DOE/EH-0481

HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Biochemist
John Randolph Totter, Ph.D.

Conducted January 23, 1995

United States Department of Energy
Office of Human Radiation Experiments
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MASTER

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FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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

CONTENTS

Page

Foreword iii

Short Biography 1

Early Teaching and Basic Research in Biochemistry (1935–50) 1

Nucleic Acid and Leukemia Research at Oak Ridge (1952–56) 3

Participation in AEC Biochemistry Training in South America (1958–60) 5

The Division of Biology and Medicine's Research Focus on Radiation Effects 7

Early Leadership of the AEC's Division of Biology and Medicine (1956–60s) 9

Attempts to Prevent AEC's Biologists From Thwarting Nuclear Power 10

Radium Oversight Becomes a Political Football Between AEC and the Public Health Service 11

Controversy Over Low-Level Radiation, Iodine From Fallout 13

Livermore Biomedical Division; Conflicts With John Gofman (1962–72) 15

Origins of AEC-Funded Research Programs 17

Advisory Committee on Isotopes for Human Use 18

The Division of Biology and Medicine's Research Goals; Bone Marrow Transplants at Oak Ridge 19

The Military's Animal Research on High-Dose Radiation 22

AEC Involvement in International Research 23

The AEC's Environmental and Ecological Research 24

Suspension of Proposed Plowshare Projects (Circa 1963) 25

AEC Program Approval Coordination 27

Fishing (for Foreign Secrets) Where the Ducks Are 28

Radiation Research on Penitentiary Inmates in Washington and Oregon (1963–73) 29

Pre-World War II, Nongovernmental Radiation Research 36

Medical Follow-Up on Occupational Radiation Exposure 37

Follow-Up of Subjects From Plutonium Injection Experiments 37

Low-Level Radiation and the "Hot Particle" Controversy 38

Support for Animal Studies 38

Early and Recent Research Into Indirect Effects of Radiation and Cell Repair Mechanisms 40

Ethics of Government Radiation Research 41

Research Interests of Commissioners Seaborg and Schlesinger Compared 42

Rise and Fall of AEC Support for Cancer Research Hospitals (1948–74) 43

Public Misperceptions About Radiation and Cancer; Underuse of Established Biomedical Facilities; and Funding of Environmental Cleanup vs. Biomedical Research 46

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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September 1995

Interview with John R. Totter, Ph.D.
Setting: January 23, 1995; Oak Ridge, Tennessee
Interviewers: Darrell Fisher and David Harrell
(DOE Office of Human Radiation Experiments)

ORAL HISTORY OF BIOCHEMIST JOHN RANDOLPH TOTTER, Ph.D.

Dr. John Randolph Totter was interviewed on January 23, 1995, by David Harrell of COMPA Industries and Dr. Darrell Fisher, a health physicist from the Pacific Northwest Laboratory, on behalf of the Department of Energy Office of Human Radiation Experiments. Dr. Totter was selected for the oral history project because of his career with the Atomic Energy Commission Division of Biology and Medicine (DBM), particularly as its director from 1967 to 1972.

Short Biography

John Randolph Totter was [redacted]. He is married with three children. He received his A.B. in 1934 and his A.M. in 1935, both from the University of Wyoming. In 1938, he received his Ph.D. in Biochemistry from the University of Iowa. From 1938 to 1939, he was an instructor at the University of West Virginia. He joined the faculty of the University of Arkansas School of Medicine in 1939, as an instructor. Dr. Totter remained there until 1952 after serving as an associate professor. He then joined Oak Ridge National Laboratory (ORNL), where he served as a biochemist until 1956.

Dr. Totter worked as a biochemist for the Atomic Energy Commission (AEC) from 1956 to 1958. From 1958 until 1960, he worked with the Rockefeller Foundation at the University of the Republic of Uruguay, in Montevideo. Upon returning to the United States, Dr. Totter took a position as a professor of Chemistry and Chairman of the Division of Biological Science at the University of Georgia from 1960 to 1962.

He rejoined the AEC Division of Biology and Medicine (DBM) in 1963 as an Assistant Director. In 1967, he was named Director of the DBM, a post he held until 1972. From 1972 to 1974, he was the Associate Director of Biomedical and Environmental Science at Oak Ridge National Laboratory. He worked as a biochemist at ORNL from 1974 to 1978 and as a scientist at Oak Ridge Associated Universities (ORAU) from 1978 to the present. Dr. Totter has published on radiation effects, amino acid and formate metabolism, and cancer epidemiology.

Early Teaching and Basic Research in Biochemistry (1935–50)

HARRELL: [We are interviewing] John Totter, on January 23, 1995, in Oak Ridge, [Tennessee].[1] Dr. Totter, can we start with your Ph.D. work?

TOTTER: Okay. I was in Biochemistry at the University of Iowa, after getting a bachelor's and master's degree at the University of Wyoming in Chemistry. And at Iowa, I was in Biochemistry and worked under Clarence P. Berg, who was a student of William C. Rose from Illinois. And I worked

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

in the field of amino acid[2] metabolism.[3] From there, I went to the University of West Virginia Medical School and taught for one year in the Biochemistry Department. And then to Little Rock, Arkansas, to the University of Arkansas Medical School, where I stayed a total of 13 years.

FISHER: Teaching Biochemistry?

TOTTER: Teaching Biochemistry to medical students and technicians. I came here [to Oak Ridge National Laboratory (ORNL)],[4] first in 1950, while I was still an employee of the University of Arkansas, and spent six months as a visiting investigator at the Biology Division under Alex Hollaender.[5] And we were doing tracer work with carbon-14.[6]

FISHER: What was it called, the laboratory, at that time?

TOTTER: It was the Biology Division of the Oak Ridge National Laboratory.

FISHER: Okay.

TOTTER: This was my first exposure to work with radioactive isotopes. It was an attempt to determine what effect folic acid deficiency had on the incorporation of carbon-14 precursors into nucleic acid, and the various other compounds that are associated with nucleic acid.

I think it's well to point out that this folic acid is required for the synthesis of the nucleic acids, which in turn are required for our immune processes. When folic acid deficiency results in a drop in the white-cell count,[7] usually infection sets in in an animal or person. If it happens to be a person, they die of intercurrent[8] infection, rather than from the deficiency itself.

But that made me very interested in other work which was going on under the leadership of Charles Congdon at the Biology Division. And Congdon had come, after working for Egon Lorenz, a physicist who had been active in the Manhattan Project doing—well, biological experiments, mostly on survival of animals after radiation.

FISHER: Where was Lorenz's laboratory?

TOTTER: I think he was at the University of Chicago when he did those [experiments]. He was a member of the National Cancer Institute, I believe. It

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2 any of a class of organic compounds that are the building blocks from which proteins are constructed

3 the rate at which chemical processes take place in the body

4 For a history of ORNL, see *ORAU From the Beginning,* written by William G. Pollard with Gould A. Andrews, Marshall Brucer, et al., which was published by Oak Ridge Associated Universities, Oak Ridge Tennessee, 1980.

5 Dr. Alexander Hollaender was the director of the Biology Division at Oak Ridge National Laboratory.

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

7 the count of the number of white blood cells in a specific volume of blood

8 occurring while another disease is in progress

was one of the Institutes [of the National Institutes of Health in Bethesda, Maryland]. And of course, that's where Alex Hollaender, who headed the Biology Division, came from. I guess it was natural for him to look back to the National Cancer Institute when he wanted a pathologist.[9] And he brought Charles Congdon here.

Congdon did much of the early work which laid the groundwork for transplantation of bone marrow in cases of leukemia[10] and other diseases in which you need to kill all of the individual's bone marrow [cells] and then transplant with the non–cancer-containing stem cells from someone else. And I understand that [E.] Donnall Thomas got the Nobel Prize for his [bone marrow] transplant work with humans. And he got much of his information and training, I think, here and in the animal work done by Dr. Congdon. I met him several times while he was here.

HARRELL: Did you do any work with worker safety when you were here?

TOTTER: Worker safety—I had never any particular concern with that. I wasn't close to any of the work that dealt with worker safety.

We had a relatively lax system, in which the responsibility for safety for the individual worker—that is, the scientific worker, the investigator—was really up to him. And there were, of course, people who checked from time to time to see that there was nothing we left around in the laboratory that was dangerous. They checked for radioactivity, and there were monitors scattered throughout the places where we worked, for the more penetrating radiation.

Nucleic Acid and Leukemia Research at Oak Ridge (1952–56)

HARRELL: So your program here under Hollaender was one of basic research?

TOTTER: Basic research, yes. I was interested in what happened to the nucleic acids after animals had been irradiated. I worked with whole animals or with bone marrow[11] from animals to see what would affect the growth or the production by the marrow of the nucleic acids.

HARRELL: And so your emphasis was on cancer research?

TOTTER: Well, it bordered on that. At that time, I was not especially interested in cancer except for the leukemias, which I had seen in Arkansas and which present a tragedy. Anybody who sees leukemic children will understand how difficult it is to face the thought that they have a death sentence on them that's going to be carried out in a year or two. I can't emphasize too much how that affects people who have any empathy at all, and it accounts for the desperation with which some people go to to try to find a cure.

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9 a physician who studies the study of the origin, nature, and course of diseases

10 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

11 the soft, fatty, vascular tissue in the cavities of bones; it is a major site of blood-cell production.

HARRELL: And during this time, was the ORINS [(Oak Ridge Institute of Nuclear Studies)][12] Clinical Division being set up?

TOTTER: Yes. When I first came here in '50 [for six months], Marshall Brucer[13] was setting up a medical program here. He was concentrating on making cobalt-60, a substitute for x rays [in cancer treatment]—developing it. And he was quite successful in doing that development. Then, later on, of course, this branched out into several fields. And it eventually got into ones that were closer to my interests; [at least] some of them [were].

FISHER: Which would be which?

TOTTER: Well, it's the whole field of nucleic acid production and its relationship to leukemia, which was what I was interested in, although I never worked directly in the leukemia field myself. But that had excited my interest.

HARRELL: So how was it decided to have this Oak Ridge program in studying nucleic acids and leukemia therapies? Did they set up the ORINS hospital, the ORAU hospital, to treat cancer patients? Who decided to put all that emphasis—

TOTTER: Well, I'm sure that came from—the original impetus should have come from Shields Warren,[14] who was head of the Division of Biology and Medicine [(DBM) of the Atomic Energy Commission (AEC)][15] in Washington[, D.C.] at that time. He was, I guess, also head of the New England Deaconess Hospital and interested in leukemia himself. He did research on leukemia in animals, as well as treating patients. I'm sure he—he must have encouraged the people here to go into that field, and [later] knowing that across the way [from this wing of the building], in the Biology Division, there was basic work going on, on bone marrow transplantation. It was addressed.

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

[13] director of ORINS; succeeded by Gould Andrews. Brucer died in 1994.

[14] Shields Warren, M.D., was Chief Pathologist at New England Deaconess Hospital and Professor of Pathology at Harvard Medical School. He joined the U.S. Navy Medical Department in 1939 and wrote with others on what was then known about radiation during World War II. Dr. Warren served on the first U.S. team to visit Hiroshima and Nagasaki after they were bombed with atomic weapons and was involved in creating what became the Atomic Bomb Casualty Commission. He was the first director of the AEC's Division of Biology and Medicine and, later, established his own cancer research institute at New England Deaconess Hospital. See "Recollections of Shields Warren" in DOE/EH-0471, *Human Radiation Studies: Remembering the Early Years; Oral History of Radiologist Henry I. Kohn, M.D., Ph.D.* (June 1995).

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

It would be inevitable that there would be some connection in the way the program here went and [that any new work would be] based on the basic work that was going on there and elsewhere.

Participation in AEC Biochemistry Training in South America (1958–60)

HARRELL: Now, in 1956, you became involved in the administrative side?

TOTTER: Yes. I was recruited by Paul Pearson, who was formerly dean of the graduate school in Texas A&M [University], [but at that time] was head of the Biology Branch in the AEC. And Paul talked me into coming [to Washington, D.C.]. Dr. Hollaender was willing to have a member of his staff absent himself for a year or two to go to Headquarters.

When it came time to come back, there were some other circumstances, including Hollaender's encouragement that I go to South America. They were trying to start to train their own biochemists. They wanted to train their own biochemists.

Most of the training—advanced training—was taking place in France or England or the United States, rather than in South America anywhere. So Uruguay felt they might be able to start that with the Rockefeller Foundation's[16] help. They asked me to come down and start research work of my own choosing. And the AEC was quite helpful. They supplied equipment and so forth for radioisotope work. I inaugurated [tracer[17] work] down there. Then I came back to the University of Georgia [in Athens] for two years.

