DOE/EH-0463

HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Health Physicist
William J. Bair, Ph.D.

Conducted October 14, 1994

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

FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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

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

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

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

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DISCLAIMER

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

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Interview with Dr. William J. Bair, Ph.D.
Setting: October 14, 1994, Richland, Washington
Interviewers: David Harrell and Cindy Shindledecker
(DOE Office of Human Radiation Experiments)

ORAL HISTORY OF HEALTH PHYSICIST WILLIAM J. BAIR, PH.D.

Conducted October 14, 1994 in Richland, Washington, by David Harrell from COMPA Industries, Inc., and Cindy Shindledecker from Kouchoukos and Associates for the Office of Human Radiation Experiments, U.S. Department of Energy.

William J. Bair was selected for the oral history project because of his participation in the University of Rochester Atomic Energy Project as J. Newell Stannard's first graduate student in radiation biology, and for his radionuclide inhalation research at Hanford Site. The oral history covers Dr. Bair's education and his career at Hanford, including his role as manager of the Biology Department and his role as Director of the Life Sciences Program.

Short Biography

Dr. Bair was born in Jackson, Michigan on July 14, 1924. He received his B.A. in Chemistry from Ohio Wesleyan University in 1949, and received the first Ph.D. in Radiation Biology from the University of Rochester Atomic Energy Program in 1954. Dr. Bair went to work for General Electric Hanford Laboratory (Richland, Washington) in 1954 as a biological scientist, and has worked for Hanford's management and operations (M&O) contractors throughout his career. In 1956, he became manager of the Inhalation Toxicology Section of Pacific Northwest Laboratory's (PNL's) Biology Department. From 1968 to 1975 he managed PNL's Biology Department. Dr. Bair managed the U.S. Department of Energy's (DOE's) Environment, Health, and Safety Research Program at Hanford from 1975 to 1990. From 1986 to 1993, he was manager of PNL's Life Sciences Center. His research focused on the inhalation of radionuclide aerosols, mostly fission products, by various animal species, primarily beagle dogs.

Dr. Bair's professional affiliations and appointments include:

- Member, Health Physics Society, President (1984–85),
- Member, Sigma Xi,
- Member, National Council on Radiation Protection and Measurements (1973–92), Honorary Member (1992–),
- National Academy of Sciences, and
- International Commission on Radiological Protection.

In 1970, Dr. Bair received the Atomic Energy Commission's E.O. Lawrence Memorial Award for Research on Radiation Biology of Inhaled Radionuclides. He has published extensively on animal inhalation of radionuclides as well as on the implications of this data for human respiratory models.

SHINDLEDECKER: This is Cindy Shindledecker and David Harrell from the DOE Office of Human Radiation Experiments. It's October 14, 1994. This is the oral history interview of Dr. William Bair at Pacific Northwest Laboratory in Richland, Washington. Thanks for talking with us, Dr. Bair; we appreciate you taking the time to do this.

One of the first things we'd like to talk with you about is your experiences as a graduate student at Rochester. We understand that you were the first Ph.D. candidate in Radiation Biology there. We're interested in knowing how you chose Rochester, how you got interested in the field, and a little bit about your experiences there.

Graduate Studies at University of Rochester

BAIR: First, I did my undergraduate work at Ohio Wesleyan University. I majored in Chemistry. There were some opportunities to see research reports, the old Manhattan Engineer District reports, when I was a student. Actually, I remember seeing papers on americium.[1] Of course, this had to do with chemistry.

I suppose in a way I became interested in atomic energy because—actually, atomic energy had saved my life. [During World War II,] I was in the infantry on my way to Japan when they dropped the bombs. I've never been one of those who deplored ending that war over there with the atomic bombs. I think [that the alternative—an invasion—] would have been a terrible disaster, not only for Americans but, also, for the Japanese. I suppose that gave me a little more incentive to look into atomic energy.

Then, I was looking at opportunities to go to graduate school. Although I had applied to Ohio State [University] for graduate work in Chemistry, I saw a notice on the bulletin board announcing the National Academy of Sciences, National Research Council fellowships in Atomic Energy. I applied and had to take an examination. I was, I think, almost simultaneously admitted to the graduate school at Ohio State and informed that I had won one of these fellowships at [the University of] Rochester [(Rochester, New York)]. I did not know about Rochester at the time. Actually, I think it won out because I was still intrigued by all the possibilities in atomic energy.

I didn't know what I was getting into because I didn't know much about the program that I had taken the test for. My physics [knowledge] had certainly not been used. I suppose I must have read some words in the [fellowship] description about radiation, I can't remember that. I remember going to Rochester, arriving there, and being somewhat surprised by the content of the program. It was more health-related than I anticipated. But it was a very interesting program. That first year was the master's degree program that they had set up.

The first-year curriculum provided an opportunity to work for the master's degree, although you didn't have to. I think some people left without getting a master's degree. Others stayed on to finish the requirements. It was an interesting year, because they really gave you a broad spectrum of classes. The class that I was in had only about eleven or twelve students,

[1] element number 95

with a mixture of backgrounds, including Chemistry majors, Physics majors, and engineers. I don't think there were any biologists in the whole class. They gave us classes in biology, genetics, statistics, physics, instrumentation—everything you can think of that had to do with radiation. It was really an outstanding program. It was the second year that it was in progress. During that year, Dr. J. Newell Stannard asked me if I would be interested in staying on as his graduate student.

Actually, at that time, they had no program in Radiation Biology. I initially was registered in the medical school Physiology Department as a graduate student in Physiology. I suppose I must have been in that [for] maybe a couple of years before they put together the [Radiation Biology] program and got it approved. [Then,] I was really in the two departments in Physiology and Radiation Biology.

The University of Rochester is really an outstanding graduate school. I know the medical students that we had classes with were really taught, given opportunities to put a research experience into their backgrounds, rather than just clinical studies. I don't know whether that's true now, but the medical school was quite unique in trying to get across to the medical students that there was a need for them in research.

When the Radiation Biology program was established, I transferred over and became a full-time graduate student in Radiation Biology. It was a guinea pig experience for me because the faculty and deans weren't quite certain what the expectations should be for a graduate student in Radiation Biology. It was a unique experience. The program was molded to your requirements and to your skills and background.

Newell Stannard was an outstanding mentor. I think, at the time, I was his only student, so I had a lot of his personal hands-on touch. I had a lab and an office right next to his. I couldn't have asked for a better experience.

AEC-Funded Research at University of Rochester

SHINDLEDECKER: Were you involved directly in AEC [(Atomic Energy Commission)] research as you were going through your graduate-school work?

BAIR: Yes, the program was supported by the Atomic Energy Commission at that time. My research interests were primarily on basic mechanisms that caused radiation effects. [At the University of Rochester], I really didn't get involved in the more applied aspects that I encountered here at Hanford. My thesis had to do with the effects of radiation on baker's yeast. That seems like a far cry from a human, but still, even today, baker's yeast is used for a lot of microbiological studies, because it's an easy cell to study, easier than using human cells.

SHINDLEDECKER: We were wondering about that because we had read that and wondered what the significance was of baker's yeast.

BAIR: In fact, I just picked up a copy of *Science* the other day and there were several articles about the molecular biology of yeast. It's a very useful tool in biological research.

SHINDLEDECKER: When you were in school, were you aware, in general, of all the kinds of research that was going on through the AEC at Rochester, or was it compartmentalized?

BAIR: As I remember, we did have to have [security] clearances in those days. I don't know if you've ever been to Rochester, but they had an annex that was certified as a classified facility; we had to have a badge to enter the facility. My lab was not in there; it was in the medical school. Of course, the teachers did have access to information that was generated there on biological effects.

A lot of the research was done on uranium toxicology[2] at that time. Probably more of it was done there than anywhere. There were also studies on polonium. I believe that was done in conjunction with the people at Mound Laboratories, maybe even with people at other AEC sites. The first I knew of that work was probably in 1950. I know that Dr. Stannard had contacts with other scientists doing studies on polonium. I did not get involved in that. Some of the other graduate students did work on polonium.

Even before then, one of the emphases was on inhalation toxicology studies. They had set up a program to look at the toxicity of various substances in the respiratory tract. That was a main part of our classes in toxicology. We actually did experiments where we exposed rats to chloroform[3] and various toxic substances.

In those days asbestos was of interest. I don't know that it was of interest [because of experiences that occurred] during the war in the shipyards or just [because it was known as] a potential occupational hazard.

Another material that they were working on there was [the metallic element] beryllium. As I remember, that stemmed more from the exposure of people from fluorescent tubes than it did from the wartime experience. In those early days, the fluorescent tubes contained beryllium oxide, or it may have been another beryllium compound. I remember stories about deaths occurring not only to workers, but also to the families of people working in the beryllium industry. There was one plant somewhere in Ohio where beryllium toxicity was a serious problem.

They set up an alpha lab in Rochester when I was there. It was the first program that really focused on the inhalation of alpha-emitting[4] compounds. Some graduate students worked on alpha emitters, but I did not get involved in that area.

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2 the branch of pharmacology dealing with the effects, antidotes, detection, etc. of poisons

3 a colorless, volatile liquid, $CHCl_3$, used chiefly in medicine as a solvent and formerly as an anesthetic

4 emitting helium nuclei during decay, possibly causing tissue damage if ingested or inhaled

SHINDLEDECKER: Were you aware of the fact that there had been any kind of human experiments done at Rochester?

BAIR: I can't remember in what context we learned about the studies with plutonium. I think they were done maybe three or four years before I arrived, so there was nothing going on then. But I think that, perhaps, during a class lecture, it was mentioned that people who were terminally ill patients were given injections of small amounts of plutonium. We knew about it, but it was not a big issue. I think the information was classified in those early days. Plutonium itself was not generally spoken of, I remember, even then, [and] that was several years after the war.

HARRELL: Code words were used.[5]

Use of Human Subjects at University of Rochester

SHINDLEDECKER: Was it very common for people to do experiments on themselves as far as work, ingesting something as part of their work?

