DOE/EH-0473

HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Health Physicist
Constantine J. Maletskos, Ph.D.

Conducted January 20, 1995

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

FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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

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

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

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

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DISCLAIMER

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

DOE/EH-0473
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Interview with Constantine J. Maletskos, Ph.D.
Setting: January 20, 1995, Gloucester, Massachusetts
Interviewers: Darrell Fisher (Pacific Northwest Laboratory)
and Karoline Gourley (DOE Office of Human Radiation Experiments)

ORAL HISTORY OF HEALTH PHYSICIST CONSTANTINE J. MALETSKOS, Ph.D.

Conducted January 20, 1995, in Gloucester, Massachusetts, by Dr. Darrell Fisher, a health physicist from Pacific Northwest Laboratory, and Karoline Gourley, a researcher with the Office of Human Radiation Experiments, U.S. Department of Energy (DOE).

Constantine Maletskos was selected for the oral history project because of his research at the Radioactivity Center at the Massachusetts Institute of Technology. The oral history includes Dr. Maletskos's work at the Center on research projects that used subjects from the Walter E. Fernald State School in Waverly, Massachusetts, and the New England Center for the Aging, as well as blood volume studies involving pregnant women.

Short Biography

Dr. Maletskos was [redacted]. He received his B.S. in Quantitative Biology (1942), his S.M. in Biophysics (1943), and his Ph.D. in Biology, Chemistry and Physics (1954), all from the Massachusetts Institute of Technology (MIT). He married in 1947 and has three children. Dr. Maletskos was at MIT in various capacities from 1942 through 1976. In addition, he has held appointments at the Harvard Medical School and the New England Deaconess Hospital, and has worked as an independent consultant.

While at MIT, he worked closely with Dr. Robley Evans and other scientists in the Radioactivity Center. There, he studied the effects of food phytates on calcium metabolism in children, and sought to determine the metabolic differences between radium and mesothorium, using residents at the New England Center for the Aging.

During his career, Dr. Maletskos has also served on many committees and panels and has membership in numerous societies, including:

- Journal Editor, *Health Physics*
- Fellow, Health Physics Society
- Trustee, Manomet Bird Observatory
- Panel on the Implementation Requirements of Environmental Radiation Standards (for High-Level Wastes), National Academy of Sciences
- Consultant—National Council on Radiation Protection and Measurements
- Radium and Isotope Committee, New England Deaconess Hospital

Dr. Maletskos has published extensively on radiation biophysics, radiobiology, and radiation measurement and protection, as well as environmental assessment and health and other topics.

Early Education and Career, (1920 to Mid '50s)

GOURLEY: Good morning. It is January 20, 1995. This is Karoline Gourley. I'm here with Darrell Fisher and we're here [conducting] an oral history with Dr. Constantine Maletskos in Gloucester, Massachusetts.

FISHER: Thank you for allowing us to come. This is an interesting opportunity for us to learn more about your personal history as well as your scientific accomplishments. We'd first like to ask what brought you into the field of science and drove your interest, in particular, toward the radiation sciences and health physics.

MALETSKOS: Well, the original interest for going into science was back in high school. Having attended the Boston Latin School, which was the first public school in the country, I decided that I wanted to go to MIT[1] and had good enough grades to just walk in—not go through what was called the College Board business at that time; it's called SAT [(Scholastic Aptitude Test)] now. I thought I would be interested, even back then, in doing something with electrical engineering in medicine. Why I got that idea, I have no idea at this stage in the game or then.

Maybe. I started out in electrical engineering and decided that [it was] not really what I wanted to do, given the way they were teaching electrical engineering. I don't [know] why I ha[d] this innate feeling toward it [(meaning biology)]. I knew I had to get something into biology, so I slowly worked my way from a freshman in Electrical Engineering—all the way into the Biology Department in time to be able to graduate, eventually, with both my bachelor's and master's [degrees] in Biology.

That's how things took place at that time; in the five-year course which was available to me at that time, you [could receive] both of those degrees [at the end]. In the middle of the senior year, for the summer between the two years, I wanted a job so I went searching around MIT and I ended [up] in Professor Robley Evans's[2] laboratory, which turned out to be a career-marking step.

GOURLEY: When you first started with Dr. Evans, what was he working on?

MALETSKOS: He had already come to MIT about 1934 or 1935 on invitation by Karl Compton, who was the president of MIT, to set up a nuclear engineering course, [which] would eventually become big and broad as it could be. At the same time, [Compton] set up a center, which was eventually called the

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1 Massachusetts Institute of Technology, Cambridge, Massachusetts

2 In the early '30s at MIT, Evans investigated the bioeffects of radium on dial painters in New Jersey and Connecticut. By 1941, Evans with others had set the first standards for a tolerance level for radium in the human body. The first "tolerance level" for radium was set at 0.1 microgram body burden: Evans judged that there would be no bone cancers below 0.1 microgram ^{226}Ra in the skeleton. Later he served on the AEC's Committee on Isotope Distribution. At a 1967 symposium, he proposed that the AEC establish a National Center for Human Radiobiology so the AEC could follow up and combine all the radium cases being studied at MIT, Argonne National Laboratory, and elsewhere. On September 1, 1969, the center opened at Argonne, headed by Robert E. Rowland; Evans maintained a satellite office at MIT. In the early 1990s, Evans's pioneering basic research earned him the Department of Energy's Fermi Award.

Radioactivity Center, in which all these new things related to nuclear physics, including radiation and radioactivity, would be disseminated in various fields. [In] that way you would get maximum effect from a place like the Institute [(MIT)] by producing students and scientists who would know a bit about this [new field] so that they could then apply it to their own particular research [in] the various different fields.

The Radioactivity Center consisted of representatives eventually from six or seven departments at the Institute: Biology, Nutrition, Electrical Engineering, Physics, Chemistry, Mathematics, and that sort of thing. It was an Institute-wide center and one of the first ones [(research centers)] that the Institute had. So, the Institute [was] quite great in doing the center business, where you bring a lot of talents together and do multidisciplinary work.

At the time that I came on board, he [(Compton)] had already started his Nuclear Physics course. They were doing a lot of basic nuclear physics studies [at the Radioactivity Center]. He had arranged to get a cyclotron that [M.] Stanley Livingston,[3] who had worked with [Ernest O.] Lawrence[4] in developing, designing, the cyclotron in Berkeley[, would provide]. Evans managed to get the funds for it from the Markle Foundation. [And as a result,] the priority for that cyclotron, because of the Markle Foundation funding, was biology and medicine, not physics. Even from the very beginning, this business of getting involved with biological and medical aspects with people, animals, and that sort of thing was basically [involved with the initial] construction of that cyclotron, which is quite interesting.

FISHER: Which would have been about which year?

MALETSKOS: The cyclotron probably got—I even helped run the cyclotron, so it was available and running in 1942. I think it was put together a year or two beforehand. It ran quite a bit and was used an awful lot during World War II, when people didn't even know what [was] going on in terms of what they were producing and what kind of experiments they were doing.

One of the important things that they [(medical scientists in the greater Boston area) conducted] was blood preservation studies, at the request of the military. [Before the studies,] they could only store blood for about a week, and that wasn't that bad for going [by ship] to [the European Theater of World War II], but going [by ship] to the Pacific [Theater], you needed a lot more time. One of the first major papers that came out of the Radioactivity Center, from the medical standpoint, had to do with this first

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[3] M. Stanley Livingston, Ph.D. (born 1905), a physicist and research associate at the University of California, Berkeley (1931–34). Livingston later served as a professor of physics at MIT (1938–70). He figured prominently in the design of high-energy particle accelerators.

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

work on blood preservation. It is very interesting to read that first paper, because it's a classic paper on how to report [results].

FISHER: Who wrote it?

MALETSKOS: Evans and the team of physicians from Harvard Medical School. This was a joint program, and they managed to extend the period [for blood storage] from one week to three weeks; that made all the difference in the world.

GOURLEY: And was that through radiation?

MALETSKOS: That was through the use of radioactivity; they used radioactive iron to study, to tag the red cells, [and] to preserve them. You take the red cells from somebody by blood transfusion, tag the red cells, put them back into a person, and then see how those blood cells that you put back worked on the basis of whatever [medium] they had been stored in. It was just a trial-and-error method until they kept on finding and homing-in on what eventually became the material [known as] ACD, acid citrate dextrose.

What is also amazing is that they [used] not only one radioactive iron, they used two [radioisotopes of iron][5] in the final experiments; [and] they used a radioactive iodine[6] as well. So there were triple tracers experiments involved.

With each extra tracer, you get ten times more information then you get from the first one. So by using three radioactive tracers, rather than one, they were getting a hundred times more information. What was really interesting is that they found out that in one part of the process the cells that you were putting back in were preserved nicely, but the medium was destroying the person's [own] good cells. Can you imagine what would [have] happen[ed] if they hadn't found that?

GOURLEY: So, how long into it did they find that?

MALETSKOS: Well, they found it before it was ready to go out into the real world.

FISHER: Did this work take place during World War II?

MALETSKOS: Yes, and the first paper[7] was in 1946. It was a classic paper, which I recommend [that] anybody read to [understand] the whole aspect of how you [conduct] an experiment from the start of producing material, to using it, to developing the process, and to do all the dosimetry[8] that you could do in those days and with what you could compare it to see whether the dosimetry was reasonable in terms of what other exposures were available in those days.

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5 iron-55 and iron-59

6 iodine-131

7 See OT-46, "Early Studies of Iron Metabolism in Red Blood Cells Using Iron-55 and Iron-59," in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (213 pages), DOE/EH-0491, July 1995 (hereafter called the *Experiment List*). Ref: W.C. Peacock, R.D. Evans, et al.; "The Use of Two Radioactive Isotopes of Iron in Tracer Studies of Erythrocytes"; *Journal of Clinical Investigation* 25(4):605–15; 1946.

8 the calculation of absorbed dose, using data from bioassay and other radiation measurements

FISHER: You didn't actually join this group until the 1950s?

MALETSKOS: I joined them in 1948 formally. After 1942, I had to finish my [academic] year, and then I had to go into the army to do my tour of duty.

Now getting back to the original question, you asked me what [research] was going on [under Dr. Evans]. Well, one of the things that was going on, for the war effort, was that they needed to make crystal oscillators [to use as receivers in crystal radios]. One of the problems was, "How do you get at [(determine)] the crystal axis quickly [so as to make the precision cuts at assembly-line speeds]?" So they decided to do it with x rays and a count-rate meter,[9] and I built the first two commercial count-rate meters during that time. Evans was doing radium[10] work already, and I helped build some of the [ionization] chambers [for radiation measurements] that were required, and because of all this work for the blood preservation (and I didn't know that [it] was coming because there was no reason for me to know in 1942 as a summer worker). They [(Evans and his team)] knew they were going to do a zillion samples.

So, I helped build the first automatic sample changer during that summer; it was a very interesting first summer. As a consequence of that [work], I did the first experiment with radioactivity ever at MIT in the Biology [Department]. As a result of my having been [at the Radioactivity Center,] my [master's] thesis was on the diffusion of phosphorus in nerve axons;[11] that was the real impetus of what got me going and why you people are interviewing me.

Afterwards, I went back to the Biology Department with the idea I would get my Ph.D. in that. I was a research assistant and I had to put two big laboratories together that had gone defunct as a result of the war. The laboratories I put together were Enzymology and Biophysics; that was my major job. In the meantime, I was supposed to be getting ready for my Ph.D. exams; I did very well in the written part [of the exams], but I didn't do very well in the orals, mainly because I'm not very good at memory. And I guess I was too apprehensive [(that I might forget what I had learned)] and didn't make the grade.