HARRELL: So, did the AEC start a lot of programs in some of these South American countries?

TOTTER: Yes, they did. They had international grants that usually managed to get treaties worked out between the countries with special kinds of help. And they would supply both money and equipment, too, under certain circumstances.

FISHER: Dr. Totter, was the interest [of] the AEC [in] helping developing countries with their biomedical program, to promote the use of tracers, or was it a general [effort] to help establish new programs in biomedical research?

TOTTER: Well, this all stems from [President Dwight] Eisenhower's speech on "turning bombs into plowshares." Well, down there [in South America],

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[16] Founded in 1913, the Rockefeller Foundation directs its grants to three areas. One is International Science-Based Development, focusing on the developing world with emphasis on the environment, agriculture, health, and population sciences. The other areas are Arts and Humanities and Equal Opportunity.

[17] radioactive tags on biomolecules, used to study a biological, chemical, or physical system

they called it "The Plowshare Program."[18] We didn't call it that up here, but that was part of that program.

FISHER: Can you remember some of the early projects that you participated in at the university in Montevideo?

TOTTER: I'm not sure just what you're asking.

FISHER: Well, what were they interested in doing with radioactive materials initially?

TOTTER: Well, I don't think there was any special interest there, officially. There were individuals who were interested, yes, in it. But they wanted to—they simply wanted to train some of their graduates in modern research, in modern biochemical research. And, since that depended heavily on isotopes at that time, that's the way it went.

FISHER: Would these be principally tritium,[19] carbon-14, sulphur-35?

TOTTER: Mainly we used carbon-14. Iodine[-131] wasn't used because, of course, its eight-day half-life precluded that. But I did set up, for them, the counting equipment.[20] They had some. Some of the individuals had a little bit there, but we greatly augmented what they had.

HARRELL: And most of this equipment, or all of this equipment, was manufactured in the U.S., wasn't it?

TOTTER: Oh, yes.

HARRELL: And had been given through foreign aid and other programs?

TOTTER: Well, that [which] I dealt with was mostly given by the Rockefeller Foundation. Aside from the, I think, $25,000 that the AEC gave for equipment; the counting equipment, the [radiation] measuring equipment.

HARRELL: And how was this work published?

TOTTER: Well, it was published, part of it in their local journal, *Montevideo Journal*, and part of it in the *Journal of Biological Chemistry*.

And actually, after I left, no one replaced me, but the work did continue. And more was published from what I had started in the *Journal of Experimental Biology and Medicine*, I believe.

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[18] a program initiated by President Dwight D. Eisenhower to identify and demonstrate uses for peaceful nuclear explosives (PNEs), such as civil engineering projects. For a variety of reasons, no such peaceful nuclear explosions ever were conducted by the United States as anything other than tests. Before its breakup, the Soviet Union reportedly used PNEs in several large-scale civil works projects.

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

[20] equipment used to count the rate of radiation emissions from radionuclides inside a subject's body, using radiation detection instruments or a whole-body counter

The Division of Biology and Medicine's Research Focus on Radiation Effects

FISHER: After your term of service in Uruguay, what did you do next?

TOTTER: I came to the University of Georgia. And for two years I was the chief of the Division of Biological Sciences there. But Charles Dunham[21] brought me back to the AEC [in 1963] as his Assistant Director. At the time, the Assistant Director he had was Max Zelle, who left to go to Argonne[22] to head the Biology Division there.

HARRELL: How did he know you at that time?

TOTTER: Oh, well, he knew me from my two years, from '56 to '58, in the AEC. And I was kind of surprised that he wanted me back—

(laughter)

—but that was nice. And I went back as Assistant Director and was chiefly responsible for the basic research program that he had.

FISHER: What was the basic research program focusing on at that time?

TOTTER: Well, we tried to classify a bunch of things that we felt were needed to understand the effects of radiation and understand how it might be used to benefit the people.

FISHER: Was the principal concern fallout from atomic testing, or was it use of radiation for other purposes?

TOTTER: Well, one branch of the biomedical division[23] was concerned with fallout. And the Sunshine Program[24] that we mentioned a while ago was a big part of that. And I mentioned that there were, I believe, four [or five] branches [in the Division of Biology and Medicine].

FISHER: What were those four branches?

TOTTER: Well, one—the smallest—was Instruments. The development of the instruments was a part of that division's responsibility. And another was the Biophysics Branch, the Medical Branch, the Biology Branch, which I had worked in earlier, and the Civil Effects Branch, which I think is self-explanatory.

The Biophysics Branch eventually split into two or three branches: fallout studies—some of the earliest research in weather by the [National] Weather Bureau was supported by the AEC. And they got started, I think, on their modern research through support from the AEC. And

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21 director of the AEC's Division of Biology and Medicine from 1963 to 1967

22 Argonne National Laboratory outside Chicago; operated by the University of Chicago

23 shorthand for Division of Biology and Medicine; the term is found again when Totter discusses the like-named division at Los Alamos.

24 Project Sunshine was initiated by the AEC in response to the urgent need to better understand the global distribution of fallout from atomic weapons testing and its potential adverse effects in people.

Lester Machta [(a weather bureau official)] was a very important part of that beginning.

We had to collect samples at high altitude to watch for fallout, [and] also to monitor for somebody else's testing beside our own. And that was supported through the Fallout Studies Branch, eventually.

HARRELL: Was the Intelligence Branch involved in some of that work?

TOTTER: Oh, yes. They had theirs, but we didn't have any connection with that—

HARRELL: No?

TOTTER: —in the Division of Biology and Medicine. But they were glad to have our data, I'm sure. But I think they had their own independent work going on.

The U-2 [reconnaissance plane]s,[25] I think, collected some stuff. You know about that aspect. I don't know much about it; I just knew they were involved.

HARRELL: Did you work on—or did the Division of Biology and Medicine do monitoring for the rad warfare tests?

TOTTER: Beg your pardon?

HARRELL: For the radiological warfare[26] tests?

TOTTER: I don't think we had anything special on that. The Civil Effects Branch was interested in effects of [such] warfare, of course.

HARRELL: Some of the rad warfare tests done at Oak Ridge and Los Alamos[27]—

TOTTER: I don't know—

HARRELL: —with lanthanum[28] and—

TOTTER: I don't know anything about—now, I mean, that doesn't ring any bell with me. I know there were releases here, just to study dispersion and that sort of thing.

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[25] a long-range, high-altitude, strategic reconnaissance aircraft with a crew of one pilot that was developed in secret for the Central Intelligence Agency by the Lockheed Corporation (now Lockheed Martin). U-2s conducted overflights of the Soviet Union from 1956 until 1960, when one was shot down and its pilot captured deep inside the Soviet Union. From the beginning, air sampling for fallout to monitor the Soviet nuclear weapons program was an important mission for the U-2. Long after its penetrations of Soviet airspace ended, the U-2 continued to be used on high-altitude flights to sample for fallout from Soviet and Chinese nuclear weapons tests.

[26] the conceptual use of fission-product radiation to kill enemy troops

[27] the National Laboratory near Santa Fe, New Mexico, where nuclear bombs were assembled before and during the Cold War; operated by the University of California for the U.S. Department of Energy

[28] From 1944 to 1962, Los Alamos conducted 254 open-air implosion physics tests in neighboring Bayo Canyon. The purpose of the program was to test weapons designs using conventional high explosives and radioactive lanthanum (RaLa), a short-lived but intense radiation source. Tests were performed specifically to diagnose material motion and compression through high-speed x-ray photographs of the earliest moments of the implosion. The sources involved contained quantities ranging from around one hundred to several thousand curies of lanthanum-140.

And our Civil Effects Branch, which eventually came under Joe Deal's leadership, really developed the aerial monitoring for radiation. I think this turned out to be a very useful thing. There would have been no [aerial] radiological monitoring at Three Mile Island[29] if that had not been developed by the Civil Effects Branch.

HARRELL: Did this include monitoring [for fallout after nuclear weapon tests] at the Pacific tests[30] and Nevada Test Site?[31]

TOTTER: Well, I don't think we had any—in the Biology and Medicine, had any strong input into that. I think we had an observer or two in Biology and Medicine at those tests. But that was before my time.

Early Leadership of the AEC's Division of Biology and Medicine (1956–'60s)

HARRELL: Do you know of the early history of the Division of Biology and Medicine, how the advisory committees became the Division of Biology and Medicine?

TOTTER: Well, I don't know how it was originally set up, but I'm sure it must have been set up by Shields Warren, who was the first leader. But when I came in 1956, it was well-established and going strong. I think these names didn't get on that [list you showed me], did they?

FISHER: Let's go through it again.

TOTTER: People like John Bugher, who succeeded Shields Warren [as director the Division of Biology and Medicine], and then he later on came onto the Advisory Committee [on Biology and Medicine (ACBM)].

Bugher was a member of the Rockefeller Foundation. It was rather interesting that the Rockefeller Foundation, just at that time, began to realize that they had overpopulated the world by eliminating so many tropical diseases [and] that they had better start finding out how to feed all these people. So they were changing over into agricultural research. And I was one of the last people sent by them as a biomedical scientist to some foreign country. But that's kind of beside the point.

The other people—there was always a member of the health physicists there, and [he] was usually a member of one of the laboratory staffs—

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29 a nuclear power generating station 10 miles from Harrisburg, Pennsylvania, owned and operated by General Public Utilities, Incorporated. On March 28, 1979, a combination of system failure and human error led to a partial meltdown in one of the station's two 1,000-megawatt pressurized water reactors. As one consequence, radioactivity was vented into the air. The event at Three Mile Island remains the most significant nuclear power plant accident to have occurred in the United States.

30 chiefly in the Marshall Islands, a group of 34 atolls in the west central Pacific where the United States performed atmospheric tests of nuclear weapons in the 1950s. Since 1986 the Marshall Islands have been a self-governing area associated with the United States.

31 the location where most nuclear weapon tests within the Continental United States were conducted

[biophysicist] Bill Bale from Rochester[32] or Leo Marinelli from Argonne.[33]

Other quite well-known radiologists—Fred Hodges from [the University of] Michigan; Jim Sterner, who was the head of Eastman Kodak's Medical Division [in Rochester, New York]. Quite well-known geneticists, like Bentley Glass from Johns Hopkins [University in Baltimore], Earl Green from Bar Harbor, [Maine,] the establishment and the source for genetically controlled mice for all of the world. And he [(Dr. Green)] was head of that operation. Phil Cohen, a biochemist from [the University of] Wisconsin [in Madison]; and Dr. Finch from Washington University.

FISHER: Clement Finch from the University of Washington?

TOTTER: Not Clement. Clement, I think, is the one who's [an] age specialist—is the aging specialist, isn't he? Maybe it *was* Clement Finch. I'm uncertain about the first name, but there are two Finches involved here.

FISHER: Clement was a hematologist. He was interested in radiation effects [and other things such as] iron metabolism.

TOTTER: That is the one then. Carl Moore from Washington University in St. Louis. He is deceased now. So is Jim Sterner. Hodges is deceased. Bugher is deceased. I don't know about the others. They met three or four times a year at different places where we had contracts—major contracts.

HARRELL: Was this in '56 to '58?

Attempts to Prevent AEC's Biologists From Thwarting Nuclear Power

TOTTER: '56 to '58, and also from—all the time, right through the '60s. When I became chief, I was the first Ph.D. to be a chief instead of mandated.[34] The others are on here.

When Schlesinger[35] came in, in '71 or so, on some campaign to reduce costs, he asked me to abolish the Advisory Committee for Biology and Medicine. I refused to do it. I don't know that his only interest was financial. I'm not sure about that.

FISHER: Was this James Schlesinger?

TOTTER: Yes, James Schlesinger.

FISHER: Can you tell us more about this, because you brought up an interesting subject. And for the Director of the Atomic Energy Commission to want

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[32] the University of Rochester, Rochester, New York

[33] Argonne National Laboratory, near Chicago, Illinois

[34] the first individual to be appointed on personal merit rather than selection based on other factors, such as personal friendship or individual politics

[35] Dr. James Schlesinger was appointed by President Richard Nixon to be Chairman of the Atomic Energy Commission and, in the early '70s, led its restructuring into the Energy Research and Development Administration (ERDA).

to abolish a well-established committee is very interesting. There must have been something more to that.

TOTTER: Well, I can—yes, I think you're right. There probably is more to it. All I can do is guess.

But I mentioned a while ago, when we were not recording, that there was some jealousy or—but that's not the right way to put it. There was some interest, in other divisions, of "boxing in" biologists so they couldn't act to—inhibit any of the things that they might feel necessary—

HARRELL: Testing?

TOTTER: —to promote nuclear energy and sort of carry out their responsibilities.

