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HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Cell Biologist
Don Francis Petersen, Ph.D.

Conducted November 29, 1994

United States Department of Energy
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MASTER

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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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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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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.

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Interview with Don F. Petersen, Ph.D.
Setting: November 29, 1994, Los Alamos, New Mexico
Interviewers: Marisa Caputo and Darrell Fisher
(DOE Office of Human Radiation Experiments)

ORAL HISTORY OF CELL BIOLOGIST DON FRANCIS PETERSEN, Ph.D.

Conducted on November 29, 1994, by Marisa Caputo of the U.S. Department of Energy's (DOE's) Office of Human Radiation Experiments (OHRE), together with Darrell Fisher of Battelle, Pacific Northwest Laboratories, a contractor to DOE.

Dr. Don Petersen was selected for the Oral History Project because of his long research career at Los Alamos and his knowledge of the Atomic Energy Commission's biomedical programs.

Short Biography

Donald Francis Petersen [redacted] He is married and has three children. He received his A.B. from Depauw University in 1947, his M.S. from South Dakota State College in 1950, and his Ph.D. in Pharmacology from the University of Chicago in 1954. His Ph.D. work was on the effects of x-ray irradiation on animals. Dr. Petersen moved to Los Alamos, New Mexico, in 1956, where he served in Group H-4, Radiobiology (later renamed Bio-Medical Research), led by Wright Langham. He worked in the Biochemistry section, and from 1964 to 1974 was the Section Leader for Cell Biology. In 1974 and 1975 he was the Group Leader for Cell Biology, and, from 1975 to 1979, the Alternate Health Division Leader. From 1979 to 1981, Dr. Petersen served as the Acting Life Sciences Division Leader. From 1981 to the present, he has been the Program Manager for the Chemical and Biological Program.

Dr. Petersen did not personally conduct experiments on human subjects, although he was familiar with several of the experiments at Los Alamos. In at least one experiment, Dr. Petersen was a volunteer subject. This iodine-131 experiment was published in *Health Physics*, Volume 9 (1963) in an article by Van Dilla and Fulwyler. Dr. Petersen also served as a volunteer subject in experiments that studied the survival times of red blood cells tagged with chromium-51. These experiments were reported in the Health Division's 1957 annual report, and may have been performed in collaboration with the Argonne Cancer Research Hospital in Chicago. Dr. Petersen has allowed his own children to be used as subjects in one or more experiments involving radioiodine.

Dr. Petersen has published articles on the following topics:

- long-term studies of plutonium exposure involving autopsies of plutonium workers, analysis of tissues from members of the general population, and tissue analysis of individuals exposed during weapons tests;
- studies on radioactive contamination levels in soil at various locations around Los Alamos and across the country, and studies performed to determine the amounts of contamination transmitted to growing plants; and
- radioactive pollution from inactive uranium mills and uranium tailings dumpsites.

Dr. Petersen is a member of the following professional societies:

- American Association for the Advancement of Science,
- American Chemical Society, and
- American Society of Pharmacology & Experimental Therapeutics.

From South Dakota to University of Chicago (1950) to Los Alamos (1956)

CAPUTO: Today is November 29, 1994. My name is Marisa Caputo from the Office of Human Radiation Experiments, Department of Energy. Also with me is Dr. Darrell Fisher from Battelle, Pacific Northwest Laboratories, and we're here today [in Los Alamos, New Mexico,] to interview Dr. Donald Petersen [of the Los Alamos National Laboratory] about his knowledge of human radiation experimentation during the Cold War. Dr. Petersen, I was hoping you could start with how you became involved in your field of work and what brought you to Los Alamos.

PETERSEN: I got into science through acquaintance at a small college in my hometown. After I got out of the Navy in World War II, I went to the South Dakota State University. I was taking a few classes, and I got a job working in the Agricultural Experiment Station there. The principal interest of that group was [the] toxicity of selenium.

I worked there for a while and the practice of the director of the Agricultural Experiment Station, Dr. Al Moxon, was to watch the kids that were working for him. One day he would come in and tell you that you couldn't work for him anymore, but that he had another job lined up for you.

Always he would place his employees with major professors in leading institutions around the country, with arrangements so that they could receive stipends that would pay for their education. Many of the people who got out of the service right after World War II really weren't very bright, and they squandered their GI Bill before it really counted, finishing up college and things of that sort. When it came time to go to graduate school I had no funds.

It was a real godsend to have [Dr. Moxon] find [me a spot]. It wasn't just for me: He did this for all the kids that worked for him. He would give them several options. Such-and-Such a school with So-and-So. Always these major professors in [other] institutions had either assistantships or some other arrangement for allowing a person to work in a laboratory and pay for their education. That happened to me.

He said that I could either go to the University of Wisconsin on a Wisconsin Alumni Research Foundation Fellowship, or I could go to the University of Chicago and work at the Tox[icity] Lab[1] for money. *(laughter)* I was not very sophisticated in those days. I looked at that University of Chicago "for money" offer and figured that was the good one. It turned out that the Wisconsin offer was the good one. The stipend there was beer money; everything was paid for. Either one of them was

[1] During World War II, the University of Chicago ran a toxicity laboratory for the U.S. Army Chemical Corps to conduct research in chemical warfare. From 1948 until 1951, the Atomic Energy Commission (AEC) used the facility for radiological warfare research. In 1948, the AEC worked with the Army and the university on a research program for the laboratory that focused on the poisonous effects of radiation exposure. Animal research was conducted on the local effects and general toxicity of radioisotopes considered for use as radiological warfare agents. Some coincidental work was also done with Argonne National Laboratory on developing occupational safety practices for radiation handling.

components or radioactively labeled[8] compounds, normal metabolites[9] just exploded the whole understanding of what was then called biochemistry. That was essentially the thing we were fascinated with.

In addition to that, of course, we were interested in the metabolism of labeled drugs, therapeutic agents. Los Alamos synthesized the first pharmaceutical made with pile[10] [radio]carbon. Not many people know that. [Art Murray,] Tony Ronzio, Lloyd Roth (who was at the University of Chicago with me later), and Wright Langham[11] [reported the] synthesis [of] carbon-labeled isoniazid[12] in 1947. [*This tracer work was done carefully, in spite of the excitement, and the dominant idea was to help, not hurt anyone—be they a volunteer, a patient, an investigator, or a bystander.*]

CAPUTO: So you got your Ph.D. at the University of Chicago and then came on to Los Alamos?

PETERSEN: I stayed at the University of Chicago for a couple of years after I got my degree.

Outlining the Agenda for Radiation Research (Early '50s)

FISHER: What was your experimental work at Chicago?

PETERSEN: We were interested in radiation-effects studies. There were "camps" at the time. Harvey Patt thought that this [(radiation injury)] was all based on cellular [effects]. There was another camp, of which Kenny DuBois and I were members, that believed that the fundamental radiation effects were probably on enzyme systems. Then there were the people that were doing the phenomenological work with intact animal models, looking primarily at acute effects of radiation.

The concerns in those days were, really, "Were we going to be attacked by the Russians?" Civil defense was a big deal. That shows up in some of the studies that we've been looking at here.

There was a molecular [level]—we didn't call it molecular at the time, that word didn't become fashionable for another five or ten years. But we were interested in radiation effects at the molecular level. [We] were looking at enzyme systems. Then there was another gang that was looking at the effects on cells and organs, thinking that the fundamental effects of radiation were due to organ failure. Muller's [drosophila] fruit flies were already well-known to everybody [as tools to study genetic

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8 incorporated with a radioactive isotope to make a substance traceable

9 products of metabolism

10 an early form of a nuclear reactor, an apparatus in which a nuclear-fission chain reaction is sustained and controlled

11 At Los Alamos National Laboratory, Langham led the Health Division's Radiobiology Group from 1947 until his death in 1972.

12 a water-soluble solid compound used to treat tuberculosis

a perfect opportunity. And the McArdle Memorial Cancer Institute at the University of Wisconsin has been a spectacular place over time, in terms of its scientific reputation.

But the Department of Pharmacology at the University of Chicago wasn't peanuts, either. I worked for Dr. Kenneth Dubois and Dr. E.M.K. Geiling. It's interesting that the director of the Toxicity Laboratory at the time was [Dr.] Franklin MacLean, who had been in the [Manhattan Engineer District's and Atomic Energy Commission's] Santa Fe Area Office during the time immediately after the war years and the early testing period. By the time I got to Chicago in [the] 1950-to-'51 time frame, he was just back from that job and back on the University of Chicago faculty again.

We knew quite a bit about what was going on down here, and the Tox Lab had very close ties with the Met Lab.[2] Argonne[3] had not yet moved out to Lamont[, Illinois]. They were still on the University of Chicago campus. That was sort of a big family where everybody, even the graduate students, knew everybody else. The formalisms of an academic environment were clearly there, but we knew a lot of the people who were major players. Zirkle,[4] for example, [Harvey Patt],[5] and Austin Brues.[6] I actually worked next to John Thompson, who later had a successful career at Argonne. He was in the laboratory at the same time I was. That whole community, the radiation community at that time, was pretty tight. They were a close-knit bunch.

This was all brand new. There was a great deal of interest, enthusiasm, curiosity, [and] anxiety to exploit this new technique. If you think about it in terms of what we know today in biology and medicine, the fraction that we would know without the nuclear components is pretty small. We exploited it mightily in terms of unraveling biochemical pathways.

A lot of the biology and a lot of the medicine that we know is because of work that was done in the two decades immediately after World War II. The pathways were already open. The cyclotrons[7] had done that in the 1930s. The concept was already well-established, but the tools weren't there. Then, all of a sudden, all of the products of operating a reactor and the possibility of getting them out and using isotopic variants of bodily

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2 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

3 Argonne National Laboratory (ANL) outside Chicago; successor to the Met Lab, operated by the University of Chicago

4 Raymond Elliot Zirkle, an experimental radiobiologist for the Metallurgical Project, Manhattan Engineer District, 1942–46

5 a physiologist at Argonne National Laboratory

6 a professor at University of Chicago and Senior Biologist, Division of Biological and Medical Research, Argonne National Laboratory

7 accelerators in which particles move in spiral paths in a constant magnetic field. The resulting beam of high-speed particles can disintegrate atomic nuclei and may be used to produce radionuclides.

down here. He had been Baird Hastings'[17] student at Harvard. There was an interest in lipid[18] metabolism that involved collaborative effort between the University of Chicago's Argonne Cancer Research Hospital[19] and people here. The cholesterol tracers and the [labeled] acetate were synthesized here in the laboratory, primarily by Lloyd Williams and Art Murray. They were part of that synthesis gang that I alluded to earlier who I first started out with. They made labeled compounds which were subsequently given to patients on the metabolic ward at Argonne Cancer Research Hospital. The pathway of acetate to sterols[20] was looked at, with special emphasis on cholesterol.

FISHER: Was that work funded by the AEC?

PETERSEN: Yes, it was AEC work.

Choosing a Career at Los Alamos (1956)

FISHER: Can you describe the process by which you chose Los Alamos?

PETERSEN: Here?

FISHER: Yes.

PETERSEN: I was working at the University of Chicago, and C.C. Lushbaugh,[21] who had been at the Tox Lab ahead of me, along with John Storer, saw me at a meeting. I had known of them for some time. There was a problem at the University of Chicago where there were three of us who were competing for a position. I guess I won the position, but I didn't want the position: I wanted to go to medical school. When the others were allowed to go to medical school and I wasn't, I just decided I didn't want that position. I resigned, and Lush[, who in 1949 had left Chicago for Los Alamos,] said, "There's a spot for you out here." So, I took it. We were really trying desperately to get out of Chicago anyway, because that's culture shock for country kids.

FISHER: Someone from South Dakota?

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17 chairman of the Department of Biochemistry, Harvard University, and a well-known figure in that field

18 compounds consisting of fat, waxes, or similar substances, that are one of the chief structural components of the living cell

19 one of three clinical facilities created by the Atomic Energy Commission in 1948. While the AEC owned the 58-bed Chicago hospital, the University of Chicago medical school administered and staffed the facility. Patients were admitted on a selective basis: physicians chose persons whose condition best suited the hospital's research and treatment applications. The hospital admitted its first patient in January 1953. The AEC terminated its contract with the hospital in 1974.

20 solid fatty alcohols, including cholesterol, derived from plants or animals

21 Dr. Clarence C. Lushbaugh, M.D., Ph.D.—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 Lushbaugh, see DOE/EH-0453, *Human Radiation Studies Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

effects of radiation]. All of this [was done] before [James D.] Watson and [Francis H.C.] Crick [explained DNA chemistry]. That was 1953, and this was before that time.

The enunciation of that very neat idea of how genetic effects came about was not really there, but the understanding that something like that was happening was very clear throughout the community. [If] you go back and read some of the advisory memos, minutes of meetings, things of that sort, [from the people] who were [directing] nuclear research programs, [it is clear that they had clear ideas of necessary research].

CAPUTO: Who was that?