But anyway, Evans heard about it, and he said, "Come on down here, take charge of all the biology in the Radioactivity Center, and we'll see how we'll get you a Ph.D."

FISHER: You had already worked with Evans for some years?

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[9] an instrument that counts the number of radioactive decays per minute, used in this application to quantify the x-ray exposure rate

[10] a radioactive, luminous white, metallic element that occurs in very small quantities in combination with minerals. Radium emits alpha particles and gamma rays to form radon gas. Radium has been used in luminous surface materials, such as the numbers on watch faces, and used in treating cancer.

[11] the appendages of a neuron that transmit impulses away from the cell body

MALETSKOS: Only that summer. And also in the following year when I did my radiophosphorus experiment, I was always down at the Radioactivity Center, so I was seeing him, but nothing was [planned]. He assigned one person to whom I could talk freely and [was told] not to pester anybody else, because everybody else was pretty busy. So, that was very nice [(having someone to pose questions to)] because I had to get the radioactivity, I had to standardize and know what I was doing. I also had my own counting equipment[12] in the Biology Department.

FISHER: Where did you get the funding to support some of these activities?

MALETSKOS: Back in those days, it had to be the AEC.[13] During the war effort, I don't know, it would be a lot of [funding from] the military. There was the Office of Scientific Research and Development[14] that had been set up, and Vannevar Bush,[15] who had been from MIT, set up this whole overs[ight] research group; they were the ones that would be funneling information back and forth to various military organizations and various departments in the universities throughout the country. So a lot of the funding must have come from that, but I don't know those details at all.

At the time I was there, the AEC was in the picture as well as the Office of Naval Research. The Office of Naval Research was remarkably good in their support of basic research, that [they] had no immediate help to them as such, but they anticipated that it [(the basic research)] would all be useful in the long run, which I thought remarkable for a military organization. So those were the two main funding [groups] when I first got there on a more permanent basis.

GOURLEY: You said there was no need for you to know about some of [the] things—

MALETSKOS: There were secret things being done [at MIT that] I said there was no need [for me] to know. There was a war on—remember. The Radioactivity Center and all of MIT was heavily involved in basic military research to help the war effort; all the initial radar work was being developed in this country over there [(at MIT)] and it turned out to be the real major resource for radar development. People knew that something was going on, obviously all over this country, but [I] wasn't involved; that's why I said I didn't need to know.[16] There were a lot of things going on

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

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

[14] Established by an executive order June 28, 1941—six days after German troops invaded the Soviet Union. The OSRD's Director reported directly to the President and could invoke the prestige of the White House when dealing with other Federal agencies.

[15] Bush, president of the Carnegie Foundation, acted as a point man in persuading the Roosevelt administration to set up a national science organization, the National Defense Research Committee, which he went on to head.

[16] Radar—Radio detecting and ranging—was a key development that allowed the Allies to detect and track hostile airplanes by measuring the direction of reflected radio waves and timing their return. Its development (continued...)

that even in the Radioactivity Center that there was no need for me to know. This was in 1942, when I came in 1948, there was still some classified work going on and Evans wanted me to have clearance, and I got clearance at that particular point.

FISHER: Do you recall any work that Robley Evans did for the Manhattan Project[17] at the Radioactivity Center?

MALETSKOS: I don't recall right offhand now; I think I could think of something, but I don't know. I'm sure the answer is yes, [Evans did some work on the Manhattan Project]. It could have been a lot of [radiation] counter [and] instrument development. Sandy Brown[18] was the counter developer in the U.S., for [all] practical purposes; he developed the theory of the Geiger counter[19] operation. [He] could end up designing any kind of a counter you wanted with anything. As a matter of fact, there is some place at MIT right now (and I don't know where it is, because I've searched and searched and couldn't find it—because it would make a terrific museum piece) whose electrodes are a fork and a spoon; this was his proof that he could [make] any doggone kind of counter you wanted.

FISHER: You mentioned the use of radioiron from the cyclotron in blood preservation studies and blood metabolism[20] studies. Do you recall other uses of radioactive iron in human studies during the period of 1940?

MALETSKOS: Yes, 1948 on. There was earlier work with the Department of Nutrition[21] in the iron absorption studies, which were to determine whether phytates[22] prevented the absorption of iron when it's present in cereals. The work that I got involved in was (now having the developed technique of using red cell tags and there were many things one could do) blood volume studies.

One of the things the gynecologists wanted to know about is, "How does the blood volume of a pregnant woman change with respect to time, and what happens after the child's birth?" When you take a venous sample from your vein on your elbow, for example, all you're doing is getting the hematocrit[23] of what's in the sample in [that] pipe, but you don't know what's happening [in] the whole body.

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16 (...continued)
and use remained highly classified until after the war.

17 the U.S. Government's secret project, launched December 28, 1942 by the U.S. Army Corps of Engineers' Manhattan Engineer District, to develop the atomic bomb

18 Sanford Brown, Ph.D., professor of physics at MIT in the field of plasma physics, retired from MIT, now deceased

19 a portable instrument for detecting ionizing radiation and measuring dose rate

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

21 an academic department at MIT or Harvard

22 Phytates are biomolecular salts or esters of phytic acid, $C_6H_6(OPO_3H_2)_6$, obtained from plant seeds or tubers. They bind to certain alkaline earth materials, such as calcium. This binding inhibits gastrointestinal absorption of the materials so that they are excreted without being taken up into blood and utilized nutritionally.

23 the ratio of the volume of red blood cells to a given volume of blood so centrifuged, expressed as a percentage

The only way to determine that is you have to determine what the plasma[24] volume is doing and what the red blood cell volume is doing. You could determine the plasma volume by using a dye and they used what is called "Evans blue" (which has nothing to do with Robley Evans), but, that's what they called it. You could inject [the dye, having] measured the concentration of the dye with a photometer[25] or something like that, [then you'd] separate the cells [to] get the plasma sample, next measure] the concentration of the dye again, and [then] by division (by ratio), you could tell what the volume of the plasma [in the body] was.

But there was no way to measure what the volume of the red cells was, and so you didn't know what was really happening to [blood in] a pregnant woman. So now along came [radioactive] tag[ged] red cells and you could say, "Ah, we'll use [tagged][26] red cells as the agent and we'll inject those [cells] and see how dilute they become when then travel around the body; then, by taking another sample, we'll make the measurement."

This is exactly what we did, and there is a paper that has been written up on that [subject].[27] The [pregnant women] were injected with small amounts of radioactive iron, and [then] small amounts of red cells [were obtained] on a periodic basis, ending up between six and eight [times], depending on when you first got ahold of the pregnant women for research purposes; the gynecologist tried to get them all as early [in their pregnancy] as possible. We had six to eight groupings, of time [to birth], up to pregnancy; and then they were measured [for red-cell radioactivity] up to two times after pregnancy to see how rapidly [the women's blood volume] came back [up] to normal.

FISHER: These studies were done back in the early 1950s?

MALETSKOS: That's right. Having done that, and by the way, one of the big things that the press got into when they were reviewing [this experiment] was the big deal that one of these women hemorrhaged at birth. Well, a woman occasionally hemorrhages at birth.

It happens that one of these women [in the study] hemorrhaged at birth, and so we mentioned it (obviously); you're doing a blood volume study, you're going to mention [that] this woman hemorrhaged at birth. She was a perfectly good candidate for the study, up to birth, but we mention that because we say we had to take her out of the group after birth.

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[24] the fluid part of blood, as distinguished from the cellular components

[25] an instrument that measures luminous intensity or brightness, luminous flux, light distribution, color, etc., usually by comparing the light emitted by two sources, one source having certain specified standard characteristics

[26] Radioactive "tags" are applied to biomolecules to study a biological, chemical, or physical system.

[27] See OT-57, "Red Blood-Cell Volumes and Hematocrit in Normal Pregnancy Using Iron-55," in the *Experiment List*, which cites the following two references: W.L. Caton, C.C. Roby, D.E. Reid, and J.G. Gibson II, "Plasma Volume and Extravascular Fluid Volume During Pregnancy and the Puerperium," *American Journal of Obstetrics and Gynecology* 57:471–481, 1949; and W.L. Caton et al., "The Circulating Red Cell Volume and Body Hematocrit in Normal Pregnancy and the Puerperium," *American Journal of Obstetrics and Gynecology* 61(6):1207–1217, 1951.

So the news media immediately connected hemorrhaging with radiation and therefore, [the press concluded] the radiation did it. And they persisted on that [subject] for a very long time [by] showing what they [believed was] very bad practice.

FISHER: Where in fact, a tracer [(minuscule amount of radioactive tag)] amount of radioiron wouldn't be enough in terms of radiation dose to cause anything to hemorrhage?

MALETSKOS: Absolutely not; not only that, but [it wouldn't be enough] to cause any other health effect at the levels that we gave, and remember this was long-lived iron-55 [(half-life of 2.7 years)], not iron-59 [(half-life of 44.5 days)]. The reason these experiments could be done [was that red-cell] donors were around because of the blood preservation studies; they still had some radioactive iron [in them]. We didn't have *new* donors, we just used *them*.

GOURLEY: So, the [tagged] red cell project to determine where you learned about blood preservation, that was a human study?

MALETSKOS: Yes, had to be. Eventually, although there was a lot of work that was done in the laboratory [first]. You could put cells in a test medium—[the first thing you want] to do is watch what happens over a period of time without doing anything [in people]. A medium could ruin the cells right on the spot; the [cells] could hemolyze and that's the end of that particular medium. Hemolysis is when the cells break apart and all the hemoglobin comes out.

GOURLEY: How many subjects were there on that study?

MALETSKOS: I don't know. It's possible to find out, but I don't happen to know; a lot of them were probably medical students.

GOURLEY: The pregnant women—they came out of the red-blood-cell preservation study?

MALETSKOS: The donors from the preservation study were available, as far as I know, (because I don't recall that we [used] new donors by injecting them with radioactive iron to incorporate in[to] the[ir] red blood cells) to use [in the blood volume study]. I don't remember that [additional subjects were injected]; I can't prove it, but I don't remember. But, the [donors] *were* available, and [though] their radioiron levels were still going down with time, we had enough sensitivity in our instruments that we could still measure th[em], not only [the blood] from the donors, but after [the blood] dilution by the pregnant women. That shows what you could do if you sit there and develop techniques and so forth that have great [radiation measurement] sensitivity; it doesn't mean that you're just jacking up the radioactivity—you are in fact jacking up the sensitivity so that you could use what radioactivity is there.

GOURLEY: What sorts of dosages are we talking about?

MALETSKOS: We're talking a few millirem[28] or something like that.

GOURLEY: The records on this—are they still at the Radioactivity Center?

MALETSKOS: All the medical records are in the medical school [and the obstetrical hospital]. I never saw any of the pregnant women; there [was] no reason for me to see them. This [research] was all done on an outpatient basis; they would come in while they were being observed during the pregnancy for regular checkups and then we'd just draw a sample of blood. On the first visit, you'd just get an injection of red blood cells, and the only time that they stayed in the hospital was when they were giving birth. They came back afterwards on two occasions, separated by a month, to see if [their blood volumes] returned to normal by the second month.

FISHER: If I read some names to you, maybe you could tell me a little bit about the investigators on different studies? Clement E. Smith.

MALETSKOS: He was the obstetrician.

FISHER: R.B. Cherry.

MALETSKOS: She was the research assistant.

FISHER: John D. Gibson.

MALETSKOS: He was the first physician who was originally involved in the beginning of the blood [preservation studies]. He was the honcho, from the medical standpoint, as Evans was from the physics standpoint.