In 1959, while I was in South America, Lewis Strauss had gone, and there was a new [AEC] Chairman.[36] And his name has always escaped me. Since I never met him, I don't remember him well. He separated the Biology and Medicine Division into two sections. And one section he called Operational Safety. And the other section was regarded as the Research Division. And that separated the people responsible for safety, say, at the test site—although that was no longer operating aboveground, it was operating belowground.

And the curious thing about it is, then, that the Operational Safety became an overhead division. So they did not appear before Congressional committees. Whereas, the Biology and Medicine Division remained as it was.

FISHER: A line-item budget function—

TOTTER: Yes, a limited function.

FISHER: —subject to Congressional review?

TOTTER: Yes. And so we had to answer questions which should have been properly asked to the Operational Safety.

Radium Oversight Becomes a Political Football Between AEC and the Public Health Service

FISHER: What was Congress interested in at that time, when it came to the budget process?

TOTTER: Well, it's really kind of hard to tell. I can tell you what the main controversies were then.

FISHER: Please.

TOTTER: At that time, they were very concerned about radon[37] from tailings[38] dumps. And [about] the situation in Grand Junction, [Colorado,] where

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[36] Dr. Totter is referring to John A. McCone, who later headed the Central Intelligence Agency under President John F. Kennedy's Administration.

[37] radon-222, a naturally occurring, heavy, radioactive, gaseous element formed by the disintegration of radium-226

[38] sand residues from the milling of uranium ores

some houses had been built with foundations made out of tailings that had a little radium in them, so the radon [emission rate] was high.

That comes back to your question a while ago, too—well, it deals with responsibility. When the Atomic Energy [Commission] was set up [in 1947], an enabling legislation had to take care of the radium situation, because the medical people handled the radium. And so—they, anyway, separated radium and its daughters.[39]

[The AEC was exempted from] responsibility for [these elements] because this was part of the U.S. Public Health Service [(USPHS)] responsibility. And that is what gave rise to the big argument, because the radioactivity in the tailings dumps were from radium. The AEC could disclaim responsibility for that. And the USPHS didn't want to accept it. They said, "You've dug the stuff up; it's not our responsibility." So it never was settled. They couldn't settle it politically.

HARRELL: And they weren't concerned about miners at that point?

TOTTER: Well, they were concerned about the miners. And the AEC felt that this was the responsibility of the USPHS.

FISHER: Even to the present time, I don't think that radium is regulated by the Nuclear Regulatory Commission.

TOTTER: I think that's right.

FISHER: It probably has historical roots—

TOTTER: Yes.

FISHER: —in that division.

TOTTER: And that gets back to your question a while ago that I've tried to recall and couldn't, about [how] the enabling legislation made a General Advisory Committee for the AEC. It was a statutory committee.

And you asked about abolishing the ACBM. Again, it was tried to get the ACBM, a statutory [requirement], [abolished] but Congress refused to do it, for some reason. We don't know what internal politics caused them not to do it.

But that's why Schlesinger could ask for the abolition of the—he couldn't ask for the abolition of the General Advisory Committee. Well, he probably wouldn't have anyway, but he could ask for the abolition of [the ACBM].

HARRELL: So he may have had help from Congress on that?

TOTTER: Yes, it's quite possible that he did. It's quite possible.

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[39] isotopes formed by radioactive decay of another isotope

Controversy Over Low-Level Radiation, Iodine From Fallout

FISHER: We were talking about some of the more interesting controversies that took place during your term as Assistant Director and Director [of the Division of Biology and Medicine].

TOTTER: Well, of course, aside from the tailings controversy, there always was the controversy about the effects of low-level radiation. And that has not yet been resolved satisfactorily.

And I'm quite sure that scientific [questions remain]. Well, you see those pictures back on the wall up there? Do either of you recognize those two people?

(pause)

They're up there to remind me who you can and who you can't trust.

(laughter)

That's [John] Gofman[40] and [Arthur] Tamplin,[41] whose names I'm sure you've heard of. And that [issue] broke out while I was [Director of this Division]. It came about in a quite complicated and odd way.

The other big controversy was over iodine and its effects. And this happened in about 1952—1962, I mean. About the time I came [to AEC] it was a hot subject—and growing hotter.

And so, Dr. Dunham thought that if he had a Biology Division associated with the Lawrence Laboratory at Livermore, [California,] which was a bomb factory, maybe he could get some control over the iodine problem.

FISHER: What was considered "the iodine problem"?

TOTTER: Well—

FISHER: Can you explain this just a little more?

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[40] John Gofman, a physician and biophysicist, held that there is no safe level of radiation exposure. His public views and outspoken style brought him into frequent conflict with Totter and the AEC. For Gofman's account of these conflicts, see "The Controversy Over Nuclear-Armed Antiballistic Missiles (1969)" in DOE/EH-0457, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. John W. Gofman, M.D.* (June 1995).

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

TOTTER: At the time the [nuclear weapons] tests aboveground were being made, the monitoring was usually gross beta[42] monitoring. They followed the cloud with the radar measurements—and gamma[43] measurement.

FISHER: So, it was a concern for iodine-131 and fallout—

TOTTER: Yes. And if—

FISHER: —the atomic testing?

TOTTER: If you followed the gross measurement and assumed that the iodine didn't fractionate [(didn't separate out of the cloud and quickly deposit on the ground and other surfaces near the point of release)], then the doses that people received were very high. But the measurements did not permit you to know for sure whether the iodine fractionated or not. It didn't [necessarily] all stay a consistent part of the cloud. A member of the staff of the Division [of Biology and Medicine] wrote up a pretty strong argument that the children had received enormously high doses of iodine, based on the gross measurement.

FISHER: Do you remember who that was?

TOTTER: Yes I do, but I can't recall his name. He later transferred to the Army, I think—the [Defense] Department, anyway. And he was very active in getting the release of a black man who had been convicted of raping a woman, from the—Maryland Penitentiary. I know you could trace him that way, because it's a very well-known case.[44]

There was nothing wrong with his arithmetic. What was the matter with [Harold Knapp's report] was that you didn't know whether the gross beta measurement or the gross gamma measurement told you how much was iodine and how much was not.

FISHER: Didn't distinguish between the radionuclides—

TOTTER: And iodine is very weird. It doesn't go very far [through the atmosphere without change].

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42 Beta particles are electrons or positrons emitted from an atomic nucleus in beta decay.

43 Gamma rays are highly penetrating photons of high frequency, usually 10^{19} Hz or more, emitted by an atomic nucleus.

44 Dr. Harold Knapp worked in the AEC Division of Biology and Medicine's Fallout Studies Branch. Following up on assessment of sheep exposure to iodine-131, in September 1962 he submitted a report that concluded that aboveground nuclear weapons tests had produced radiation doses around the Nevada Test Site significantly higher than previously announced by the AEC. This brought him into conflict with Dr. Gordon Dunning, also of the Division of Biology and Medicine, who had taken the position that radiation from the Nevada Test Site was at safe levels. Dr. Charles Dunham convened a meeting of scientists to review Knapp's paper. For a participant's account of that meeting, see the section "Livermore Biomedical Department's Work on Fallout and Plowshares (1963–65)" in the Gofman transcript (*op. cit.*) In the early-1960s criminal trial referred to by Dr. Totter, Dr. Knapp's inquiries legally challenged the conviction and led to the release of three men. For his actions, Dr. Knapp received the Oliver Wendell Holmes Award of the American Civil Liberties Union. Source: Philip L. Fradkin; *Fallout, an American Nuclear Tragedy*; 1989; University of Arizona Press; Tucson; p. 192.

So they argued. He[, Knapp,] wanted to make a WASH number paper[45] out of that. And a lot of the Commission [members] didn't want that, because it [could lead to serious consequences] and they were not sure of it. So there was a hell of an argument. And finally, I guess—and I don't know exactly what the disposition on the paper was. Maybe it was kept secret from all—but people knew about it, so it didn't matter.

Livermore Biomedical Division; Conflicts With John Gofman (1962–72)

HARRELL: And so then, Dunham wanted to find out more about this and set up a—

TOTTER: Yes. [In 1962] he set up a [new] division—[the] Biomedical Division—in the Lawrence Livermore Laboratory. And it was done over the summer, when the ACBM didn't meet. And so it was not checked very well with them.

And when it came time to put up or shut up for Lawrence Livermore Lab— John Foster was chief [of the Livermore Lab] then—John Foster says, "We'll do it as if you'll allow me to put John Gofman[46] as head of it. Otherwise, we won't do it." And Dunham was terribly upset, because he knew Gofman, [at least by reputation], and he suspected that—and he knew—Gofman had been a little bit wild in his behavior earlier, and he was afraid of him.

He[, Gofman,] was a physiologist, and he had had a grant from one of the National Institutes [of Health] which had been [terminated] for what they considered bad science. So, he didn't want him, but he was—he felt that he was forced to accede to Foster's requirement. So it was set up with Gofman as chief. And the trouble commenced then.

And it turned out that Mr. Ramey, who was on the Commission, was not aware of it either. He was away somewhere when it all happened. And it happened within two or three months, a very short space of time. So, neither the ACBM nor Jim Ramey had access to the information before it was a *fait accompli.*

Mrs. Ramey[, Ramey's wife,] was a good physiologist, and she knew all about Gofman. It was in her field that he made what was considered a big mistake. I'm not so sure it was a mistake, but it looked like it then. And so Ramey was very upset about it.

Some of the members of the ACBM got wind of it in a roundabout way, and they were upset about it. But there was nothing—it was already done—nothing you could do about it. So it eventually erupted again when they [(Gofman and Tamplin)] decided to allow local politics to enter into their science.

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[45] A WASH number paper was an official AEC research report widely distributed to libraries, usually dealing with nuclear health and safety.

[46] For insight into discussions leading to establishing this laboratory from Dr. Gofman's perspective, see "Establishing Livermore Laboratory's Division of Biology and Medicine" and "Jack, all we want is the truth" in the Gofman transcript (DOE/EH-0457).

HARRELL: And so, what did Dunham feel about what had happened, if he had gotten what he wanted out of this?

TOTTER: Well, they set up a program and started monitoring. We tried to make it strong in ecology [and other environmental sciences], but they only had one ecologist. And he hired people who would compete with the other National Laboratory work already going on, rather than trying to start something new. So, we were disappointed about his inability to assemble the kind of a group that we thought he should have assembled. But they were pretty good scientists, most of them. Some of them were excellent.

And then Gofman, himself, started pushing some research that the geneticists on our staff in the Biology Division [(the Biology Branch of AEC's DBM] thought was—made claims far beyond what he had actually shown. So, it kind of went sour.

And then [Tamplin and Gofman] announced that there were thousands and thousands of cancers [caused by] fallout and so forth, which we [(the AEC)] were covering up and that we should change the standards—without any knowledge, real knowledge of how the thing was put together, [how standards for radiation exposure were recommended by] the National Committee for Radiation Protection, National Commission—whatever it is.

FISHER: The National Council [on Radiation Protection and Measurements].

TOTTER: The NCRP[47] [experts] were the ones who really set the standards, and the AEC was required to follow them. Of course, the Federal Radiation Council had been established, too, but it had a short life, because it was sabotaged, I think, by the Secretary of Health, Education and Welfare, or whatever it was then, because he didn't want to accept the Federal Radiation Council's recommendation [even though other members of the Interagency Committee did].

HARRELL: Had you made trips out to Livermore and Gofman's lab as part of the ACBM?

TOTTER: Yes, yes. As a matter of fact, when Dunham resigned[48] [in 1967] and I became chief, it was announced by Gofman—at the meeting at the Livermore Lab, the program directors meeting there—and I was out there quite a bit.

HARRELL: And had you ever thought of stopping funding for that lab or closing it?

TOTTER: [I] didn't want to stop funding for it. We didn't give them as much as they asked for, and we tried to squeeze out some of the work they were doing.

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

[48] Dunham left to take a position at the National Academy of Medicine.

I told Roger Batzel, who was then chief at Livermore, that I did not want Tamplin doing any responsible work, that I couldn't trust his judgment. And there were several million dollars involved in the whole laboratory.

And I didn't feel that Gofman had used good judgment either, and [I said] that I would like to see him replaced. But of course, he was an employee of the University of California. I couldn't fire him. I couldn't fire either one of them [(Gofman or Tamplin)] and probably wouldn't have anyway, as it's quite a difficult thing. But Batzel slowly got rid of both of them [by 1972].

Origins of AEC-Funded Research Programs

HARRELL: That brings to mind a question of how these various programs and experiments were originated and approved.

You mentioned that Shields Warren may have encouraged Oak Ridge to do cancer work, and Dunham decided to set up this lab in Livermore. How many of these programs came from Washington's directive, and how many came from the Lab's own desires?