PETERSEN: [About fifteen people, including] Stafford Warren,[13] Hymer Friedell,[14] and Louie [(Louis)] Hempelmann met regularly from all the institutions. Just on the transition between the Manhattan Engineer District[15] and the formation of the AEC[16] and the formalization of the laboratory system that eventually became the National Laboratories was really remarkably prophetic, in terms of the things that they wrote down. In a certain sense, we're still working on that outline to this day. They knew fifty years ago, in general, what form this was going to take. You have to remember that in those days, they were just kids; they were young [men] then. Most of them are dead now. They were young guys when they wrote these directives.

Anyway, that whole environment, that whole atmosphere, pervaded university campuses, the laboratories; and the Radiation Research Society was brand new. People were in pretty stiff competition to determine what the effects of radiation were, but it was a subset of a much larger puzzle being [worked] on by a very much larger number of players: the unraveling of basic metabolic pathways.

MET Lab Research in the Metabolism of Radionuclides

FISHER: Who were the persons at Chicago who were most interested in the metabolism of radionuclides by man?

PETERSEN: Those were [Met Lab] people. We had a few. George [T.] Okita in our department was very interested. And of course, George [V.] LeRoy was the dean at the time. He was interested, too. That was a connection with Los Alamos, as a matter of fact, because Gordon [(R.G.)] Gould was

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[13] 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 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.

[14] 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).

[15] the U.S. Army Corps of Engineers organization set up to administer the development of the atomic bomb under the top-secret Manhattan Project

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

Nuclear Weapons Fallout Studies (1946–54)

FISHER: In particular, what kind of answers were being sought?

PETERSEN: If you recall, the schism in the scientific community on the effects of fallout from [atmospheric] nuclear weapons testing was almost immediate. [Operations] Crossroads[23] [and] Sandstone[24] were [complete, and supporters and opponents were] very clearly polarized by the time of the Greenhouse,[25] [Ivy,[26] and Castle[27] nuclear test series]. [Some] thought that we had already caused a disaster. Others [said] that we simply had to test in order to defend ourselves, the way the world [was].

FISHER: Which tests were you thinking of: biological tests at the cellular level [or] at the [whole] animal level?

PETERSEN: I'm sorry, the weapons tests. The business of bombs.

But you see, the questions as far as the effects of fallout were concerned were not geared to the kinds of work that people were doing at the time. The acute [nuclear weapons] effects work that marks the end of the war

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23 a series of two atomic bomb tests conducted at Bikini Atoll in the Marshall Islands while a fleet of surplus U.S. and captured German and Japanese warships were anchored in the lagoon as a test array. Many of these ships were damaged and set ablaze by the first shot (Able), which was dropped from a B-29 bomber and detonated in the atmosphere, July 1, 1946. Many more were sunk by the shock waves from the second shot (Baker), detonated in the lagoon July 15, 1946. Yields of both tests were in the 21-kiloton range. (A kiloton is equivalent to the blast effect from 1,000 tons of high explosive.) Source for yields: Office of External Affairs; *Announced United States Nuclear Tests, July 1945 Through December 1988*; U.S. Department of Energy Nevada Operations Office; September 1989; pp. 2–4. (hereafter referred to as *U.S. Nuclear Tests*)

24 a series of three nuclear weapon tests, detonated between April 14 and May 14, 1948 and ranging in yield from 18 to 49 kilotons (Source: *U.S. Nuclear Tests, ibid.*). The Sandstone shots were reportedly the first proof tests conducted by the U.S. since the July 1945 Trinity test and to have been intended to assist in developing design principles for second-generation nuclear weapons. All three shots were detonated on 200-foot-high towers. 10,200 people participated in Operation Sandstone. Source: Robert S. Norris, Thomas B. Cochrane, and William M. Arkin; *NWD 86-2 Known U.S. Nuclear Tests, July 1945 to 31 December 1985*; February 1986; Washington, DC; Natural Resources Defense Council (hereafter referred to as *NWD 86-2*).

25 the first of three series of nuclear weapon tests conducted at the Pacific Test Range. Operation Greenhouse included four tests detonated between April 7 and May 24, 1951, from towers at Eniwetok Atoll in the Marshall Islands. The only confirmed blast yield was for Shot Easy, said to be in the 47-kiloton range (Source: *U.S. Nuclear Tests*). To collect data on nuclear effects, 15,000 animals were reportedly used in the Greenhouse series. Source: *NWD 86-2*.

26 the second of three series of nuclear weapon tests conducted at the Pacific Test Range. Operation Ivy, held at Eniwetok, involved two tests on October 31 and November 15, 1952. Shot Mike, a surface burst that yielded a blast in the 10.4-megaton range (*U.S. Nuclear Tests*), is reported to have been the first test of an experimental thermonuclear device, in which substantial portions of the energy released came from fusion of hydrogen isotopes. (A megaton is the equivalent of the blast effect from one million tons of high explosive.) Shot King, an airdrop from a B-36 bomber, had a yield in the 500-kiloton range (*U.S. Nuclear Tests*) and was the largest fission weapon detonated by the U.S. Source: *NWD 86-2*, p. 14.

27 the third of three series of nuclear weapon tests conducted at the Pacific Test Range. Operation Castle involved five tests at Bikini Atoll and one at Eniwetok detonated on barges and at the surface between February 28 and May 13, 1954. One test fizzled, yielding a blast in the 110-kiloton range. The rest were in the 1.69- to 15-megaton range (*U.S. Nuclear Tests*). Reported to have been the capstone series for the project to develop the hydrogen bomb, begun in 1950, all of the Castle tests were planned to produce multimegaton yields. The yields of the first two tests, Bravo and Romeo, were well above those expected. Source *NWD 86-2*, p. 16.

PETERSEN: Right.

FISHER: You came down to Los Alamos in 1956?

PETERSEN: I came here permanently in 1956.

FISHER: And you've been here ever since.

PETERSEN: Yes.

FISHER: Uninterrupted.

PETERSEN: That's right. No interruptions.

FISHER: You probably worked first with Wright Langham.

PETERSEN: Wright Langham hired me. But I worked first, actually, with Gordon Gould. I dabbled a little bit in the sterol business, just trying to get started here with something. My interest had been in radiation effects on enzymes. That was start[ing] to develop here [and there was some suspicion that radiation affected lipid metabolism]. We looked at transaminase and things like that. Actually, I did a little bit of work with lipid metabolism. Just enough to decide that lipid metabolism wasn't how I was going to spend my career. I had some other interests.

The Laboratory at that time had a very benign research policy. Today we prepare formal research proposals, we have them internally reviewed, critiqued, [and] if they pass that, eventually they—I forget what they call them now. They were called 189s[22] when I last [worked] with them. They were the formal proposals to the AEC for funding. We didn't write [189s] until almost 1970. The other [National] Laboratories did, but we didn't.

But what [Lab Director] Norris [Bradbury] said was that this was an important part of his Laboratory, and he would like to have it funded. Langham was given a lump sum for a large number of years, on which he ran his group, all of us: bought the equipment, paid the salaries, published the papers, did everything. It was a group that, in the amount of enthusiasm, was just incredible.

FISHER: Working somewhat autonomously from the AEC?

PETERSEN: Autonomously in a sense, in that what Langham was given license to do was poke around in interesting science. Every time a good idea surfaced, it became sort of a part of the program. If it failed, then that individual was expected to move on to something else. Of course, this exploratory poking around mode of doing work was superimposed on a time when there were very, very urgent requirements for answers.

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[22] 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

Institute of Technology and said, "Those would be perfect scintillation[32] solutes."[33] Langham is really the [one] who [recognized what would] eventually [become improved] liquid scintillation counting.[34]

FISHER: He also had license from the Laboratory Directorship here to pursue studies that he wanted to pursue.

PETERSEN: Well, no. Within limits he did, but I can tell you stories that prove that that's not the case. Norris Bradbury was every bit as canny as Wright Langham. He never relinquished to Wright Langham any hunting license, so that Langham could go do anything he wanted to.

Langham operated under the directorial constraints, but he was insulated a little bit, in that he was doing biology and the Lab was run by physicists, and physicists are notoriously—how can I say this, I don't want to use a pejorative word. They [were] not in direct, understanding contact all the time about what biological interests are.

The upshot of this is that biology has never been the major factor in the activities of this Laboratory on a comparable basis, for example, to what it was at Oak Ridge, [Tennessee,] where [Alex] Hollaender[35] was running about a quarter of the Laboratory's effort in biology, or as [involved as] you guys[36] became up at Richland[,Washington,] later on. Chicago always had at least a fifty-fifty biology mix. Brookhaven,[37] even more. The universities almost entirely focused on biomedical medical effects.

FISHER: To a large degree, the biology program at Los Alamos supported the ongoing efforts of your Physics Division and the weapons [design program].

PETERSEN: Not really. The Division of Biology and Medicine (DBM) had anxieties, the need for answers.

FISHER: At [AEC] Headquarters.

PETERSEN: At Headquarters. They also had a very astute senior advisory committee, the ACBM, the Advisory Committee on Biology and Medicine. There were directives that put fences around wh[at] each of the Laboratories did, in terms of trying to even out and focus on specific areas in specific Labs. The three Labs with hospitals were Oak Ridge, Brookhaven, and Argonne. While there was interest in biology here [at Los Alamos], the

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32 a flash of light from the ionization of a phosphor struck by an energetic photon or particle

33 substances dissolved in a solution

34 measuring radioactivity by registering the number of scintillations it produces

35 director of Oak Ridge National Laboratory's Biology Division. Hollaender is noted several times in DOE/EH-0475, *Human Radiation Studies: Remembering the Early Years; Oral History of Health Physicist Karl Z. Morgan, Ph.D.* (June 1995).

36 a reference to interviewer Darrell Fisher's associates at Pacific Northwest Laboratories, in Richland, Washington, which is operated for DOE by Battelle Memorial Institute

37 Brookhaven National Laboratory (Long Island, New York)

in the early '50s didn't match what Willard Libby wanted to do with the Sunshine Program.[28] [The purpose of Project Sunshine] was to find out exactly what the world [contamination as a result of atmospheric nuclear weapon testing up to that time] looked like and what the potential effects, presumably [from] strontium-90, would be. Was there going to be a catastrophic increase in the amount of osteogenic sarcoma[29] in kids?

Strontium is metabolized like calcium. You have a certain amount of it in bone. You have malignant transformation and most likely a bone tumor. Since all kids were being exposed to strontium-90 from fallout, this had to be a concern.

Then there was the group that said, "This can't be a concern in a vacuum. It has to have some kind of a quantitative understanding associated with it. How much fallout have we dumped? What is the concentration of this fallout on the land surfaces? What is the contamination [of the] food chain? How fast is it going into people, with special emphasis on the very young, because they are the ones that are laying [bone] down at the fastest rate?" That whole business, the next generation of radiation effects, the early carcinogenesis[30] studies which were not terribly successful, [are examples of studies resulting from that group's concern].

Carcinogenesis studies to this day are not very successful. It's extremely difficult to get into the [low-dose] radiation regime that we live in and demonstrate any effects at all. You always have to look at the effects that were observed at much higher doses and then extrapolate back down into the extant radiation field in order to say something.

The Radiobiology Group's Research Project Approval Process

CAPUTO: I want to go back for a second. You said that Los Alamos pretty much got a lump sum, and the AEC wasn't approving specific projects. Was there an internal approval system set up at Los Alamos?

PETERSEN: Yes: Wright Langham.

CAPUTO: Everything was submitted to Wright, and then he said "Yes" or "No"?

PETERSEN: Well, that's a half-facetious answer. He was not a *laissez-faire* person. He was on your case all the time. In retrospect, he was one of the most astute guys that ever had a position in the AEC. The kinds of things that [he] saw, just as flashes, are scary. Let me illustrate that. He took one look at the oxazoles[31] that Newt Hayes brought with him from Illinois

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[28] Project Sunshine was initiated by the AEC in response to the urgent need for radiation biomedical information. The Project began as an evaluation of the hazards associated with nuclear war and grew into a worldwide investigation of radioactive fallout levels in the environment and in human beings.

[29] a malignant tumor that arises from bone-forming cells and chiefly affects the ends of long bones

[30] the development of a cancer

[31] a series of chemical compounds that scintillate when irradiated. The use of one oxazole—terphenyl—for scintillation counting was pioneered at Los Alamos in the '50s. Terphenyl remains a staple for scintillation counters.

isotope that were pretty hefty. Here [the diagnostic work was done] with just minuscule doses.

FISHER: When you say "these people," who do you mean?

PETERSEN: Patients who were being diagnosed for a specific disease. That, of course, was an interest of [Lushbaugh's]. In addition to that particular application, there was also the development of physical measurements. [Physicist Marvin A.] Van Dilla came here from MIT[43] via [the University of] Utah. He was interested in a very low-level detection. Marinelli-type[44] crystal counters were his interest. This whole [effort] developed into a highly sophisticated, extremely low-level detection capability that was sitting here available for a lot of the [Project] Sunshine problems that were plaguing DBM in the mid-'50s.

Participation as a Subject in Human Radiation Studies

FISHER: This capability that Los Alamos developed to measure radionuclides in man at low levels made it possible to conduct experiments on human subjects. Do you remember some of those early human studies?

PETERSEN: Yes, I participated in some of them.