BAIR: There's no doubt that use of human subjects was more casual than now. I know that the Medical School over there had a large dental research group. Many of us signed up and made $25 brushing our teeth with some compound for whatever length of time and going in for periodic checkups.

I also remember one day walking down the hall and someone said, "Do you want to make $10?" A graduate student is not going to turn down $10. I went in, and they were in the process of developing cinema fluoroscopy, taking moving x-ray films. "Stand here and put your hand in front of this and wiggle your fingers," [we were instructed]. We signed nothing: we were given $10 and that was the end of it.

I think I would not say that it was a rare event, [but it was certainly not a] callous disregard for human life. People did things then, you would not do now. Now, you would not ask someone to be a human subject in a research project without going through all the procedures that are required.

HARRELL: So with the dental school, were there actual radioactive toothpastes?

BAIR: No. These were experimental [toothpastes, I believe, to test the benefits of fluoride additives]. I don't know that there was any radioactive work done at the dental school. I believe the only radioactive materials being used in those days were in the Atomic Energy Project and possibly in some of the other departments, where they would use tracers.[6] Tracers, such as carbon-14, were used to study chemical and biochemical processes.

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5 One common term for plutonium was "product."

6 small amounts of radioactive materials used in place of stable forms of the same element to track biological or chemical processes

AEC Direction of University of Rochester Research

SHINDLEDECKER: You may have been too far removed from this: Do you have any knowledge of how much involvement the AEC actually had in directing what work was going on there, and essentially how all that was coordinated, who was going to do what?

BAIR: Just in general. I know that the mission for the Atomic Energy Project at Rochester was pretty well defined. It was going on in conjunction with work being done at Argonne,[7] Berkeley,[8] and other places. Each one had definitive missions as part of the overall effort. I would say—at least it appeared to me at the time—that the Atomic Energy Program and research was pretty well-organized and -directed. They [(the Atomic Energy Commission officials)] probably didn't review and approve every experiment; I'm sure they didn't. The people that headed the biomedical research in the AEC were really outstanding people. I don't know how they were able to do it, but they attracted some of the best people in the country to that. It was a high-priority effort for the Atomic Energy Commission. Shields Warren and Stafford Warren and those people are just truly outstanding people as well as outstanding scientists and physicians.

HARRELL: They did have the Committee on Isotope Distribution of the AEC and there was the Allocation of Isotopes for Human Use Committee, as well. Are you aware of the procedures for approving—the forms and things?

BAIR: I've read about it. At the time, I wasn't even aware of that committee. My work was not involved. There was one human study that I was aware of when I was at Rochester, because some of the people I knew became involved in it after I left. That was the work in trying to treat brain tumors with neutron exposures after injection of boron and uranium compounds. The work was done at Brookhaven and in Boston and people at Oak Ridge were involved. I can't remember whether any people at Rochester were involved or were just aware of it because some of their colleagues had gone to Oak Ridge and become involved in the program. Dr. Sweet is a name that I remember being involved in the studies in Boston.

HARRELL: Sweet?

BAIR: [I believe the studies were reported.] In recent years they've been referred to a lot because they've been trying to revive that whole concept, with neutrons.

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7 Argonne National Laboratory outside Chicago, Illinois

8 Lawrence Berkeley Laboratory (then UC Radiation Laboratory) on the campus of University of California at Berkeley

Contacts With Researchers Into Radiation Effects

SHINDLEDECKER: Did you personally know people like the Warrens? Of course, you knew Newell Stannard. Had you ever met Wright Langham?[9]

BAIR: Yes, of course. These people were the real pioneers; I was a Johnny-come-lately. Lushbaugh,[10] who else did you say?

HARRELL: Friedell?[11]

BAIR: Yes. These people were there when it really began. I don't know if Hymer Friedell told you about the book that he's writing.

HARRELL: I've seen parts of it.

BAIR: I talked to him last summer and encouraged him to try and get it written. In fact, Ray Baalman and Newell Stannard might be interested if they could find somebody to cause it to happen. All of his information really ought to be documented. It's a fascinating tale; he could talk for hours.

HARRELL: He did.

BAIR: I also knew Dr. Louis Hempelmann; I'm sure you've run into his name. He was on the faculty at Rochester. Blair was the director there. He was certainly one of the pioneers.

HARRELL: How about Joe Howland?

BAIR: Howland, sure. We were very fortunate in having these people on the faculty at the time. Information about atomic energy and the health effects was concentrated in a few people worldwide. We were fortunate in having some of them at Rochester.

HARRELL: Did Howland ever talk about his time at Oak Ridge?

BAIR: I don't remember his talking about it. He probably did; I don't remember specifically. Of course, at that time, people like K.Z. Morgan[12] and Alexander Hollaender were well-known at Oak Ridge. I did know Wright Langham at the time.

HARRELL: You knew of his work?

BAIR: I knew of Dr. Hollaender's work at Oak Ridge and the work of a number of people at Argonne that we had contacts with as students.

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9 At Los Alamos National Laboratory (Los Alamos, New Mexico), Langham led the Health Division's Radiobiology group from 1947 until his death in 1972.

10 For the transcript of the interview with Clarence Lushbaugh, see DOE/EH-0453, *Human Radiation Studies: Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

11 for the transcript of the interview with Hymer Friedell, see DOE/EH-0466, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Hymer L. Friedell, Ph.D.* (scheduled to be published later in 1995).

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

SHINDLEDECKER: Was there anyone that was in school with you that subsequently went on to be pretty prominent in the field as you are?

BAIR: There are quite a few of them. The first one that comes to mind is Bob Thomas. I don't know if you've run into him or not. He was the second one in line. He went down to Albuquerque and initiated the inhalation toxicology program at Lovelace [Clinic]. He and Tom Mercer. He subsequently went to Los Alamos and DOE.

Marv Goldman[13] is another. He went to the University of California and subsequently headed up the program there. He is retired and is currently president of the Health Physics Society.

Dr. Melvin Sikov is another. He went to Wayne State University [(Indiana)] from Rochester. We talked him into coming to Hanford back in the '60s, and he is still on the staff here. He's probably done more research on the uptake of radionuclides[14] into the fetus and fetal membranes than anyone in the world. His work has all been done with animals.

I could go on. There are others. Some that their careers kind of advanced off. Another was Lyle Roberts, who went to Georgia [Institute of] Tech[nology, Atlanta] and turned out a number of students there. He currently works up in Buffalo. I can't remember at the time.

SHINDLEDECKER: Anything else on this? Any other Rochester things you'd like to share with us, or shall we move along?

BAIR: I think one of the experiences I had at Rochester that has been invaluable to me, was working with G. Hoyt Whipple, a fellow graduate student. His father was a Nobel prize winner and was the Dean of the University of Rochester School of Medicine while I was there. Hoyt had worked out here at Hanford and then came back to school. I learned a lot from being able to have him as a lab partner, as he was a few years older and had a lot of experience outside the University.

No Knowledge of Uranium Injections at Rochester

HARRELL: Did you do some uranium injections at Rochester?

BAIR: In people?

HARRELL: Yes.

BAIR: I did not. I don't know that others did.

HARRELL: I think those were early on, maybe before your time.

SHINDLEDECKER: And they also did some polonium injections in the time before your era also.

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[13] For the transcript of the interview with Goldman, see DOE/EH-0468, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Marvin Goldman, Ph.D.* (scheduled to be published later in 1995).

[14] radioactive nuclides (atomic species in which the atoms all have the same atomic number and mass number)

HARRELL: So, in terms of your early work, that and the Strong Memorial [Hospital] plutonium work, those weren't part of the curriculum and part of the general discussion? Were things kind of compartmentalized, or run as they were in the MED[15] days—with a lot of security?

BAIR: In part, the work that was done in this particular building [(referred to as the Atomic Energy Project Annex),] was not necessarily all classified; but the classified work that was being done and discussed would have been done there. The uranium toxicity work, probably, began in the Annex but moved to a new building at the medical school about the time I arrived. It could have been that a lot of that work was classified before I got there, but I don't remember uranium toxicity research being classified.

HARRELL: Was some of that work closely involved with the defense plants or production plants and may have been classified because of that?

BAIR: Probably; I can only speculate. Uranium was not as sensitive a word as plutonium. Polonium was not—somewhat surprising; I don't remember that being a sensitive word. Tritium,[16] I don't remember anyone ever talking about tritium there or not. When I came to Hanford, tritium was referred to by a code name.

HARRELL: And for uranium hexafluoride?[17] I think they called it "six one six" for a while.

BAIR: They might have.

HARRELL: At least at Oak Ridge they did.

BAIR: It's possible.

SHINDLEDECKER: One of the fun things when you do this research is trying to figure out what all the code words for the things you are looking for. The next thing we're ready to move to is, how did you end up at Hanford?

Beginning a Career at Hanford

BAIR: When I finished at Rochester, I determined I wasn't going to stay at Rochester. I felt that you needed to get away from where you did your training. I looked into an opportunity as a postdoc down at Oak Ridge with Alexander Hollaender, which was really an outstanding opportunity. There was also an opportunity in the East, I think at Yale [University, New Haven, Connecticut]. Then one out here at Hanford.

I suppose that several things influenced my decision to come here. One was, it was more money. Even though I'd gotten married in Rochester and my wife had a good job at Kodak, money still meant something to us. Also, the Northwest seemed like an exciting place to come because it was

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15 Manhattan Engineer District

16 an isotope of hydrogen with an atomic mass of 3, the only radioactive isotope of hydrogen

17 a colorless, water-insoluble, crystalline, volatile solid, UF_6, the chemical form in which ^{235}U is separated from ^{238}U

farther away from where we had lived all of our lives. Hoyt Whipple had been at Hanford, and his comments were not all favorable. Louis Hempelmann was another one that I talked to about Hanford. In the long run, the people I met from Hanford were very impressive. Frank Hungate was the person who hired me. We drove clear across the country.

SHINDLEDECKER: And you're still here.

HARRELL: What kind of work did you start doing when you arrived? You were talking about your early work here with yeast.