TOTTER: Well, it's very difficult to assess the blame or praise for any of the—almost any of—these [programs]. Sometimes we were desperate for something to be set up, like plutonium toxicity in animals. So we asked somebody we knew who might be interested in it if they would be willing to set up that sort of [study]. And there were a few programs set up that way. Most of the individual [programs] came spontaneously from the individuals who were interested in radiation and thought they could use it for some good purpose. And if it looked like it was something useful to the AEC, then we supported it if we could.

FISHER: Were you more interested in supporting research from your division at the AEC [National] Laboratories or at nonaffiliated universities?

TOTTER: Well, it didn't really make much difference to us. If we wanted the research, we would take it anywhere we could get it, if we could find competent people to do it.

But the majority of the work was spontaneously developed at the [national] laboratories, and they would come to us to see, then, if we would like to support it. And if we could and if it looked like it might be [scientifically] profitable for us, we would support it.

But we always were criticized by other agencies that we didn't have a peer review system. But that's not correct. We had a peer review system and we used it. It just wasn't like *their* peer review system, because we didn't have formal meetings.

We sent proposals out to people either in or out of our program, to whoever we thought would be most knowledgeable about these things, to have them reviewed individually. Then [the outside] reviews and our internal review were put together. And my first job, when I went back

[to DBM] as Assistant Director [in 1963], was to chair the meetings to determine what research offsite would be supported.

Now, the onsite research usually came in the budget document, and that was reviewed separately. And probably, the onsite research got less peer review, except that it was reported in our program directors meetings and to a large audience, local and visitors, and was reviewed then, although that wasn't very formal. That was an informal meeting.

Advisory Committee on Isotopes for Human Use

HARRELL: In the early AEC days, there was the Advisory Committee on Isotopes for Human Use?

TOTTER: Yes.

HARRELL: Did they [have to] approve all experiments or requests for isotopes?

TOTTER: In my understanding. I had no direct contact with that committee.

HARRELL: Do you know when that committee ceased to exist?

TOTTER: No, I couldn't tell you.

HARRELL: And after that committee, there were various human use committees at individual labs and universities—

TOTTER: Yes.

HARRELL: —and yet those came into existence at different times.

TOTTER: Yes.

HARRELL: Was there ever any AEC program to review human experiments between the end of the Advisory Committee for Human Use and when the Human Use Committee started?

TOTTER: I don't think so, if I understand your question correctly. What we did at the program directors meetings, or the ACBM meetings, was to have people talk about [their research]. Then we would get a letter from the ACBM giving their assessment of the work.

HARRELL: So each experiment went through a process?

TOTTER: Well, the human ones always went through a very severe process. I didn't have a great deal to do with that, but I'm aware of some of the stuff that went on.

HARRELL: Did the AEC encourage individual institutions to set up their own review committees?

TOTTER: I can't now recall the details. We required them to meet whatever ethical standards were available and set up for either their state or for [the] national [level].

And the ACBM was very jealous about that. They were very careful to make sure that everything we did was ethical. Because there were always two or three or more M.D.s on the ACBM Committee.

The Division of Biology and Medicine's Research Goals; Bone Marrow Transplants at Oak Ridge

FISHER: As you served as Assistant Director and then Director for the Division of Biology and Medicine, what were the principal research objectives that this division wanted work done on? Was it radionuclide toxicity? Was it general radiation effects? Was it basic research and scientific—or basic research on mechanisms and radiation damage? Where were the major emphases placed?

TOTTER: Well, we had a whole string of those things. You can put them together. You enumerated some of them. But what we were interested in was effects and uses—

FISHER: —of radiation?

TOTTER: —unofficial uses and effects—yes—as a broad general view of the thing.

FISHER: So your office supported a number of research projects focusing on the development of radioactive materials for medical use—

TOTTER: Yes.

FISHER: —either in diagnosis or therapy of disease.

TOTTER: Yes.

FISHER: And can you tell us about some of the work that was conducted under your leadership?

TOTTER: Well, I can't give you very much detail, because my personal interests were not so much in that as in the basic sciences. But a lot of that went on right here, as you know.

FISHER: Were you a strong supporter [of] the ORINS[49] medical program?

TOTTER: Oh, yes.

FISHER: One of the things that has crossed my mind more than once: you mentioned that Dr. [E. Donnall] Thomas[50] was learning techniques here in Oak Ridge, techniques that would later prove highly successful in bone

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

[50] Dr. E. Donnall Thomas was awarded the Nobel Prize in medicine in 1990 for his pioneering work in bone marrow transplantation.

marrow transplantation in the treatment of leukemia. We understand that ORINS tried to develop a bone marrow transplantation program here which was never really very successful.[51] Do you know the reasons for that?

TOTTER: Well, maybe the reason that it was not successful is because we didn't support it with a large enough staff. But I think the—perhaps the alternate reason is that other things beside radiation began to show more promise as suppressive of the immunity—immune blocking agents.

FISHER: Such as various chemotherapeutic agents?

TOTTER: Yes, those. You see, they were not available when the radiation room [(LETBI)][52] was built here. The radiation was, at that time, the best way of blocking immune response. And so we supported that wholeheartedly. But when other things showed more promise than radiation, then we lost some of our interest in it.

FISHER: Now, part of the reason that cancer therapy was not as successful here as was hoped for was that the whole-body radiation was delivered at low-dose rate, but not at levels that would exceed what was then considered, what—more than about 50 percent of the whole-body lethal $LD_{50/30}$.[53] And so total body radiation doses that were administered here in general, were quite low compared to those being used today in the treatment of leukemia.

TOTTER: Yes, I think that they were. It was not easy to build a stronger one [(total body irradiator)] at that time. I don't know the details of that here. I supported the building of [the room] here, because I thought—I had been exposed to those kids with leukemia, too; and I thought that anything we could do was worthwhile. It turned out there are better ways.

FISHER: And it turns out that the [cancer treatment] program [at ORINS] was discontinued about two years after you left AEC. Do you remember the reasons for discontinuing?

TOTTER: I stayed as much out of that as I could—

FISHER: After you left?

TOTTER: After I left, yes. I didn't want to try to influence anybody, because I might be out-of-date, then, anyway [in my technical knowledge of the field].

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[51] For a discussion of the ORINS bone-marrow transplant research, see DOE/EH-0453, *Human Radiation Studies: Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

[52] the Low-Exposure-Rate Total Body Irradiator (LETBI)

[53] $LD_{50/30}$ is the dose at which 50 percent of humans, within 30 days, will die.

HARRELL: You mentioned that you were a supporter of—was it the LETBI[54] facility, the low-level irradiator?

TOTTER: Yes.

HARRELL: What was the theoretical basis behind building that?

TOTTER: Well, that was based on the [bone marrow] transplantation business and generally the way to suppress cell growth. You didn't really know what you might eventually get into, so you tried it. But you knew that there was a rationale for using it.

HARRELL: I understand, from talking to Dr. Vodopick,[55] that there weren't any animal studies on that particular amount of radiation to show its effectiveness.

TOTTER: That may be. I was not aware of that myself. I knew that there had been lots of animal studies of one sort or another; but maybe at that particular dosage rate, perhaps not. Although it would be pretty hard to find something that might have been missed.

(takes a book down from a shelf)

Here's something from Argonne. And the dose rates. You see, here—

FISHER: You've pulled down a—

TOTTER: —here is a set of exposure rates, going from nothing to 56 rads a day in six [days]—in, I guess, [judging from the modest slope of the curve,] logarithmic entries.

FISHER: What you've done is you've pulled down a book, *Delayed Effects of Radiation*, edited by Dr. Fry.

TOTTER: Yes.

FISHER: —Doug Grahn, Melvin Graham, and John Rust, *Proceedings of a Colloquium,* University of Chicago, May 1969. And on page 105, table 1 shows the mean survival [rates] and standard errors for exposure to radiation at different daily dose rates. It looks like these are all in mice.

TOTTER: Yes, mm-hmm. These are mice.

Well, it would be hard to have missed the area. Maybe precisely what this one did was not duplicated with animals, but had been bracketed by other studies.

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[54] For contrasting views on the medical ethics of the LETBI studies at Oak Ridge, see the oral history transcripts of Lushbaugh (DOE/EH-0453) and Karl Z. Morgan, Ph.D. (DOE/EH-0475, June 1995).

[55] Dr. Helen Vodopick, M.D., was the Senior Clinician in Oncology Research at the Oak Ridge Associated Universities Medical Division and participated in the treatment of patients with total-body irradiation and chemotherapy. See DOE/EH-0482, *Human Radiation Studies: Remembering the Early Years; Oral History of Oncologist Helen Vodopick, M.D.* (August 1995).

HARRELL: How early was NASA's[56] interest in studying the effects of low levels of radiation discussed?

TOTTER: NASA?

HARRELL: Yes.

The Military's Animal Research on High-Dose Radiation

TOTTER: Well, I don't know that they studied it very much. They used our people a lot for information. And of course, the people who really put a lot of effort on high doses, were the AFRR[I] group, the Armed Forces Radiation Research [Institute].

HARRELL: At Los Alamos?

TOTTER: No. That is in Bethesda, Maryland.

HARRELL: Oh.

TOTTER: Most—a lot of the work. But maybe some other places, but I was familiar with some of the [work] going on in Bethesda, Maryland, across from—or in back of the Naval Hospital[57]—

HARRELL: Yes.

TOTTER: —the laboratories in back of the Naval Hospital in Bethesda.

HARRELL: What kind of work did they do there?

TOTTER: Oh, high-dose radiation usually—mostly. They wanted [to know] the effects on flyers and operators at high doses of radiation.

HARRELL: On mice and?

TOTTER: Well, they used all kinds of animals.

HARRELL: Monkeys?

TOTTER: Monkeys.

HARRELL: They did a lot of work of that kind at Los Alamos, too, didn't they?

TOTTER: Yes, I think there was some there, too. But we generally—the AEC generally stayed out of that, because there was no real legitimate interest in that high a dose.

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[56] National Aeronautics and Space Administration. NASA sought to determine whether astronauts should be protected from the radiation flux in the Van Allen belts and from radiation in space in the event of a highly energetic stellar event (such as a supernova). Such exposures, NASA calculated, would amount to about 1.5 roentgens (R) per hour. Some LETBI patients would receive similar rates of exposure for days at a time, as astronauts might. Accordingly, NASA paid ORINS to report on the effects of such exposure on patients in order to develop techniques that could be used to diagnose whether an astronaut was developing radiation sickness. The funding led to charges that NASA was dictating the exposure rates that the LETBI staff administered to patients. See "NASA Support for LETBI Research" in the Vodopick transcript (DOE/EH-0482, August 1995), and "NASA-Sponsored Studies" and "Questioning the Propriety of NASA-Funded Studies" in the Lushbaugh transcript (DOE/EH-0453, April 1995).

[57] now the National Naval Medical Center, Bethesda, Maryland

HARRELL: What kind of work were they doing at the Naval Radiological Defense Lab?

TOTTER: They did some basic work on it [(high-dose radiation)], too, which is all—the only part that I was interested in, so I can't tell you much about the rest of it. There was some jealously between the people working at the various places like that. But they tried generally to stay out of each other's way.

The AEC's Involvement in International Research

HARRELL: You mentioned a lot of AEC involvement with South American countries.

TOTTER: Well, really countries all over the world.

HARRELL: Right.

TOTTER: This was just part of it, yes.

HARRELL: What other countries was the AEC most involved with?

TOTTER: Well, I had no personal connection with any of that except where I went in South America—Uruguay. But I know that they had an active group in the AEC that was conducting negotiations with many other countries in the world. And I don't—I can't tell you what the—

FISHER: [Much of the early nuclear medicine research took place at ORINS] while you were director of the Division of Biology and Medicine. So, I'm real pleased that we could have this.

TOTTER: Well, I'm sure glad to hear it, because I didn't realize it then—how basic it turned out to be. I really didn't.

FISHER: And radiation therapy is a very important part of it. Radionuclides are still very important for diagnostic nuclear medicine—becoming increasingly so. And you know that—was it last year or the year before—in 1993, strontium-89 chloride was approved for use in this country—

TOTTER: Strontium-89?

FISHER: —for bone pain and the treatment of metastatic[58] prostate[59] cancer [in bone].

TOTTER: Well, I've got a friend dying from that over in the place where I live now.

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[58] relating to metastasis, the spread of disease-producing organisms or of malignant or cancerous cells to other parts of the body by way of the blood or lymphatic vessels or membranous surfaces; or, the condition so produced

[59] a partly muscular gland that surrounds the urethra in males at the base of the bladder and secretes an alkaline fluid that makes up part of the semen

FISHER: When—we've talked a little about some of your research priorities when you were Director of the Division of Biology and Medicine. This included both animal research and some human studies.