FISHER: Do you want to describe some of this for us?[45]

PETERSEN: That is, I participated in them as a subject, not as an investigator. The studies were being conducted by Lushbaugh in medical diagnosis, and whenever he needed a normal [subject] he would seek out some people who were [physically] close enough so they could come [in] and be counted[46] frequently. He didn't want people from other parts of the Laboratory, simply because it was too inconvenient to come in and get counted. But, if you could zip down there and be counted in five minutes, and be back at the bench, you were convenient. Those were the normals he used.

There was no [teaching] hospital. The Los Alamos Medical Center was essentially a hospital devoted to the care of sick people, [and] not in a research mode. Occasionally, patients could be obtained that required a diagnosis that was of mutual interest. Hyperthyroid[ism][47] is a good example. Some of the blood dyscrasias[48] are other examples. I took some [intakes of] chromium-51 to determine what the normal red cell

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43 Massachusetts Institute of Technology (Cambridge, Massachusetts)

44 named for its developer, Leo D. Marinelli, a researcher at Argonne National Laboratory

45 For a description of 24 experiments, with references, see "Los Alamos National Laboratory" in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (213 pages), DOE/EH-0491, July 1995.

46 to have the rate of radiation emissions counted from radionuclides inside their body, using radiation detection instruments or the whole-body counter

47 overactivity of the thyroid gland, resulting in increased metabolism rate

48 imbalances of the constituents of the blood or bone marrow

medical focus here was peanuts compared to the other places, in terms of the distribution of the biology budget.

Mission of the Los Alamos Biomedical Group in the 1950s

FISHER: How would you describe the major mission of the biomedical group at Los Alamos in the '50s?

PETERSEN: It was a group that had broad capabilities. You've got Langham with his background. You had [Payne] Harris, who was an M.D. but knew as much physics as the top ten percent of all physicists. He's remarkable. You had Lushbaugh in medicine and pathology. You had John Storer in that same area. Gould in lipids. [Harry] Foreman came from [Dr. Joseph] Hamilton['s group] out in California.[38] He was interested in [chelation[39] as a means of removing] nuclides. Spalding was interested, or [became] interested, in long-term effects of radiation. He was at the time heavily involved in acute-radiation–effects studies.

But all of these guys had a dual role. They [did research] work [that] people said was like [playing] pinochle in the fire house: What [they] were really here for was to be a part of a team in the event that the bell rang.[40] That was partially true. We all had disaster responsibilities as well as programmatic research responsibility.

I was going to go back a little bit and show you how this apparent *carte blanche* that Langham had, developed into a very cohesive and remarkably astute program. The scintillation counters are an example of that.

Ernie Anderson[41] designed [a] counter big enough to hold a person, and [that] could [achieve] extremely low-level [radionuclide] detection. There was a stampede [to do new research with this instrument]. That counter was dedicated on Bastille Day[, July 14,] in 1956. It opened this whole area of clinical diagnosis and of the development of new diagnostic methods, mostly by Lushbaugh.

Metabolic work was done in parallel by Chet Richmond[42] [and] Jeff Furchner. Really neat stuff that contributed [fundamentally to radiation] protection. But the diagnostic work, I think, was especially clever, because these people, if they went someplace [else], were getting doses of

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38 Joseph Hamilton, an M.D., 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, in Berkeley, California.

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

40 that is, in the event of an emergency

41 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.

42 For the transcript of the interview with radiobiologist Chet Richmond, Ph.D., see DOE/EH-0477 (August 1995).

because of the sensitivity [of the whole-body counter], because of the fact that you didn't have to give enough [tracer[53]] so that a urine sample or a blood sample would have enough [radio]activity in it to be [measured].

[The subject was counted], time after time, and what they [retained] was what gave you the information. You simply looked at the rate that they lost [the] radioisotope. You were essentially counting their total dose of isotope over and over again and watching it both physically decay and disappear metabolically, and because of that you were using doses that were a tenth or even a hundredth of what was common practice in the community at the time. Nobody here ever gave a second thought to radiation effects from doses like that because they are nominally fractions of what you're living in as a natural radiation background, but you can see them with some resolution, and they work very nicely.

FISHER: So chromium-51 was injected, I suspect.

PETERSEN: No, the technique for the chromium studies was to withdraw 40 cc's[54] or 50 cc's of blood, incubate [the washed red cells] with the chromium[-51], wash again so that the chromium that didn't stick was discarded, and then readminister the labeled red cells. Quick count [provided] the baseline, and then you watched the disappearance by counting and re-counting over a period of [several] months.

FISHER: Did you crawl into that first HUMCO [whole-body] counter for evaluation?[55]

PETERSEN: Yes, lots.

FISHER: How many of these experiments do you remember? How many different experiments?

PETERSEN: There were studies with iodine that looked at thyroid[56] function. There were studies with iodinated lipid that were true diagnostic studies, looking at fat malabsorption syndrome. There were studies with iodinated albumen. There were studies of vascular properties. There were studies [with] iodinated rose bengal.[57]

One of the counters that we had was a small animal counter that was built just like the big scintillation counter and had roughly the same sensitiv-

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53 a radioactive tag on biomolecules, used to study a biological, chemical, or physical system

54 cubic centimeters; a 40-cc sample is about 1.4 fluid ounces.

55 HUMCO I was 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.

56 an endocrine gland located at the base of the neck and secreting two hormones that regulate the rates of metabolism, growth, and development

57 an intense magenta dye that can be measured colorimetrically in blood samples. Rose bengal was useful because it was cleared efficiently by the liver, just like the hormone thyroxin, which it was intended to simulate.

survival time was. That was also being done on patients being treated for leukemia[49] [for] an assessment of red cell survival in leukemic patients.

FISHER: Some of that work was in conjunction with other investigators at the Argonne Hospital, wasn't it?

PETERSEN: The Argonne Hospital collaborations were largely in lipid biochemistry. Del Bergenstall, George Okita, George LeRoy, [and] Gordon Gould. There are probably more that I'm forgetting. Those were the people [who] were interested specifically in atherosclerosis.[50] That was their principal focus, the role of cholesterol in the formation of atheromata.[51]

FISHER: Studies using labeled compounds?

PETERSEN: Yes.

FISHER: Which?

PETERSEN: The labeled compounds that were used there were carbon-labeled acetate and tritiated[52] cholesterol.

FISHER: Were you the subject of any of those studies?

PETERSEN: No.

FISHER: Let's go back to the chromium-51 blood studies. How were you selected as a subject, and what was your experience in participating in that experiment?

PETERSEN: My lab was across the hall from his lab, and he hollered over and said, "I need a volunteer!" And I said, "Okay."

FISHER: This was Dr. Lushbaugh?

PETERSEN: Yes.

FISHER: Did he tell you what the levels of activity would be?

PETERSEN: Oh, sure.

FISHER: What was your recollection of that?

PETERSEN: Well, of course, all of these doses were very small because we were using th[e] human counter[, which had dramatically reduced the amount of administered activity that would be needed for detection]. Doses in metabolic studies [at] that time were in the multiple-microcurie ballpark, twenty microcuries, fifty microcuries. If you were doing [a study in] a big hospital setting, at Chicago [(Argonne Cancer Research Hospital)], for example, which [was] the big, classy, teaching hospital that I have some familiarity with, [only a small amount of radionuclide was needed]

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49 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

50 a common form of arteriosclerosis in which fatty substances deposit on the inner lining of arterial walls

51 abnormal deposits of plaque and fibrous matter on the inner wall of an artery

52 labeled with tritium, a radioactive isotope of hydrogen having an atomic weight of three

mate] the attenuation [by] the intervening tissue by the change in the attenuation of the two decays. One is a gamma ray and the other is an x ray [with an energy] about ten percent of the gamma ray. The crystal can see and resolve both those radiations. He had a big 9- by 5-[inch] [sodium iodide, thallium-activated] crystal. The only problem was getting kids to hold still. That was the real genius of the experiment, other than the double label.

FISHER: For what counting periods?

PETERSEN: Half a cartoon. They had put a little television set up on top of the counter and the kids would [look] at that television set and hold absolutely still for the count, whereas if it wasn't there, they'd fidget.

CAPUTO: Were these children of Los Alamos workers?

PETERSEN: Yes.

CAPUTO: Did you use your own children?

PETERSEN: Yes. I had three: a four-year-old, a six-year-old, and an eight-year-old. The six-year-old and the eight-year-old decided they wanted to participate. The four-year-old looked around and she decided she didn't want any part of it, so she went home. Chet Richmond's kids participated. Some of Marv's kids, I know, participated.

The reason for the experiments, as I explained, was the concern that if you were dealing with environmental iodine, which has a very short half-life anyway, there was no way that you could do good dosimetry on fallout iodine and make the appropriate predictions for what the hazard for thyroid cancer might be in kids exposed to fallout iodine. This was an experiment that, once and for all, solved the problem of attenuation, of biological half-time, and those were the principal problems that were necessary to address.

FISHER: For the benefit of future researchers who might review this transcript and wonder why children were used [at] a prestigious national scientific laboratory, can you describe the decision-making process by which the use of children was justified?

PETERSEN: I can only speak for myself. The doses that were given to these children were very small: nanocurie[63] doses. The dose of radiation, even [considering] the concentrating metabolic effect of sequestration of iodine by the thyroid, [is] very small. What many people who criticize these experiments don't appreciate is that whether they like radiation or not, they live in a radiation environment that is many many times larger than the doses that these kids sustained in this experiment.

People have to realize that the sensitivity of these instruments was exquisite. The resolution was very good. The hazard from radiation was

[63] one billionth (1×10^{-9}) of a curie

ity.[58] [If] you [placed] your arm in the [counter], it would count your arm while your liver got rid of the rose bengal, and that was the liver function test. It was Tamplin's test,[59] but done with a radiolabel that made it much more sensitive and much more precise. It was marvelous.

At the time, bromsulfalein[60] clearance was the liver function test. And bromsulfalein was just about as damaging as anything else. The test was almost worse than the disease. Rose bengal [clearance] made that a neat test.

FISHER: Did you get some ^{131}I-labeled rose bengal also?

PETERSEN: Yes.

FISHER: So, you participated in quite a few of these experiments, as a normal, with Dr. Lushbaugh as principal investigator?

PETERSEN: Yes.

Measuring Iodine-131 Uptake in Children (Circa 1963)

FISHER: Marvin Van Dilla published some work on iodine-131 uptake in the thyroid also in this series in *Health Physics*.[61]

PETERSEN: There are a couple of reports. That was a study that was driven by the fact that the dosimetry in little kids subjected to fallout [radio]iodine was at some disagreement, largely because of the geometrical problems associated with looking at a [tiny] thyroid that had taken up iodine. You didn't know how much tissue there was between the iodine and the [detector] and that attenuation factor. Also, the fact that biological half-times [in children] were not well-known, prompted a study I thought was elegant.[62] He used iodine-131.

FISHER: "He" is Marvin Van Dilla?

PETERSEN: Marvin Van Dilla and Mac Fulwyler did this. They used [the] two iodine isotopes simultaneously, one with an energy that was 10 percent of the other, so that if you [measure the] two different energies shining through the tissue barrier between the thyroid and the detector, you can [esti-

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58 The scintillation counters detected virtually all nuclear disintegrations; Geiger-Mueller detectors, by contrast, had been able to detect only about 5 percent. Their superior sensitivity is how such counters made it possible to reduce the radioactive dose by 90 percent or more.

59 a clinical test of liver function using rose bengal to measure a sequence of blood samples. It made bromsulfalein injections unnecessary by allowing the subject to simply stick an arm into a scintillation counter after swallowing rose bengal. Art Tamplin conceived the test; Lushbaugh made its use practical.

60 Unless administered by a highly skilled phlebotomist, bromsulfalein was likely to damage blood vessels.

61 M.A. Van Dilla and M.J. Fulwyler. "Thyroid Metabolism in Children and Adults Using Very Small (Nanocurie) Doses of Iodine-125 and Iodine-131." *Health Physics*. Vol. 9, 1963, pp. 1,325–31.

62 The study's purpose was to determine the retention of iodine in the thyroid as a function of time, with a particular interest in radioiodine metabolism in children. Nineteen normal male and female subjects ranging in age from 4 to 46 drank approximately 10 nanocuries each of iodine-125 and iodine-131 mixed together in water. Subsequent measurements showed that there was little difference in radioiodine metabolism between children and adults. The work was supported by the U.S. Atomic Energy Commission.

FISHER: Did any of this process involve the AEC in Washington?

PETERSEN: Well, clearly, from early times Shields Warren[67] had set AEC policy. [But] he didn't like the use of human subjects. He was especially vocal in yelling at the military. In terms of the way patients were used in the teaching hospitals, that was fairly clear. What we were talking about out here is not quite clinical and involves people using some patient material; but by and large, the questions of normal metabolism have been answered within the confines of the staff of the Bio-Medical Research Group, H-4.[68] Most of the volunteers came from there. A few of them came from H-5 [(Occupational Health)]. We had volunteers from another research group merely because they were in the same building and this business of convenience of repetitive counting was really the driver that identified who was a volunteer, or a potential volunteer, and who wasn't.