FISHER: The blood preservation studies: did Robley Evans advise the physicians on the amounts of radioiron that could be considered safe for these administrations?

MALETSKOS: That's why I go back and tell you to read that first paper: it's classic. Everything that was done with anybody was a [close working] relationship. It was not a case of, "Yeah, we'll provide you with radioactivity; and you can do whatever you want." Both of them [(Gibson and Evans)] had to sit down—remember nobody had ever done anything like this before to start with. You had to sit down and figure out, "Can we do the experiment with what we know right now? And what is required [to ensure that the procedure will be effective]?" Keep in mind that when I was there in 1942, they already had decided [that] they were obviously going to do [the study]; [because] they were going to have so many samples, they weren't going to be able to sit and measure them by hand one at a time, [so] they needed an automatic [measuring] instrument.

We eventually built four of those things; [they] became the impetus for the first nuclear instrument company, Tracerlab[, which] then built better versions of those [instruments]. There was so much going on. We had just [designed] a battery of electroplating units because we electro-

[28] A millirem is one-thousandth of a rem. A rem is a unit of radiation dose equivalent, or "rads times the quality factor, Q." The limits for occupational exposure of workers to radiation range from 2 to 5 rem per year for most countries.

plated the iron [to be] able to measure the x rays because they were so easily attenuated; the beta rays[29] of the iron-59 were no problem.

Evans insisted that there be total knowledge on the part of everybody involved, no matter who it was, regarding the dosimetry, which had to be considered in-depth. That's why I ask you to go back [and read that article]; the section on dosimetry was [written by] Evans himself.

FISHER: It may be difficult [for] the general public in the 1990s to understand why normal pregnant women would be injected with a radioactive material.

MALETSKOS: You have to keep in mind that there was one step beyond that, that you had to consider: that woman is going to develop a child[, and] that child is going to get some of that radioactive iron. The critical organ—if you want to, call it the critical person—was the *fetus*, not the woman.

FISHER: What were some of the ways in which the patient was informed of the study of the potential risks, or the benefits to her and to science? Do you recall any of this?

MALETSKOS: I don't know any of those details, and the reason for this was because the experiment was set up at Evans's level, and then I would eventually be notified. Evans would just keep an eye on [the progress] and I would constantly inform him [on research progress] and that sort of thing; or he'd get involved when we ran into a snag and what-have-you.

It was worked out in such a way (and I'm just guessing, because I wasn't there) in that there was total trust between the two [interacting research] groups, and in this particular case, it happens to be the Radioactivity Center and the Department of Obstetrics, let's say, at the Harvard Medical School and the Boston Lying-In Hospital; it could be the Radioactivity Center or some other group.

There was an intimate relationship at the top level and everybody really knew each other before they did any [research] because they would know each other for other reasons. And everybody was counted on to do their job; the physician was counted on to do the correct thing with respect to the [research] subjects and I personally do not know what that [researcher] did. I'm sure in those days that it wasn't as formal, anywhere near as formal, as we have it right now. But I'm sure there would have been a conversation between them [(doctor and subject)].

I don't even know who the females were, for example: I don't know whether they were all white, all black, [or] a mixture of everything. In Boston, everybody would come in, so I cannot even tell you that [(their race)]. I don't even know what their names were; they had code numbers so we would never know who it was, and that was it.

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[29] electrons or positrons emitted from an atomic nucleus in radioactive decay. Unlike iron-55, iron-59 emits beta and gamma radiation.

So, I'm assuming that it [(the research)] was done properly, but informally. And in those days, keep in mind, that physicians were looked upon by the patients with sort of admiration, if you want to look at it that way, that they were people that they would trust themselves to and that was the custom of the time; and what actually literally went on, there is no way for me to know at that instant. That was not my level of getting involved.

FISHER: Not being the physician in charge, you probably don't know about such issues as informed consent?

MALETSKOS: Not relating to these experiments.

FISHER: Is Dr. Gibson still alive?

MALETSKOS: No, he's been dead quite a while. Keep in mind, he was on that paper, he was the one who really knew about radioactivity and medicine and people and everything else. So Clement Smith, for example, learned a lot and had to do his part of the job, but it was Gibson who was really looking at the medical aspects of it, from the standpoint of the research.

Early Dosimetry Research (1940s to 1960)

GOURLEY: And your aspect was the dosimetry of it?

MALETSKOS: Dosimetry had already been done. But, I'm sure I did it on my own just for the practice of doing it, because we were learning how to do dosimetry. As I remember, all this business of new units were coming in[30] and everything else. The only thing you had back in the old days was this little old Roentgen[31] and that's it.

FISHER: Do you remember a Dr. Caton?

MALETSKOS: Yes. What's the subject of the paper?

FISHER: "The Circulating Red Cell Volume and Body Hematocrit in Normal Pregnancy."[32]

MALETSKOS: Excuse me, I gave you [some] wrong information. Caton was the gynecologist and obstetrician and Clement Smith was the pediatrician, because that was the paper that had the children in it—didn't it? Go back to where you read Clement Smith.

FISHER: Smith was on "Persistence and Utilization of Maternal Iron for Blood Formation During Infancy."[33]

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[30] The Roentgen was extended to the new concept of REP (Roentgen Equivalent Physical), a measure of absorbed dose to tissue after exposure to an external source of x- or gamma rays; it is now called the "rad" or "gray."

[31] the amount of radiation exposure in air required to produce one electrostatic unit of charge of either sign per cubic centimeter of air

[32] *American Journal of Obstetrics and Gynecology* 61(6):1207–1217; 1951. See OT-57 in the *Experiment List.*

[33] *Journal of Clinical Investigation* 34(9):1391–1402; 1955. See OT-60 ("Studies of the Metabolism of Maternal Iron in Newborn Infants Using Iron-55") in the *Experiment List.*

MALETSKOS: Yes, he was the pediatrician. See, that's a follow-up [study]. We knew that the infants would have some radioactivity in them, and [that] if we [used] sensitive enough [radiation detection instruments] we might be able to measure the radioiron in their red cells. And we could [also] see how that iron lasted over a period of time as dietary iron started to come in [(be ingested)]. And that's what we did.

We followed those infants for a year or two, whatever it was, and for the first three months or something like that, nothing changed [in the child's blood radioactive concentration]. This meant that the child had the iron that it needed in its own stores and was not getting anything significant from the mother's milk, if they were on that [(mother's milk)], or where ever the [(formula)] source was. It was only later on that you see this [iron] curve start to going down and being diluted.

FISHER: After being corrected for decay.

MALETSKOS: Yes, all that kind of stuff; everything and all that was done correctly; we always had standards along with [the subject's samples], so you were automatically corrected for decay.

FISHER: Did you just measure radioiron in blood in these studies? There wasn't any counting of the infant in, say, a small whole-body counter?[34]

MALETSKOS: We didn't have a whole-body counter in those days[35] and you couldn't measure the x rays very well.

FISHER: That's interesting to note for historical purposes.

MALETSKOS: We were doing whole-body counting with the radium subjects[36] at the time, but that was out in the open air.

FISHER: In an unshielded counter?

MALETSKOS: In a classroom. I did an awful lot of those measurements; they'd take all-day-long, and it's tough not only on the subject, but it's tough on the researcher.

GOURLEY: What makes it so difficult?

MALETSKOS: It takes so long and you have to repeat and alternate because you don't know what background [radiation] is doing during the day.[37] So, you have to keep repeating the experiment all over again, so that you could

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[34] an apparatus that measures radionuclides in man, using shielded detectors and multichannel energy analyzers

[35] The first whole-body radiation counter, HUMCO I, became operational at Los Alamos National Laboratory in 1956. The sensitivity and noninvasive nature of this instrument permitted studies at levels 10 to 100 times below established limits of exposure. It opened an entire area of clinical diagnosis and the development of new diagnostic methods.

[36] Robley Evans and his group were still investigating the bioeffects of radium on dial painters who had been working at watch factories in New Jersey and Connecticut.

[37] In the United States, an individual's exposure to background radiation averages about 350 millirem per year; the amount will vary with elevation and other factors. Daily fluctuations in the background occur proportionately with the amount of cosmic radiation striking the earth.

average out [the radiation readings] throughout the whole day and hopefully take into consideration whatever variations took place.

Radium Dial Painter Research (Early '50s–60s)

GOURLEY: So that was with the radium dial painters?

MALETSKOS: Those were the methods that Evans had already developed [to measure] the radium dial painters by and then that was all translated when we got a whole-body counter at MIT, which was one of the first ones [(counters)].

GOURLEY: About what year did that arrive?

MALETSKOS: Between 1955 and 1960. We went through a whole investigation of what materials to use and everything else because there were various kinds of soils that could be used to make bricks. For example: dunite bricks[38] are the ones that I remember that could have been used for a low-background walls and, therefore, would be a cheap way to go, and people could build it easily. But it turned out that good old-fashion battleship steel (pre-World War II) was the best way to go, and that's how we built it.

FISHER: During the early '50s, right at the time you were completing your doctoral degree, you were working with Robley Evans on a number of different studies—

MALETSKOS: That's right, I was in charge of the biology part of the whole Radioactivity Center and a number of things were going on; some of them were my own.

FISHER: You were measuring radium and radium dial painters.

MALETSKOS: That's right, both in terms of the radon[39] [level] that was released [in their breath] and the gamma ray[40] content of the body; so the sum of those two measurements was the total radium in the body.

FISHER: By this time, radium dial painting had ceased to be a practice, hadn't it?

MALETSKOS: Yes, the original dial painters were back in the late '10s and in the early '20s, and that's when the effect of the radium was observed by Harrison Martland, who was the medical examiner in New Jersey. Evans got involved in 1934 with somebody [visiting] him who was worried about radium being used in California; that was the beginning of Evans getting involved for the rest of [his] life with the study of radium toxicity.

Then when he came to MIT, he continued this interest, and slowly was making measurements on people [(individuals who had ingested radium)], as he could find them. After World War II, it was decided that this was really an important group of people [(the radium dial painters)]

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[38] bricks made of a dunite, a coarse-grained igneous rock composed almost entirely of olivine and low in natural radioactivity

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

[40] a highly penetrating photon of high frequency, usually 10^{19} Hz or more, emitted by an atomic nucleus; in this case, from decay products of radium

that had to be studied; here [they] had this radium in them for all these years; some of them were getting into [medical] trouble,[41] and some of them were not. What was the story and what did this mean for us in terms of setting standards?[42] [Being] in the war [effort, we] had to have a standard, because they had to make all this military equipment that had to have self-luminous[, radium-dial instruments], especially for airplane pilots and tank people and that sort of thing, where you could not turn the light on because then you'd be seen by the enemy. So, there was this big impetus to develop a [radium exposure] standard. The standard was set in 1940 with the available people that had been studied; a total of 27 [people with intakes of radium who] fell into two groups: [those exposed to an internal dose] above one microcurie,[43] [who] get in trouble [(become sick)], [and those exposed to an internal dose] below one microcurie, [who] don't get into trouble.

FISHER: You're talking about one microcurie [of radium deposited in the body].

MALETSKOS: Yes, internally, inside your body, integrated into the bone structure.

GOURLEY: That was a determination after they had been ingesting the radium for how many years?

MALETSKOS: Some of them were [ingesting radium] for 20 years or more; some of these people that were studied were people [who] had to be treated medically with radium. This was a big thing, to use radium for medicinal purposes; radium was going to cure, I don't know how many zillion problems. Some of them got large amounts, some of them got injections, some of them drank what was called *radiothor*; you drank it about once a month or something.

FISHER: Did you count any people who were radiothor drinkers?