The AEC's Environmental and Ecological Research

TOTTER: Yes. I didn't mention another important segment of that, the ecological studies.

FISHER: Ecological and environmental.

TOTTER: Environmental and ecological. I think that one of the reasons Dr. Dunham wanted me back there [to the AEC and its Division of Biology and Medicine] was that I had good rapport with the ecologist, the only ecologist they had at that time. And he was the one that really built the beginning of the basic ecology program, the first the AEC had, [except for] ongoing practical ecological studies at two or three of the major Labs.

John Wolfe was the person who came from Ohio State [University in Columbus] to head up the beginning ecology program. And he built it. And eventually it became a branch, Environmental Sciences Branch. It had four or five ecology people there by the time I left.

Somehow, the ecologists were kind of looked down on by a lot of the more basic[-science-]type people. And he felt he could communicate with me, when he couldn't with some of the others. So, he worked on Dr. Dunham, I think, to bring me back to the outfit. And under John Wolfe's aegis, the ecology program grew quite well there. And he had a big part, I think, in the International Biological Year through those ecological studies.

HARRELL: What were the major sites of the ecological work?

TOTTER: Well, this was one [(Oak Ridge National Laboratory)]. Savannah River[60] had a strong ecology [program], based at the University of Georgia. Eugene Odum was there, and he had a group always on the Savannah River Site.

This, too, is a large site, and so it was easy to inaugurate a pretty good-sized program here, have a watershed program and that sort of thing, tracing everything that came down and everything that washed down and everything that came from the soil. [Stanley Auerbach created the large ecological program here.] There were also quite a bit [of activity] at Hanford[61] and [at the Nevada Test Site led by UCLA scientists].

HARRELL: Are you aware of studies conducted in Alaska? I know they surveyed Eskimos there and cesium fallout above the Arctic Circle.

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[60] a Department of Energy weapons site in Aiken, South Carolina, that, during the Cold War, was the major source of tritium and plutonium for atomic bombs

[61] Hanford Site is the Department of Energy's 570-square-mile former nuclear weapons complex near Richland, Washington.

TOTTER: Yes. Yes, I was concerned with those quite a lot, because I had experience in Alaska, and I knew something about the Eskimos. I lived with them for a while. The Russian fallout came into Alaska pretty badly, and we had to have studies up there. But they were based at Hanford, the people that went while I was there.[62] I don't know, you're perhaps thinking of something later than that, or?

HARRELL: Well, I know they did a long-term cesium study on the Eskimos.

Suspension of Proposed Plowshare Projects (Circa 1963)

TOTTER: Well, when I came back [to the AEC in 1962, the Commission] was urgently trying to [promote], the Plowshare Program; you know what that was. They were trying to dig a harbor at Cape Thompson in Alaska. And I think I'm responsible for them not canceling that shot [(nuclear detonation)]; although it has never been shot. They postponed it after I argued with them that they should not cancel it out-of-hand.

HARRELL: What were some of your arguments?

TOTTER: Well, I knew that one of the problems [that] the natives there bitterly resented having was a high lighterage[63] fee for unloading shipments from the United States, from other places. Forty percent of the [freight charge] was the cost of lighterage. And they had to keep the big lighter boats for transshipment through the winters and so forth there. And they felt that the Boston financiers were getting rich off of them. And I felt that [the natives would] benefit very strongly from a harbor anywhere up in the northwest part of Alaska, and that's a perfectly good place for it.

FISHER: Was it really the desire to put in a harbor, or was it the desire to demonstrate the harbor-carving power of atomic energy?

TOTTER: I think the latter, as much as the former.

FISHER: You said that you were responsible for postponing or delaying—was it Project Chariot?

TOTTER: Chariot, yes.

FISHER: Up at Cape Thompson?

TOTTER: Yes, Cape Thompson.

FISHER: Or Point Hope, Alaska?

TOTTER: Well, not Point Hope. Kivalina is the closest [settlement].

FISHER: Okay.

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[62] For a firsthand account of Hanford's detection of Soviet atmospheric testing in 1946, see "Monitoring Successfully Detects the Soviets' Entry Into the Nuclear Age" in DOE/EH-0455, *Human Radiation Studies: Remembering the Early Years; Oral History of John W. Healy* (May 1995).

[63] Where harbor facilities for docking a large ship are inadequate or nonexistent, cargo must be transshipped to and from shore by means of smaller, often bargelike, shallow draft vessels (hence, "lighter").

TOTTER: I had stopped at Kivalina.

FISHER: Was your concern for the contamination of the Alaskan environment?

TOTTER: Well, we were all concerned about that, but we felt that could be controlled. There were other people who felt it couldn't, so, it was a [debate]. I thought the argument was spurious, and so I didn't feel that there was enough against it to stop it.

FISHER: So what was the main reason, then, that you argued against it?

TOTTER: Well, because I thought it—as I argued against canceling it.

FISHER: Against canceling it?

TOTTER: Yes, that's what I argued, against canceling it. See, the [Atomic Energy] Commission met, and I had just gotten back. I hadn't really gotten settled-in very much, when they decided to cancel that shot. And when I heard about it, I went and argued, as I have now, that it would help the Eskimos.

FISHER: I see. I misunderstood that. That would have been in 1963, wasn't it?

TOTTER: Yes, about then.

HARRELL: So what kinds of meetings did they have to plan this shot? Was it a meeting of division heads?

TOTTER: Well, we had lots of small meetings, yes, concerning that, that the Plowshare [Program] wasn't getting along very well. I mean, they weren't making much headway. But I thought [the harbor] was a very splendid idea.

They had a chance of digging a harbor in Australia, too, because [the Australians] decided to develop the big iron deposits on the northwest coast. And they needed a harbor, so they were actively interested in it.

And then a little later, of course, they talked about a sea-level canal to help out the congestion in the Panama Canal. And I was put on the safety committee of that one.

I hadn't been acquainted with the Alaska one, but nobody knew I had a particular interest in Alaska, having spent some time up there. And when they found out, they invited me to the meeting at which they planned to cancel it, just a *pro forma* meeting of the Commission. And they gave me a chance to talk[, figuring I'd support their decision]. And so we'll see where it's at. "Well, let's talk, and so just postpone it." And that's what they did.

HARRELL: Indefinitely?

TOTTER: Indefinitely.

(laughter)

HARRELL: Were you aware at that time of similar work that the Russians may have been doing?

TOTTER: Well, I was vaguely aware of some of that. [(AEC colleagues in the Intelligence Division] talked about it, but we didn't seem to know much about—

HARRELL: Right.

TOTTER: —what was going on.

HARRELL: Right. Those [Soviet nuclear explosions] were just detected, and people didn't know what they were for? Or was there—

TOTTER: My memory is very vague about it, but I do remember hearing about something.

HARRELL: So, did the Safety Committee ever present any objections to any of the Plowshare projects?

TOTTER: Well, the sea-level canal was stopped on the basis of safety. The committee that I was on that had to do with fallout thought that we could handle the fallout. But the shock, the ground shock, couldn't be handled. And that stopped it.

HARRELL: Would that have caused earthquakes or—

TOTTER: [There was] a city of 40,000 within 40 miles, and I think that was too close. It would have leveled the town, probably.

So, it might go now, because they have probably fixed it so they can set off smaller explosions. So no one of them would be big enough to hurt.

AEC Program Approval Coordination

HARRELL: When you were Director of the Division of Biology and Medicine, did you participate in regular meetings with the other division heads to discuss projects and funding?

TOTTER: Yeah, I don't know how regular the meetings [were], but they had them pretty frequently, if there was some special announcement or . . . But most of the meetings we only went to if we were concerned with—only the division heads who were concerned met, not the whole group, as a rule. There were meetings of the whole group once in a while, but not regularly.

HARRELL: And for your programs in the Division of Biology and Medicine, would you submit a proposal to the [AEC] Commissioner, or how would you get your things that you wanted done?

TOTTER: Well, it was fixed so you rarely had to go to the Commission. And if it was really something out of the ordinary, you would go to the Commission. But the normal routine in the operation went on with just budget meetings. And if the budget was handled, then I could sign, as responsible agent, for expenditure of money through the normal operation. When something extraordinary came along, then we went to the Commission.

HARRELL: So for normal programs, did you have authority to approve whatever you felt necessary?

TOTTER: Yes. Of course, there was a routine that had to be gone through. And if it concerned any other division, you had to circulate it to the other division. And sometimes that happened. We sometimes got mixed up with operations that—the Research Division, for instance, had certain rules and regulations about secrecy for certain kinds of isotopes. And if we did—inadvertently—pick up a program which had involved one of these secret isotopes and we didn't know it, then we were in trouble unless we went through the other division. And they—

HARRELL: But how would you even know there was a rule unless—

TOTTER: Well, sometimes you didn't.

HARRELL: Then that came out later—

TOTTER: Yeah.

HARRELL: —that you had violated some rule?

TOTTER: Now, iodine, what is it, iodine-128, -129, iodine-131? What is the one that's so top-secret, among the iodines? Is it -129 or -128?

FISHER: Well, iodine-129 is very long-lived—

TOTTER: 17 million years.

FISHER: —and continues to accumulate in the environment.

TOTTER: And that's the one. That's the one. That was the topmost secret—I never knew it even [existed]—because you could calculate how much fission was going on [during detonation of a nuclear weapon if you knew the rate of change of that isotope].

FISHER: For fission yields.

TOTTER: Well, there are a few [telltale isotopes] like that, that we had to check with other divisions. Normally we handled [approval for research programs] through a regular process which was credit-approved in advance by the Commission.

HARRELL: And other divisions checked with you for their proposals?

TOTTER: Yes.

Fishing (for Foreign Secrets) Where the Ducks Are

HARRELL: Do you know what kind of work the Intelligence Division was doing, to get back to that?

TOTTER: Well, that brings up some fine memories, but they're not appropriate here. No, I didn't know what they were doing.

HARRELL: There seems to be a lack of documentation.

TOTTER: The only thing that we had any connection with [the Intelligence Division for] was getting soil samples from countries. And we did—we managed that through the [United States] embassies [overseas]. We'd dig it up in [our embassies'] backyards.[64]

HARRELL: Oh, really?

TOTTER: Yes, in getting fallout samples. And we were [concerned then] with Intelligence. That's the only thing that touched us.

HARRELL: And they would ship this soil back in a diplomatic bag?

TOTTER: Yes. I don't know whether those things are still secret or not. I don't know whether you're supposed to talk about them—[activities] like catching ducks, seeing what's on their legs.

HARRELL: Mm-hmm.

TOTTER: And that was top-secret, too, that sort of stuff. It's amazing to think that anybody would make that secret. Almost every hunter knows where the ducks come from.

(laughter)

And somebody, quite seriously, from MIT[65] came about 10 years ago to me and asked me. He had a wonderful idea, and it turned out that this was something I had known about that was top-secret for years. So he was quite disappointed when I told him, "Well, sure, they were [already] doing that."

(laughter)

Radiation Research on Penitentiary Inmates in Washington and Oregon (1963–73)

FISHER: That had been already thought of. You were one who would review proposals for their suitability for funding?

TOTTER: [Yes, with others].

FISHER: You would also review the progress of work that was conducted?

TOTTER: Yes.

FISHER: You conducted many program reviews. We're interested in some of the policies concerning the experimentation on human subjects. This was before—well, you served as assistant director of the division before there were formal policies for human subjects committees, and you also served as director of the division after human subject committees were formally established. Do you remember this process?

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[64] Under international law, a diplomatic embassy is the sovereign soil of the nation being represented (in this case, the United States). Similarly, diplomatic pouches are immune from inspection by the host country.

[65] Massachusetts Institute of Technology, Cambridge, Massachusetts

TOTTER: Yes, I don't remember exactly when they were established, but—

FISHER: Do you remember this coming up?

TOTTER: Oh, you mean, in connection with the human experiments?

FISHER: Yes.

TOTTER: Yes. The one that I was most familiar with was, of course, their [ir]radiation of the prisoners in the Northwest. What is it, Washington or Oregon?

FISHER: Well, there were two studies, one in Washington State[66] and one in the State of Oregon.[67] These were taking place while you were—

TOTTER: Well, they were being started. I can't remember exactly when they were started—

HARRELL: About '63.

TOTTER: —but I was involved with some of the negotiations there.

FISHER: Can you tell us about this, in your recollections?

TOTTER: Well, I do remember that the ACBM took a great interest in it, and they were very anxious. So was Dr. Dunham. I think it started under him. And then I—

FISHER: To support this work?