FISHER: So, the authority really didn't come from the AEC?

PETERSEN: The AEC defined [the authority] in correspondence between [Los Alamos Medical Director] Tom [Shipman] and [the AEC's head of the Division of Biology and Medicine,] Chuck Dunham in 1956. [The] human counter [was available] and everybody was salivating about getting in and doing work with it, because now you could use really small amounts of isotope and get some answers that hadn't been available before.[69] Shipman in those circumstances wr[ote] to Dunham and said, [I'm paraphrasing], "Here's how we propose to do [these studies]. Do you approve? We hope you approve and hope you will give us some guidance, because if you don't, these things are going to get done anyway." That's essentially what his letter said.

Dunham wrote back and said, "You have a good protocol except that...." And, then, he defined a protocol. That defined protocol of Dunham's, circulated by Shipman to Langham and all of the other isotope users, is essentially the ground rule that prevails throughout all of [the studies from 1956 to 1966].

FISHER: So, what you're saying is that, in effect, there was Headquarters' authorization for these experiments?

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[67] Shields Warren, M.D., had been Chief Pathologist at New England Deaconess Hospital and Professor of Pathology at Harvard Medical School. Dr. Warren served on the first U.S. team to visit Hiroshima and Nagasaki after they were bombed with atomic weapons and was involved in creating what became the Atomic Bomb Casualty Commission. He was the first director of the AEC's Division of Biology and Medicine and, later, established his own cancer research institute at New England Deaconess Hospital.

[68] In a May 1947 reorganization, the research functions of the Health Group became the responsibility of a new group, H-4 (Radiobiology), under the direction of Wright Langham. During the late 1940s and early 1950s, research with human subjects at Los Alamos was limited to tritium studies. The human subjects were researchers in Group H-4. In 1949, the group's name was changed to Bio-Medical Research. Langham headed this group until his death in 1972. At the time of his death, H-4 had grown to 70 staff members working in molecular biology, cellular radiobiology, mammalian biology, biophysics, veterinary biology, and pathology.

[69] because the research would have required that the subject receive a much larger dose

essentially nonexistent.[64] Therefore, we felt that there was no [danger]; we would never put our kids in harm's way.

If we had any thoughts at all that there was any hazard associated with this, the first thing we would have to do is move from Los Alamos. Because Los Alamos [residents, at 7,500 feet elevation,] live with the natural background [radiation] on an annual basis in the order of [350] millirem. What we are talking about in this particular study was a very small fraction of that as the total dose of that iodine before it either disappeared metabolically or simply disappeared through physical decay.

AEC Authorization of the Use of Human Subjects (1956)

FISHER: This is very useful, what you're saying. How far up the management chain did the authority go for the use of human subjects in experiments such as this?

PETERSEN: The division leader was given that authority by DBM about the time I got here, and that ground rule prevailed throughout the entire time that the major human experiments were done prior to the appearance of the Institutional Review Board.[65] From the time of the Institutional Review Board's assembly in 19[69] on, there has been a continuous review of protocols by a duly constituted board.

FISHER: Before this human-subjects review?

PETERSEN: Prior to that, the process was different. The process was that if you wanted to use human subjects in a study, you made that proposal to Wright Langham.

CAPUTO: In writing?

PETERSEN: In writing. [The request] identified the subjects that you wanted to use, identified the doses that they were going to receive, and Langham then forwarded that request to Tom Shipman, who was the [Health] Division[66] leader and also the Medical Director of the Laboratory. He then approved the request, and it came back down through Wright Langham to the investigator, and the experiment was performed.

CAPUTO: Langham only forwarded if he already approved it, or would he be able to say "No," and then never sent it up to Shipman?

PETERSEN: Sure, Langham could say "No" and never send it forward. In which case the [study] didn't get done.

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[64] Because the instruments were so sensitive, patients were able to be given minuscule doses of the isotope.

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

[66] 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 had safety responsibility for all weapons tests conducted by the Laboratory.

use human [subjects], so put it down on paper that we aren't responsible for this."[75]

CAPUTO: Does this paper exist today?

PETERSEN: I'm sure it does. I would imagine in the J Division [(weapons design and testing)] files. I found the original memo of this exchange between Tyler and Fields.[76] I believe that's in your hands now. I think I've made that available.

Valley Forge Hospital [in Pennsylvania], which is an Army installation with a pretty crack ophthalmologic[77] group, did the original studies. Then, when it came time to start evaluating protective devices[78] on volunteers, [the Air Force and] Sandia[79] got into the act. Their opaque visor designs [for military aviators] were outgrowths of the original Army experiments. Buster-Jangle [was] the first formal [study], Upshot-Knothole[80] [included] a trailer [facility for conducting studies] with dark-adapted people.[81] Upshot-Knothole, I forget. But anyway, by the time Teapot[82] came along, they had standard operational techniques that are written down for flying personnel to dodge the flash and avoid bleaching their visual purple. That whole [effort] was essentially dealt with in operational terms over a period of about three or four years. As far as an ongoing program now, I'm not aware of any.

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[75] The initial memorandum from Tyler to Fields, requesting a letter releasing AEC from responsibility for military use of troops as human subjects, is the document that was found by Dr. Petersen in the files at Los Alamos National Laboratory. However, according to Dr. Petersen, a search at Los Alamos has failed to turn up a reply from Fields.

[76] for example, CIC document #720376, a letter from Fields to Tyler acknowledging Tyler's distaste for the flashblindness tests but stressing the importance of such programs to the Department of Defense.

[77] relating to the branch of medicine dealing with the anatomy, functions, and diseases of the eye

[78] devices that would protect the eyes of subjects, such as bomber or fighter pilots, from the intense flash of a nuclear blast

[79] Sandia National Laboratory, based in Albuquerque, on Kirtland Air Force Base, was then and remains today a principal research and development facility for nuclear weapons design and nuclear weapons effects.

[80] Operation Upshot-Knothole was a series of eleven nuclear tests, including tower and airdrop tests and one nuclear artillery test (Shot Grable), conducted between March 17, 1953, and June 4, 1953, at the Nevada Test Site. Yields ranged from 0.2 kiloton to 61 kilotons (*U.S. Nuclear Tests*). During the series, 21,000 people from four military services participated in exercise Desert Rock V. Source: *NWD 86-2*, p. 15. Shot Harry in this series, a 32-kiloton surface burst, was detonated from a tower on May 19, 1953 and involved 900 troops in trenches 4,000 yards from ground zero. Harry produced fallout problems off the test range that were exacerbated by weather patterns. Source: Philip L. Fradkin; *Fallout, an American Nuclear Tragedy*; 1989; Tucson; University of Arizona Press; p. 3 and pp. 102–4. (hereafter referred to as *Fallout*)

[81] subjects whose eyes had become adapted to seeing in the dark. These studies were necessary to determine the extent of acute and chronic impairments that might affect personnel flying at night when suddenly blinded by the flash of a nuclear explosion.

[82] Operation Teapot was an atmospheric nuclear weapons test series at the Nevada Test Site, involving 14 shots detonated between February 18 and May 15, 1955. With yields ranging between 1 kiloton and 43 kilotons (*U.S. Nuclear Tests*), the tests were part of the development of various tactical nuclear weapons. During Operation Teapot, 8,000 Department of Defense personnel participated in troop exercise Desert Rock VI. Source: *NWD 86-2*, p.16.

PETERSEN: Yes, you can interpret Dunham's letter pretty clearly in that direction, saying, "If you're going to do this, do it under these conditions." He outlines the [requirements] and then that set of conditions is reiterated to the investigators. [Those conditions are adhered to] through the period.

FISHER: But the impetus, the proposal came from—

PETERSEN: —From here [(Los Alamos National Laboratory)].

FISHER: From the principal investigators at Los Alamos?

AEC and Military Differ on the Use of Human Subjects

PETERSEN: There were problems, as you know, early on, in the whole business of worker protection. Pinson, Langham, and Anderson were interested in what kind of a hazard tritium[70] posed. The two exceptions are flash blindness, which was dealt with in sort of an *ad hoc* way at Ranger.[71] That's when it was noticed that people knew enough not to look at [detonations]. There was some question about what would happen if you did. There was at Buster Jangle[72] a C-54[73] with 19 volunteers that [circled] seventeen miles away and exposed the airplane sideways during the detonation so [volunteers] could look at it. Then they were evaluated to [see] how long the flash blindness existed.

FISHER: Evaluated by staff at Los Alamos?

PETERSEN: No, Los Alamos had nothing to do with the experiment. As a matter of fact, there was a heated exchange [of letters] between Tyler and Fields[74] that says, "If you are going to do stu[dies] like this, we want a letter absolving AEC of any responsibility. [Shields] Warren doesn't want to

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70 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.

71 Operation Ranger was the first series of nuclear weapon tests for which the Nevada Test Site was used. Starting with the first test in the series with a one-kiloton weapon on January 27, 1951, a total of five weapons were airdropped, each from a B-50 bomber. Yields ranged up to 22 kilotons (*U.S. Nuclear Tests*). The series is reported to have been preparation for the May–April 1951 Greenhouse series in the Pacific. Source: *NWD 86-2*, pp. 12–13.

72 Operation Buster-Jangle was a series of seven nuclear weapons tests conducted at the Nevada Test Site, in which nuclear explosives were detonated between October 22, 1951 and November 29, 1951. Ranging in yield from 1.2 kilotons to 31 kilotons (*U.S. Nuclear Tests*), the tests included four airdrops and a tower, surface, and crater shot. The last three types of tests generated large quantities of fallout because the explosion sucked up rock, soil, and debris from the crater it created and from the surrounding surface area. During Buster-Jangle, the first three of eight Desert Rock troop exercises were conducted by the Department of Defense to explore nuclear battlefield conditions and tactics. Source: *NWD 86-2*, p. 13.

73 a four-engine cargo plane built by Douglas Aircraft for the military as the C-54 Loadmaster and for civilian airlines as the DC-4 passenger plane.

74 Carroll Tyler was a nuclear weapons test manager at Los Alamos Scientific Laboratory. At the time of this exchange, Brigadier General Kenneth D. Fields was assigned to the Armed Forces Special Weapons Project, an antecedent of today's Defense Nuclear Agency. Later, Fields became the director of the AEC's Division of Military Applications (DMA).

other words, classical pathological description of radiation effects in animals was one of his principal responsibilities.

He was also the pathologist at the Los Alamos Medical Center, and he supported the Medical Center as any pathologist would, not as a research individual. That led to his contacts. If you know Lush, you know that he's a very gregarious and likable individual. He had a great many friends in the regional medical profession, and he bent their ear at every opportunity, looking for ways to exploit the capabilities that the lab had. It was in that vein that a lot of this work was actually done.

FISHER: At that time, was the Los Alamos Hospital an official part of the Laboratory or a separate entity?

PETERSEN: It has always been a separate entity.

FISHER: Did Dr. Lushbaugh then have separate paychecks? One as a pathologist at the hospital, one as a biomedical scientist at the Laboratory?

PETERSEN: I can't answer that authoritatively, but I do know that in the mid '50s, the arrangement became that he was paid entirely out of University of California Health Division funds.[83] I don't remember the date of this either, but mid '50s is as close as I can come. That, of course, implies that there was some sort of [previous arrangement].

FISHER: The reason I'm asking this is that I'm wondering if his position as a medical doctor in the community and his position as a scientist in the Laboratory facilitated his experimentation on human subjects with radioactive materials.

PETERSEN: I'm sure that there was some facilitation of access to patients, but in a sort of "reverse English" way. He had certain diagnostic procedures that he was working on, trying to improve. Physicians in the surrounding community had patients that needed those diagnoses. Paul Lee[, who was running the standard diagnostic capability in the Hospital,] wasn't particularly interested in this sort of thing. His interests lay elsewhere. As a consequence, physicians could send patient[s] here, have them housed at Los Alamos Medical Center overnight. If they couldn't afford to pay, Lush could pay their bill, [perform] the diagnosis, and [provide] a specific written diagnosis of the patient evaluated for the condition requested by the attending physician.

That happened all the time. Physicians would send him patients for diagnosis. These diagnoses eventually accumulated into a publishable series and, whenever they did, a paper got written. He did not, as in a major teaching hospital, assemble [patients, saying in effect]: "Get all the guys to line up, send me your anemia [patient]s today, send me your leukemia [patients] next week, I want to see your thyroid patients the week after." That never happened.

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[83] The University of California was responsible for managing and operating the Laboratory.

CAPUTO: I was hoping you could describe the institutional decision-making process. If Defense wanted to do a certain experiment, what process would it go through so it was actually conducted?

PETERSEN: We have a pretty fair record of the institutional interrelationships, especially with regard to biomedical experiments. Some of it is not very complimentary correspondence.

There was, in the very earliest times, a steering committee that decided among candidate experiments, which ones would be performed. You can [appreciate] that the primary reason for setting [off weapons] is to acquire information about [performance] so the next time you set one off, you [have] fixed what bothered th[at] particular device. The underlying [reason] for the entire test program is design improvement.

CAPUTO: Of the bomb?