MALETSKOS: Sure, half of our people had mesothorium [(radium-228)] in them; some of it was because they spiked the paint with it, and some of it was because they drank the radiothor and that sort of thing. That's why you'll see in the bibliography, the experiment that had to be done on the absorption of the thorium and radium, because until we knew what the answer on *that* was, there was an opportunity for us to lose half of our study group.

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[41] i.e., were experiencing the development of bone tumors

[42] See H.S. Martland, "Occupational Poisoning in the Manufacture of Luminous Watch Dials," *Journal of the American Medical Association* 92:466–473, 1929; and R.M. Macklis, "Radiothor and the Era of Mild Radium Therapy," *Journal of the American Medical Association* 264:614–618, 1990.

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

Fernald School Calcium Metabolism Studies (1948 to Early '50s)

FISHER: We'll get to that in a little bit and ask you some more specific questions about the radium and thorium. You were also involved in some controversial studies in the early '50s on calcium metabolism and uptake in man; was this using calcium-45 produced at the [MIT] cyclotron, or was this calcium obtained somewhere else?

MALETSKOS: Let's make sure about the use of the word "controversial." They weren't *controversial* at the time they were done; they became controversial in 1993[, when Secretary of Energy O'Leary launched her openness campaign to "come clean" about the alleged abuses of the past].

FISHER: That's what I meant.

MALETSKOS: I had mentioned that before I got [to] the Radioactivity Center in 1948, an experiment had been done by the [MIT] Department of Nutrition on the absorption effects of phytates on radioactive iron because radioactive iron was available; [it had been used] for the blood preservation studies.

Here was a situation where we wanted to know what phytates do to [iron absorption] because we're sweating out [a problem]; the world eats cereals left and right, especially the people who aren't very well-off [financially], and especially now in what we now call developing countries, and it could be a serious [malnutrition problem] if you're just holding the iron away if they happen to eat the wrong "cereal."[44] So they did the experiment. (I don't know who started it, but Evans and Bob Harris[, who] were in charge of nutrition, obviously got together at some point, and they both knew what they were doing, and I'm sure Bob Harrison said, "Boy that's terrific!")

FISHER: Can you describe this experiment?[45]

MALETSKOS: Well, I wasn't there, [but] I can describe now what the next step was, which is: later on, the same problem would occur with calcium, so calcium and phytates became an important thing to study.

When I came to the Radioactivity Center, this experiment had already been conceived and designed, and was waiting for animal experiments to be done. [These animal experiments] were being done at MIT because [we] didn't know what the dosimetry was going to be like [on humans] and how much you could give that would [be] consider[ed] safe.

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[44] Phytates are natural ingredients in grain cereals that were thought to interfere with natural iron absorption.

[45] In the early to mid-1950s, various radiation-related studies were carried out at the Fernald State School in Waverly, Massachusetts, using mentally deficient students as subjects. In a study addressing calcium metabolism, nine adolescent males, institutionalized for mental inadequacy but otherwise physically normal, ranging in age from 10 to 15 years, and one 21-year-old male participated as subjects. A second study addressed thyroid function in Down's syndrome subjects and their parents. Twenty-one male and female Down's syndrome students ranging in age from 5 to 26 years participated, as did 5 female and 2 male normal parents of these students. These studies were supported in part by the U.S. Atomic Energy Commission. For details and references, see OT-19 ("Radioisotope Studies at the Fernald State School, Massachusetts") in *Human Radiation Experiments*.

Nobody had ever done a calcium experiment before using radioactive calcium, calcium-45, which you could now get from the U.S. Atomic Energy Commission program; so all the initial work that was developed ([namely,] use of the students from the mental institution, any informed consent, the general design of the experiment; not the details, because that would come when you knew everything) had already been decided upon. So I was not involved in any of that part of it. I was involved in the dosimetry, the development of techniques for making the measurements and how to analyze the data eventually, and all this kind of stuff.

FISHER: These are important aspects of dosimetry; but do you remember how the subjects were chosen by the physicians?

MALETSKOS: I do know, because I was involved; there were criteria set for what the status of the subjects had to be. Obviously, you couldn't have a subject that was sick from something that had to do with the alimentary tract.[46] Is that what you are referring to? All of those considerations were all in there: how you chose which subject actually became a part of the group that was going to be in the experimental group had a lot to do [with them]; how easy it was for that [decision] to be managed; and was the subject willing and not afraid of the experiment—that sort of thing.

They were essentially young kids, you see; I saw those kids eventually, when I was invited to one of the Christmas dinners. This is what they called the Science Club Group, and it's been misinterpreted from the very beginning; I can't believe it! They were very nice young kids. My first reaction was, [when] I'd [visited] the mental institution in the earlier phases of it, and boy there were some sights in there that still bother me today.

GOURLEY: Like what?

MALETSKOS: Well, the kind of physical situation some of the people, some of them were plain skeletons, they couldn't move and everything else; it was really difficult to see, you'd see one person and you'd feel sorry for [him], [but when] you [would] see a lot of them[, it was very difficult].

[But with regard to the boys in the experiment,] there was nothing wrong with them. These were people that had to be in an institution like that and they were being cared for well, as far as I could see; the beds were cleaned, there were no smells around, at least in the places that I had gone to.

So as far as I was concerned that place was being run very well; now what was going on in detail, I don't know, and I don't know how they were chosen. But the kids that I saw that had been chosen [for the experiment] looked fine to me and essentially looked normal to me. It turns

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[46] the alimentary canal, a tubular passage functioning in the digestion and absorption of food and the elimination of food residue, beginning at the mouth and terminating at the anus

out that a lot of them *were* normal; they were just put in there because the[ir] families couldn't handle them.

FISHER: Were they considered a part of the Science Club because they had participated in the experiment?

MALETSKOS: It sounded to me like an afterthought; it *wasn't* originally set up as an enticing method; and remember, even today, there are ads in the paper inviting you to participate and stipends [are indicated], and I've seen one stipend that goes up to 3,000 dollars. Is that enticement or is that not enticement? The object is that you've got to pay this person for their time. But you don't want to overpay the person, to make sure that you [are not] forcing them, in effect, to come off the street [to jump at a rare chance to earn good money].

The [children] were picked—and it was an afterthought, as I gather—that somebody was talking about: "It would be nice [to do something for them because] these kids have been involved, we've had to jab them [with needles], and they had to eat a meal—every little drop of it, because you wanted to be sure they got 100 percent of the radioactivity— wouldn't it be nice to do something for them?" So to make them feel like they were special, they called it the Science Group, and the only thing they got was one meal, maybe twice in their whole career, outside of the institution at the MIT Faculty Club and as I remember, and they got a [small] present.

FISHER: So this was done at the MIT Faculty Club?

MALETSKOS: Yes, because it was easier to bring them there; you could control them and all this sort of thing.

FISHER: Did the parents receive any stipend?

MALETSKOS: No, there was no stipend in money; the meal was the stipend, and a little-baby gift [(i.e., a gift of small value)].

FISHER: You mentioned the payment of money for participation.

MALETSKOS: I mentioned that relative to current times; it had nothing to do with money back in those days. I was using the illustration [that] if you wanted to call this enticement, it's just as enticing these days when you have a stipend. You've got to remember that there are a lot of things that are going [on today] that are identical, only we think they're okay now.

FISHER: Did the parents bring them to the [Radioactivity] Center or did the hospital [provide transportation]?

MALETSKOS: They had already been brought there; they were already residents of the Fernald School.

GOURLEY: How did MIT hook up with Fernald?

MALETSKOS: I don't know; could have been a happenstance situation, for all I know, but I don't know. I literally do not know. It must have been done by Professor Harris that I mentioned already, and some contact who knew about the school, and then he eventually contact[ed] the superintendent

of the school. There was no way for the school to know that MIT wanted to do this.

FISHER: Do you remember if the parents were involved in this study?

MALETSKOS: I don't know anything about that. All I know is what's been found out since then, and we can talk about that later on if you want. Anyway, as far as I could see, things looked liked they were being handled well. Again, remember what I said earlier, the school was responsible for getting their subjects, and it was up to them to do that properly, it was up to MIT on its part to do everything properly in terms of the radioactivity and the handling [of] the venous puncturing and [associated sampling].

The Radioactivity Center's involvement was making sure they got the right amount of [radio]activity, which essentially fell into my lap, and that we had done the dosimetry correctly and everything else [related to measurements]. Remember, I mentioned there were animal studies going on. Well, those went on two years before Evans would give an okay to do this [experiment]. That just shows you how much care was involved in those days. Here the Department of Nutrition [at MIT] was champing at the bit to want to do the experiment and Felix Bronner,[47] who was going in to do his Ph.D. on this subject, was champing because he didn't want to stay there [in grad school] forever. And Evans [was] saying, "We [have to learn about the research for] a couple of years before we can make any decisions about how much [of the radioactive substance] we're going to give them [(the human subjects)]." Because [back then,] you didn't know anything about what the metabolism [of calcium] was.

GOURLEY: Was it, at the time, considered a metabolism experiment or a dietary experiment?

MALETSKOS: No, it was a *dietary* experiment as far as the children were concerned; the animals were rats: *that* was just an animal experiment. This was a dietary experiment on those subjects who are likely to get in trouble because they were growing kids [who drank] a lot of milk and [ate] a lot of cereal. Even at the institution they were getting a lot of cereals because it was a cheap food, but it was a nutritious food.

If you want to look at it from a broad scope, it was a benefit to them to know this [(about the calcium and iron uptake)]. It was benefit to the whole world to know it because the whole world eats cereals.

GOURLEY: How did the funding come in for this?

MALETSKOS: The funding [for] the Radioactivity Center was [from the] Atomic Energy Commission at the time and there was Office of Naval Research [funding as well], but I assume the bulk of it was from the AEC. (I assume I wasn't there watching the balance books as to how much money was [being]

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[47] Felix Bronner, Ph.D. (born 1921, Vienna, Austria), naturalized U.S. citizen, physiologist, and nutritionist who worked at MIT on his dissertation and later worked at the Rockefeller Institute, the Cornell University Medical Center, and the University of Connecticut.

taken out of each pocket.) The other side was Quaker Oats[, the cereal company], which was supporting the [nutrition] research, mainly the stipend for the graduate students and a few chemicals [reagents] and that sort of thing; the institution [(the Fernald School)] was supporting it in the sense that they were housing the [children] and taking care of them.

GOURLEY: I was looking through some of the papers over at the Center, and in Robley Evans's personal collection and I noticed some letters—

MALETSKOS: Where was this personal collection—you mean those bound books?

GOURLEY: The Robley Evans collection at MIT Archives.

MALETSKOS: That's the set that I have over here, I assume.

GOURLEY: I noticed that there was some correspondence and that sort of thing with the [United Nations] World Health Organization. Were *they* particularly interested in the results?

MALETSKOS: I don't know, because I don't recall any of that; I have no recollection about that, even if I did know [at that time]. But I'm sure, he [(Evans)] was involved in an awful lot of things because this Radioactivity Center was one of the sources of development of this whole field [of radiation measurements and application of isotopes in medicine and biology]. You have to keep in mind, keep it in perspective, [that] this was a real challenging era that people looked forward to as being a way to learn a lot of things that you were dying to learn but couldn't do it because there was no way to [do the research].

GOURLEY: Could you just name some of those things "Joe Q Public" wanted to learn that radiation experiments helped them learn?

MALETSKOS: They were doing absorption experiments; nobody wants to listen on how you used to do an absorption experiment [and] I'm not going to take a long time to do that experiment; it took one week.

