation Studies: Remembering the Early Years; Oral History of Biophysicist Robert Edmund Rowland, Ph.D.* (June 1995), pp. 3–12.

[71] By examining data plotted on nonlinear "log paper," researchers can discover numerical patterns or trends that would elude them if the data were plotted on linear graph paper.

Public Misperceptions About Radiation

CAPUTO: You've been working on this project now since January [1994], and you've seen how this issue has been perceived by the public. Do you feel that there are any misconceptions that should be corrected?

MOSS: There is a misperception that the people at Los Alamos had little regard for radiological risk. The public has no concept of radiation. They are almost inclined to think that any kind of radiation is bad for you. Any concept that they are living in a radioactive world is missing from their point of view.

CAPUTO: Is there anything else you want to add? Anything you felt we've missed?

MOSS: I just think that we should emphasize that the people doing these studies were reputable people. There was no question about the quality of any of them. You know George Voelz personally. You've meet Don Petersen;[72] you may have known him before.

FISHER: I've known Clarence Lushbaugh for twenty years.

MOSS: These are fine people. In the case of Lushbaugh, when I had a serious illness in my family, I went to him on several occasions and he said, "Give me your doctor's number, I'll call him up and I'll tell you what I think." He came back to me and said, "This is what I think." He gave me what I consider ethical advice about what I should do, and so on. I thought he was a fine fellow.

FISHER: Thank you very much for your time. We greatly appreciate it. ❑

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[72] Dr. Petersen has worked as a cell biologist at Los Alamos since 1956, originally in Group H-4, Radiobiology (later renamed Bio-Medical Research), led by Wright Langham. From 1964 to 1981, he served successively as the Cell Biology Section Leader and Group Leader, Alternate Health Division Leader, and Acting Life Sciences Division Leader. Since 1981 he has been the Program Manager for the Chemical and Biological Program. See DOE/EH-0460, *Human Radiation Studies: Remembering the Early Years; Oral History of Cell Biologist Don Francis Petersen, Ph.D.* (August 1995)