PETERSEN: Of the bomb. The rest of these things are, in a sense, piggyback experiments of various degrees. Some of them are piggyback to the extent that they're *ad hoc* as far as the weapons test is concerned, and they don't get very much sympathy [(priority)] because there are enough things going on, in terms of gathering data, gathering debris from the detonation, how it was set off, whether it was an airdrop or whether it was a tower. All of those problems were being addressed right at that time, in real time, as fallout issues.

Sitting on top of all this is the test director, who has to decide how many kilowatts, how many square feet, how many trailers, how many trucks, all of th[e] logistical [details] in addition to the technical requirements of ancillary experiments that don't have anything to do with performance of weapons.

During the taped interview, part of the beginning of the interview was accidentally recorded over. At the end of the interview the interviewers tried to recapture the last portions. Thus, the interview at this point in time switches to a new topic.

PETERSEN: I don't believe so. I think the leukemia work was done, or the ^{32}P work on blood dyscrasias, was done largely at UC San Francisco, and it's very, very early.

Dr. Lushbaugh's Diagnostic Use of Radioisotopes (1956–63)

FISHER: Getting back to Dr. Lushbaugh and his specific responsibilities in the Lab, what was his primary responsibility in terms of either developing diagnostic techniques for isotopes or utilizing isotopes in biomedical research? What was his mission, his charge, and his responsibility?

PETERSEN: His work was as a principal investigator. He was the head of the section. He had responsibilities in approaching radiation effects in the animal work that was going on here from the standpoint of the pathologists. In

FISHER: These events took place during the birth time of nuclear medicine.[84] Was the diagnosis and treatment of cancer ever a part of Lush's responsibility?

PETERSEN: The diagnosis of cancer was clearly his. He did have an interest in the behavior of formed elements in the blood in leukemic patients. That was primarily surveillance. In other words, the physician was treating the patient somehow, [for example] using chemotherapy;[85] it was brand new. There was nothing like we know today, in terms of aggressive chemotherapy. That was clearly a part of what he had an interest in. But the iron work was largely related to anemias and iron metabolism.

Use of Humans to Calibrate a Whole-Body Counter; Study of Iron-59 Metabolism

FISHER: Can we come back to the iron-59[86] studies?

PETERSEN: Sure.

FISHER: This is interesting, too. One of the early uses of the whole-body counter was to look at iron-59 metabolism. There has been a suggestion in some of the papers that have been uncovered that Los Alamos firemen were used in some of these studies. Can you recall?

PETERSEN: There were ten of them.

FISHER: And how firemen got to be chosen for this?

PETERSEN: This was a one-time study that was done on the transition between the original human counter and the new one.

FISHER: The HUMCO II?

PETERSEN: HUMCO II. HUMCO I and HUMCO II are the way they're referred to in publications.

The HUMCO II was equipped with sixteen-inch photomultiplier tubes in an array, I think hexagonal,[87] [with] twenty-four tubes. The result of this design change from the many multiple little photomultiplier tubes of HUMCO I was about a tenfold increase in sensitivity so that, whereas you had to look fairly carefully and for some time to do natural potassium [counting], for example, in HUMCO I, HUMCO II [made the measurement] in about 100 seconds. Depending on the energy of the emission, it was between one and two orders of magnitude more sensitive.

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[84] diagnostic and therapeutic medical techniques using radionuclides or radioisotopes

[85] the treatment of disease by means of toxic chemicals that kill cells or inhibit their ability to grow and multiply

[86] Iron-59 has a half-life of 45.1 days. Unlike iron-55, iron-59 emits beta and gamma radiation.

[87] shaped like a hexagon, a closed, six-sided figure

It was as it came into practice setting and, many times, not enough material was ever accumulated to write a paper, largely because even the surrounding community was a pretty young, healthy community. There weren't a hell of a lot of sick people.

AEC Procedure for Authorizing the Use of Radioactive Materials on Humans (Late '40s Onward)

FISHER: Was it understood in the 1950s that only a physician, such as Dr. Lushbaugh, was authorized to administer radioactive materials to normal subjects?

PETERSEN: That was a ground rule that was set up by AEC early and never changed: the supervision by medical person for any isotopic work. [Chet] Richmond was not an M.D. His activities were always supervised by one of the staff M.D.s, either [Harry] Foreman or Lushbaugh or [Irene U.] Boone. Anyway, that was a rule. Implicit in the permission to do the experiment [was to have] an M.D. in attendance and supervising. We've uncovered quite a bit of the paper traffic, but we've never uncovered a written consent.

FISHER: For that time period. That's interesting.

PETERSEN: It wasn't done. It was not a custom.

CAPUTO: Was consent and having two physicians sign a medical chart done?

PETERSEN: [Howard] Wilson thought that would be a good idea early on. In the case of our diagnostic work, no, it didn't happen. But what went on was that a physician would indicate to a patient that they needed a diagnostic test. They would request that Lush do the test. Lush was providing the isotope from his research funds. The patient was not charged for the procedure. The diagnosis was evaluated in a quantitative sense from the data collected. That was handed back to the physician in writing to put in the patient's chart. There was usually an acknowledgment note by the physician, and then, that's in the patient's chart.

We've actually dug up a couple of those to see if, indeed, that's the way it happened. Howard Wilson, one of the practicing pediatricians here, used Lush a lot for diagnosis in little kids.

FISHER: Primarily for thyroid diseases?

PETERSEN: He suspected thyroid in a couple of cases. It was a thyroid [case] that I actually was able to uncover. She's [the subject's] a fourth-year medical student this year.

FISHER: Can you think of other diagnoses, other than thyroid, that Dr. Lushbaugh performed?

PETERSEN: Yes, lots of them: a lot of blood-forming disease problems, liver function. I mentioned his acute rose bengal variation. Vascular competence. There's a long list, and I'm embarrassed that I can't rattle it off.

PETERSEN: Just a nice easy convenient population, and Lush was always the one that gave them their orientation lectures on radiation effects, crew by crew.

CAPUTO: So, they were Los Alamos National Laboratory firemen?

PETERSEN: Yes.

FISHER: Were women ever chosen as the subjects of either radionuclide metabolism studies or label[-ed compound] studies? Can you recall any specific examples?

PETERSEN: I think the women, normal women, were in the chromium study. There w[ere] normal women in iodine studies. There were normal women in iron studies, lots of them.

FISHER: "He" meaning Dr. Lushbaugh?

PETERSEN: Lush. [One problem he studied] was iron metabolism in menstruation difficulties. A number of gals were on that study, as I recall.

FISHER: Is this published, or are there documents that relate to it?

PETERSEN: [A book chapter in 1965 is the best summary].

FISHER: That's one that I'm not aware of.

PETERSEN: I suppose I could dig out some documentation on that. His notebooks are being sent in. If you read through there, you would find it. His notebooks are hard to read, of course, just like everybody's are. If you know what you're looking for, you'll find something. It took me a hell of a long time to find [mention of] the firemen.

FISHER: Have you been able to identify the names?

PETERSEN: I know several of them personally, and they have remembered most of the rest of them.

FISHER: Is this being written down for record?

PETERSEN: One of the things that's being done is the callback. We've been so busy with some of these other things that they're still tail ends. [The] callback [is dealing] with a couple of the firemen on that [issue]. I think, other than that, probably there is nothing.

If you're going to use iron in a patient, with a machine like that, then you want to have your first dose and your last dose inside the dynamic range of the counting capability of the incident. You don't want to either have the patient too hot[88] to count to begin with or not hot enough to count at the end.

Ten firemen were asked to volunteer to take [oral iron-59]. They were all given the same dose, as I recall, and the dose was [about] 200 nanocuries of iron-59. Nanocuries! This two-tenths of a microcurie dose was given, and then they were counted over a period of, I think, ten days. Several times during the ten-day period.

FISHER: Who was the principal investigator?

PETERSEN: That was Lush. Phil Dean was actually [in charge of the] HUMCO II [development] during its initial phase. Ernie Anderson designed it, but Phil Dean operated it and tweaked it.

FISHER: Was the purpose of the experiment to look at iron metabolism, or to determine capabilities of the counter, or both?

PETERSEN: There are two ways to do this. People work better [as calibration subjects] than big sugar phantoms.[89] You can put a big sugar phantom in there with that amount of iodine and count it, but the geometry is not the same as it is if you are circulating iron dynamically in an individual. The counter can tell the difference between those two. It was sort of a direct calibration attempt, so that all the patient work with iron-59 would be in the right ballpark in the new counter. [The metabolic data are also an integral part of the study].

FISHER: Did patient studies with iron-59 follow these early calibration studies?

PETERSEN: Yes, I think there were quite a few patient studies that followed. I think I even know where I could go to count them if you needed to know. They would be in [the semiannual reports of the early '60s]. Lush left here in '63, so '56 to '63 are [the years for] his involvement, but the involvement goes much later than that. As a matter of fact it's still going on with other people interested. [However, the] big [liquid scintillation] counters have been [retired]. The interest in that kind of research [is] gone. The answers have been found. There is not a contemporary counterpart to that kind of study. In biology here, we're gene sequencing.[90]

FISHER: Why were firemen chosen?

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88 radioactive

89 a cylinder of sugar (sucrose) weighing about 150 pounds that serves as a surrogate for a human being during calibration of radiological counters. Sugar's molecular makeup (largely carbon, oxygen, and hydrogen) absorbs ionizing radiation almost as effectively as the human body.

90 Petersen is referring to Los Alamos National Laboratory's participation in the Human Genome Project, a broad-scale program sponsored by the National Institutes of Health to map the location of every gene of all 47 human chromosomes.

over the reactor top, and retrieving him was an activity that went on for several days.

CAPUTO: So, they died of the trauma of the accident, not from radiation exposure?

PETERSEN: Yes, that was our conclusion.

FISHER: What was [the reason for] the autopsy? To determine cause of death or to decontaminate the bodies?

PETERSEN: Both. "Decontaminate" is the wrong word, but there were some areas of quite severe trauma where debridement[94] was possible and decon[95] by usual scrubbing techniques simply couldn't be performed because of the intense radiation field.

CAPUTO: Before the autopsies were performed, was permission gained from the families?

PETERSEN: I don't have any idea, because these were military [operations] and conducted under circumstances where there was some question about the circumstances of the critical excursion. I'm sure that there would be some authority invoked at some level. I presume that somebody in their reactor training program would have authority to request that.

FISHER: Were the bodies transported to Los Alamos for analysis?

PETERSEN: No: pretty standard autopsy tissue specimens were fixed and returned to Los Alamos. For example, I removed substantial hair specimens from various parts of the bodies and brought them back here for analysis, because the hair sulfur could be used as a threshold detector [for neutrons over 2 MeV (million electron-volts)].

FISHER: You published extensively on this topic, the analysis of neutron criticality accidents by activation analysis.

PETERSEN: Yes, that was done first with the accident[96] here at Los Alamos out at DP.[97] It worked fairly well, with some guesses. We've done this on several occasions. We were able to do something about dose estimates and provide evidence for the multiple excursions at the United Nuclear Plant at Wood River Junction, Connecticut,[98] for example. We were able to show that that [tank] went critical more than once. That's the only one.

FISHER: In the specific handling of the worker's bodies from the SL-1 accident, was there anything that you observed that may have been inappropriate in any way?

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[94] surgical removal of lacerated, devitalized, or contaminated tissue

[95] decontamination

[96] the "Kelley case," December 30, 1958. For details, see "Investigations of Radiological Accidents" in the Lushbaugh transcript (DOE/EH-0453), April 1995.

[97] DP West was the site of the early plutonium chemistry labs before the CMR building was built.

[98] an industrial accident involving the chemical processing of spent fuel. The accident was essentially identical to the Kelley accident.

Postmortem Assistance Following the SL-1 Reactor Accident (1961)

CAPUTO: I was wondering if you helped Dr. Clarence Lushbaugh with the autopsies after the SL-1 accident at Idaho Falls?[91]

PETERSEN: Yes, I was there. And, I did assist him. That was a problem that was dictated largely by the intense radioactivity of the bodies themselves. We had to be very, very careful and efficient and be sure that we got things done properly the first time because there was no time to be leisurely.

The calculated doses for each procedure were very carefully surveyed by a monitoring team that was also there. They would calculate how much time you could spend in that intense radiation field to accomplish each of the activities, and then you would retreat to a shielded position and think about what you were going to do next. Nobody stood around in that radiation field.

At one time, there was a very short period of time when that field was on the order of 1,500 R [(roentgen)] an hour. It was pegging[92] a Jordan [radiation counter], and we had to do something about that very quickly. The reason for the intense field was because the fission products were blown up into the bodies [of the victims] when the [fission] excursion[93] took place. You had some very, very hot focal areas that had to be dealt with in order to get the general field [of exposure] down.

CAPUTO: Did you use remote controls at all for this?

PETERSEN: There were no remote controls. The only remote controls were used here for analysis purposes after we got the samples back. That was done under circumstances where it was largely improvisational from start to finish. There were hoists and things like that that helped [but no] remote control from the shielded position.

FISHER: What was your responsibility during the autopsy?