BAIR: The project that I was going to be working on was one that Frank Hungate developed. He was trying to determine how radioactive materials incorporated in living cells caused genetic effects. When radionuclides emit radiation, they may become another chemical element. The idea was to determine whether it was the emission of a radioactive particle, alpha[18] or beta particle[19] or gamma ray,[20] or whether it was the transmutation of an element such as sulfur to another element within a biological molecule that initiated the subsequent genetic event. It's still an intriguing question, because you have a complex molecule, made up of carbon, sulphur, hydrogen, and mineral elements in a biological molecule. When one of them all of a sudden changes to another element, something has happened.

That was the thing we were looking at when I came here. The first two years, that's what I did—trying to find ways of identifying that particular mechanism.

HARRELL: And this was related to plant safety and workers here?

BAIR: It was basic research; it was hard to relate it directly to plant safety. It was not a large program, but it did have the basic research element in the program. We have maintained that throughout the years. It has never been a dominant program, but we have maintained basic research here.

Radionuclide Inhalation Studies at Hanford

HARRELL: Then you became involved in inhalation work in '56. Was that an abrupt change from your basic research work?

BAIR: Very much. As I said, we came here for two years. Probably near the end of those two years, I had an opportunity to go to the University of Illinois to start a radiation biology program in Urbana. We visited Illinois at a bad time. We visited in August, when it was humid and the allergies were the worst that you could possibly have. The job opportunity was really outstanding. It would have been fun to do that. We rec-

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[18] a positively charged particle consisting of two protons and two neutrons, emitted in radioactive decay or nuclear fission; the nucleus of a helium atom

[19] an electron or positron emitted from an atomic nucleus in beta decay

[20] a highly penetrating photon of high frequency, usually 10^{19} Hz or more, emitted by an atomic nucleus

ognized that both Barb and I would have problems with allergies if we moved there.

We hadn't made the decision, actually, when Dr. Kornberg asked me if I would be willing to head up this program in Inhalation Toxicology. I didn't jump at that, because I hadn't been working in the field. However, Dr. Kornberg said I had to know something about it because I had been in Rochester, where the program had developed. With that, I agreed to give it a try.

HARRELL: Was that program starting up at that time?

BAIR: No, it had actually been in existence for maybe two years, maybe a little longer. Ralph Wager had been here, and I think he was really the person who got that going. They had developed some technology. They hadn't really done a major inhalation study at that time. They had introduced small amounts of plutonium into mice. That was tricky with a syringe. Obviously, their work was directed towards introducing material by inhalation.

That program was driven by plant safety problems. It was known that aerosols[21] would potentially leak and then you had a potential dust coming out. At that time, they had a real hot-particle[22] problem at the plant. That's the first time I ever heard of hot particles. Maybe I did at Rochester, I'm not sure. Radioactive ruthenium[23] particles were being released from the stacks at the chemical separation plant.

Although the ruthenium particles were small, some were large enough to be visible. Since radioruthenium has a relatively short half-life,[24] it doesn't stay around forever, like plutonium.[25] We began studies with both plutonium and ruthenium as aerosols and developed the facilities to expose animals to these aerosols.

HARRELL: So you were instrumental in creating the whole inhalation procedure?

BAIR: The inhalation really got started after I became involved. It would have, anyway; it was on track. I just happened to step in at the time this was taking off.

HARRELL: Who were some of the people that you brought in to staff your program?

BAIR: There were people here who were really quite good. They didn't have a lot of formal training; I don't think any of them had more than a master's degree except for one. There was Louis Temple, whose interest was

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21 airborne particles dispersed in a gas, as smoke or fog

22 a multiaction particle of radioactive material that emits many alpha or beta particles

23 ruthenium-106, half-life = 373 days

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

25 Plutonium-239 has a half-life of 24,4000 years.

in histology[26] and pathology.[27] Another was Don Willard, who could do all kinds of things with his hands; one of those types. He was quite useful in rigging up some of the devices to expose animals to [radioactive aerosols]. We did bring in a respiratory physiologist[28] who stayed here for a short time. We had a veterinarian from Minnesota who decided to go back to faculty work. The first person I actually hired was Jim Park from Ohio State, who was a veterinarian. I had a friend on the faculty of Ohio State and I told him I needed some help and identified Jim as a possibility. He's still here. He's been really great, a very good person, really outstanding work.

We never did have a large crew of people; we didn't have very many on the staff. For a long time, we only had maybe a dozen people, including a secretary and the technicians. They might have exposed some animals to plutonium aerosols at Rochester before we got started, because they were heading in that direction[, but our efforts were more intensive]. We first exposed mice to aerosols and then began to expose dogs to aerosols.

Use of Animals in Radiation Studies

HARRELL: Did you use dogs because they were a closer approximation of the human?

BAIR: The real problem in these [kinds of studies is in] extrapolating [the results from animals to] humans. You need to do more than one species. It was true then, and certainly is true today, that if you try to extrapolate from one animal species to man, you get all kinds of objections. You always get objections, but if you try to use just one species, you have a [serious] problem.

When I was at Rochester, they had been doing some work with dogs. As I remember, one of the studies that I had to do was the effects of radiation on the testicular function [in beagle dogs]. I think they were also beginning to use dogs in their aerosol studies.

Most of this was based on work that had been done at Cornell [University in Ithaca, New York], where they had a large beagle colony. They had determined that the beagle was a good experimental animal for extrapolating to man. I don't remember what they were doing at Cornell. It wasn't radioactive studies. Basic biology was done there with respect to the beagle. Also, there was an early program started in Davis, California, using beagle dogs exposed to low-level radiation [from external gamma or x-ray sources].

HARRELL: Once you found the beagle, you were reasonably satisfied that you had a pretty good model there and there was no desire to try any human variation in that?

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[26] the study of the structure of tissue

[27] the science or the study of the origin, nature, and course of diseases

[28] a biologist who studies the functions and activities of living organisms and their parts

BAIR: We always did preliminary studies in rodents—mice or rats—before we did a dog experiment. Dog experiments were expensive, so when you used dogs in experiments, you really wanted to be sure you knew what you were doing. You did all the experimenting with the rats or mice so you knew what doses to use and everything else you needed to know about the study, before you actually did it on dogs.

Identifying Health Effects of Inhaled Radionuclides

HARRELL: What do you think was the main contribution of that work to the literature today or the community today?

BAIR: The first contribution was that we were able to really find out for certain that if we tried enough materials like plutonium, lung cancer was a possibility. I think, before we did that, there was one study, that was only reported as an abstract, where we gave plutonium to a rat. I'm not even sure about that. There were some fairly large pieces of metal—radiostrontium and some other things—and they produced tumors. But our study was the first that truly demonstrated that inhaling radioactive substances such as plutonium could result in lung cancer. That was the primary effect.

A lot of the early work resulted in plutonium being concentrated on bone surfaces. It's certainly true that plutonium was introduced as a chelate,[29] a soluble form, and was deposited primarily in the bone and some in the liver. Bone tumors were certainly introduced.

In the practical world, exposure to workers was not in soluble forms of plutonium: it was by inhalation of mostly insoluble oxidized forms. There were some wounds occurring in weapons plants, machinists that would occasionally get a sliver of plutonium metal in their thumb or finger. But, here again, this didn't result in very much plutonium entering the blood and the bone. From a practical standpoint, the inhalation study was probably more relevant than other studies, where materials were injected [into the blood].

HARRELL: Were you concerned with developing exposure rates for long-term exposure or for short-term doses or both?

BAIR: The initial work was to look at metabolism or the behavior of the material in the body. How long does plutonium stay in the lungs? [Knowing this,] we would be able to calculate doses to the tissues, particularly in the respiratory tract of man.

We had some idea how much would stay in the lungs after they were inhaled. I would say most of our earlier work was concentrated on just knowing something about the behavior of material when it was deposited in the respiratory tract, primarily by inhalation. When you injected it in the solution, in suspension, if the animal was laying [on] its side you weren't measuring where it went: gravity has an influence on things

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

like that. Inhalation gives you a different distribution of material in the lungs than when you inject it in a suspension. That was an important route to take with inhalation studies.

HARRELL: Did you do lung imaging, as well?

BAIR: We really didn't have equipment to do that in those days. We certainly thought a lot about doing it and we developed a scanning device. We could put an animal on a track and take readings [along the length of the animal]. We did that with both rats and dogs. At that time, the gamma camera was not available to us. Besides that, we were working with plutonium, which was an alpha emitter which did not lend itself to being detected by those devices.

The Air Force was interested in plutonium, and they actually supported the first major dog study that we did here. Kirtland Air Force Base down in Albuquerque was interested in knowing something about the behavior of [inhaled] plutonium. They were interested in [a study of] two or three years, maximum.

Interesting enough, when we did that study with the dogs, some of the dogs that survived for more than two years, at about three years began to shrivel up with lung cancer. This was really an unexpected finding. Even at that time, we hadn't seen very many [lung cancers in our rodent experiments]. We'd seen a few, but not very many.

HARRELL: What kind of negative or health effects did you expect from the plutonium inhalation?

BAIR: We were mostly interested in the acute effects if somebody got a high dose. We were also interested in knowing how long it stayed in the lung and if it left the lung where it went.

The acute effects that we saw were also surprising. Wright Langham was the world expert on plutonium at the time, and I don't think he anticipated that you would be able to cause an animal to inhale sufficient plutonium to result in an acute death. I think the earliest deaths that we had after inhalation of plutonium in dogs was about two months [at high levels of exposure to the lungs from plutonium alpha particles]. I don't think we were ever able to get them to inhale enough that it would cause death earlier than that. This was just due to massive destruction of the respiratory tissue. It generated a lot of fluids and essentially drowned them in their own fluids. I think that was perhaps unexpected.

Interestingly, the animals that survived that, many of them lived on for many years. As I said, only a few of them showed up with cancer. Having plutonium in your lung does not automatically mean you're going to have lung cancer sometime down the road.

HARRELL: Was human inhalation of plutonium more dangerous or less harmful than what happened to the dogs?