To do that experiment the old-fashion way, you had to find your people [in] the same fashion and everything else, put them on a fixed diet that would essentially make them normal, hold them on that diet and measure every day—or not every day, but frequently enough for weeks and weeks and weeks—until they became stabilized. [And then you had to] give them another diet and do the same thing again for weeks and weeks and weeks; and there [would be] variations in the answers of both of them; and unless there was a big change, you didn't know what happened. In the meantime, you were collecting blood and excreta and you were doing chemical analysis on excreta until you were going blind.

FISHER: What types of samples did you measure?

MALETSKOS: We measured feces, urine, and blood; and, of course, the original material and the aliquot[48] that actually became part of [the administered radioactivity].

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

FISHER: Over what length of time were the calcium-45 nutrition studies [conducted]?

MALETSKOS: It would be a week's worth of collection—that's *it.* By then, we were giving so little [radioactivity]—now remember, the doses turned out—(I don't remember what the calculations are that I made, but when this whole "scandal" started in December of 199[3], I made a quick back-of-the-envelope calculation and I [came up with], like, 10 or 20 millirem). It turns out the average was ten millirem.

Back in early 1950s, we had good detection sensitivity, so that we could do an experiment and on those young kids give no more than ten millirem; pretty doggone good fundamental radioactivity physics and instrumentation. You can't do any better now, [though] you might do better with calcium-47.

FISHER: It's amazing that you were able to do this with the equipment and the instrumentation that was available. What did you use for calibration standards back then?

MALETSKOS: [A good] part of the [work at the] Radioactivity Center was [applying] nuclear physics. There were a lot of people [developing] methods of how you standardize, and they developed a technique of coincidence counting,[49] which you couldn't do with the calcium because it was a pure beta emitter. [It emits no alpha or gamma energy.]

They [(nuclear physicists)] calibrated a lot of different [nuclides] for us, and then we used the beta ray part of it, developed it, and then transferred it over to calcium. Meantime, I had someone working on developing a 4π counter;[50] no 4π counters [existed then]; and eventually we proved that we were all right, but that was the best that we could do at the time. [There were] a lot to 4π counters that people thought they knew [something] about and they didn't know about it.

As a matter of fact, the beta spectrum turned out to be an important consideration. I had a [colleague who] was doing physics; he was helping me with the student that was developing the 4π counter. And we saw what the problem was; he went back and started studying the beta spectrum.

FISHER: The 4π counter was used to [increase counting efficiency by counting low-level standards.][51]

MALETSKOS: [It was used to make the detection 100 percent [thorough]; you surrounded the sample entirely by the counter, and the only difference in the two halves of the counter would be the thickness of the source that

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[49] using a coincidence counter, a radiation detector that matches two simultaneous emissions from a single radioactive decay

[50] Spherical or 4 pi (π) geometry, in this context, refers to allowing all radioactive disintegrations to be recorded, giving the highest possible counting efficiency.

[51] The purpose of the four-π geometry was to improve the detection limit of the detector.

happened to be on one side. And, if you made your source properly, there would be as little [material or absorption] as possible.

Iodine-131 Thyroid Research (Early '50s); Additional Calcium Metabolism Studies on Elderly Subjects (Early '50s)

FISHER: Also during this period of time, you started doing some iodine-131 measurements in thyroids[52] in conjunction with the medical school. Do you want to describe some of this work?

MALETSKOS: The iodine work started probably about the time I got to MIT as a freshman. That was one of the first things that the [Radioactivity Center] did [in the biomedical field]. They made iodine-128 by just bombarding it with [a] neutron[53] source and use that in conjunction with people at Mass[achusetts] General Hospital to see how the iodine is handled [by rabbits] and how you could use it.

FISHER: And who did this?

MALETSKOS: A fellow by the name of Saul Hertz.

FISHER: Was he in the medical school?

FISHER: I think he had an appointment at the Harvard Medical School [and] at Mass General, and somebody told him, "Go see Evans and see what you can do." Evans had developed contacts through the radium work because we had physicians from the Mass General who would look at patients from a medical standpoint, the radium subjects, [for example,] and the message could have gotten through that way.

So they started in the early days to do whatever they could, but remember iodine-128 has a couple of hours or something like that for a half-life [(25 minutes)]; you couldn't do very much with it [before it's gone]. And so, you had to wait until the cyclotron came around [to develop longer-lived isotopes of iodine] and they tried to do some [work but] didn't do very much. And eventually ^{131}I came along [with a half-life of 8.05 days]; and there was a lot of work that was done with ^{131}I. We had to develop the techniques for measuring it, the same thing all over again. [It was] a brand new ball game with a new radionuclide, and an awful lot of work went on with all those studies.[54]

FISHER: Do you remember what the source of the iodine-131 was?

MALETSKOS: Yes, ^{131}I—every radioactive material that you couldn't produce on the cyclotron came from Oak Ridge,[55] in those days, through the AEC.

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

[53] an elementary particle found in the nucleus of most atoms and having no electrical charge

[54] Radioiodine (^{131}I) is still a highly effective therapy for hyperthyroidism, Graves' disease, and thyroid cancer.

[55] 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 (continued...)

That's why you had to go through the equivalent of a human use committee at [the] Washington Headquarters of AEC to get approved in the first place.

Then you got permission by [submitting a] separate application, to get the radioactivity from the Oak Ridge National Lab [(ORNL)],[56] [which] sent [it] to you; they wouldn't give it to you unless you had all the right facilities in the first place and you demonstrated your expertise that you knew how to standardize and do all the various things that were required. We didn't have any trouble [at] the Radioactivity Center because we were *developing* the techniques. But this was a general program throughout the country.

FISHER: What were the major isotopes being produced in the early '50s at the Radioactivity Center?

MALETSKOS: By the cyclotron, we were making zinc-[65], and we started to do a lot of work on leukocytes[57] at the Harvard Medical School. I don't know if they're in there, as such; and there was some cobalt work, I had to develop techniques for cobalt, but they never developed [into much research].

FISHER: Cobalt-57?

MALETSKOS: Yes, it would have to be cobalt-57. But eventually cobalt-60 was used, once I developed the techniques for doing cobalt.

FISHER: In 1956, you were third author on an interesting paper, and the title is "Studies in Calcium Metabolism: The Fate of Intravenously Injected Radiocalcium in Human Beings."[58] First author is Bronner, second author is—

MALETSKOS: Yes, he was the doctoral candidate that did the absorption studies.

FISHER: Right. Do you remember the injection of human subjects with calcium?

MALETSKOS: These were the older people and the Fernald School.

GOURLEY: Older *students* or—

MALETSKOS: —they were not students, but the [organization] was called a school.

FISHER: Older patients, subjects, mental patients?

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55 (...continued)
assumed control of these facilities upon its creation and, today, they belong to the Department of Energy.

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

57 white blood cells

58 F. Bronner, R.S. Harris, C.J. Maletskos, and C.E. Benda, in *Journal of Clinical Investigation* 35: 78–88; 1956. See OT-19 ("Radioisotope Studies at the Fernald State School, Massachusetts") in the *Experiment List.*

MALETSKOS: Yes, mental patients, subjects.

GOURLEY: I just wanted to be sure it wasn't elderly people.

MALETSKOS: While that was in progress—because there was a whole series of test meals, you know, it took a period of about a year or two to do the whole absorption experiment. I was involved in studying calcium metabolism because [it] was the analog for radium. [It metabolized in the same way.]

The Nutrition Department was interested in the metabolism of calcium, because it was something that [they] had to know how to handle and everything else, and a lot of work was being done on bones and teeth [because of the radium dial-painter studies]. While I working with animals, the Department of Nutrition decided, "Well, we better see what we can do about studying the metabolism of calcium by actual injection [into human beings]." And so that's the consequence of that paper.

FISHER: In the 1990s, readers may not be familiar with what the rationale would have been for choosing mental patients as subjects of a metabolism study, particularly one where the activity was injected intravenously. Can you recall how those subjects were chosen?

MALETSKOS: No, again because I [was] not actively involved in that part of it. There are different ways to look at it. People can look at it [(the use of these subjects)], read it in a bad way. Let me put it in context. Once you find the source [of people to participate in an experiment], then you tend to go back to the source because you've already done all the [lengthy administrative] machinery leading up to getting the permission to use people who are involved in that organization.

So, I'm sure that sort of thing was playing in[to the decision to] go back to the Fernald School to get other subjects. Because in all of these experiments, you have to have control of the subjects. You just can't let them walk around; you have to collect 100 percent of the excretions, you have to see that they're eating properly, and all this kind of thing[, including the maintenance of a stable metabolism]. Unless you do it that way, you're not going to have a good experiment.

Ideally, you put them in a clinical research center; but there was no research center of that kind around. MIT was designing one and building one, but it wasn't available [then]; so [instead] you go to a place where you could control the people. I know that even with researchers themselves, who might have been subjects in other places ([I know] because I've talked to them), they failed even on their own to go collect that particular sample, just because they were in a rush. You can't afford to lose samples if you're [going] to collect and [account for] 100 percent of the radioactivity out of the body that went into the body.

So that's probably why we went back there [to Fernald], and the concepts [involved] were identical: nothing was going to be seriously done to these people, they were just going to be injected and we were going to collect samples. We had the experience of doing [experiments] with little kids, then these were older people, and therefore if you're going to

give them about the same [radioactivity] per kilogram of body weight, the dosimetry would be identical.

FISHER: Did you go to the school to collect samples?

MALETSKOS: The samples were taken by the people [from] the Nutrition Department; they took them and they "digested" them. We ended up getting a clean solution, and we took over from there to make our samples for measurement. We precipitated[59] them as the carbonate [for] urine, feces, blood, and the original aliquot.

FISHER: What has become of Felix Bronner?

MALETSKOS: He is [a professor] emeritus, I believe, at the University of Connecticut and he's still there, or at least he's associated with them in some fashion.

Iodine-131 Research and the Fernald School (Early to Mid '50s)

FISHER: Interesting. We'd like to move into the area of iodine with the Fernald School.

MALETSKOS: This was a situation in which the Director of Research, Clemens Benda,[60] had a theory [about] people with myotonic dystrophica.[61]

FISHER: What is that?

MALETSKOS: It describes the symptoms [in which] you cannot relax your muscles. But it's a very complicated disease that involves brain, muscles, body, and everything else. These people don't live very long, and it's a very tough thing to see them with it. Everything they do is peculiar because they can't relax their muscles; they shake when they walk—it's a peculiar walk because they can [contract muscles] in one direction easily but they can't do the relaxing part of the muscle.

[Benda] felt, on the basis of a lot of previous work, that this was probably related, mainly, to a deficient thyroid, because pathological studies of the thyroid indicated that it was not a normal thyroid. So when [radioactive] iodine came around and you could make measurements of the metabolism of the thyroid, you say now, "Here is another opportunity to see what we can [do]."

Now this is a medical problem, [and to start finding a solution, you] start with a physician who is in charge of these people, just like you would do today. You go to your physician, you got something wrong with your thyroid, or you feel shot [(fatigued)] or you're hyperactive or whatever it

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[59] the act of separating a substance in solid form from a solution

[60] Clemens F. Benda, M.D., a physician specializing in psychiatry, had an appointment as a professor at the Harvard Medical School. He was Director of Research at the Fernald School. He is now deceased.

[61] a rare, slowly progressive, hereditary disease transmitted as an autosomal dominant trait, characterized by myotonia (lack of muscle tone) followed by atrophy of the muscles (especially those of the face and neck), cataracts, hypogonadism, frontal balding, and heart abnormalities. Also called *myotonic dysrophia* or *myotonic distrophy.*

is, and your physician suspects that there is something wrong with your thyroid, and now you're going to get a tracer dose of radioactive [iodine].

FISHER: As a diagnostic?