PETERSEN: I did whatever I could to help. My principal responsibility was trying to get an estimate of the doses to the individuals. One was just barely alive when they [(the rescuers)] first went [in]. I think it was Ed Vallario who carried the one off the reactor top who was still alive. He died very shortly thereafter in the ambulance. The other two were dead on top of the reactor. One had been [thrown] up and tangled in the superstructure

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91 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).

92 making the indicator needle swing all the way to the high end of the scale

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

PETERSEN: I've always been a cheerleader. That's kind of a tough question. Without having [it] sound self-serving, that's a difficult [question] to answer. I would guess that, over time, it would have to be the progress in cell biology. Because we went from [pursuing] an interest that we couldn't convince Wright [Langham] was important to [being] probably the leading lab in cell growth and kinetics in the early '70s. Exploiting and actually [pushing], during that time, the development of instruments that became commercial successes.

A lot of the understanding of cell growth and division dates from that time. That's the work of a lot of people. Ernie Anderson, George Bell, Ron [Walters], Paul Kraemer, Marv Van Dilla. We had a big gang of people. Van Dilla and Mullaney, in parallel, ran the development of the instrumentation.

The idea that you could do cell sensing and sorting was being [pursued] in parallel. That was Phil Dean, Dick Hiebert, and Scott Cram. The litany—it's a large number of people that performed those tasks. As I say, I sort of was the cheerleader—front man and cheerleader.

FISHER: We've read and reviewed your list of publications, and there are many on radiation effects on enzymes, of course. Different aspects of cell growth and radiation, the cell cycle, and differential effects of radiation on cells and at various stages in the cycle. This is an opportunity that you have to describe, for those who read this, those accomplishments in your career that you're most proud of.

PETERSEN: I would have to stay with *that* [(my hand in making the Lab the leader in cell biology)]. I think the assembly [of a great team] and the fact that those people worked so well together. I worked at the bench with them. We saw the sun come up a lot of times: Bob Toby, Ernie Anderson, and I would run experiments that would last for 96 hours or longer, and we would simply stay with them that whole time. I guess that's the thing that I'm most comfortable with, that part of [my] scientific career.

Leadership of the Los Alamos Divisions and Groups Following the 1974 Reorganization

FISHER: Then you had major responsibilities in division management, life sciences.

PETERSEN: Yes, in 1974, I think, I don't remember the date. You can date it exactly because it dates from when Chet [Richmond] went to Oak Ridge.[101] Chet was the alternate division leader and when he left to go to Oak Ridge, I took over as the alternate division leader of the Health Division. The division of responsibilities between George Voelz and me was that he, with his industrial medicine background, ran the operations of the division related to health and safety: H-1, the monitors; H-2, the health group;

[101] Richmond left Los Alamos in 1974 to join Oak Ridge National Laboratory as Associate Laboratory Director for Biomedical and Environmental Sciences.

PETERSEN: Well, let me put it this way: The trauma resulted in non-decontaminable wounds with sufficient radioactivity that the bodies could not have been returned to the families for burial. Some debridement was necessary. In a nonradiation field, I suspected you could argue that it would have been done with more artistry than we were able to do hurrying.

FISHER: What was the ultimate disposition of these bodies?

PETERSEN: They were all returned to the families for burial by them.

FISHER: Okay.

CAPUTO: Do you remember how Los Alamos became involved in the autopsies?

PETERSEN: I remember that quite vividly, as a matter of fact. That was a call that came from AEC, and we were asked to assemble a team. The call came in at ten o'clock Sunday morning.

FISHER: From?

PETERSEN: From Washington.

FISHER: Do you remember who?

PETERSEN: I think it was General Leudecke,[99] but I'm not sure. I can't remember. It was probably DMA[100] at the time, whoever that was. The request came to provide the team to assist the people at Idaho Falls. By two o'clock, we were in the air on our way up there. The next morning at seven o'clock, we started the examinations. Two bodies were available at the time, and they had been misidentified. One of the major early contributions of the autopsies was the unequivocal identification of who was who.

CAPUTO: Why was Los Alamos called?

PETERSEN: I guess we simply had a reputation that we could do things like that. Certainly, Lush had that reputation.

FISHER: And perhaps the fact that Dr. Voelz had come from Los Alamos, he might have recommended them to Washington.

PETERSEN: I suspect that might be true. I don't know the details [of] how the decision was made. My knowledge starts with [our instruction to] "Get ready to go."

Contribution to Cell Biology Research

FISHER: Would you like to describe for us your major scientific contributions during your work at Los Alamos?

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[99] General Leudecke, who was a general manager of the AEC at that time, was deeply involved in the investigation of the accident.

[100] the AEC's Division of Military Applications

competition. There's a watershed in about 1970, when people had to start writing 189s. That essentially defined a program, because people were writing, first of all, about what they were doing. At the time there had been some guidance from AEC, ERDA,[103] and [then] DOE developing a stable-isotopes program and things of that sort. I think that if you were to summarize, in just a few words, the budgetary conflicts have essentially destroyed what was a golden age in the mid-'50s and up until about 1960.

FISHER: I often refer to this era as the "golden age of radiation research." Not because funding was unlimited, but because so much was accomplished in so little time. And so much information was learned by so few investigators.

PETERSEN: There was a feeling. I'm sure it exists today and the reason I don't appreciate it is I [am not familiar with] the details of the research that the people are doing [now]. I [feel that feeling] doesn't exist, but maybe it does.

But back when we were working, the only way you could tell the difference between night and day was whether the lights were on.[104] The people worked around the clock, and they worked in an almost driven mode. And yet, I've never seen enthusiasm and excitement and the sense of urgency and mission replicated that was existent during that mid-'50s to very early '60s time frame.

I suspect that it has to do largely with the funding arrangements. Also the burgeoning legal requirements and ramifications. [They] simply didn't exist! It's amazing that people find it so hard today to believe that nothing was written down. The whole time, [we] operated under a sort of handshake mentality that required documentation only in terms of what was published. The record keeping, even the early notions of patentable [ideas], the first time we ever were careful about patentability documentation was with the cell counters. The human counter doesn't have that. Lyle Packard[105] is a millionaire, time and time again. Newt Hayes never realized a nickel out of that business. Yet, Newt Hayes and Wright Langham are really the genuine inventors of workable scintillation spectrometry.

FISHER: That's interesting.

CAPUTO: The J Division at Los Alamos was responsible for atomic bomb testing, and the H Division was responsible for biomedical aspects?

PETERSEN: And health and safety.

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[103] [U.S.] Energy Research and Development Administration, predecessor agency to the Department of Energy

[104] that is, whether the lights were on in the offices or labs of coworkers who would leave at 5:00 or 6:00 pm. There were no windows in some of the defense-related laboratories such as Los Alamos.

[105] founder of Packard Instruments, which used LANL's unpatented developments in scintillation spectrometry to develop a successful commercial line of well counters. Using a well counter, a researcher could simple place a tissue sample into the well counter, and the radiation rate would be counted automatically.

[and] H-3, the safety engineers. I had H-4 [(Bio-Medical Research)], Wright's old group; and some of the environmental people and the research and service functions of the division were divided in that way.

The assistant director of research delegated to the Health Division the responsibility of assembling the entire biomedical program package that, by that time, had branched out so that Nuclear Chemistry had a small bunch of people that were actually working in stable isotopes, as an example. T Division [(Theoretical Physics)] had a large theoretical biology effort going. There were nests of biological activity related primarily to what would eventually be called structural biology, but was clearly not structural biology then. There were nests of these people scattered throughout divisions in the Laboratory. There were some in T, some in CNC.

FISHER: CNC standing for?

PETERSEN: Chemistry and Nuclear Chemistry. Theoretical Division is T. Those were the major ones. There were other people in other divisions with interests that were funded by what was by then the Office of Health and Environmental Research. I babysat that whole [effort in terms of attending to programmatic funding].

FISHER: Your responsibility then evolved into Program Manager for Biology and Chemistry for the Laboratory, which is a position you held until retirement?

PETERSEN: Except that the program management position was a little broader. It was actually designed originally to be not an interface with the DOE, but an interface with the branches of the [armed] services. In later years, I learned to speak "military" without an accent. [That was] hard to do.

FISHER: Who was the primary contact with [AEC] during this same era?

PETERSEN: Mark Bitensky had the division, but he came at it from a little bit different direction. A lot of that responsibility fell on [Marty Holland,] Dwayne Enger, and Ron Walters. Eventually Ron took it over, and Ron was the principal contact out of the ADR's[102] office. But it took [quite] a while. There was a lot of groping and trying to figure out how to do that.

FISHER: Was part of this due to the changing nature of [AEC] Headquarters?

The Research Culture at Los Alamos During the "Golden Age" (Circa 1955–60)

PETERSEN: Yes, but a lot of it also had to do [with the Laboratory]. If you have my advantage of being able to look back over a long stretch of time, the relationships and the camaraderie and all of the other things [that were] hallmarks of the Los Alamos operation over time have deteriorated monotonically as the struggle for funds ha[s] developed more and more

[102] Assistant Director for Research

Relationship Between the Health Division and the Weapons Design Division

CAPUTO: What is the relationship with the H Division and J Division when it comes to atomic bomb testing and biomedical planning?

PETERSEN: The biomedical planning for the tests was in early days done by largely the same. That is, the screening of experiments and the design of experiments was a multiple representative arrangement where AEC had input, but delegated to Los Alamos as its testing capability large chunks of responsibility for getting weapons tests done.

The experiments on Crossroads,[106] the Crossroads was really done as a quasipolitical demonstration that we could do it again.

FISHER: Meaning?

PETERSEN: Meaning that we had more [atomic bombs], and [therefore, Soviet leaders], "Don't step out of line." It was a pretty strong [message]. Instead of bombing Japanese cities, we'd set them off [(the bombs)] in a remote lagoon in the Pacific, but the bang would still send that political message.

Sandstone[107] [began the] interest in the diagnostics and the improvement of weapons design. Greenhouse[108] [was] a full-fledged set of design tests with a large biomedical component.

George LeRoy from the University of Chicago was the biomedical director at Operation Greenhouse and [Wright] Langham was his assistant. Much of the work that was done [in the Pacific], and the preliminary work that provided the baseline for it was conducted here.

The continental series starting with Ranger[109] [became] more and more sophisticated as you move forward, too. The idea of involving troops start[ed] with Buster-Jangle[110] and then Tumbler-Snapper,[111] Upshot-Knothole,[112] Teapot,[113] and on up the line.

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[106] See earlier the descriptive footnote under "Nuclear Weapons Fallout Studies (1946–54)."

[107] *ibid.*

[108] *ibid.*

[109] See earlier the descriptive footnote under "AEC and Military Differ on the Use of Human Subjects."

[110] *ibid.*

[111] a series of eight nuclear weapons tests at the Nevada Test Site detonated between April 1, 1952, and June 5, 1952. Four were airdropped; the other four were tower shots. Yields ranged between 1 kiloton and 31 kilotons (*U.S. Nuclear Tests*). During the series, 10,600 troops were present to participate in Exercise Desert Rock IV. Tumbler is reported to have been designed to collect data on the effect of height of burst on blast overpressure; Snapper is reported to have tested potential warhead designs and techniques to be used in the Ivy series conducted in the Pacific between October and November 1952. Source: *NWD 86-2*, p. 14. Shot Easy in the series is reported to have produced fallout incidents as far away as Salt Lake City, resulting in a letter of protest from the Governor of Utah to the Chairman of the AEC. Source: *Fallout*, p. 101.

[112] See earlier the descriptive footnote under "AEC and Military Differ on the Use of Human Subjects."

[113] *ibid.*

But the question that you asked originally, "What's the relationship between H and J?" J is largely a physics operation. It has to do with what makes bombs go off. It has to do with how they're designed, what the design parameters would provide in terms of information of the kind that permits the design people to go back and redesign for improvement.

The biomedical problems were twofold. There was the [radiation] effects problem, and th[at] was done, in the test series, exclusively (I can't emphasize that enough) with animal models, with two exceptions, both introduced by defense community entities.

The relationship between J Division and the Health [(H)] Division was that J Division was overall responsible for the weapons tests.

In the Pacific tests, th[ere] was a joint task force where the AEC and the military departments [shared] responsibilities. In the continental tests, starting with Ranger, this was done differently. Los Alamos had a much larger role to play in terms of overall logistical responsibility in the continental tests than they did in the Pacific tests. That doesn't diminish their responsibilities in the Pacific tests, but all of the Pacific tests were done by a joint task force. And in the continental tests, the AEC assumed responsibility for those tests and everybody else piggybacked on top of that fundamental responsibility.

So there is a thread that goes through from early times, where Los Alamos had virtually entire responsibility for health and safety. Then, the military started to involve their people beyond Buster Jangle, which incidentally [annoyed] Tom Shipman here because he had th[e] whole responsibility dumped in his lap at a very late time, when it was very, very difficult to acquire enough competent people to assume the extra health and safety responsibilities that [the] combined Buster and Jangle tests [required]. You see, in the early times, Los Alamos had a pretty good handle on everything. That is, Los Alamos H Division in health and safety, and also in describing what's going to happen in the biomedical tests.