BAIR: We still don't know. To our knowledge, there have not been any new workers, particularly people that have shown health effects or that have

died as a result of inhaling plutonium. Although I wouldn't want to say this as a final answer, the studies with the rats and with dogs may have, in a sense, overestimated the effects that might occur in humans. I don't think they underestimated them; they might have overestimated them.

HARRELL: Did you alter the safety procedures in the [nuclear] plants at all, or [alter the] allowable doses?

BAIR: There certainly was not a decrease. I can't remember what's happened with the permissible limit of plutonium. I don't think it's changed much since the '50s. [The recommended annual limit on intake of plutonium by workers published by the International Commission on Radiological Protection in 1979 allowed a slight increase in the amount deposited in the lungs. This was because it took a somewhat different approach in calculating doses.]

HARRELL: So they were conservative from the start?

BAIR: I'm sure they were. There's nothing to indicate that it had not been.

Expanded Customer Base for Inhalation Studies

HARRELL: You were manager of the Inhalation Toxicology [Section] from '56 up until '68?

BAIR: Yes.

HARRELL: What happened to that section or your work when PNL[30] took over from GE[31]?

BAIR: When PNL took over [in 1965], our program expanded. It allowed us to expand into other kinds of studies. Until then, most of our work was with radioactive materials. I can't remember doing anything that was not. When Battelle [Pacific Northwest Laboratory] came in, it gave us an opportunity to seek funding from other agencies. We didn't take off right away.

HARRELL: What were some of those other agencies?

BAIR: This was before the EPA[32] was formed. [The National Institute of Environmental Health Sciences was one of the first.]

HARRELL: Any military service?

BAIR: [We continued work for the U.S. Air Force, this time on plutonium-238, which was used in weapons, but more importantly it was to be used as a heat source to generate electric energy on space vehicles.] One of the first studies I remember doing was with the Tobacco Research Institute.

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30 Battelle Memorial Institute, headquartered in Columbus, Ohio, operates the Pacific Northwest Laboratory for the U.S. Department of Energy.

31 General Electric Company

32 [U.S.] Environmental Protection Agency

We did some studies to see if we couldn't learn about the behavior of tobacco smoke deposited in lung tissues.

HARRELL: Was that with humans?

BAIR: The work with humans here—we've never done any work with inhalation except that tritium experiment that was done before I arrived here. We just have not done that.

There have been times when we were interested in doing it. Because, as I said, there is always this question of extrapolating from animals to man. The radiation protection industry has always had a strong interest in trying to verify models with some human study. Even if you just have one human data point, people are happier.

We thought, at one time, it would be possible to do some human studies with a very long–half-life plutonium.[33] Very long–half-life plutonium is essentially not radioactive. We were never able to get enough of that material to do it. There's also another plutonium, -237, that you could get in pure form. It's not an alpha emitter, so you wouldn't have that problem. We were never able to satisfy ourselves that we could get it pure enough that it would be acceptable.

HARRELL: Were there any dangerous toxic chemical effects of plutonium?

BAIR: That was one of the things we wanted to learn by getting this long half-life, plutonium-244.[34] We felt if we could obtain enough of that, we could find out if there was some chemical toxicity. We did get some and we were able to do some fairly small studies. But we never found any evidence for chemical toxicity in those studies. I'm sure that it's the radiation that you're concerned about, not the chemical toxicity.

Limited Involvement With Human Studies

HARRELL: How much management of other people's work, work other than inhalation, did you do when you were manager of the Inhalation Toxicology Section during those years?

BAIR: At that time, I didn't do any managing of anybody except the people in that section, which was almost entirely involved with inhalation studies.

HARRELL: When did you move onto a larger managerial role?

BAIR: It must have been 1968. I became manager of the Biology Department. At that time, there was a much broader effort. It had a large pathology section. We had Microbiology.[35] We had a large animal section. We did some early work with strontium, trying to determine whether it would

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[33] Because it releases its radiation over millions of years, a human could receive only an inconsequential dose.

[34] Plutonium-244 has a half-life of 76 million years.

[35] the branch of biology dealing with microscopic organisms

cause leukemia.[36] We had some evidence in our case that that might be: it did cause leukemia in pigs. We were trying to find out whether there were viruses involved in that whole process.

HARRELL: We do know about several human studies that were done by GE or PNL, several involving Earl Palmer. Did you manage him?

BAIR: No, he was in a different department.

HARRELL: What department was he in?

BAIR: I can't remember [the exact name of the department but it was concerned with radiological physics and chemistry]. Earl Palmer was one of the pioneers in developing whole-body counting[37] techniques. He was involved with people at the University of Washington Medical School. They were interested in trying to apply nuclear energy techniques to their clinical work.

HARRELL: Do you know how the relationship with the University of Washington began?

BAIR: No, I don't. I just knew that he had a close relationship with those people over there.

HARRELL: So, you were involved in the high-level AEC approval and all that managerial kind of decisionmaking?

BAIR: No.

HARRELL: Did you know about the Palmer promethium[38] studies in 1967?

BAIR: I knew about those, because we had done some studies with promethium in animals. They were interested in that. The department he was in was pretty much focused on supporting the radiation protection programs in the laboratory.

HARRELL: Was there coordination between the departments to compare your animal studies to their human studies?

BAIR: We worked with each other quite a lot. For one, we used Earl Palmer, Ken Swinth, and Bill Roesch, all experts in whole-body counting. We had them set up counting facilities for our animals. They received our reports and collaborated in some of our studies.

HARRELL: Did you suggest different experiments for each group that they might want to do to enhance the others'?

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36 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

37 measurement of radionuclides in man using shielded detectors and multichemical energy analyzers

38 a radioactive metallic element obtained as a fission product of uranium or from neutron-irradiated neodymium

BAIR: I can't remember any specifics, but we probably did. I'm sure that the work we did with animals raised a lot of questions that they wanted to follow up [on] with respect to applying the data to humans.

AEC Headquarters Monitoring of Experiments

HARRELL: Was there AEC overall coordination of the various experiments?

BAIR: I can't say that there really was. Our interactions with Headquarters in Washington[, DC] have always been greater than with the local offices of either AEC or DOE. [Compared with other research done at PNL], the biomedical studies have been somewhat of an exception. They have been more directly monitored by Headquarters than by the local office.

HARRELL: What kind of monitoring would that be?

BAIR: Periodic reviews. We sent our proposals for funding to DOE or AEC Headquarters for review. We provided annual reports from back in 1952 to Headquarters. There has always been a strong Headquarters interest in and knowledge of our research. I think the approval would be the fact that they provide the funds to do the work. They did not get involved in the day-to-day design of experiments. There were times when they might have suggested that there was something worth looking into. I can think of lots of examples.

For example, there was a uranium exposure at Oak Ridge National Laboratory. It was an accident, of course, and these people demonstrated a totally different rate of clearance of uranium than [had been found in] any animal studies or in previous human exposures. We did some studies with animals to see if we could duplicate that behavior.

HARRELL: Was that an inhalation kind of exposure?

BAIR: Yes.

SHINDLEDECKER: What kind of role did the Advisory Committee play, like the [AEC's] Division of Biology and Medicine, Advisory Committees, and those kinds of groups that had members from all the contractors?

BAIR: And noncontractors, too. [University scientists were included on the committee.] We did meet periodically at the various sites. Their influence in this case was probably more along the line of something you might recommend to Headquarters. They might have suggested something to us, but it would have been on an informal basis. If, subsequently, we felt it was something we wanted to do, we would put in a proposal or a request for funding.

Differing Effects on Humans of Plutonium-238 and -239

BAIR: I do remember that Merril Eisenbud[39] was on one of those [committees] back in the middle '60s. He suggested to me that we should take a look at plutonium-238 relative to man. We subsequently did. I think that's something that was a very interesting finding. We generally just assumed that two isotopes of the same element were going to remain exactly the same. When we gave animals an inhalation exposure to plutonium-238 oxide, the material [did not remain in the lungs a long time like we had observed for] plutonium-239.

HARRELL: Did it go throughout the body?

BAIR: It would leave the lungs very rapidly and go to the liver and bone.

HARRELL: So, it was much more dangerous?

BAIR: Plutonium-238 was much more prone to irradiate other tissues [in the body as well as] in the lungs. I remember the first time we observed that.

It seems as though I was interested in plutonium-238 because it was a component in the weapons systems. Also, it was being used in the early '60s as a heat source for some of the satellites. There were several of those.

I remember going to a meeting, it might have been at the Pentagon, and to Las Vegas, where I reported on the plutonium-238 studies. People thought it was crazy: How could I stand up there and say that plutonium-238 would not behave exactly like plutonium-239? That really put a fly in the ointment, because you couldn't use the same dose compilations that you had used for plutonium-239.

We subsequently demonstrated that several times. It's also been demonstrated in other laboratories in Europe and also in Lovelace [at the Inhalation Toxicology Research Institute in Albuquerque] that plutonium-238 is different. In terms of long-term effects in our dogs and the animals at other places, inhalation of plutonium-238 oxide will result in both lung tumors and bone tumors, whereas inhalation of plutonium-239 oxide only results in lung tumors. I think it was an interesting finding of this laboratory. Much of my work, I should say, too, was supported by Air Force funds.

HARRELL: When you decided to start with the -238 or -239, the new isotope, did you have to get special approval to acquire that?

BAIR: I can't remember about the approval, but I remember that [the AEC's] Mound Laboratory [(near Miamisburg, Ohio)] was a source of [plutonium-238 ceramic microspheres], and I visited the laboratory to talk to the people there. I think at that time we just requested a shipment of plutonium-238 and it went through the process.

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39 For the transcript of the interview with Eisenbud, see DOE/EH-0456, *Human Radiation Studies: Remembering the Early Years; Oral History of Merril Eisenbud* (May 1995).

HARRELL: Did the requirements and procedures change over time? Did it get more difficult to request?