MALETSKOS: As a diagnostic, exactly.

FISHER: What was the outcome of this?

MALETSKOS: He [(Benda)] came to the Radioactivity Center because he knew [us by reputation]. He was involved in all the original planning of the previous experiments, because that was one of his responsibilities, and he was the one who was arranging all the permissions and what-have-you.

He said, "What can you do?" I said, "You're coming about at the right time: we've just finished developing a device that has a circle of sensitivity that is large instead of very small, so that subjects won't have to sit stiffly for the [entire] counting time, [however long] it might be. And, it would be ideal for your people [with myotonic dystrophica] because they can't sit still, and this [device] is going to go over to the Beth Israel Hospital [in Boston]."

[An earlier version of this device used] four G-M [(Geiger-Müller)] counters set up in four quadrants, [but the] sensitivity wasn't very [high] with the G-M counters. [In the newer version,] we had converted to sodium iodide crystals [as the detectors], and by adjusting the controls and so forth, you could get a plateau, where you'd have a large region of [equal] sensitivity and high sensitivity, because it's now a sodium iodide crystal instead of a G-M counter. [The crystal was equally sensitive from center to edge.] It was described in the paper as the first time.

The news media made a big deal that we were experimenting on these guys [in developing] brand-new equipment; they implied that [the new equipment] didn't work. Everything was done in the proper fashion and, I didn't know what had gone on in terms of what he did for informed consent.

FISHER: When you say "he," was this Benda?

MALETSKOS: Yes, Benda. But you know in a diagnostic situation, outside of telling the subject that you're going to get this [material], which you did, you don't need to go through the informed-consent process in a really formal way.

FISHER: Benda's initials were C.E.

MALETSKOS: Clemens E. Benda. When the task force that was set up in Massachusetts to review what had gone on back there, they found out that everything was done perfectly for this experiment. He went, not only through us, he went through the AEC and all that [regulatory] part of it; that part was done correctly.

But he [also] went to the Department of Mental Health, to the Advisory Committee on Research or whatever it was called, and he had to go before them, and they gave him permission to [do] this so that Massachusetts was giving permission for him to [do] this. Then all these people got excited

about the fact that this was bad[ly] done and this, until they found these things [(proper procedures) were followed,] that shut them up.

And the doses that were given were 50 microcuries [of iodine-131], as I remember, [to] a subject because we didn't know what to expect. We didn't know what to expect with the hyperthyroid [situation] when the standard dose was already 100 microcuries, so we gave half of it instead of the regular one. We wanted to be sure that we gave enough, because you don't want to reuse a subject unnecessarily, and this [was a case of being] blind on the disease that you don't know what the thyroid is going to be doing.

FISHER: This was before you had good imaging capabilities: you simply counted the activity in the thyroid.

MALETSKOS: That's correct. That was the standard way to do thyroid diagnosis. You measure how much [radioiodine] went in—we called it the uptake—and it was an uptake [at] 24 hours and maybe at 48 hours. That gave you enough information on the basis of the previous other studies of patients that had both normal thyroids, hypothyroids,[62] and hyperthyroids,[63] and so you knew where people would normally fall [in the distribution curves].

FISHER: It was a range of normals in-between "hypo" [(too little)] and "hyper" [(too much)].

MALETSKOS: That's correct.

FISHER: Simply by a number of counts.

MALETSKOS: That's correct. And it turned out in this particular case, everything was done according to Hoyle[64] by today's standards, let alone back [then]. It really bothers me [about] people [when they] make a big noise before they know the facts; they didn't find the equivalent [permissions sought] for the calcium-45 studies.

FISHER: When you say equivalent you mean [what]?

MALETSKOS: All these informed consents and everything else and the approval from the Massachusetts State and everything else. All this record business was not found; whether it existed or not, I don't know. And if it didn't exist before, I don't know what made them do it [correctly] out of the clear blue sky for the ^{131}I study. They tracked it down, and there is nobody who can say a word about it now on the basis of what I've seen [from the Massachusetts report]. Everybody was worried that these were high doses and everything else; it was *half* of the standard dose back in those days.

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[62] deficiency in thyroid secretions, resulting in goiter, myxedema (thickening of the skin, blunting of the senses and intellect, and labored speech), and, in children, cretinism (stunted growth, deformity, and mental retardation)

[63] overactivity of the thyroid gland, resulting in increased metabolism rate

[64] "according to Hoyle"—an expression or colloquialism that means "done properly." Edmond Hoyle (1672–1769) was a British writer on games and who prepared an encyclopedia of the rules of card games.

FISHER: Were these patients with myotonic dystrophica found to have abnormal thyroids?

MALETSKOS: It turns out that even though pathologically you see lots of colloid,[65] and you would normally suspect that they were highly hypothyroid. They turned out to be metabolically, from a radioactive standpoint. Therefore from the standpoint of the actual development of thyroxine[66] and everything else, that they were absolutely normal. From that standpoint, if [the] thyroid is a problem [with this disease] it's not obvious from the radioactivity studies.

FISHER: That's interesting.

MALETSKOS: It is fantastically interesting; and that was the beginning of a change in philosophy as to what the roots of the disease are. Clemens is one of the big sponsors of the concept of thyroid at that time; he wrote a lot about it, and there were other people, too, that agreed with him, but not everybody.

This was an opportunity, it was the right thing to do from his standpoint, when the radioactivity became available and all the research had been done, so that you would know what the result would mean. So it isn't a case that you were experimenting with these people: this is a straight—as you said earlier, a straight diagnostic test. And, it should not have even been classified as being experiments on human beings; it wasn't an experiment, it was a diagnostic test.

FISHER: Was the iodine-131 from Oak Ridge?

Robley Evans's Role in Experiment Oversight and Funding Information

MALETSKOS: Yes; and all that paperwork is perfect. The [Massachusetts investigators] found all that, and I knew it was perfect because Evans wouldn't sign his name to anything that was not perfect.

FISHER: Did Robley Evans have management oversight over such things as which type of experiments were appropriate versus those that may not have been considered appropriate? Did he make [those types of] decisions?

MALETSKOS: Yes, from two standpoints. One is: no matter what the experiment was, it had to be a collaboration between equals. The Radioactivity Center was not a service organization, and the experiment had to be something [done] on the basis of what your knowledge was at the time you started, that it was really going to make a contribution and that it could be done. And he [(Evans)] had management control during the execution as well on his part of it.

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[65] a substance in the body, as a stored secretion, that is a colloidal suspension (a suspension of minute solid particles)

[66] a hormone of the thyroid gland that regulates the metabolic rate of the body; *also:* preparations of it used for treating hypothyroidism

FISHER: Even back in the '50s, he was a very respected scientist.

MALETSKOS: That's correct, even before that. He was like a mecca: If you were involved in something that you thought was going to involve radioactivity, you considered coming over there [(to the Radioactivity Center)] to learn. You could be coming over there and be a temporary fellow or the equivalent of it or something like that. You could get your hands dirty working with people right beside the working people and you were treated as a worker, only you weren't getting paid; you were just contributing.

You were learning, and you would be given more and more responsibility; and eventually, after you stayed there for six months or something like that, you would have a pretty good grasp of what it involved, and you could probably go back and—with a little bit of extra help from somebody else—get yourself going. Alternatively, you'd come in with a nice interesting problem and the problem would be a joint problem between that person's organization and the Radioactivity Center at MIT.

FISHER: During the early '50s, what fraction of the Radioactivity Center's funding came directly from the AEC? Was it most of it?

MALETSKOS: No; I don't know, but I would say probably half.

FISHER: And the other half coming from grants or—

MALETSKOS: There was the Office of Naval Research, and I don't know when ONR went out [of business]. Then there were other grants and all that kind of stuff. And there were specific grants to do specific research for graduate students, and all this kind of stuff.

FISHER: Did the medical schools provide support for their own studies for the Radioactivity Center?

MALETSKOS: Yes, if it was a joint [study] with a group from a medical school, the medical school supported its own experimenters through whatever funding they had; some of it could be directly from the AEC as well, if it involved radioactivity. But that would be because of a separate application.

Even in those days you didn't just do the diagnostic tests: you had to go through all the formalities, as well. You had to be able to establish to the AEC that you had the capability of doing this work: doing it safely, and also knowing that either you could standardize the radioactivity yourself, or you had a source of standardization that would be acceptable. You just couldn't just say [that by having the correct aliquot of the starting solution] you probably got the right activity.

Experiment Safety Protocols, Clarified (1950s)

GOURLEY: We've gone through some of the layman's terms of what the purpose of and what has been learned from some of these studies. [For example,] the muscle disorder study showed that the thyroid wasn't involved. We started out the discussions with the blood preservation: Increase the preservation from one week to three, I think you said, but, I didn't quite

follow, in layman's terms, what was learned from the infant study and the pregnant women. I know it had something to do with the red blood-cell volume. Could you elaborate on why that's important?

MALETSKOS: Blood volume changes with time [during] a pregnancy. Also, the woman is starting to provide all kind of nutrients for that child. You'd want to know eventually by a hematocrit sample, if you're suspicious that there may be a problem with blood volume.

GOURLEY: What problem with blood volume?

MALETSKOS: I don't know; I'm not an obstetrician. I can't tell you what to look for, I'm sorry. What you want to know is whether there was a sufficient blood volume. Remember, the woman's body is completely changing in [her] metabolism to support this child, and it[, itself,] has an increased circulation, which means you have got to "fill the pipes" [with blood] and you got to be able to generate enough materials to fill the pipes. But you can't fill the pipes only with just plasma: you've got to have enough red cells there to carry oxygen. So, you've got to know that.

Now you don't go around taking a woman apart to find out how much blood volumes she has. The only thing you could do is measure [the] hematocrit. So in the end what we did was determine what the body hematocrit was from these measurements and related it to the venous hematocrit. I can measure [that] hematocrit, which is the ratio of red cells [in] the whole body, to the whole blood volume. And so you know what's going [on] on that basis, and if it's out of line, it could be out of line both ways, it can be [because you don't have] enough [blood,] or [because] you've got too much, and then you've got too [much] pressure.

GOURLEY: Was there any way to diagnose this at all before these studies?

MALETSKOS: No: you just hoped—you could diagnose only the plasma. Remember, I said by the dye [(tracer)] technique, this experiment is identical, only it is nonradioactive: it's just an ordinary dye, a blue dye. The other one is a radioactive material. You can't you see the radioactivity [with the naked eye], but you can measure it. But the concept is identical in both.

[To do this technique,] you get some material, you dilute it, you measure the dilution, and you can calculate what the [whole blood] volume is. And so, once you've done the experiment on a few women, (these were normal women, as far as everybody knew), then you could relate [your findings to] the hematocrit that you could get from an ordinary blood sample. [That is done the same] way you draw blood from a person under normal circumstances and relate it to what it may be going on in that woman if you're suspicious of something going on.

You also learn how fast they [(the pregnant women)] come back to normal [blood volume levels], because we follow[ed] them after birth. And that's important to know, too, because you may have problems; I don't know all the problems you could run into [because I'm not an ob/gyn]. So that experiment provided the information that the obstetricians wanted to know, relative to blood changes.

That was the definitive [experiment]. I don't know that it's ever been done again. It was done well; it was done with sufficient accuracy and well-executed, and well-analyzed, and well-described; and there it is for posterity. Keep in mind, what I'm saying right now is [that] everybody takes radioactivity for granted from the medical standpoint. (You go into the hospital and a doctor says you got to have a tracer or dose of this, you usually say, "Yes.") You don't think about [it], you don't bat an eyelash.