TOTTER: Yeah, we wanted to support it, but we were awful cautious about it, because we didn't think that could be done the way the person planned to do it. I mean, not scientifically—but for ethical reasons, we didn't think it could be done, but [the researchers] insisted it could.

And I know the ACBM [advisors] were very ticklish about it. And finally it seemed to be okay from the reports we got, that they had cleared it with everybody.

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

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

They told us that the prisoners asked to be put on the program, and they knew that they would be sterilized [surgically afterward] if they were irradiated. But the researchers [planned to use only] the ones who wanted to be sterilized.

HARRELL: Did the idea for this kind of research come from an overexposure at Hanford? I heard that at one time.

TOTTER: No. I guess the background of it, as I see it, was that we had understood the progression of cell development in the testicles of mice—work that was done here in the [ORNL] Biology Division—very well, so that when you irradiated them, you knew exactly what stage—what time to look for what stage—of irradiation at the time, what stage the cell was in at the time of irradiation.

But it wasn't known, for humans, whether they behaved in the same way as mice. Mice have a coordinated development as—the germinal cells before they are in their last division—spermatogonia, I guess—a spermatogonium.

In mice, the whole testicle works simultaneously. In humans, apparently it doesn't, but that wasn't known at the time, and that's what we wanted to find out so we could tell what the probabilities of mutation were.

But in humans, it turns out that apparently there are areas which are in simultaneous synchrony, but other areas are in a different outfit. So you have a mixed population in the whole area.

But it was to try to elucidate that that seemed very important information for understanding the genetic effects.

FISHER: What were the major ethical concerns expressed against these projects?

TOTTER: Well, I don't know how to answer that in words. It's just that they shied away from doing something on humans that they thought people might object to.

And we found that the [political] atmosphere changed very rapidly about that, because at the time when the fellow started these experiments, there seemed to be no strong aversion to the fact that the experiments were given—[it was accepted] that the people were fully informed and knew what they were getting into. But when [the researchers] tried to have the slides read by some University of California pathologists, [the pathologists] wouldn't touch them, because [the slides] hadn't passed their human [use] committee operation.

HARRELL: What year was that?

TOTTER: Well, it's hard to remember exactly, but it was about the time I left the AEC, or maybe shortly after that, that they had the setback, as far as getting the slides read.

FISHER: These were testicular biopsies.

TOTTER: Yes.

HARRELL: Were there concerns because the radiation was too much—

TOTTER: Well, I—

HARRELL: —or permanent effects?

TOTTER: No, no. I think they were afraid of the reaction if they collaborated in the study. Because there was, by that time, a pretty big hullabaloo about informed consent. And since their own committee had not acted on this—and, of course, there was no reason they should have—they just were not going to touch it.

FISHER: How was that resolved?

TOTTER: I don't know. I lost track of it after that.

FISHER: And this was after the AEC had adopted the NIH guidelines for protection?[68]

TOTTER: Well, their committee was formed after we had started the work there, yes. And I think all the other committees were formed after. You mentioned the timing; I don't know exactly when it was.

HARRELL: That was in 1970 when you adopted those guidelines?

TOTTER: It was about then. I don't know exactly when it was. But—

HARRELL: Was there concern over the use of prisoners as subjects?

TOTTER: Yes. I think—well, there was that—there was that concern. That concerned all of us, because we wanted to be sure that they really freely elected to enter the study, and we were assured that they had.

HARRELL: Prisoners had been used before, though, for AEC experiments?

TOTTER: I don't—I don't know.

HARRELL: I think there were some in [the Federal prison in] San Quentin[, California].

TOTTER: There may have been, but I don't know. I don't know.

HARRELL: One of the interpretations of the use of human subjects for radiation research has been in the popular media, that the Government pushed this, that it was a coordinated Government program, as opposed to a collection of small research projects proposed by individual investigators. Was the prisoner study in the State of Washington and Oregon—was that project developed within the AEC, or was it brought to the AEC as a proposal?

TOTTER: It was brought to the AEC. We never heard of it before they came and asked for support.

FISHER: Was it an unsolicited proposal?

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

TOTTER: Unsolicited. Completely unsolicited.

FISHER: There were two. One was from a Dr. [Carl G.] Heller[69] in Oregon.

TOTTER: That's the one that I know most about.

FISHER: There was another one from Dr. [C. Alvin] Paulsen[70] at the University of Washington.

TOTTER: Well, Heller is the one I remember. I don't remember the other one.

FISHER: So these were unsolicited proposals from well-established medical schools—

TOTTER: Yes.

FISHER: —to do research on the response of gonads to penetrating radiation. What were some of the advantages that you saw in doing this research? How was this to benefit the Atomic Energy Commission?

TOTTER: Well, I think it's largely a part of unraveling your real genetic danger from radiation. And I don't know whether you remember that far back or not, but the early—in the early days of atomic exposure, radiation exposures, the greatest danger was considered to be from genetic [mutations].

But later on, the fear of cancer overshadowed the fear of mutations. And the present feeling is that they thought it was cancer, and that is the one thing to be feared. So, we were in-between these two stages when [the proposal for the prisoner experiment] came along. And the mouse studies had shown beautifully what you could do in detecting the danger—genetic danger from it. We would like to get as good a fix on human genetic danger as we had on the mouse.

FISHER: Now, the prisoner studies at Washington and Oregon focused mainly on high-dose—high-dose-rate exposures to testicles. But they didn't focus on the low-dose-rate, long-term effects of gamma radiation. Was there a similar program to look at low-dose rate?

TOTTER: Not that I'm aware of.

FISHER: What are your feelings now, some 30 years later, about these experiments? And as you think back on your service as—or your tenure as director of the division, what are some of your feelings now about these projects?

TOTTER: I don't think they have changed much. I think if those experiments are carefully done and the people are willing or eager, as we were told—

FISHER: —to participate.

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69 principal investigator for the Oregon prisoner studies

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

TOTTER: —to participate in the experiments, then that is information that would be useful. It's very difficult to tell—

I think if the opportunity were offered again, I would not hesitate, provided all the safeguards are clearly present.

FISHER: What would those safeguards be?

TOTTER: Well, I think—I think you must guard very carefully not to entice people into participation in an experiment. It's a little like an autopsy. When I think of myself being autopsied, I don't have any objection to that. But if I think of my wife being autopsied, then I do object to that. I think this is somewhat the same kind of [feeling] these people have about those experiments.

HARRELL: Do you think the money involved [(the payment the prisoners received)] was an enticement?

TOTTER: Well, obviously, there is some enticement in the benefits they got. They figured they wouldn't do it without some benefit. But according to what we were told, they had psychological help. They had—

FISHER: There were some counseling sessions.

TOTTER: Well, yes, and that they were very carefully informed of all of this.

FISHER: And even the wives consented along with their husbands.

TOTTER: I was not aware of that, but I think it should have been, yes.

FISHER: Was there a—did the agency have a review committee at that time?

TOTTER: Specifically for that?

FISHER: Yes.

TOTTER: I don't think so. We presented that to the ACBM, and they were very cautious about it.

FISHER: When you say "cautious," [do you mean] guarded?

TOTTER: Guarded. Guarded. They wanted to be sure that they were assured that the prisoners were not being coerced.

FISHER: Maybe "guarded" isn't the right term.

TOTTER: Well, it's satisfactory—one that might not be precisely the right one.

HARRELL: And you had several reviews during the course of that program with visits to—?

TOTTER: Yes. And there were visits there. I never made one, but some of my colleagues did.

FISHER: Was this project assigned to someone in your division to oversee?

TOTTER: Yes. It was in the Medical Branch. And I can't remember the name of the person who was head of the branch at that time. He was an old Army man, [*Totter adds later:* Joseph Goldstein, a physician].

FISHER: Not Howland?

TOTTER: No. Not Howland. But Dave [(H.D.)] Bruner[, also an M.D.,] had been the head of the Medical Branch, and then Bill Burr was. But I think—under part of that, Bill Burr was probably head. But he was my deputy director after the second year, I think.

FISHER: Who was your deputy director?

TOTTER: Bill Burr. And he's here now[, still living in Oak Ridge]. You know him?

FISHER: Sure, mm-hmm—met him several times.

TOTTER: And—see, I had—it scares all those names out of my head.

FISHER: Was there concern expressed at the time by members of the Advisory Committee about potential long-term, late effects of this experiment?

TOTTER: No. They didn't—there was no reason to be concerned about that, because the people who agreed to it, agreed to be sterilized.

FISHER: Yes. But what about—

TOTTER: For personal.

FISHER: —somatic effects,[71] cancer—long-term cancer?

TOTTER: No, I don't believe they had any concern about that, because [the committee members were] physicians—some of them. They were physicians, and they had irradiated cancers with 2,000 rads or something more on an arm, or a leg, or breasts. So, they were not really concerned about it.

FISHER: These inmates were of all ages, weren't they? Or were they primarily young—young prisoners?

TOTTER: That I can't say. I don't know what the spread of age was.

HARRELL: There was a proposed neutron experiment as part of this work that was approved by the AEC, but [which ultimately] the University of Washington decided not to do.

TOTTER: Yes, I—I have a vague recollection of that, but not—I don't know whether it happened while I was there or later.

HARRELL: So you don't know anything about why they wouldn't want to go ahead with that part of it?

TOTTER: No. I think they were feeling the heat, the change in attitude of people about it—those things. I don't think you could get anywhere near something—a program like that nowadays. It's because of the change in feelings [held by the general public about radiation].

HARRELL: And what were the results of the program?

TOTTER: I don't know. I was gone—

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[71] affecting somatic cells—any cells of the body that are not sexually reproductive cells

HARRELL: You were gone.

TOTTER: —before the results came in, and I have not been briefed on them.

FISHER: One of the interesting results of those studies was that sperm production was—the sterility was a short-term phenomenon—

TOTTER: Yes, it was, mm-hmm.

FISHER: —and that sperm production resumed in these patients later on. It wasn't a permanent sterility.

Pre-World War II, Nongovernmental Radiation Research

TOTTER: There's an interesting sideline on this. There was a physician in New York who irradiated a lot of women to prevent conception. And those data—he kept very careful data on it. He died, left the data with his widow, and the AEC tried to get that information from her, but she wanted an enormous price for it. And they were never willing to pay for it. So as far as I know, that data has never come to light.

FISHER: That's interesting.

HARRELL: Do you know the name of that doctor?

TOTTER: No. I wouldn't now remember it.

HARRELL: Do you know when he did this work, in the '50s or '60s?

TOTTER: Well, I heard about it in the early '60s, but he was already dead then. So it must have been done in the '30s and '40s.

HARRELL: Wow.

TOTTER: Well, you know, they did a lot of things with radiation back then we wouldn't even think of doing now. Have you, either of you, happened to see the museum, the Radiation Museum, in another part of this institute? You've seen it. And it scares you, doesn't it, what people were willing to do.

But I saw that in South America. People who came to the beach, which was known to be quite radioactive, and they rolled in the sand and piled it up over their joints and so forth. That was just natural. And they claimed it helps them.

HARRELL: —natural radiation?

TOTTER: Natural radiation, yes. Black sands.

Medical Follow-Up on Occupational Radiation Exposure

HARRELL: While you were director of the Division, Pat Durbin[72] was doing a search for survivors of the original plutonium injection series. And this must have caught your attention to some—

TOTTER: It did.

FISHER: —high degree at that time. Can you recall this process and the currents of events?

TOTTER: Oh, I can't recall enough of it, no. I was aware of her search and so forth. But I was more interested in the group from Los Alamos, which was being followed by the Medical Division there. And, of course, Chet Richmond[73] was in the AEC's Division [of Biology and Medicine] while I was there. He had a hand in some of that, too. And you will see him tomorrow, is that it? He will know much more about it than I do.

FISHER: That was a group of primarily occupationally exposed workers—

TOTTER: Yes.

Follow-Up of Subjects From Plutonium Injection Experiments

FISHER: —at Los Alamos. And you were interested in the long-term follow-up of people who had been occupationally exposed to plutonium. But at the same time, there was this other earlier population of non-occupationally exposed people who were hospital patients who were injected with plutonium. Did you initiate any reviews of the original plutonium injection experiment?

TOTTER: No, I didn't. I don't think it ever—ever occurred to me. Or the question was not raised with me about those people. And so I spent very little attention on it. I was aware that work was being done, and that was all I really cared about, to make sure they weren't lost [historically], because it should be followed.

HARRELL: How widely was that population known about?

TOTTER: Not very widely.

HARRELL: Not at all? It wasn't discussed as—

TOTTER: No. The occupationally exposed ones were the ones that we were concerned about. I think it must have been that people wanted to forget that and never talked about it—

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[72] From 1951 to 1977, Durbin worked as a chemist and radiobiologist at the Crocker Laboratory of the Lawrence Radiation Laboratory (Lawrence Berkeley Laboratory). For the transcript of the November 11, 1994 interview with Durbin, see DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (June 1995).