The first of the Pacific tests, [Greenhouse], [that] did have a major biomedical component, had a director from the University of Chicago, George LeRoy. Much of the preliminary [investigation] was done here. Baselines were [established] here [and at] Oak Ridge. The whole AEC community participated in those tests. The whole AEC community representatives largely were responsible for defining what the test program looked like from the standpoint of experimental design, what biological questions were asked, what biological questions were answered, what test object would be involved, would it be mice [or] would it be something else[, etc.]

Then, as the military started using the continental tests as a familiarization tool for units—combat regiment-sized groups of people, five thousand [troops at a time]—the health and safety headaches had to be spread out, and military responsibility for health and safety started. But at the same time, there were military experiments that were proposed,

and some of those were disallowed by the screening committee, and there got to be some real confrontations.

The committee actually fell apart [but] then was reconstituted by Shields Warren and stayed as a committee. I've identified all of the players on that screening committee. They looked at both AEC and DoD [(Department of Defense)] [studies]. [The committee included] representatives of both and looked at the AEC and DoD experiments and decided which ones would be conducted.

It's within this framework that I mentioned that exchange between Tyler and Fields, suggesting that the flash blindness studies were something that the AEC disapproved of, and if they were going to proceed, they needed some assurance that responsibility would be assumed by the military.

That is a thumbnail sketch of how experiments were included in tests. It's a question of comparative examination [and] evaluation: "Is this an important question, is it more important than that one?" Because, there are a limited number of things that can be done [in a given weapon test], when you stop to think that most of what's going on out there has to do with bomb building, not biomedical experiments.

With those two exceptions—the flash blindness and the psychological impact on troops on the ground—[human subjects have] never been involved in weapons tests. There was some problem about interpretation here, I realize, but the guys flying airplanes are asking questions about crew performance. They're asking questions about dose. They're asking questions about things that have to do largely with the occupational exposure of people in that environment.

In other words, it's much easier to look at radiation doses on people by other techniques, than it is by shooting off a bomb. It's a terrible way to do experiments.

If you just think about that for a minute, the idea that there is [a] ghoulish, sinister effort on somebody's part just doesn't wash, simply because the kinds of answers that you get are so unsatisfactory. You do the best you can to see how much dose you gave to somebody. But it's an occupational record, not an attempt to do something to him that will cause some anguish at a later time. The whole notion is really one that I know is quite popular in some quarters, but if you know anything at all about experimental design, it's such a rotten way to do experiments that you'd abandon it and go to something else.

Now, so much for the Health Division and the health community responsibility for examining the experimental package that went into these [nuclear weapon] tests. Almost everything of a biomedical nature, you can examine within the test planning records under Item 4.0. [For example,] 4.1 would be such and such; 4.1A or 4.2 [would be such and such]. These will all be biomedical experiments. In the case of the flash blindness, that shows up as a four-point experiment in the planning documents, so it's easy to track.

The things that might be harder to track, but still have no evidence of human involvement, are the civil defense activities that were also starting to be included, by the mid '50s. These are experiments on structures: If you get bombed, what happens [to buildings, bridges, et al.] in a structural sense? What happens in the biological sense as you move out away from ground zero?[114] These were designed and conducted largely [as] a civil defense responsibility. It involved other contractors as the people that had responsibility.

Overpressure, [is] one of the [hazards] that is of immediate concern. Can somebody stand overpressures [of] about 2 psi for a protracted period, for example.[115] These experiments were conducted largely on [human]-sized animal models; goats, sheep, pigs, that sort of thing. Organ damage was well-defined, from an overpressure standpoint. You can infer, I think pretty readily, what the shrapnel and flying debris kinds of problems are.

The radiation effects work had already been done probably well enough so that it could be extrapolated to populations. People were beginning to be fairly comfortable with [exposures of] 400 R and 1,200 R. Those numbers had started to become part of our assessment conversation. People seemed to think they knew where the thresholds of the various kinds of effects were, and this could be extrapolated to humans. I sat in a meeting in the mid '70s where AFRI[116] was trying to pull all this [information] together. The gaps in human activity were largely such questions as possible synergy between trauma and radiation or between thermal burns and radiation. Second-order curiosities like that were still of interest then. The primary radiation effects were pretty well accepted in everybody's mind.

FISHER: No studies were of interest to the military?

PETERSEN: Yes. And much of it designed in the later studies by military investigators.

Health Division Biomedical Responsibilities and Nuclear Testing

FISHER: You described some of the responsibilities of H Division during atomic testing. What were some of the biomedical responsibilities?

PETERSEN: Biomedical responsibilities were very large. That whole testing crew was really one of the apples of Wright's eye.

The spleen and thymus weight-loss studies were done in house for example, [and] remained within a very few rem[117] of ever changing, in terms of the risk tables. Some of the neutron [work] was done later.

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[114] the point on the earth directly below or at which an atomic or hydrogen bomb explodes

[115] Normal atmospheric pressure—the pressure exerted by the earth's atmosphere at any given point—is about 14.7 pounds per square inch (psi). In the aftermath of a nuclear blast, this value would rise to about 16.7 psi for hours or days.

[116] Armed Forces Research Institute

[117] a unit of radiation dose equivalent, or "rads times the quality factor, Q"

The other day, I ran across a really neat summary that I had known about for a long time. But it was never really needed because it's a summary of all of the neutron measurement techniques that we ever [used] here in Los Alamos. It was written in 1967 by Dale Hankins, and he talks about all of the foil work done [with] Godiva.[118] If the question of neutron dosimetry ever comes up, I sure would like whoever asks it [to] have that document. Despite the fact that it has nothing to do with human studies, it's a methodological bible of the kinds of things that were done over a period of a decade and a half.

FISHER: Were there any human studies involved in this work?

PETERSEN: No, I think that, I say no. We're kind of cross lines with the [Advisory Committee on Human Radiation Experiments] right now on this subject.

There was some hint in the early '60s Rover experiments,[119] [and] the Kiwi reactors,[120] that there might have been some exposure to thermal neutrons during the stem flights.[121] The dosimeters that suggest this never did more than hint at it. There w[as] never any definitive information that this had actually happened. But it set people to scurrying to devise methods so that if you did have thermal neutron exposure, you could find it.

Ed Ballinger and Payne Harris worked on these problems. They flew big bags of saturated [sodium] chloride [(table salt)] aboard the RB-57s[122] to be sure [they could detect sodium activation].

FISHER: And a short half-life.

PETERSEN: Yes, sure, it goes away in a hurry. You have to get back on the ground and hurry up and get people to where you can make some measurements. That whole effort was set up with the help of Godiva, and they actually had

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[118] a remotely controlled, uranium-235–fueled critical assembly reactor operated by Los Alamos Scientific Laboratory. Godiva first went critical in 1967. Source: *Directory of Nuclear Research Reactors*; STI/PUB/853; International Atomic Energy Agency; 1989; Vienna; p. 456 (hereafter referred to as *IAEA*).

[119] a research and development program initiated by the AEC and the U.S. Air Force in 1957 to develop nuclear rocket propulsion. The National Aeronautics and Space Administration (NASA) replaced the Air Force as cosponsor in 1960. AEC had responsibility for the nuclear aspects of the program. AEC work was assigned to Los Alamos Scientific Laboratory and Lawrence Radiation Laboratory; field experimentation was conducted at Jackass Flats, Nevada. The Kiwi series of experimental reactors was part of Project Rover. The relatively long development lead time associated with nuclear rocket propulsion caused the program to lose funding priority to chemical rockets in the 1960s. Source: Linda Neuman Ezell, *NASA Historical Data Book, Vol. II: Programs and Projects 1958–1988*; National Aeronautics and Space Administration; Washington, DC; 1988; p. 473 (hereafter referred to as *NASA*).

[120] a series of reactor experiments related to development of a direct-cycle nuclear rocket engine for propelling space vehicles. A Kiwi reactor went critical in 1965 and thereafter was shut down (*IAEA*, p. 788). While the program struggled on with developmental problems, most of the funding was cut in 1963. For a detailed history, see *NASA*, pp. 476–88.

[121] posttest air-sampling flights through the "stem" of the mushroom-shaped cloud produced by a nuclear detonation

[122] a twinjet U.S. Air Force reconnaissance/bomber (hence *RB*), nicknamed the *Intruder*, that was used for air-sampling missions in connection with some U.S. atmospheric nuclear weapon tests

Jezebel[123] and Topsy[124] in Nevada[125] for a time, doing these same [measurements]. These are all critical assemblies of fissionable material; you can bring [them] into close proximity and get [bursts] of neutrons [of] various energies out of them. They all have different spectra.

FISHER: Which one of those blew up?

PETERSEN: Godiva. Known as Godiva because it was unshielded, a naked reactor.

Dr. Wright Langham's Postwar Studies of Plutonium

FISHER: We've just concluded discussions about the role of H Division in atomic testing in support of both the Laboratory mission and the Department of Defense. Now I'd like to ask a question about plutonium research, research on the metabolism and biokinetics of plutonium by H Division and, in particular, [by] Dr. Langham after the 1945-to-'46 period and up to the time he died. Could you describe some of the work that was conducted here, not so much on occupationally exposed individuals, but on humans if there were any studies?

PETERSEN: The answer to your question is that Wright had an abiding interest in plutonium and enjoyed a reputation as one of the people most knowledgeable about the metabolism of plutonium throughout his career. But the experiments were largely experiments that were peripheral to human exposure and experiments that were designed to provide answers to problems of human plutonium exposure.

The example would be if a nuclear-powered rocket aborted on takeoff, or if one of the Explorer vehicles[126] that carried plutonium power supplies aboard burned up on launch and rained down particles. This was essentially where [John] Gofman[127] and [Arthur] Tamplin[128] and [Tom] Cochran [of the Natural Resources Defense Council] got their idea that one of these particles of plutonium embedded in the lung would be far more carcinogenic than diffuse exposure to plutonium. That was from the time of that first enunciation, the so-called "hot particle" project.

[The] hot particle problem was an interest of Wright's. It was approached in a number of ways. He collaborated with Chet Richmond, for example, on the question of whether or not particle density made a dif-

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[123] a plutonium-fueled critical experiment facility at Los Alamos Scientific Laboratory

[124] a critical assembly

[125] the Nevada Test Site, where most nuclear weapon tests within the Continental United States are conducted

[126] a series of spacecraft launched by the United States for interplanetary exploration

[127] For the transcript of the December 20, 1994 interview with Gofman, see DOE/EH-0457, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. John W. Gofman, M.D.* (June 1995).

[128] Tamplin worked with Gofman in the Biomedical Department of Lawrence Livermore Laboratory, where he gathered international literature on the effects of nuclear fallout on animals and humans. Tamplin's close work with Gofman and involvement with the human radiation research community are discussed throughout the Gofman transcript.

ference in transit time through the gut.[129] They used heavy particles [of uranium carbide]. In order to get something light that would also be traceable [by] radioactivity, they used zirconium microspheres laced with manganese-54. They administered these particles and watched the transit time through the human gut. This didn't involve plutonium, but it was simulating a problem that would be encountered, and human volunteers were used in looking at the transit times of these particles of high density or high mass through the gut.

FISHER: Do you remember who the subjects were?

PETERSEN: Yes, they are all identifiable. There are 57 of them. We know them all.

FISHER: Were they Lab employees?

PETERSEN: They are all Lab employees, except one, the wife of one of the investigators [(Richmond)].

FISHER: Who was the principal investigator?

PETERSEN: Chet Richmond, I think, is the principal on that program, but this was one of Wright's real interests. He was enthusiastically and deeply involved in space exploration. Before Randy Loveless's death, I think it was more Randy than anybody else that got Wright involved in this program.[130]

Of course, we had all of the potential astronauts up here. We ran them through the human counter and did all of the tests on them that were supplemental to the kinds of things that Loveless did to them down in Albuquerque. All of the Apollo guys and all of the Mercury guys were subjected to [that test battery].

But the gut transit time experiment is directly related to the possibility of a radioactive source, a plutonium source, burning in such a way that particles of this kind could be [swallowed or] inhaled and lodge in the lungs of people. If Gofman was right, this could really be a tragic situation because it would expose people in fairly large areas to an almost inevitable lung hazard. Designing ways of looking at that potential hazard were major conversations between Wright and Chet and Wright and Ernie Anderson.

FISHER: What about the long-term follow-up of the plutonium injectees? Do you recall?

PETERSEN: I don't recall much of that. That was never an area that I was personally involved in. Th[at] was sort of hallway [conversation] more than anything else. The people [who] were actually subjected to some [plutonium], at least some of them were still surviving [and] were followed up later. Of course, Pat Durbin in her 1972 urgings was really one of the driving forces

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[129] For a summary of these experiments and a list of references, see "LANL-12, Gastrointestinal Passage of Radioactive Particles Containing Manganese-54 and Uranium-235" in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (213 pages), DOE/EH-0491, July 1995.

[130] Dr. Randolph Loveless was the director of a hospital in Albuquerque. He was intimately involved in the clinical aspects of health and well-being of workers involved in overpressure tests.

in getting *LA-1151*, the original, paper released.[131] It had been referenced and the contents of the paper had been public from earlier publications. But [*LA-1151*] and the ERDA report pretty much put that kind of inquiry in perspective. I think they said as much as [was] known then.