You [could] do that because this work was done back in those days; that's why I said it's a tremendous era, an unbelievable era of having such a potent new tool and used so well over this period of time, irrespective of what you hear in the newspaper. There very well may be some bad instances, but there [are] always bad instances, [in] everything, and you can't make generalizations from a few to the whole thing.

And the gain, and the results, and the information that we've gotten is just totally unbelievable, and [it] spread through all science, through all medicine throughout the whole world. And not only at the Radioactivity Center. The Radioactivity Center was one of the stars in this; work was going on [at] Berkeley and maybe two or three other places. The Radioactivity Center came [as one of the] first because [starting in 1942] they even had the cyclotron [right there at MIT] to use directly for this sort of stuff.

GOURLEY: You mentioned bad instances and, you know there has been this whole big uproar in the media and Massachusetts. Can you think of some examples where you saw some work and you thought, "Gee," [and] you kind of shook your head and said, "No, no, no, that was [conducted in the] wrong [manner]?"

MALETSKOS: No, [in] my personal involvement, I have not seen anything done incorrectly.

GOURLEY: With others? Others' work?

MALETSKOS: Again, it's all hearsay. You can talk about it; [but] I'm not ready to. I have a talk that I give, and I mentioned some of these things, and there are instances where some of them *look* bad, and are *not* bad, once you look at them. Also, it depends on who's looking. If it's [Energy Secretary] Hazel O'Leary looking, everything is bad. That's on the record [for the transcript of this interview] on purpose, and, I'll say it that way.

FISHER: Costa, do you recall the amounts of radionuclides chosen for ingestion or injection at a level that was low enough that were considered not harmful, yet high enough that it could be measured with the equipment that you had? Were those the basic considerations?

MALETSKOS: Yes. The rule is, you give the least amount of radioactivity that you can, but you don't want to make it so low that you [ruin] the experiment and use radioactivity on somebody unnecessarily. You have to be able to measure it, so it cannot be the least—the least is zero—and then you don't have an experiment. It has to be enough so that you can do the experiment well. Once you try to aim for that, then you ask yourself, "Can my sensi-

tivity do it right now?" [("Can my instruments measure such a low amount of radioactivity accurately?")] And there were instances where we said, "No, we can't do that now, we've got to work more on it."

When we come to that big experiment on the absorption of radium and mesothorium, we didn't know that we could do that; I didn't know that I could do that safely. I had to go through a tremendous amount of work ahead of time to find out where I stood, and then eventually, by doing the experiments—even on dogs—to find out if the system worked. And [if] my ideas worked, [then it] was possible to say, "Maybe now we can do it on human beings, and we could do it safely, and it can stand [up against] anybody's scrutiny that's going to be involved in reviewing this experiment before we do it."

That's a good classic example of taking a long time to do the job well. But there were many people who would come in and say, "We want to do this and we want to do that." And the answer would be a flat "No, because you could do it by other [nonradioactive] methods just as well, and we'll give you two suggestions." A lot of people went right out the door just rapidly [in a huff] that way.

For other people, we would study it, because it looked like an interesting thing to do, and find out either you could do it [or not]. If we had the time we would get involved, and if we didn't have the time we'd say, "You could do it, but you'll have to go someplace else." Or if it looked like it could do it and it was really worth doing, then [it] was worthwhile spending time to get the sensitivity [of the instruments] down low. And so you got it low enough so you could make sure that the dose was low enough.

FISHER: What you're saying then is that there was a driving philosophy within the Radioactivity Center that when radioactive [materials] were used in humans, [the amounts used] would be as low as possible for radiation protection?

MALETSKOS: It was two-sided; when you're using human beings, what you've said is correct. The general [thinking] was to use as little as possible, in general, because you can get contaminated; and why get contaminated with a lot of material instead of a little material, if something should go wrong? Evans deserves great credit for maintaining that philosophy from the very, very, very beginning: "We do everything well; we do it with as low a dose as we can; and we do it with minimum difficulties to anybody that is involved, especially by human subjects." That is carried through with all his students and all the people having been involved with him.

FISHER: On the studies involving pregnancy and fetuses, do you recall discussions on radiation safety aspects of protecting the fetus?

MALETSKOS: What we did was try to get the lowest amount to the mother and see what we could guess with whatever knowledge was there, at the time, as to how much radioactivity would get into the fetus and what the dosage would be to the fetus.

FISHER: Was there any concern about the effects of low-level radiation on the developing embryo/fetus?

MALETSKOS: Not with the knowledge you have now, but with the knowledge that we had back then. If we had decided that we wouldn't want the fetus to get this amount of radiation, whatever the radiation dose was calculated—(I don't even have the slightest idea what the numbers are right now)—if we had decided that the fetus was going to get too much, the experiment would not have been done. We would have worked our way around and tried to get higher sensitivity [in our instruments], or something else. It would not have been done until it [(safety to the developing fetus)] was adequate.

FISHER: And what was the basis that you had for deciding whether a certain value of administered activity was safe or unsafe? What standard did you compare against at the time?

MALETSKOS: In the early phases, only background [radiation. At] the time we were doing some of these experiments, more work had been done. We had changed from the Roentgen to the REP (Roentgen equivalent physical) [as our measure of absorbed dose to tissue], and so we knew a little bit more and therefore we could use whatever information we had. And you didn't do anything differently then, than you do now: you used what information you had [in] the best way that you could to make a judgment. It was, if you want to call it, the best-informed judgment you could make at the time. It certainly was discussed and it was certainly considered, and it was certainly an option not to do [the experiment].

FISHER: Were you part of those discussions?

MALETSKOS: Sure. Every time I was involved in an active fashion and it was a starting [(new)] experiment—yes: as a matter of fact, you brought in everybody you could. You wanted to make the best decision possible, and it would be [made] at different levels. It might start with me, for example, and then go to somebody else, and eventually it would work its way up to Evans. Certainly for people in-house, it would go through the whole routine that we could talk about it, review it at MIT.

It was not a simple thing. MIT is great in many respects, in terms of taking care of people, whether they were employees or anything like that; we had the first human use committee ever at MIT.

FISHER: About what time?

MALETSKOS: I don't remember. It was between mid to late '60s. I forgot—

FISHER: About the middle '60s, '67. The first human subjects committee at Oak Ridge was about 1965 or '67.

MALETSKOS: They [may] have been close, but [I] remember it being the first one, but I can't prove that. I could find out, but I just don't happen to know. Are you finished with your layman's terms?

GOURLEY: Yes, you explained that nicely.

Radium and Thorium Ingestion by Human Subjects (Late '50s to Early '60s)

FISHER: You were concerned about the gastrointestinal absorption of radium and thorium in the late '50s, early '60s, and you prepared some materials for human ingestion. Can you talk about the ingestion of radium and meso-thorium ingestion by human subjects?

MALETSKOS: Okay, [but] tell me if this is too detailed as I go along.

We were interested in it long before that [experiment]. We were interested in what—we finally realized that we couldn't pay attention to radium-226, which is what we thought was the only radium [that] was in the body, but actually [there's] radium-228, as well. That was known way back, even back in the days when Harrison Martland, the medical examiner of New Jersey[, where some of the dial painters worked]—he knew about that and I'm sure even Evans knew about [it], but we had forgot about it. What started the whole thing was is that in the group of patients that we had at that time.

GOURLEY: These were dial painters?

MALETSKOS: These are the people who got radium in them however they got it, that we were studying. They fell into two groups: people who got very sick and people who didn't get very sick. And [populations] don't [usually] do that, [divide into two groups of] people, [one of which is] ten times more sick for the same thing at the same stage.

So we decided that there had to be some other agent or something was going on; we didn't know what it was. We went through all kinds of theories where they [(the less-sick group)] did more exercise and their bones were better, or bone metabolism turned over[67] [more], and all this sort of thing.

Finally, after trying a million things, we find that, somebody recalled that there was radium-228 in there. Radium-228 had a short half-life [(6.7 years)] as opposed to the long half-life of radium[-226 (1,620 years)]: it [(radium-228)] died away quickly, and we would not measure in our usual measurements what [amount of this] radium is in the body. Therefore, we did not know what that radiation dose was; it could be quite high. [So] we did not know the effect of the radium-228 contain[ing] thorium-228, which is its first progeny [(daughter isotope)] in the [watch dial] paint itself.

So, if you ate that paint[68] or got it into you as radiothor and the thorium itself was absorbed well, then you got a dose from the thorium[-228] and

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[67] in this context, the replacement of bone mineral with new deposits

[68] The radium dial painters usually "tipped" their brushes to a fine point with their lips, and inadvertently ingested some of the radium dial paint.

its daughters that you would never be able to measure [because] its half-life [(1.91 years)] was even shorter than the radium-228. Then thorium-228 would build in from radium-228 as you metabolize radium, but *that* you could account [for] because you knew that was part of the radium-228, and you could measure that.

That initial dose represented a long, big dose. If you couldn't measure that, then we [would] not be able to use half of our subjects, which is a real loss because you don't have that many subjects to start with. So it was important to find out, and we knew way back then that we had to eventually do an experiment. Well, we talked about it, we talked about how we might do it, but you know it was planned in stages over quite a number of years, and then it fell to me to be the one to set it up.

Now what we had to do, the end experiment had to be an experiment in human beings. We didn't want to know it in animals because we wouldn't trust [the result], it had no bearing directly.[69] We were doing it [(the experiment)] in human beings and [planned to] give them mock radium paint, because this is where the bulk of the mesothorium [(radium-228)] and thorium-228 would come from. We would give them short-lived radionuclides of the long-lived counterparts. We chose thorium-234 [(half-life: 24.1 days)] for the thorium and radium-224 [(half-life: 3.64 days)] for the radium.

Then we had to determine how to prepare these things; we had to determine how we were going to do the experiment to make these, and actually then try them out—through a lot of physics and chemistry experiments, first trials, and that sort of thing. Then a whole rehearsal with dogs, and then finally make a decision as to what is allowed to give [the human subjects] and this sort of thing. Then [you had to] go through the whole routine of informed consent and choice of subjects and everything else; and then actually execute the experiment.

The first thing you had to do [was decide if] you could even *get* the radionuclides, because you don't go out and buy these; you don't get [them] from the AEC. These are naturally occurring radionuclides and there is only one way to get them: to find some natural source that has them in them and in enough quantities so that you can do something with them. It was very fortunate Robley Evans had a vial of [thorium-228], which we could milk to get the [radium-224].

The development of that procedure was long. We had to develop iron exchange procedures for separating it and everything else and getting it into final pure form. That, in itself, took a while. But the [thorium-234] was a tough one, because the only [adequate] source of [thorium-234] that we had was uranium slag, which comes from the melting of uranium, which is the material that floats to the surface and then hardens,

[69] Animals had been suspected, and were later confirmed, to metabolize and excrete radionuclides at rates that differed, often substantially, from the rates in humans; animal metabolic rates are usually higher than man's.

and it contains the bulk, if not all, of the thorium-234 [that is in uranium]. But, that's [not a pure metal but] an oxide, absolutely nearly insoluble to start with. This was available to us from the [Army's] Watertown [(Massachusetts)] Arsenal, which was making all kinds of natural uranium and depleted uranium configurations of it for military purposes or whatever they [did]. They may have even been making the bullets [(depleted uranium projectiles are used for penetrating battle armor)] for the missiles for example, for all I know.

So, we started off with that. I developed some ideas and I got a radiochemist Ph.D. student on the job and we worked, and worked, and worked, and saw that we were going to be successful on that. So now we knew we could go to the next step.

Now, remember: we were going to be doing an experiment, a major experiment on human beings at the Institute, which is not a medical school. My decision was, everything was going to be done according to Hoyle and *beyond,* all the way through [to the completion of the experiment,] so that there would be no arguments about how we do it.