[73] Richmond, a Los Alamos researcher, was on loan to the AEC from 1969 to 1971. For the transcript of the January 24, 1995 interview with Richmond, see DOE/EH-0477, *Human Radiation Studies: Remembering the Early Years; Oral History of Radiobiologist Chet Richmond, Ph.D.* (August 1995).

FISHER: Of course, that worker study continues, and Dr. Voelz[74] and his collaborators, have just published a very interesting follow-up, again, in the December 1994 issue of *Health Physics*.

TOTTER: *Health Physics*? I must get that, because I always maintain an interest in those. I would like to see how they are now, because we all predicted, of course—wanted to predict—what was going to happen [to those exposed workers] and see how far off we are [from our estimates of long-term outcome].

Low-Level Radiation and the "Hot Particle" Controversy

And, of course, those two characters there [(Gofman and Tamplin)] made much of a hullabaloo about that and how bad it was going to be. And the question arose about plutonium in the dust and how much we breathe in from the ground. And that was a great concern to everybody at that time.

FISHER: You're referring to the hot-particle[75] controversy?

TOTTER: Yes, the hot particles. And, of course, somewhere during my stay there, there was an aborted rocket [which put] plutonium in the atmosphere, and there was a big argument about that.[76]

FISHER: Was there a follow-up study of the plutonium contamination from that explosion?

TOTTER: Oh, I don't really know. I know we kept track of it in the atmosphere, as it fell down, pretty closely. But it faded away, and I don't remember what happened after that.

Support for Animal Studies

FISHER: You supported animals studies on radionuclide toxicology[77] in a number of institutions—

TOTTER: Yes.

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[74] See DOE/EH-0454, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. George Voelz, M.D.* (May 1995)

[75] multiatom particulates of radioactive material that emits many alpha or beta particles

[76] In 1964, a U.S. Navy *Transit* navigation satellite failed to reach orbit and disintegrated in the atmosphere. The satellite received its electrical power from a 4.5-pound, grapefruit-sized radiothermal generator that produced energy from the heat of its decaying radioisotopes. The device, known as a SNAP or System for Nuclear Auxiliary Power, disintegrated, scattering plutonium particles in the atmosphere over the southern hemisphere. Today, plutonium-238 is used as a thermal source to keep instruments warm in outer space where it is very cold, such as on the *Galileo* space voyager.

[77] study of the effects an detection of poisons; in this case, the study of the hazardous effects of internal radioisotopes

FISHER: —including [radionuclide toxicology studies] at the University of Utah, Battelle[78] at Hanford, Lovelace ITRI[79] in New Mexico, probably at the University of California at Davis; Argonne—

TOTTER: Argonne, the dog studies.

FISHER: Dog studies. And these studies were quite active during your tenure as director of the division.

TOTTER: Yes, they were.

FISHER: You were a strong supporter of animal research.

TOTTER: Yes.

FISHER: And someone once told me that it has gone downhill ever since.

TOTTER: So I understand. I understand the dog project was aborted just a year before it would have paid off handsomely. And I don't understand why that happened.

FISHER: Well, of course, there have been changes in administration, and office directors have changed over the time. But this was an era when much was learned about the toxicity of radionuclides, the metabolism of radionuclides, their uptake,[80] retention, distribution, and excretion. Can you remember some of the more important scientific contributions of that period?

TOTTER: Well, I have a particular interest in one at the University of Utah, which was—of course, none of those projects have gone as far—enough to draw final conclusions for them. So—but who was the director there, at Utah, in the early days?

FISHER: Well, Dr. Dougherty.

TOTTER: Dougherty. That's the one I'm thinking of.

FISHER: Thomas Dougherty.

TOTTER: He was an expert in the hormonal interactions and so forth. And he was the one that proposed that the white cell drop was a consequence of shock, rather than direct effect of radiation, a theory that was never followed up. And I would very much like to have seen it followed up, because I think that they underestimated the importance of hormonal influences. Because too much was attributed to the direct effect of radiation, the direct or indirect, but too little to the reaction of the body—to the hormonal system from the radiation.

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

[79] Lovelace Inhalation Toxicology Research Institute, Albuquerque

[80] an excess assimilation of radioiodine in the thyroid, indicating abnormality

Early and Recent Research Into Indirect Effects of Radiation and Cell Repair Mechanisms

FISHER: You always had a big interest in the indirect effects of radiation—

TOTTER: Yes.

FISHER: —and the importance of free radicals[81]—

TOTTER: Yes.

FISHER: —on the biochemistry of living systems. Did you support research in this area, also?

TOTTER: There was some—yes, quite a little, in fact. And I think it made very great progress. Of course, Dr. Denham Harman, who worked some with—at the University of California with [Melvin Calvin]. But he has gone into the aging effects of radiation at great length and studied aging. A little of that work was done here [at ORNL], very early studies, showing that oxygen was toxic. And at Johns Hopkins, the doctors there were producing mutations [just by exposure to oxygen]. And I feel that has not been adequately pursued, perhaps because there were more interesting things that people could follow. But it seems to be paying off now, widely believed now. And I can show you the latest I have had to say about that. They're here.

(pulls out a letter)

This is a little letter chiding people for misclassifying things as antioxidants when they are, in fact, something else.

FISHER: You've pulled out for us a letter to the editor of *Science*, which appeared in the October 7th issue, 1994. This is one, in fact, that I do remember reading, now that I look it over again, because of my interest in antioxidants and especially the work of Dr. Ames.

TOTTER: Bruce Ames.

FISHER: Yes, Bruce Ames, in this area.

TOTTER: Yes. And Dr. Ames is particularly interesting in this field. The Atomic Energy Commission supported him when he couldn't get support anywhere else. And he has always been grateful for getting a good start—

FISHER: I can imagine—

TOTTER: —on some of this work that wouldn't have been done had it not been for this AEC support. And he's doing marvelously well, I think, for first developing a test for mutational action of chemicals,[82] and then showing that [the] mutational effect from natural products is ubiquitous. And so, I think, [he] debunk[ed] the idea that the little bit of some mutational

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[81] molecular fragments that have one or more unpaired electrons and are therefore highly reactive, being capable of causing rapidly oxidizing reactions that destabilize other molecules

[82] the "Ames Test"

agents that we might get from the environment is not going to have much effect because it's overwhelmed by the natural ones we get all of the time.

FISHER: That's really the point that oxidation from the consumption of normal food—

TOTTER: Yes.

FISHER: —is much greater at the cellular level than all of the environmental insults—

TOTTER: —that we get.

FISHER: —from radiation or chemicals. And that those effects can be neutralized, to some degree, by antioxidants within our diet.

TOTTER: Yes. But we must not forget what Claude Bernard said about the constancy of the cells' milieu. It's very difficult to raise the antioxidant level above what the cell feels comfortable with.

FISHER: And *Science* this year has been highlighting the important mechanisms of DNA repair—

TOTTER: Yes. There was a big issue on that just recently.

FISHER: —which overcomes the normal effects of oxidation on DNA. It seems like the cell has a marvelous ability to repair DNA damage—almost flawless.

TOTTER: Much more fantastic then we ever dreamed.

HARRELL: So you would doubt the effectiveness of these vitamin supplements?

TOTTER: I do doubt the effectiveness. If you're low on the vitamin, sure, they're going to be effective. But if you're not low on them, you can't raise your level very much higher. And they're just excreted as fast as you take them in.

Ethics of Government Radiation Research

FISHER: I'd like to come back to a subject we've already talked about. And it may sound like I'm beating a dead horse to death again. But I'm quite amused at the popular press, which has written extensively that human radiation experiments were coordinated by and conducted by the U.S. Government—almost in a Naziesque fashion.

You served as Director of the Division of Biology and Medicine during a period of time when nuclear medicine was in its infancy, when new isotopes, were—applications for those isotopes were being developed;

when brachytherapy[83] was being developed; cobalt teletherapy[84] —as you've already mentioned, some high-voltage therapies were being developed. And you were looking for a lot of applications of radiation in the healing arts for both diagnostic and therapeutic purposes.

But how would you counter some of the arguments that it was the Federal Government experimenting on people in an unethical manner, that it was directed by the Federal Government and not the result of [normal research proposed by] brilliant scientists in the field?

TOTTER: Well, if I can answer that a little facetiously at first—and before I get serious—I think that the critics of the Government are implying a much greater degree of organization in the Government than really exists. It would be very difficult to get together three agencies to do some kind of nefarious operation. It's hard to get two agencies together to do something good. But the idea that various agencies collaborated in doing something which was not quite proper is kind of laughable. It's an impossible situation.

Research Interests of Commissioners Seaborg and Schlesinger Compared

FISHER: Who was the Commissioner of the AEC during this period of time? And who did you interact with?

TOTTER: Well, Glenn [T.] Seaborg,[85] for most of the time that I was there. My first tour at AEC was with Lewis Strauss.

FISHER: Okay.

TOTTER: And then he left at the same time I did. And Glenn [T.] Seaborg was there when I came back, and he left just a year and a half before I left. And James Schlesinger succeeded him.

FISHER: Was James Schlesinger less supportive of human research than Dr. Seaborg?

TOTTER: I don't think so. I think he was not very interested in it. I don't think he was at all critical of what we were doing, but he didn't take a great interest in it.

He did have a little biological interest. He was a birdwatcher. And one of our ecologists used to watch birds with him early in the morning.

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83 placement of sealed radiation sources into cavities of the body for treatment of cancer, such as uterine cancer; these sealed sources are later removed when treatment is completed.

84 radiation treatment in which the radiation source is located outside the body

85 U.S. chemist, born 1912. A professor of Chemistry at the University of California, Berkeley, Seaborg discovered plutonium in 1940 and went on to play a key role in the discovery of more than half a dozen heavy elements through the 1950s, winning the Nobel Prize for Chemistry in 1952. Seaborg later served as Director of the Atomic Energy Commission.

He was more businesslike. He wanted to [help industry] sell [commercial nuclear] reactors as fast as he could and develop some new kinds of them. He didn't take a great interest in—

FISHER: —in the biological and medical research program. Would you characterize Dr. Seaborg as a chemist more interested in furthering biological and medical research?

TOTTER: He was quite interested in it. In fact, he used to tell a story about iodine, which was discovered or developed—some of it was developed—in his laboratory. Radioactive iodine was [later] used to cure his mother's thyroid difficulty.[86] He was very proud of that.

And he had the usual small interest, I guess, not a large interest, in biological [research]. But he was quite supportive of it.

And he never questioned what we wanted to do. If we could give him good reasons for it, he would support us. But I don't think any of the Commissioners were "antibiology," in any sense of the word.

I think people who were afraid of us were in the reactor development sort of thing, the commercialization of nuclear energy, because they didn't want any obstacles. And I think that's perfectly natural for them to worry a little about that, because it was the biological stuff that often was the confining border of what they could do.

And I think the [cancellation] of the digging of the sea-level canal [in Panama by nuclear explosives] is a good illustration of that. As soon as it was found out that it would hurt the cities—close—they took it off the [Plowshare Program].

Rise and Fall of AEC Support for Cancer Research Hospitals (1948–74)

FISHER: Did you have policies in the Division of Biology and Medicine on human use of radionuclides as early as 1962, '63?

TOTTER: Oh, I don't know exactly how to answer that question, because our work, of course, was done by contractors or other people. But we did have a policy of making sure that there was some sort of oversight for that. That was the policy. And I don't think we wrote any restrictions ourselves.

FISHER: You funded, or supported, about four different [AEC] hospitals at this—during this period of time—

TOTTER: Yes.

FISHER: —which provided—I think it was free medical care to cancer patients being treated with experimental therapies.

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[86] Radioiodine (^{131}I) is widely used to diagnose thyroid function and also is a highly effective therapy for hyperthyroidism, Graves' disease, and thyroid cancer.

TOTTER: Yes.

FISHER: And could you elaborate on this, this system of hospitals, one of which was here in Oak Ridge,[87] one in Chicago?[88]

TOTTER: Well, I believe that there was a requirement placed on the AEC by an act of Congress. I'm not sure I can cite all the history of this. But we regarded [the hospitals] as excellent sources of information about the [medical] use of isotopes and radiation.

And of course, a lot of this grew out of the original people who were involved with the Manhattan Project. Stafford Warren[89] had founded a large program when he organized the UCLA School of Medicine. And there was another Californian, whose name I can't remember now, who was involved deeply in the Manhattan Project, who also was interested in high-energy x-ray treatment.