BAIR: Certainly. You look back and see it change conservatively over the years. It was a good example. In the '60s, we had collaborative studies with the English on plutonium. In fact, there were some studies down at the Nevada Test Site. They did these without animals as well. They did several studies with animals where they just blew up a nuclear weapon and exposed it [(the animal)] to see what would happen to plutonium—how far it would go [from the explosion site] and so forth. Animals were exposed to this material in the desert. We had only marginal involvement in that.

We did participate in one of the later studies. People who were still at Rochester at that time were heavily involved in that, and I think we were coordinated. Anyway, the British were involved in that, and that's some collaborative efforts.

The British had a particular form of plutonium over there that I wanted to use in some studies. Lo and behold, it showed up in the mail. They sent me a teak box container with plutonium in it. It was inside, a very thin layer. It was something like three grams of plutonium in that container.

HARRELL: How much would that have cost in those days?

BAIR: I didn't pay anything for it. It was sent to us as a test substance. That was a shocker for the people who were keeping the records, because we had three grams of plutonium we shouldn't have had. They did keep very careful records on the amount of material you had on hand. It was inventoried periodically and these people were responsible. They came around and everybody had on our books three grams of plutonium that we shouldn't have had.

SHINDLEDECKER: A little opposite of what they probably expect[ed].

BAIR: At the time, I didn't think anything about it. Our people who store our material, I don't remember calling anybody else and telling them I had it.

Study of Emissions From Proposed Nuclear-Propelled Aircraft and Rockets

BAIR: Another thing we got interested in, back in the '60s, was the inhalation of fission products. I don't know whether you knew or not, but back in the '60s we had several plans and they were really moving ahead towards equipping airplanes with reactors, nuclear-propelled aircraft.

HARRELL: Until 1961, pretty much.

BAIR: Actually, GE [Aircraft Engine Group in Evendale, Ohio] was involved in that. These plans would essentially spew out fission products as they went along.

HARRELL: Did they fly—actual planes?

BAIR: They didn't fly. I don't think they even built one. They [(the AEC)] were also interested [in nuclear-powered rockets], and developed some rockets that were propelled by several reactors.[40] We were involved in the discussions [of] that, and did some [animal] research along those lines.

HARRELL: I noticed you wrote an article, "Carcinogenesis from the Inhalation of Nuclear Engines." It was with the Naval Radiological Defense Lab.

BAIR: That's right, and Dr. Charles Sanders was involved.

HARRELL: Was that the same—

BAIR: —That was probably the question that we discovered. They never reached a point where the potential human risk ever entered into the equation with respect to that program. I think it got phased out for other reasons. It could have at some time.

HARRELL: Were there any studies done on exposure that had happened at the sites when they were testing these engines?

BAIR: Not to my knowledge. I'm not aware of anybody being exposed. I saw the setup down there at the test site.

HARRELL: You did some in Idaho, I think.

BAIR: [No, I didn't. The only tests] I remember [were at the Nevada Test Site, and] there were very few firings [of atomic devices]. I'm not aware of anybody being exposed. They probably were. At least not directly. They might have subsequently, if some of that stuff was stirred up by the wind and blowing around.

Technical Support to Human Studies by Universities in the Northwest

HARRELL: You mentioned the uranium exposure at Oak Ridge prompting a study. I guess there was a gamma exposure at Hanford that prompted the testicular irradiation studies at the University of Washington?

BAIR: I know about that study, but I didn't remember that it was prompted by [the Hanford accident]. I read in the newspaper a few months ago, when Dr. [C. Alvin] Paulsen[41] was interviewed. We got involved in that study in an incidental way. University of Washington people contacted us. I can't tell you who, because I was not involved at the time.

[We were asked] to help them in areas where they didn't have the expertise. One was dosimetry, and some of our pathology people did some of the sperm histology. I didn't remember what prompted that study. I remembered instances where people were exposed. [I believe one was a] criticality [accident]. I remember that the staff from Oak Ridge—Mike

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[40] See *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records* (310+ pages), DOE/EH-0445, February 1995). In that report, Figure 52 shows a nuclear reactor on its testpad at Nevada Test Site, awaiting testing for potential future use in an interplanetary spacecraft.

[41] principal investigator for the Washington State Prison, Walla, Walla, Washington, testicular irradiation of inmates study, 1963–70

Bender, for one—came out to determine what kind of a dose they had received.

HARRELL: You weren't aware of any connection between those instances?

BAIR: I wouldn't say that there wasn't, but I was not aware of it.

HARRELL: Do you know what the relationship was between the University of Washington prisoner studies and the University of Oregon studies?

BAIR: I don't know how much contact there was between those two groups. I'm not sure that they always saw eye to eye on things. Our involvement with them was the same way.

In those days, we were encouraged to cooperate with the universities in the Northwest. I suppose that, compared with other sites, we probably had less interaction in the Northwest than Argonne [in Illinois] did with the Midwest and Brookhaven [in New York State did with the Northeast] certainly. We participated in those studies as a means of being a good citizen.

HARRELL: Technical assistance?

Contractor Reluctance to Engage in Human Studies

BAIR: I mentioned when we met before that some of our people were very unhappy about that. I remember Herb Parker being absolutely astounded that we allowed ourselves to get involved in that study. That was while it was still in progress.

HARRELL: And he didn't approve of the whole idea?

BAIR: He just didn't have a very good feeling about that whole study. The attitude here towards doing human studies was pretty much, "Don't do them."

The General Electric Company[, which operated the Hanford Laboratories] was very conservative. Management would not have approved a study that ran the risk of embarrassing General Electric Company. In those days, the medical people were a part of the directors, so the Medical Department was a part of the Laboratory, the overall organization.

When Battelle came in, we were separated, so we had a new medical group. Battelle very deliberately has stayed away from anything that would look like clinical research. It's even more difficult with Battelle, doing studies with human subjects.

Most of the work that involved human studies has been done over in Seattle at the Battelle Seattle Research Center, where they have the human component and a sociological setting. Not studies where you actually treat people with drugs or invade the body.

HARRELL: Did you keep informed of the data that came out of those prisoner studies, since you were doing the lab work for them?

BAIR: Only in general, not very closely. I probably didn't, at Battelle, know more about it than what I saw in the reports.

HARRELL: I know that in one, I think it was the Oregon study, they wanted to give vasectomies[42] to the recipients because they were concerned about producing mutants and having genetic defects passed along. I was wondering to what extent they were able to assess the genetic damage and what kind of studies they did?

BAIR: The only information that we had, [which] is the same today as it was then, came from mouse studies, Russell's studies that were done at Oak Ridge. We didn't have any genetic work going on here, so we didn't contribute to that at all.

Review Procedures for Research Proposals

SHINDLEDECKER: Do you have any sense of how that work was funded by the AEC? What group within the AEC would have managed or approved of that kind of work?

BAIR: It would have been funded by the Division of Biology and Medicine, I'm sure. I'm sure that the process was the same as for anything. Universities [might have submitted proposals to the AEC] a little differently. They sent proposals in to Headquarters directly, I think. I'm sure they were all peer reviewed. I suspect that when they sent a proposal in it was sent out for a peer review. Headquarters did not always send out, probably rarely in those days, proposals [to] more Laboratories for peer review. Those decisions were made primarily in-house, based on their own in-house review of the proposal. You know the term, 189s?[43] There was a budget docket committee. The decisions were pretty much based on what was in that proposal.

SHINDLEDECKER: I noticed it appeared to be pretty thorough.

BAIR: Yes, but I think in those early days that you looked at probably being more thorough. Subsequently, as time went on, it was a little more frequently pointed out that they were actually budget documents.

In more recent years, they would come in wanting full-fledged proposals in the NIH[44] format, which they might send out for peer review. Most of the early work was funded on the basis of what went into the 189 format.

Not Involved in Studies of Phosphorus-32 or Iodine-131 in Food

HARRELL: There were a couple of other studies at GE. There was the phosphorus-32 fish study that also involved some injections.

BAIR: Was that the one where people ate fish [from the Columbia River]?

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[42] surgical removal of part or all of the sperm duct to sterilize men

[43] Form 189 (Research Proposal), a funding document required by the AEC

[44] National Institutes of Health (Bethesda, Maryland)

HARRELL: Yes, they were eating fish from the river and then I think some volunteers received some injections.

BAIR: That was a different department, so I really didn't know very much about that first hand. Of course, we were aware of the fact that they were doing [that]. We saw the data subsequently. But the Biology Department was not directly involved with it, to my knowledge. They might have provided some assistance in terms of back up; I don't know.

HARRELL: Was that a subject of a lot of discussion, since they were eating, regularly, fish that some people might be concerned about eating?

BAIR: I know that the dosimetry was very carefully looked at and those calculations were very rigorous before they allowed them to go ahead and do that. It was not just casually done. I don't remember who did the injections—whether they were done by the Hanford Environmental Health Foundation or whether it was done over at Seattle.

HARRELL: Do you know anything about E.C. Watson's iodine-131 milk ingestion study?

BAIR: No, I don't know any of the details. I know what you're talking about, but I don't even think I was aware of that at the time.

HARRELL: What about Dick Cuddihy? What kind of work did he do?

BAIR: Dick Cuddihy was not here. Dick Cuddihy was down at Lovelace in the Inhalation Toxicology Research [Institute in Albuquerque].

HARRELL: His name was just mentioned as someone who was in this area and you might know about.

BAIR: Dick Cuddihy has been very helpful to the legal counsel. What's the DOE legal department called?

SHINDLEDECKER: General Counsel.

BAIR: [I believe Dick] supported them in various litigation cases.

HARRELL: As an expert witness?

BAIR: Yes.

Oversight by Battelle Human Subject Committee

HARRELL: Battelle had a Human Subject Committee in '68. What kind of oversight did they exercise?

BAIR: Very thorough. I can't remember if [Herb] Parker was Chairman; he must have been. I was a member of it. He really expanded the influence of the committee beyond what was intended. He not only looked at the potential impact on the subjects, he also wanted to look at the science, all the legal implications, the whole spectrum. Much to the displeasure of some of the people who sent proposals in, because they didn't expect to have the science challenged.