FISHER: All other studies on plutonium metabolism dealt with the follow-up of workers with accidental exposures?

PETERSEN: Yes, that was the extent of the human studies. This is not to say that their work on plutonium stopped. It didn't, by any means. There has been an active investigation of plutonium in various phases that spans the entire existence of the Health Division. And, it [has] evolve[d] from excretion through this final aspect where [potential] space vehicle accidents are the source of the exposure.

FISHER: Very good.

CAPUTO: Do you have any questions?

FISHER: This will complete my list of questions for you in this oral history interview.

Public Misperceptions of Radiation

CAPUTO: I have two more questions. One, do you feel there are any misperceptions of the public right now that need to be corrected?

PETERSEN: Yes, I think the misperception that I feel most strongly about, the one that is most important as far as the public is concerned, is the fear of radiation in the amounts that are in existence. They have no quantitative idea of what the hazard is. They simply know that radiation is dangerous and therefore become hysterical. You can't talk to them in terms of quantity, because it only infuriates them. You can't use arguments related to how much radiation they've been exposed to.

Most people don't realize that much of the radiation experimentation that has been done, has been done in situations where the quantity of additional radiation either was less than, or approximated the amount of, natural radiation that they are exposed to anyway. Many people, I'm sure, feel that if it weren't for mad scientists they wouldn't be exposed to radiation at all. That, of course, is not true.

Particularly here in the west, we live on granite rock outcroppings that seep radon all the time. Because of the way annual doses are computed, it doesn't sound like much, but if you compute the lung dose due to radon every year, just from inhalation and living in a fairly tight house, it exceeds the natural background by almost an order of magnitude. It's three to five rem to the lungs.

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[131] 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).

PETERSEN: My other real concern is also a radiation exposure issue. It's the hysteria associated with plutonium. I guess it's our own fault, because the notion that plutonium is the most toxic material known to man is ascribable to [misquoting the original statement that it is the most toxic *element*]. I don't think that there's any way in the world that you could actually prove that plutonium [on a] weight-for-weight quantity even comes close to some of the biological toxins. They're all toxic, and they kill you *now*. In the same quantities, plutonium, even put in the most strategic place that it could be located, probably [spends] most of the remainder of your life as a non-actor, in terms of your well-being. Plutonium enjoys its reputation [(notoriety)] largely because this notion is perpetuated by popular press. And it's the sort of things that they enjoy doing: it's dramatic, it's the kind of thing that should be an attention getter in the context of any story that's being developed. [The claim is] simply false to begin with, and I have no notion of how to change that idea.

CAPUTO: *(smiling)* I have one more question. Rumor has it that you've eaten your way through the periodic table?

PETERSEN: *(laughing)* No, that's an exaggeration. I have not come close. I have had several isotopes. I think it was Gary Sanders that accused me of "noshing" my way through the periodic table.

CAPUTO: That was my last question. Thank you very much.

PETERSEN: I've enjoyed talking to you. It's been a pleasure.

(Interview continued in an attempt to recapture the part of the interview accidentally recorded over.)

CAPUTO: What I would like to do now is [revisit some questions], since I taped over about ten minutes [of our conversation]. It was after our discussion of Shipman, Langham, and Headquarters and the decision process that related to what human experiments occurred at Los Alamos, and then the conversation picks up again discussing leukemia and ^{32}P [(phosphorus-32)] and blood tracers at the University of [California at] San Francisco. I remember we had somewhat of a discussion on leukemia versus anemia, and why the studies were stopped. I think we're missing a portion of the discussion on Clarence Lushbaugh and how he got his experiments approved. I would like to quickly go over the Shipman, Langham, Headquarters association and then move into Clarence Lushbaugh and, hopefully, that will cover the missing information.

Researchers' Use of Human Counters

PETERSEN: In 1956, the first of the human counters [(HUMCO-I)] was coming on line. There was intense interest on the local staff to use this very sensitive counter to reduce the amount of isotope that was needed to do studies. More studies would consequently be feasible; studies not only involving the use of isotopic tracers for diagnosis, but also, studies measuring the metabolism of trace elements for purposes of establishing

protective levels were being considered. The main people involved in the interest in clinical use of isotopes were Irene Boone, Harry Foreman, and C.C. Lushbaugh.

Lushbaugh was, by far, the most vigorous in terms of the program that he was interested in. He was a very active and very curious guy, with a broad range of interests: essentially unbridled curiosity, very undisciplined in that regard, and interested in everything.

Harry Foreman was interested primarily in the treatment of radiation exposures with actinide elements by using chelating agents. EDTA [(ethylene diamine tetraacetic acid)] and DTPA [(diethylene triamine pentaacetic acid)] were his principal interests, and he wanted to know if you could remove deposited doses by treating patients with chelating agents.

Irene Boone had a clinical interest in isoniazid. She was also fascinated by microbiological systems, and her work involved those two things. The microbiological systems did not involve human volunteers. But the isoniazid studies were done because of her interest, and her work with tracers at the Indian hospital at Fort Defiance, [Arizona], which was the last of the tuberculosis hospitals in existence [in the U.S.]. If you remember, isoniazid and streptomycin virtually eliminated the [tuberculosis] sanitorium industry in this country after World War II.[132]

Protocol for the Use of Human Subjects

PETERSEN: The availability of [the whole-body] counter and the pressure that was building up to do these studies prompted Shipman to write to [AEC's] Chuck Dunham and ask him to approve a procedure that Shipman described in his letter. Dunham wrote back and said, "It's a nice protocol, but this is what I want you to do." It was a modification of the protocol that was actually submitted [to him by Shipman].

Once Shipman had this guidance, it came to Langham, and from Langham to the people [in the Radiobiology Group] who were interested in doing research on human volunteers using isotopic tracers, either for diagnosis or for metabolic studies. The ground rules were the same. As a matter of fact, that *one* set of ground rules has been in existence, superimposed on virtually everything else that has come on since.

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[132] a highly communicable, potentially fatal disease that in humans is manifested primarily in the lungs. Known as the "white death" and more commonly as "TB," tuberculosis was common in the United States and once was treatable only with bed rest, extending from months to years, and surgery. Hence the need for sanatoriums that could be found throughout the country. New drugs gradually transformed treatment of TB to an outpatient basis. Tuberculosis had been brought progressively under control in the United States until the late 1980s early 1990s. Then, drug-resistant strains of tuberculosis began to emerge, in part because, during the 1980s, Federal funding was withheld for continued research and development of new TB drug treatments and for subsidy of early treatment of TB for the poor with drugs. As of 1995, the reemergence of tuberculosis has advanced to the point that some authorities have proposed reestablishing TB sanatoriums.

The salient features of that exchange were that the patients would be true volunteers, understanding what the study was about, [and] that the doses were *bona fide* tracer doses—that is, on the order of a fiftieth of the maximum permissible level, or otherwise defined, but [as a] rule of thumb, a microcurie or less with short-lived isotopes. For example, strontium-90 [(half-life: 29 years)] would never be given to a human volunteer, under any circumstance. You would go to a metabolic surrogate for strontium[-90], [such as] strontium-85 with a much shorter half-life [(65 days)]. And actually, volunteers here in the protection [studies] did get some strontium-85.

The other constraint was that the administration and supervision of the volunteers required participation of a physician. This worked with the people whose names I've mentioned until 1963, when Lush left for Oak Ridge, Irene went back to finish her residency, Harry left for University of Minnesota, and Group H-4 suddenly wound up with no M.D.s on the staff.

CAPUTO: That all happened within the same year?

PETERSEN: Yes. And so, there is a memo from Tom Shipman to everybody still interested in this sort of [study]; principally Chet Richmond [with] his metabolism [program], and of course Wright was still interested in all of these things. The word was spread that Harry Whipple, then the H-2 group leader, would assume that responsibility [for] supervision of isotope administration. Any experiments that were done would have to be coordinated with him. That's what happened.

Incidentally, Harry later became the first chairman of the Institutional Review Board. [The studies that were done at Los Alamos were programs that spanned years.]

Providing Patients for Dr. Lushbaugh's Research

PETERSEN: [Because there was no clinical capability at Los Alamos comparable to the hospitals at Argonne, Oak Ridge, and Brookhaven, the] kind of studies that Lush did here were opportunities that were developing as a result of requests from individual patients and their physician. A physician knowing Lush would send him a patient within the sphere of interest that he had for patients. [They might be] a thyroid or a blood dyscrasia or an iron malabsorber [case], or whatever the interest in the clinical technique development was, and he would do the diagnosis and add that person as a statistic to his collection for that particular procedure.

Then, after enough of them had been accumulated, such as it's done even in teaching hospitals with rarer diseases that you don't see all the time, [he would] write a clinical paper on method development or some facet of modification of an existing technique. Whatever it happened to be.

He never had great access to patients here, largely because of two things. A very healthy young community [then], [so that] the [doctors]

who were really stretched in their practice those days were the pediatricians and the ob/gyn [(obstetrics and gynecology)] people. They were busy all the time. The patients that he did get, he got by referral. He did diagnoses, procedures, according to the things he was working on.

CAPUTO: Did he pay for referrals, do you know?

PETERSEN: That's an interesting question. He had a little slush fund [(discretionary fund)] provided by the Director of the Laboratory. It was five thousand dollars. If a patient at some distance couldn't afford to get here for financial reasons or couldn't afford to pay for the stay in the hospital overnight, which in those days (including supper and breakfast) was $14.50, he had a five-thousand-dollar kitty to support these patients. They're being described as indigents.

CAPUTO: Right, but he didn't pay the doctor?

PETERSEN: He didn't pay the doctor, and he didn't charge the patient. The five thousand dollars, under those circumstances, went a long way.

Over the years that he worked here, he must have done diagnostic procedures on close to four hundred patients. Of that number, maybe ten percent were given either partial or total support through his little five-thousand-dollar slush fund. It was done on a completely different scale from the sorts of things you envision when you think of a clinical trial soliciting for volunteers in a teaching hospital today. Although it's described with the same words—the dimensions, the intention, the whole flavor—the activity differed from the business of hunting up patients as volunteers in a study, like [the way] happens today, [where] you can see ads in the newspapers. This [use of printed ads] never happened; it was strictly by word of mouth within the medical community.

He [(Lushbaugh)] was widely known and well-respected by the medical profession in the region—[a] region extending from Denver to El Paso. He got patients, on occasion, from distant sources and paid for them out of this little fund.

[An] amusing sidelight is that some people out of Naval Radiological Defense Lab at Hunter's Point, [in San Francisco Bay], were interested in body composition, and they had given potassium-42 to some kids working out there, sailors, who had been doing weightlifting, and they had bulked up; they were all-muscle. Then, when they [discovered] that they could see potassium-40 and resolve it from [potassium-]42 here, the [investigator] out at Hunter's Point wanted to send [his weightlifters] here for a count in the whole-body counter. It boiled down to the fact that they couldn't get this done, because the Navy, for some reason [or] regulation, apparently could provide transportation but no per diem [allowance]. I forget the details of that. Anyway, it turned out that in order to solve the problem of getting these sailors paid, they made a raid on Lush's slush fund. He was just mad as hell about that.

FISHER: They were counted here in Los Alamos for potassium-40?

PETERSEN: Yes, and potassium-42.

CAPUTO: I think we discussed ^{32}P. Was any ^{32}P work done at Los Alamos?

PETERSEN: Not on humans. There was no ^{32}P work on humans done that I can remember. ^{32}P, very early on, developed a bad reputation that was actually referred to in Shipman's letter, when he says that there had been, I forget just exactly what words he used, but an unfortunate occurrence at an earlier time. He was referring to the treatment of [polycythemia] vera[133] patients with phosphorus-32.[134]

FISHER: I believe ^{32}P is considered leukemogenic [(causing leukemia)].

PETERSEN: Yes, now. But in the early '50s, [information was accumulated] on which we base our conclusions today. At that time I think it was a different problem that ^{32}P caused in a patient. As I remember, it was a [polycythemia] vera patient, or maybe a series [of patients].

CAPUTO: Anything else you want to add? This is your last chance. Not necessarily your last, of course. We'll come back if you want us to.

PETERSEN: I can't think of anything.

FISHER: We'll check the tape and see what's missing.

CAPUTO: It'll be easier once I can read the whole thing.

PETERSEN: If you need a "mulligan,"[135] we can always do that.

CAPUTO: You can always add. Once you get a copy of the transcript, you can supplement.

PETERSEN: *(smiling)* We'll have to turn it into sentences first of all. I think there was a comma someplace along the line. There may have been one period.

FISHER: Thank you again. It was a delightful experience.

PETERSEN: I've enjoyed it. Be sure to give my regards to Ron [Walters] when you see him next. I hope the boys [who are still at Pacific Northwest Laboratories] are doing okay. ❑

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[133] a disease characterized by overproduction of red blood cells

[134] See the interview with Dr. John Gofman (DOE/EH-0457) for a discussion of treatment of polycythemia vera patients with radioactive phosphorus (^{32}P).

[135] an opportunity to "mull again"