FISHER: Dr. Tobias?[90]

TOTTER: No. Tobias—no, Tobias was at Berkeley. But there was another—another man who died.

FISHER: At UCLA?

TOTTER: Not at UCLA, but on the West Coast.

FISHER: San Francisco?

TOTTER: He had a 750,000-volt machine, which I think the Navy has now. I can't think of his name, but he died before I was much involved with this. So, I never—I only met him once, so I can't remember.

Then, of course, the Brookhaven[91] [Medical Division] and then the Argonne Cancer Research Hospital was also in this organization. I think

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87 The Medical Division at ORINS had approximately 30 beds for people with certain types of cancer; this contract was terminated in 1974.

88 the Argonne Cancer Research Hospital, one of three clinical facilities created by the Atomic Energy Commission in 1948. While the AEC owned the 58-bed Chicago hospital, the University of Chicago medical school administered and staffed the facility. Patients were admitted on a selective basis: physicians chose persons whose condition best suited the hospital's research and treatment applications. The hospital admitted its first patient in January 1953. The Energy Research and Development Administration terminated Government support for Argonne and the other AEC-created research hospitals in 1974, three years after the hospital's name was changed to the Franklin McLean Institute. The facilities are now used by the university's medical school for studies in radiology and hematology.

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

90 Tobias was a professor of medical physics and radiology at the Donner Laboratory and the University of California at Berkeley. Dr. Tobias's main research focused on the biological effects of radiation; cancer research; and space medicine. For the transcript of the interview with Tobias, see DOE/EH-0480, *Human Radiation Studies: Remembering the Early Years; Oral History of Biophysicist Cornelius A. Tobias, Ph.D.* (July 1995).

91 Brookhaven National Laboratory (Upton, New York)

those were set up by special—these three were set up by a special act of Congress. The others were spontaneous growths.

FISHER: They were fully funded while you were director of the Division of Biology and Medicine.

TOTTER: Yes.

FISHER: And the costs of treating patients were covered by the program.

TOTTER: Yes, I believe so.

FISHER: And these hospitals also served as training centers for physicians from all over the country.

TOTTER: Yes, they did. Of course, I can't say how many people were trained, but I know that there was always a turnover of people, coming in for training.

FISHER: Why—why was this program discontinued after you left?

TOTTER: I don't really know. I suppose it was a funding problem. But I don't have any information about it.

FISHER: It has been mentioned to us before that there may have been two contributing factors. One, it was getting kind of expensive. It was a very expensive program for the agency to administer. And maybe another reason might have been that excellent cancer therapy was being provided at the university medical centers and other facilities not supported by AEC, and perhaps the responsibility gradually shifted.

TOTTER: I think that's probably correct. I do recall that when I first was at the AEC, that there were agreements reached between the director of the National Cancer Institute and the director of Division of Biology and Medicine about which areas were suitable for us to go in and would not be in direct competition with the National Cancer Institute.

But of course, in 1972, when the Crusade Against Cancer was [started]—when they allowed the National Cancer Institute to double their funding level, then we stopped, I think, building cyclotrons[92] for the therapy then, because it was much easier for the Cancer Institute to do it. That was one step in this decline of AEC support.

And I think that probably the larger funding for the National Cancer Institute did affect the funding possibilities of funding these hospitals.

FISHER: There was a proposal to build a large cancer-treatment facility in Knoxville[, Tennessee]. Were you involved in the decision not to build that?

TOTTER: No, I was not. I know the research hospital was built over there. When I thought it should have been on this side of the river, they put it on the other side, to make it difficult to get isotopes from here. But is that the one you're thinking of? Or has that have some connection with it?

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[92] accelerators in which particles move in spiral paths in a constant magnetic field

FISHER: It might have. I don't really know enough of the history.

(pause)

Public Misperceptions About Radiation and Cancer; Underuse of Established Biomedical Facilities; and Funding of Environmental Cleanup vs. Biomedical Research

HARRELL: Should we just follow up? What would you like people to know about the AEC and the biomedical programs that has sort of been overshadowed by the hysteria that has come out in the last few months?

TOTTER: Well, I think I could go on at some length about that. But to put it very succinctly—I don't know whether I could or not—but I believe that the fear of cancer has frightened a lot of people away from many of the things we should do in the basic study of what cancer is and what causes it[—activities] that we *should* be doing and we're not because of this fear that has turned us away from a lot of the things that we can do, not just in the cancer field but in the development of atomic energy in general and so forth.

I think the fear has perhaps [placed us] one step—or has resulted in one step away from the United States leadership in science. And I greatly fear that this is going to cost us dearly in the future.

HARRELL: Do you feel that the work on the safety of various nuclides has been stopped short of—

TOTTER: Well, I hope it really hasn't. What I think is, that the fear of cancer and so forth has driven the standards that we must follow to the impossibly low [exposure level], in which it's impossible to get things done. That's another factor. If I can say it right.

We no longer do hands-on experiments in chemistry simply because of the fear of the chemicals, which is engendered by the fear of cancer. And I think that the chemists that are being trained now are being cheated. And we can lay it all, I think, to this fear of cancer, which has been orchestrated [by] a few people to the impossible level.

I have been looking at the statistics of cancer lately, and I find that the increase in cancer—which is undeniable—comes all in people over 70 years of age; so that cancer is not shortening our life span any more now than it did 50 years ago.

FISHER: The longer we live, the greater our chance of—

TOTTER: —getting cancer.

FISHER: —getting cancer. Certainly.

TOTTER: People don't understand that, apparently. And I am particularly depressed by—well, the state of this floor [of this building at ORISE], for example. Here are laboratories going to waste, when they should be working on things that could better our conditions. There are animal

laboratories over at the Biology Division that cost millions to put in, and there is not a single animal in them. And this is a horrible state of affairs.

FISHER: What do you think has brought this on? As I visit these same facilities, I see the same thing that you do. We know it's related to decreasing budgets from the Department of Energy in biological research. But what is the causative factor in the declining budgets?

TOTTER: Well, there are too many old people like me still alive. Perhaps that has something to do with it.

(laughter)

But we do—we have no real good way of separating the needy from the people that can be responsible for their own upkeep. Politically, we don't have any way to do that. And I think this has a lot to do with it.

FISHER: Can you expand on that just a little bit? There seem to be a lot of hidden, unsaid messages in that last comment.

TOTTER: Well, I worry that we're overtaxing our food supply and everything else. That is, we're living at a dangerous level [of dependence on the Government]. And I think that if individual responsibility were functioning like it was 100 years ago, we probably wouldn't be in this shape. But we expect, perhaps, too much from the Government, or it's giving us too much—however you want to put it.

I don't see any solution to this problem. I haven't any solution to offer. I just worry about it.

FISHER: How could these facilities be better utilized?

TOTTER: Well, that would take more money, of course. And I'm sure the young people are ready and waiting to make use of them somewhere. And we have—we have an enormously lot more to learn about ourselves, about science in general. And I think we should do it.

We have worked ourselves into a state where we're extremely dependent on technology, and there are lots of people who want to throw that overboard. And I think that—I think that it's not bad that we've worked ourselves into a dependence on it, if we keep it up properly.

FISHER: What are your current activities focusing on now? You're almost—your age is about 80, isn't it?

TOTTER: Yes, 81.

FISHER: 81?

TOTTER: Yes.

FISHER: This is a spry age to be actively coming to work. What are your pursuits right now?

TOTTER: Well, you saw one thing. Maybe I can show you another. I have been interested—since I worked in the Institute for Energy Analy-

sis—interested in what the fear of cancer has been doing to us, as I have been mentioning. And so I have been looking into the epidemiology of cancer, and I find that people who study cancer epidemiology[93] have not been very frank with us.

And I've written a book here, but it's still being reviewed, so it's not available. But in it, I have tried to show that our cancer is internally caused, and at least 95 or more percent of it is *not* environmentally caused.

FISHER: Natural, as opposed to direct interaction with our environment?

TOTTER: It's not an accidental cause of death. It's a natural cause of death, yes. And we're being misinformed by the people who have, perhaps, a vested interest in keeping us ignorant about that. Or perhaps they so badly want to believe it can be prevented, that their outlook is warped.

We are throwing away the best chance we have of maintaining our supremacy in science and maintaining our level of support by being misled about cancer, by not understanding that it's a natural cause of death.

It's very difficult to prove a thing like that. But I think, on the scientific weight, that the evidence is strongly in favor of it being a natural cause of death.

And that you will not—the easiest way to do it—and it should have been done when this book was published—

(begins to reach for a book and hands it to Fisher and Harrell)

—are you familiar with that one?

FISHER: You're showing us now a book entitled *Causes of Death, Life Tables for National Populations* by Samuel Preston, Nathan Keyfitz, and Robert Schoen.

TOTTER: Published in 1972.

FISHER: 1972.

TOTTER: And it has all the information needed to show that cancer is a natural cause of death, if you believe the statistics.

FISHER: Published by Seminar Press, New York. What is the course [of action] that you recommend as a result of this research and the writing of this book?

TOTTER: I think that you should take the emphasis off environmental problems with respect to cancer—not with respect to beauty and cleanliness and all of that sort of thing and safety in general. But you should take the emphasis off of that.

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[93] the branch of medicine dealing with the statistics of incidence and prevalence of disease in large populations and with detection of the source and cause of epidemics; *also:* the factors contributing to the presence of absence of a disease

And the money that is spent to clean up things beyond any reasonable—way beyond any reasonable level, could better be invested in developing our nuclear energy or whatever other source of energy we have, to make sure we don't run out.

FISHER: In reviewing the Department of Energy budget last year for environmental cleanup versus biomedical research, the—I believe the numbers were on the order of about $7 billion for cleanup and a fraction of a billion for biomedical research. That probably has contributed to this.

TOTTER: The decline of these facilities and so forth and the programs—yes, I'm sure it has. And I don't know how much of that $7 billion is warranted, but I think that the division is obviously wrong, $7 billion [for cleanup] to one-half or six-tenths of a billion [for biomedical research], whatever it is. It should be more nearly half and half.[94]

FISHER: You mentioned even the risk from mill tailings in Grand Junction earlier in our discussions today. Just the cleanup of mill tailings and moving tailings from one spot to another has cost the Department of Energy over $2 billion, for a savings of about one to two lives in the next century, according to—

TOTTER: Sounds about right.

FISHER: —the most optimistic estimates.

TOTTER: Yes.

FISHER: That's a large expenditure of money for perhaps a real negligible benefit.

TOTTER: Yes, I'm sure we're doing a lot more of that. I don't object to cleanups if they have some result. But if they don't have any result, it's a terrible waste of effort.

FISHER: Our [oral history] project has been concerned with identifying and learning more about the uses of humans as subjects in radiation research, both for tracer studies and other medical uses of radionuclides. Would you—if you were serving as the member of a human subjects committee, would you consider use of human subjects in the 1990s to be ethical?

TOTTER: That's a really difficult question to answer, because you haven't any boundaries on it that I can see very clearly.

FISHER: Nor have I described the nature of the experiments, but—

TOTTER: No, I—but let me say this. Every time you take an aspirin or a Tylenol[95] you're doing a human experiment. And we mustn't get down to where

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

[95] Tylenol™ is a popular nonprescription pain reliever whose active ingredient is acetaminophen.

we have to have a committee if you're going to take an aspirin. But we've approached that closely.

FISHER: We may have made it much more difficult for human experimentation to take place in the future.

TOTTER: We must quit getting down to these—into the area where the dangers are negligible. We like to keep them low, but taking them to nothing is impossible.

FISHER: Well, I believe we have come to the end of our list of questions that we have prepared. We were particularly interested in the policies and decisions that you were responsible for in the Division of Biology and Medicine. We have touched a little on your academic and professional career. In conclusion, is there anything that you would like to add that we may have overlooked or missed?

TOTTER: I can't think now of anything we haven't touched on that is very important, except to say that I had a heck of a good time all these years.

(laughter)

And I appreciate being supported in the work I've done as well as I have been.

FISHER: You're very fortunate to have lived and done these things during this era.

TOTTER: It's fantastic, isn't it?

FISHER: It will never be the same again, will it?

TOTTER: It certainly won't. It's a wonderful age to have lived in.

FISHER: Well, it has been a pleasure for me to meet you again. I think I met you in the past, but we haven't had a close association. And I came very close to being one of your students in 19—

TOTTER: I'm really surprised at that. I'd forgotten about that.

FISHER: —in 1975.

TOTTER: Yes?

FISHER: It was probably by the flip of a coin that I went elsewhere [to graduate school]. We appreciate you taking the time to let us ask these questions and hear your opinions and recollections of the past. Thank you very much.

TOTTER: Thank you.

HARRELL: Thank you very much. ❑