That committee really got off to a very good start in a very thorough way. I would say its review of proposals was probably as thorough as they are today, even in today's climate, primarily because of his insistence. He was really very, very conservative with [regard to the] deliberate exposure of people.

HARRELL: Do you know what kind of volume of work they had? Like how many things would be submitted in a particular quarter or year?

BAIR: Not very many. We didn't see a lot of proposals.

HARRELL: And, you would approve a small percentage, or a large percentage?

BAIR: I can't remember that.

SHINDLEDECKER: For example, when you were a manager if someone came to you and said, "Gee, I want to do this human study." What would happen?

BAIR: They were generally discouraged. As a manager, I'm sure I did that more than once. I pointed out to them the difficulty they would have in getting approval. If they got the management approval, then the difficulty they would have in getting it approved by the Human Subjects Committee. If they still wanted to go forward with it, then I'd give it a try, unless I felt it was really something that we wouldn't do.

Many of the proposals that I remember, other than those that came from the Seattle group, were submitted by the Radiation Protection people, who wanted to verify the models that had been developed from animal studies.

International Cooperation in Assessing Worker Exposure

HARRELL: Who were some of the people that worked in the Radiation Protection Group?

BAIR: You mentioned Earl Palmer and Ken Swinth. Iral Nelson and Dick Foster were there, too.

HARRELL: Was it a large group?

BAIR: I would not say it was a large group; they probably had fifteen people. You mentioned Ed Watson. Joe Soldat might have been in that group.

I think these people were mostly involved in assessing exposures of workers, and so they were the ones using the models, and periodically they questioned the models and would clamor for verification. I don't think that they were unique.

I think this was probably occurring at other sites. I know that in Europe this was the nature of things. They did a lot more studies in Europe with people than we've ever done here. We recently got this rolled out on these sheets that we had participated with the English and having people counted.

HARRELL: The in vitro?

BAIR: I don't think any of us really felt that that was something that we shouldn't do. I don't think that we felt that we were obligated to send it through for a complete review by the Human Subjects Committee, since it had been approved normally by the European labs [and the IAEA[45]]. We knew their standards were very high. That also was done with the knowledge and approval of DOE or AEC, whichever agency it was at the time. When these people came here at the time, we [processed them through our whole-body counter] without any hesitation.

HARRELL: That was pretty much a request from the British to use your facilities and you said, "Sure?"

BAIR: These were all coordinated through [AEC] Headquarters; it was not an independent thing of any kind. I think it was a very important thing to be able to verify that the in vivo[46] counting at one laboratory was as good as [that in] another laboratory. Or, if one of us was not up to snuff, then we should know about it.

HARRELL: Were you aware of chromium-51 blood studies that they did at the University of Washington?

BAIR: I saw them on the list, but I wasn't aware of them at the time.

HARRELL: There was a mention there that they might have had some subjects who were in Bangkok[,Thailand,] as a part of that study.

BAIR: I don't know.

HARRELL: Do you know of any other international coordination between the U.S.—the AEC—and a foreign government?

BAIR: With respect to humans?

HARRELL: Yes.

BAIR: No. I should have mentioned that with [respect to the] whole-body counting, the IAEA was involved in all those, as far as I know. Maybe they might have actually been coordinating them. I can't think of any.

Cancer Research

HARRELL: Did you get involved in any cancer research work at Battelle, or were you mostly involved with safety issues?

BAIR: That's really what we were doing: cancer research.

HARRELL: Or treatment of cancer?

BAIR: No, we did not get involved with the treatment. There is an exception: [In] one of the earlier findings in our plutonium studies, we found that dogs that inhaled plutonium showed a continuing reduction of circulating lym-

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45 International Atomic Energy Agency, an organization of the United Nations

46 inside the body

phocytes.[47] I think I brought that to Frank Hungate's attention—the fact that this was happening in every dog and we didn't know why. It might have some applicability to leukemia. And also possibly for knocking down the immune system for organ transplants. Frank picked that up and developed the in vivo blood irradiator. I remember going with Frank.

We went down to the University of Virginia and a few other places where they had these radiation units in place. These were big, desk-sized devices that were wheeled in next to a patient, and the patient would have to lie there all day long and have his blood circulated through an irradiator.

We thought we could develop one that could be temporarily implanted in a person and may be able to accomplish this treatment a lot easier. Also, if you allowed this to happen over a longer period of time, it also might have longer-lasting, -reaching effects. The people who received that treatment in a hospital had to go in every few months and have that done again. We were hoping that maybe this device would eliminate that and provide a more permanent, more lasting treatment.

HARRELL: Did you ever develop the small device?

BAIR: It was actually the first [CRADA,[48] a DOE program that supported technology transfer from the laboratory to the private sector] with the Fred Hutchinson Cancer Research Center over there [in Seattle, Washington,] trying to develop it. We carried it as far as we could do it [through tests in animals]. The next step is for some organizational assistance, [such as from] the Fred Hutchinson Cancer Research Center, to do the other research and take it into clinical tests. It has had a lot of interest among the cancer-treating people throughout the world, but it needs further development.

[An advantage of the in-vivo blood irradiator is that it would avoid] the side effects when you gave people some of the drugs that they used for treatment of cancer and depressing the immune system.

HARRELL: How is that device implanted?

BAIR: It was used in goats [sheep, dogs, and a baboon]. It was actually just put on the neck with a collar here *(points to a part of his neck)* and blood was shunted through the irradiator from the carotid artery. It did reduce the circulating lymphocytes. The only problems and side effects was that there were some clotting problems carrying blood through the device. Those are the kinds of technical things that need to be worked out before it should go into humans. As far as radiation is concerned, that part was perfectly safe for people to be exposed to it. No lasting effects of it.

HARRELL: It was just the gamma radiation?

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[47] white blood cells important in the production of antibodies

[48] Cooperative Research and Development Agreement

BAIR: [No. It was the beta radiation, which is much less penetrating than gamma radiation.] We could use other kinds of radiation if we wanted to—just killing the cells.

HARRELL: You could develop adequate shielding of the rest of the body?

BAIR: Yes, we used the beta emitter, thulium-170.[49] What we really wanted to use was alpha emitters. Any shielding would [have been even less of a problem]. Then we had the problem of getting the alpha particles transferred.

SHINDLEDECKER: Where they need to go?

BAIR: Into the blood, to different cells.

Cleanup of Nevada Test Site and the Marshall Islands

HARRELL: You were also involved with the Transuranium Technical Group to advise the AEC in the '70s. What was the role of that group? What was the function?

BAIR: We were asked to advise the AEC on some of the cleanup activities at the Nevada Test Site and Marshall Islands. Interestingly enough, I'm sure some of those have thought about it for years, but this small group really achieved what we were trying to achieve around here and at all the other DOE sites.

We sat down and we developed a cleanup criterion for these sites. [We were interested in] what level we should clean up to reduce the [risk] or keep the dose below acceptable levels. In those days, back in the '70s, we developed a criterion that we're trying to develop here, today.

HARRELL: Were they using your data?

BAIR: No, probably not. We were doing it primarily with alpha emitters.

There is a certain problem because we were dealing with sites that were contaminated with plutonium. We knew pretty much where [the contamination] was and all that. This wasn't just the same. The process was the same. We had to develop a criterion, and, then, measurement techniques that were used to determine whether you had cleaned up to whatever level was acceptable.

We provided more limited advice on other problems such as barrels [leaking plutonium at Rocky Flats]. We got involved in [providing an assessment of the health risks associated with plutonium-238 in] the Miami Canal near Mound Laboratory. So, we put together a document that outlined criteria that could be used in cleaning up.

HARRELL: Did you use some of the data that the Navy had developed as part of the tests out at the Marshall Islands?

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[49] a rare-earth metallic element (Tm) found in gadolinite and other uncommon minerals; Tm-170 has a half-life of 129 days.

BAIR: We used data from all sources. I would say probably the most data we used was from the work done by Lawrence Livermore and Brookhaven [National Laboratories] out there. Incidentally, you said you wanted to talk about [the Marshall Islands]. *(hands her some illustrated, foreign-language booklets)* I thought you might want to see these if you've never seen them.

SHINDLEDECKER: I've seen them.

BAIR: You're welcome to copies if you would like.

SHINDLEDECKER: *(to Harrell)* These are the books that I was telling you about for the natives. *(to Bair)* I read in Stannard's book that you were involved in these. I have seen these before, essentially describing to the natives what radiation is and what the effects are.

BAIR: I still don't know whether it was a good idea or not. As a member of this committee—I guess I was chairman—I was asked to go out [to the Marshall Islands] and explain to the people the measurements that had been made out there, the doses they would receive. I said the only way I would do that would be if we could take something out there and leave it with them. I felt that so many people had gone out there and talked to these people without achieving anything at all. They had to do a better job.

I said we could take out some kind of report and give it to them so that they could have it after we left and I would be able to do it. That's what led to writing these. I don't really know whether it had any effect at all. I know first we went out there and gave it to them.

We had Alice Buck, a missionary out there, who spoke Marshallese. She translated for us and we made presentations to the people out in the islands. We talked and we saw these books on roofs and huts and scattered everywhere. Whether they ever did anything with them or not, I don't know.

I guess I always felt that our biggest obstacle was the lawyers that were advising these people. I'm not sure their lawyers really wanted these people to have this information. They didn't want them to get too smart with respect—

HARRELL: To damages?

BAIR: I had a real problem because I felt that everything I was dealing with, with respect to these people and certainly with DOE and AEC, was done in good faith. I would not have participated in any of this if I felt that it was not the case. I felt the people of DOE and AEC wanted us to help them on this, really wanted to be fair to these people and give them the information and help [the Marshallese] make the decisions. I also kind of felt that the lawyers out there did all they could to make those people feel otherwise about DOE efforts and to be suspicious. That always has bothered me. That's not why we were out there.

HARRELL: What was your assessment of the exposure that they were having?

BAIR: We tried to do assessments on an island-by-island basis. We did this with various situations with respect to the food they ate. If they ate food from the islands or whether it was imported, to give them some idea of what the radiation doses would be if they lived on these islands.

It was not our data. We used data that was generated primarily by the Lawrence Livermore people. I was confident of the data then and I am today. I think those people have done an outstanding job of sampling the islands out there and analyzing the soil, vegetation, and animal life and doing dose calculations. I thought the information we were giving out was the best we could. We tried to translate their technical reports into something that [the Marshallese] might be able to put their finger on.

In retrospect, we did some things that were a little naïve. I think that it certainly caused a problem at Rongelap, because we used a color code to indicate dose levels. We only used four gradations. We put some islands in this highest category that was a pretty broad range. In other words, this category was all islands that had levels above a certain level. The lower end of that. We should have broken that into about three different dose groups.

The Rongelap people then looked at these books and they found islands that were that dark and felt that this should have been cleaned up like Eniwetok. In fact, if we had done it like we should have, they would have found that the levels were less than they were at places on Eniwetok where they cleaned up. In a way, this *created* a problem rather than solving one.

I'm hopeful that at least some of the Marshallese gained some knowledge about radiation from these [booklets]. The biggest problem for us in dealing with people like the Marshallese was that they had no scientific vocabulary. We had to try to find ways to express some of these concepts in nonscientific terms. How do you explain risk? They don't know what risk really is.

HARRELL: Apparently that term had been excised from the Russian vocabulary during the Communist years, so even talking about Chernobyl, they didn't have a term for risk.

BAIR: I was involved in Chernobyl, too. That was another experience.

Contribution of Langham Studies to Understanding Plutonium Exposure

SHINDLEDECKER: Two things that I wanted to ask you about that I don't think we touched on. One was, back to those early, original [Wright] Langham plutonium studies: How important were they to the body of knowledge of those that came after? How important is that data? Did it have any impact, in your knowledge, of what you learned about plutonium in your work, and how important were those studies?

BAIR: I think the data that were generated from those patients that were given small amounts of plutonium, were invaluable. It was the only thing that was available to estimate body burdens for people who were exposed.

You still cannot really detect body contents of plutonium with external devices: There isn't anything strong enough to detect it. The only way that they could monitor workers was to collect samples of urine and excreta, and they had to relate it to something. The only thing they had to relate it to was the amount of plutonium that was given to these people.

Langham was able to construct excretion curves that were used to assess exposures for lots and lots of workers. If they hadn't had that, I don't know what they would have used.

Subsequently, there has been some verification of that in the registry, the Transuranium Registry,[50] [that] is contrary to what you see in the papers and other places. It has nothing to do with looking at effects of these materials on people. Strictly, the data was used to verify the models that we used. After all these years, where their body burdens were assessed using this model, now you look at it (if these people actually died) and look at the content of the tissues, how close did that Wright Langham model predict the body burdens?

I think that probably did more than anything else to reassure the workers themselves that they were not exposed to truly harmful amounts. That was also assur[ance needed by] the AEC and other interested agencies as well as the employer. People were not overexposed. —You were going to say something?

HARRELL: What was the reaction to Pat Durbin's[51] follow-up in the '70s to some of those plutonium injectees?[52]

BAIR: Very positive. I think it was looked at as a necessary second stage, because at that point we had more information. She did a very thorough job on that.

Comparison of Inhalation and Injection Studies of Plutonium

HARRELL: Did it change any of your assumptions that you'd made about long-term effects?

BAIR: No, I don't think so. The one thing that we did—I mentioned this earlier—[was to revise] this excretion curve that Langham developed [that] was based on material that was injected directly into the blood. Most exposures were occurring because they inhaled it; most of it was inhaled.

What we were trying to do with our animal studies was to link the two. [In] some of our earlier reports, we did report on the excretion of pluto-

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

51 For the transcript of the interview with Durbin, see DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (July 1995).

52 See "Following Up on Human Subjects of the Plutonium Study" in the Durbin transcript (ibid).

nium from, primarily, dogs. About the only similarity we had was [that] if you had plutonium sitting in the lung, you had a very slow dissolving of that material and it eventually goes into the blood. That gives you sort of a chronic introduction of material into the bloodstream, sitting in the lungs.

That was different from what they got from the studies where they injected people. They gave them a [single acute injection of plutonium into the blood] and that was it. They did some extrapolations of trying to build into their models that were using the idea.

I think that for those of us doing the inhalation studies, it was something we kept in front of us all the time, looking at how the inhalation studies differed from that. I think that the result after all these years has been that that model overestimated the body burden—was conservative—which was the way it should have been. If you have a choice, you'd better make it conservative.

Herb Parker's Influence on Research Standards at Hanford

SHINDLEDECKER: This is sort of a broad question. You referred to Herb Parker a couple of times. I know that he was just a giant person here [at Hanford].

BAIR: In radiation protection.

SHINDLEDECKER: What was his impact as far as the kind of research work or whatever that was done here at Hanford overall?

BAIR: That's an easy question to answer. His objective was good science. I never felt that he wanted anything, that he anticipated any of the results of the search or any information, to influence it. That man, I swear, read every report that we wrote. There are many of us still in the Laboratory who remember getting a phone call from Herb, "What do you mean? What are you trying to do here?"

He looked at reports on the basis of whether they were good science, whether the study was well conceived, whether the concept was valid, how it was carried out; and he looked at the interpretation of the data. He did not allow people to overinterpret the data, either on the side of favoring higher dose limits or lower ones. His interest was strictly in whether it was good science. He did not want to have the possibility of any of the decisions regarding health protection being made on the basis of bad science.

HARRELL: As far as that is concerned, I noticed that you did a report analyzing some of Gofman's[53] work on the fallout. Was Herb Parker's influence strong in evaluating that science?

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[53] For the transcript of the 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).

BAIR: I would say Yes. I think that anything we did in those days, there was a possibility of his reading it, a strong possibility of his reading it. We tried to do the best possible job. We did not want to suffer the consequences of his finding something wrong with it.

Controversy Over Interpretation of Radiation Effects Data

HARRELL: How big was the controversy in those days between various camps of interpreting the fallout data?

BAIR: I'm not sure what you mean by "interpreting the fallout data." There were certainly controversies with respect to understanding the mechanisms that were occurring.

HARRELL: There's Gofman's article and then there [are] some articles in *Esquire* [Magazine], I believe by a former dentist who had exposed his own daughter to x rays. Then he apparently wrote a series of articles examining the fallout data and making epidemiological conclusions.

BAIR: There was [Ernest] Sternglass, too.[54]

HARRELL: I think that was Sternglass.

BAIR: I think the controversies in my view were essentially whether it was good science or bad science. I think the feeling among many of us is that some people wanted to try to find something, for whatever reason. They would select data, interpret it in a way that suited their purpose. I think the controversy, that I'm aware of, was if it was good science or not.

I've thought a lot about where these people are coming from, because I have known many of them. I knew Gofman. I knew Art Tamplin[55]—there's another name. In fact, Tamplin and I were on a committee back in the '60s that looked at the nuclear rocket program. You think back and see some of those people meeting in those days and wonder if it had any influence on such thing[s] as behavior.

I think that some of these people who have reputations for being good scientists have tarnished their reputation in a way, and I don't understand why. They obviously have stepped back from using accepted methodology. They've invented their own sometimes. In those cases, it doesn't stand up to scrutiny, to achieve a result that they had in mind when they started.

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[54] Sternglass had done some calculations and was cited in *Esquire* in a article entitled "The Death of All Babies." His estimate was that 400,000 children would be hurt with genetic disease as a result of the weapons program. For a discussion of that article and AEC's response, see "The Nuclear-Armed Antiballistic Missile Controversy" in the Gofman transcript (ibid.).

[55] 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. He wrote a response to Sternglass's findings, arguing that Sternglass's projections were a thousandfold high. Tamplin's close work with Gofman and involvement with the human radiation research community are discussed throughout the Gofman transcript (ibid.).

We've had this series of symposiums here since 1961 or 1962. We invited Sternglass and these people to meetings to get an opportunity to present their case before their peers. Some of them came, but it didn't stop them. We always felt that they deserved an opportunity to really demonstrate that they had a valid approach.

Even though we were supported by AEC and DOE, no one ever told us our results had to come out a certain way, that we had to support lower limits, or assure that people were safe when they were not, or anything like that. We were paid to do the research and produce results that could be used by people whose job it was to set standards.

And trying to, well, it's like the Mancuso Study,[56] which I'm sure you've heard of. I don't know if you've talked to Ethel Gilbert or not, but she's somebody you really ought to meet. She's our epidemiologist. She is, in my view, someone you just cannot find any possible motivation other than doing good science. She is not about to deliberately bias her results in any fashion. She has taken on our English lady, Alice Stewart,[57] many times. I think Ethel is a very good one to do it. She must come across to people as every bit as legitimate as Alice Stewart. Congressmen are impressed by Alice Stewart because she shows up as this little old lady, benign. But, this little old lady does an outstanding job of keeping herself employed.

I don't think these controversies are wrong in any way. I think it keeps people on their toes, although I think that there's a limit of how far you should go. I know that our research was supported longer and with more money than it might have been if the controversies hadn't existed. That's a positive. It certainly benefited some of our people. I think the taxpayers have to pay the bill for some of this. It's also had an impact on decisions with respect to nuclear power and other major decisions in this country.

Most of these people have gone to the courtroom and testified as expert witnesses. In every case, I know of, they've come out [on the] short side and they've been criticized by the judge. But, the fact that they continue to do it is a mystery to me.

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[56] In the mid-1960s, a contract was awarded to Dr. Thomas Mancuso and his colleagues at the University of Pittsburgh for studies at Hanford, Oak Ridge National Laboratory, and the Y-12 Plant and K-25 gaseous diffusion plant at Oak Ridge, to analyze radiation exposures and health outcomes in the nuclear weapons workforce. However, when preliminary (and controversial) reports from the Mancuso team suggested that the occupational risk of cancer was significantly higher than then-current beliefs, the Mancuso contract was abruptly canceled. Epidemiologic research was transferred and confined to the agency's own Laboratories (thus raising the real possibility of conflict of interest) and divided among them, rather than conducted as an integrated effort. Source: U.S. Department of Energy, Office of Energy Research, Epidemiologic Task Force, *DOE Epidemiologic Research Program, Selected Bibliography* (DOE/ER-0437), January 1990.

[57] In 1956 and 1958, Alice Stewart had written articles claiming that a dose as small as half a rad to a rad received by children in utero would raise by more than 50 percent the risk of cancer in the first 10 years of life. She and John Gofman later became professional friends. For a discussion of their friendship, see "The Low-Dosage Harm Controversy" in the Gofman transcript (DOE/EH-0457), June 1995.

SHINDLEDECKER: I'm trying to think if there is anything else. One question: Did you ever experiment on yourself?

BAIR: No, absolutely not. Never was tempted.

SHINDLEDECKER: It wasn't terribly uncommon, was it?

BAIR: I really don't know how common it was. I'm sure that we had people in the Laboratory who might have done things using themselves that we never knew about. The tritium study was not a secret, and those people obviously knew what they were doing. I imagine they must have had approval. I do know of people in other sites and laboratories that did. If they did not use themselves, they certainly did their work in a way that would cause problems for themselves.

I know a toxicologist at a university who set up a lab. I didn't see it, but I think he had a lab set up in his garage where he was testing materials for companies on the side. You can do that in universities. I think he really poisoned himself, because he died fairly early on—very cavalier with research; they would not have lasted here [professionally] if they would have been found out.

HARRELL: That seems to be a theme that has run through your work. Good science and staying with one topic until you've really understood it.

BAIR: I think that I've often admired people who did not stay with one topic. I know some really outstanding scientists who, throughout their career, moved into three or four different fields and really were outstanding in all of them. I admire those people. It was all I could do to stay here.

Reflections on Colleagues

HARRELL: Can you think of any other people? You've mentioned Ethel Gilbert, any other early pioneers, people who would have been involved in MED or AEC decisionmaking and had a lot of knowledge of different programs, people that we would want to interview for those reasons?

BAIR: Have you talked to any people at Headquarters?

SHINDLEDECKER: Actually, I don't think that we do have very many decisionmakers from the Headquarters on our list. What would be a good thing to know is names there.

BAIR: Bob Wood has retired, but he's been in, I'm not sure what they call the organization now, the old DBM [(Division of Biology and Medicine)], the OHER[58] for many years. I think he went back, arrived in the '60s. Murray Shulman has been there even longer than that. He's still there as far as I know. He was my vintage. I can't remember when he went to Headquarters. He's still there. Bill Burr.

SHINDLEDECKER: Someone suggested him to me.

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[58] Office of Health and Environmental Research of DOE

BAIR: John Totter[59] at Oak Ridge.

HARRELL: Totter?

BAIR: Yes. He should be somebody to talk to. You really need to move fast. Louis Hemplemann just died not too long ago. He would have been a really outstanding person to talk to. I don't know whether Robley Evans was—

HARRELL: We're looking into whether he is still alive.

BAIR: He's still alive, but I don't know what his health is like. He had to go down to Arizona. He would certainly be a good person to talk to. You ought to talk to Gofman, too.

SHINDLEDECKER: We are, actually. Not the two of us, but our group.

BAIR: You should talk to these people, Sternglass. See where they're coming from. I think you should get all [views].

K. [(Karl)] Z. Morgan, he's another person. People have very high regard for him. All of a sudden he has seemed to— The story is that he was going to give a paper at a foreign meeting and when he got there—he went there early, before the paper had gone through clearance—he was told that the [paper] was not cleared, he couldn't give it [(the paper)]. That turned him. I don't know. I've heard that.

Peer Review and Publication of Research

BAIR: We've had to clear papers here, and at one time it was to make sure it wasn't classified information that was in them and that it was legitimate. To my knowledge, none of our papers in the biomedical environmental sciences have ever been rejected because of anything. The only person who would have rejected them in the first place would have been Herb because he felt they didn't meet the scientific standards that he was trying to achieve in the Laboratory.

It's interesting about biologists. You've got to find effects, or you can't publish your work. We've done it to ourselves and we've, in a sense, I think, caused biologists to design experiments to find effects. They know if they don't find effects, no one is going to publish their paper. All of the studies that we have done here, and [that] biologists are doing everyplace, are maximized to find effects. I think that there are occasions where investigators will over interpret their data to show effects that really aren't there. They know that the chances of getting the paper published is going to be less.

There have been occasions when we have sent papers back to authors and said, "You better be sure about this before you go out on a limb

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[59] Totter headed the AEC's Division of Biology and Medicine in 1969, when the Sternglass findings were published in *Esquire*. See "The Nuclear-Armed Antiballistic Missile Controversy" and "Testifying Before Congress on Radiation Effects" in the Gofman transcript.

here." We always tried to make people repeat their results. If somebody does a study and gets results that are unexpected, they generally aren't encouraged to publish until they have been able to repeat that.

HARRELL: You've always had pretty good control of the quality that came out.

BAIR: We've tried. You can't control people totally, and we haven't tried to. I think management here has been trying to advise people and help them develop their careers. I think that we would have been remiss if we would not have advised somebody to take another look at their data, because it's there for years and [if it's wrong they] can suffer, if not somebody else. I think part of management's responsibility is to ensure that papers go out that are as good as they can possibly be.

Our job here has been to develop scientific careers among our staff as well as obtain results that have been useful to AEC and DOE. It's not different; it's all the same process.

SHINDLEDECKER: One thing that occurred to me when you were talking earlier is how much of the biological research was classified at the time that it was going on, or was there a point at which that sort of research was no longer kept secret?

BAIR: I think, in my memory or my experience, it has been sort of a gradual thing. When I got here, you could say *tritium* and certainly *plutonium* and all the other things. At one time, even those words were classified. By the time I got here, they were not. I guess in my experience, the thing that has been classified, more than anything else, is anything that would have allowed somebody to determine the rate of plutonium production at our site.

I'm trying to remember if I ever had a paper that was classified. We wrote documents, and I don't know if anything I ever wrote was stamped SECRET. As far as biological research is concerned, ever since I've been here, the emphasis has been on publishing in the open, peer-reviewed literature. In fact that's been a battle, because people have been reluctant to publish when they should have. There's an awful lot of work around here that has not been published that should be. I even did studies on alligators.

HARRELL: That was published. Something appeared in your bibliography.

BAIR: It was reported at a meeting, but I never published the full paper. It didn't seem like it had a high priority. We really were encouraged to publish our results since I arrived, and I'm sure before then. All the iodine work that was done in the late 1940s and early 1950s—for example, the sheep work. Those papers were published before I got here.

I don't feel that it was ever the intent of the AEC or DOE to withhold studies on health effects or effects on studies of the environment, unless they did provide information that could be used to get information on the rate of production of plutonium.

Preserving the Written Record

SHINDLEDECKER: Do you have any kind of collections of personal papers or any of that sort of thing that a researcher coming behind us might be interested in looking at, in the future?

BAIR: Actually, when I cleaned out my office, I hauled most of it down in my basement. I did have lots of reprints from years back which I would be very happy to provide you. I suppose that's something that people like myself, who have ended a career, tend to see and it bothers us. That is, that younger people aren't really aware of some of the earlier work. You're always seeing research repeated that really wasn't necessary. If it duplicates your results, you feel good about it. And, I don't know if it actually hasn't duplicated the results of some of the older studies.

There are some studies going on now throughout the world, in Japan and England and France and Germany, that are repeats of things that we did here. This is confirmatory. But on the other hand, in today's climate, research funds are so limited, you'd rather see money spent on studies that have not been done before in the next phase, rather than just repeating. Some of it is getting done. I think the more available the materials in the past are, the better chance we have of them not being blindly repeated.

HARRELL: Do you have internal memos and office correspondence that you saved?

BAIR: I've got a mess of stuff. I don't know what I have, actually.

HARRELL: We're finding that a lot of the office, day-to-day stuff was just destroyed and gone.

BAIR: I think everything that we had here during GE days[60] was sent over to the federal storage facility in Seattle. I don't think I have anything that goes back to those early days. We have set up a Radiation Biology Archive under Dr. Charles Watson. I've given them a bunch of stuff. I gave them all of my Marshall Islands papers. I don't know what they're going to do with it.

There is an effort at the Washington State University to put together an archive and put in everything that's available. It's someplace that would be used, I think, by students, who are more likely to go there and get things out than maybe you would go to an archive located on the Hanford reservation.[61]

I think all of this material, at least a lot of it, ought to be saved for historians. My wife calls me a pack rat. I haven't saved some things I should have saved.

HARRELL: *(to Shindledecker)* Do you have anything else?

SHINDLEDECKER: I don't think so.

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[60] General Electric became the prime contractor in charge of running Hanford for the AEC in 1946; Bair began work at Hanford in 1956.

[61] a colloquial term commonly used to refer to the Hanford Site

HARRELL: This will conclude our interview.

BAIR: As far as contributing to your objective about getting information about human studies, I haven't been able to help you very much, because my emphasis has been on animal research.

SHINDLEDECKER: The historical perspective is helpful.

BAIR: Obviously, human studies are very important, and I think what you're doing is extremely useful. Again, I think that you can prevent a repetition of what's happened. There have been a couple of previous efforts to get information compiled. Obviously, they didn't go far enough. I think what's happening now is probably needed. I think back to a lot of other people and I'm appalled sometimes when I see how supportive the media is, [when their research] doesn't really deserve that kind of [aggrandizing] interpretation.

HARRELL: Probably ninety percent of it has been published already.

BAIR: Yes, it has. But then it's rediscovered by the media and they attach to it significance that is totally out of place. Even [DOE] Secretary [Hazel O'Leary], she said some things that I think should not have been said. She deplores some of the studies, and I don't think she really understood what they were before she said it. I think those kind of things are unnecessary.

HARRELL: I think we should conclude the taped portion of our interview. Thank you very much, Dr. Bair. ❑