We would have to use only our internal physicians for the actual injections, and that's all I needed. After that, the oral[-dose] part, we could give it ourselves and that sort of thing, so that would be it. The problem now was, the thorium-234 had very low-energy gamma rays and the radium-224 had a whole series of gamma rays all the way up to 2[.6] MeV [(million electron-volts)] or something. The thorium absorption, we expected from everything that we knew; and in relation to plutonium, for example, which is an actinide[70] also, [it] was going to be very low, and that is what we hoped would happen. And the radium absorption was going to be very high.

The consequence of high radium absorption with lots of gamma rays and high-energy ones was to produce a gamma ray spectrum, at the lower energies of what we called the "Compton"[71] part of it, so huge that we might not ever see a peak of the thorium (gamma ray) in this high background. And [so] the question was, "Is this a 'doable' experiment?"

So we started to play with whatever we could get that would be high- and low-energy and so forth, and see how we could work it out. I decided that we had a good chance of doing it, but to make these measurements on a human being, we had to do it two ways.

We wanted to do everything—the experiment could not fail from an operational standpoint. Once you started the experiment, it *had* to work, or else you spent a tremendous amount of time—we're talking about a year or two going down the drain if you didn't do the experiment correctly—so we made every conceivable measurement that was possible.

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[70] an element of the actinide series—the series of mostly synthetic radioactive elements whose atomic numbers range from 89 (actinium) through 103 (lawrencium)

[71] named for Dr. Arthur Holly Compton, University of Chicago, a key member of the scientific team that established the Manhattan Project

For example, we would take all the blood samples we could take, we would take all the excretion samples we could take.

When it came down to making whole-body measurements, we would do the critical ones but we would do everything else, we would learn everything we could, because we would consider it an absorption experiment but it could also be a metabolic experiment: [it] could also be a method of verifying the methods of counting that Evans had developed—that [meter-]arc method where you could do it independently of a phantom[72] and everything else, and all this sort of thing.

Everything was plan[ned], and the logistics of doing this eventually turned out to be hairy, real hairy. We developed the procedure for getting the radioactive material, and we had to get somebody that could handle that radioactive material that would check for pyrogenicities,[73] so that we could inject it. It doesn't make any difference how perfect it is from the standpoint of swallowing it—you wanted it to be good [(safe enough to ingest)] anyway—but if it's good enough to *inject,* it would always be good enough to give it for swallowing purposes.

So, one had to develop a technique of [determining] how much radioactivity you needed when you were ready to do the experiment, so that by the time you made this material [(radium-224)], which had a short half-life [of], like, three-point-four days or whatever it is [(3.64 days)], there was enough material for you to end up doing the experiment over the period in which it was being tested for; whether it [contained] pyrogens[74] in it or not, which takes about ten days at somebody else's laboratory.

This is a process that has to be done for anything that you put in the human body, so that you don't give people fever or infection or anything else. You had to filter it to get the bacteria out and show that there were no bacteria. And you had to do tests in rabbits and see if you give rabbits fever or not. And it takes a while to follow them and so forth. This is a fixed period: whether you like it or not, it's the last thing you do before you do the experiments. So we went through all [those] techniques and found that [they] could work and so forth.

In the meantime, we were developing measuring methods. What we wanted to do was to measure what is in the body in such a way, right from the very beginning, when most of the material was still in the stomach. So the question was, "How much of the body could you get separated from the stomach by shielding [all areas but the stomach from gamma rays]?" [The] shielding had to be huge, which means big in size, because the [radium-224] gamma rays were so potent.

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[72] a cylinder of sugar (sucrose) of another material that serves as a surrogate for a human being during calibration of radiological counters

[73] tendencies to produce heat or fever

[74] substances, such as bacterial toxins, that cause the body temperature to rise

So we developed what we called the skull-and-crossbones technique, where you force the upper part of the body over a huge shield at the [correct] angles so that what was in the stomach would not be seen by the detector, which would be up here. *(holds up his hand, palm-down)* You put the bulk of your body, which is your arms, your head and your neck; [the bulk of the radioactivity was] going to go to the bones, so you could measure them this way. You surrounded the [sodium iodide] crystal [detector with shielding] at the crucial [moment of] measurement. We did mockup experiments and we did it with the dogs and all this kind of stuff to show that, "It could work, therefore, now we were ready."

[We] also made sure we didn't have to process excretion samples, so we had be sure that the sensitivity [was adequate] for measuring excretion samples that all had to be diluted to the same volume, which I think was a liter [(about 1.06 quarts)] or something like that. We could measure whatever was coming out [in] the excretion adequately enough over a period of time, as well. All those things were done on the basis of just assumptions of what the absorption might be and what we then thought we knew about the metabolism of either one, which wasn't very much, certainly, in the early period after administration. We're talking about a five-year period to do the experiment.

During this time, as we decided that we could see that we were going to do the experiment, we started developing the concept of, "What are we going to do about our subjects? How are we going to get our subjects? What are we going to tell them?" And then eventually, "What kind of a document do they sign?" And also, "What kind of a review process do we want so this whole experiment would be approved by whatever system that was going to be developed?"

We decided—we had used, [during] the radium studies, as reference control subjects, people that came from a place that was then called the Age Center of New England. This was a voluntary organization that had been put together to study aging. It was made of a collection of the most vital old people I had ever seen in my life. I remember telling my wife, "Boy, I'd like to be like that when I grow old!" because these people had the right attitude in life and that's why they were there, because they had the right attitude in life. But the object was that they were supposed to be [used for] studying aging and without the introduction [of substances into them or doing anything to them]. You talk about them, or you might do some medical tests on them, or you might draw blood, but you never would give them anything. And that's what we used them for [at] first[: as control subjects].

We took [about] 120 of them that would be representative people that fell into the category of the radium cohort, in terms of all the variables that are involved, from a health standpoint and size and all that kind of thing. [Then] we measured them for the normal amount of radioactivity in them and for the status of all their bones, in terms of x rays; they got the equivalent of whole-body x-ray measurements. That was the only insult to them, if you want to look at it that way. It's not an insult, but they got [via diag-

nostic and imaging procedures] what radium subjects were getting [by injection] every time [the elderly people's radioactivity and bones] were measured, which was frequent. And that was all calculated out, and it turned out to be an acceptable value for a one-shot [evaluation]. Those people were separate; they were part of the radium toxicity experiment.

Now we went back to this group of people [who were running the Age Center] and said, "We would like to do this kind of experiment, but we would like to introduce radioactivity into these people. But we don't ask you, as the organization, to do this; we ask you as an organization to ask your people, because these people are [already] volunteering [for medical studies] and, therefore, would be likely to volunteer for this kind of experiment. Once they knew what the situation was, would they volunteer on their own?

"So you [as administrator of the Age Center of New England] have two decisions to make: 'Do you want your organization to get involved in this sort of thing?' And, if so, 'Do you want to ask the people that are a part of your organization to do this?'"

So that was a separate decision they had to make on their own. We told them, "Go talk to your legal people and everything else and make your decision." It isn't the case that we grabbed—to hear Congressman [Edward] Markey talk about it,[75] we just picked them off the street and jabbed them with the first needle we had. We went through that routine, and they decided that they could do that, that it would not be out of line from what they [had been] put together for. Therefore, they could do that and therefore, they could announce to them if [our need and ask if there were any] volunteers. And there was no problem—there were volunteers.

Volunteer Inducements and Informed-Consent Procedures

GOURLEY: Was there any sort of stipend or inducement or—

MALETSKOS: Yes, we'll go through that. I don't know, there was a small stipend for them because they were doing it, but it was—by today's standards, we'd call it trivial. Another thing is that they [(the elderly subjects)] were going to get one health investigation to be sure that they were the right subjects for us, and they would know their health status better than they ever knew it [before]. So they would get a tremendous physical, and that would be a gain for *them,* as well. And [just] to participate—their other gain would be the gain to humanity: that if we [could use all the subjects in the radium cohort] we would be able to make a better analysis of our radium data and therefore, we would be able to set better standards and would know better what radiation does to [permit to] people, and at what levels, and so forth.

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[75] Maletskos is referring to the 1986 Congressional report issued by Representative Edward Markey (D–Mass), entitled *American Nuclear Guinea Pigs: Three Decades of Radiation Experiments on U.S. Citizens.* It discussed the 1945–47 injections of 18 human subjects with plutonium and about 30 other experiments, under the sponsorship of the Manhattan Project and subsequently the Atomic Energy Commission.

So, that's the usual routine you have now, and it's the usual routine you had then; it was no different. Nothing [was] different except it was being done without all the regulations that you now have that are spelled out; it was being done by people who wanted to do the job right in the first place. Irrespective of what the regulations are, matter of fact, we had to develop the regulations, there was no informed-consent form in those days.

So now I had to do two things. I had to develop some kind of presentation for these people so that they would understand so that they could truly volunteer and become part of this whole thing. From the Institute's [(MIT's)] standpoint, I had to develop a whole informed-consent procedure, because none existed. Then, finally, I would have to develop a procedure for having an approval system, and that would not be really my problem. In those days we had—do you know the name Harriet Hardy, Dr. Harriet Hardy?[76] She was the head of occupational medicine. She was the first person that was the head of occupational medicine [at the Center for Radiation Studies]. It's called Environmental Medical [Service] now, but it really is occupational, but now it has to do with outside—

FISHER: MIT?

MALETSKOS: At MIT. It was one of the first occupational [medicine programs], if not *the* first, in academic circles. She was [a] very good person to be the first one. She not only had [access to] the [university] presidency—the president brought her in, so that she could get things done when they needed to be done—she was [a leader] in her field and an equivalent of another Evans, [in] that she wanted him to do the thing very well and "it's going to be done right."

So the machinery would be that the proposal would be funneled to her for review from every aspect. The other proposal would go to the organization to tell them exactly what these people are going to be getting into, so that they make their final decisions. See, a lot of this stuff [had been] done by word of mouth, but now we had the formalizing of all this sort of thing. This is a lot of work alone, as well as the execution of the experiment. It took me about a year to get what we ended up calling a "waiver," but it should have been called an informed consent, but that was the way it was set up, which does [in one page] what an informed consent form now does in [many] pages.

The nitty-gritty stuff, they got directly from me. They got it several different ways: the formal proposal went to the Age Center of New England and they gave their formal approval so we could [proceed]. Then they asked for volunteers, and they had a *bunch* volunteer. [At our request,] they got more than we were going to use because we wanted to be sure we had enough when we finished, because we gave them the option of dropping out at all times; this was the first time that I know

[76] Harriet Hardy, M.D., a physician in occupational medicine, was the head of Occupational Medical Service of the Medical Department at MIT. Hardy was known for her research in berylliosis (toxicity of beryllium). She is now deceased.

that option was offered, for that matter. I can't prove that, but I just don't know. And one person did drop out, even later on as we just started the experiment—not started—but just as we were starting.

And I got them together, and I described this whole experiment in great detail as to what was going to be done. I had a physician on the team, and that person came along with me and helped me [by answering] questions [from the elderly residents] that I could not directly answer myself. So they got a first-class picture of what was going to happen to them, how they were going to be treated, [and] what facilities they were going to go in.

Then we took them to the laboratory and everything else. They saw the whole-body counter, they saw the kind of positions they had to get in [to be counted]. They had to tell us whether they could get in these [cramped devices], because we were going to verify the meter-arc method and they had to get into a sideways position, this way *(holds his hand straight out, palm facing to one side)* and then in the reverse fashion. They would have to lie there for a half-hour, at least, for each of those positions, and if they felt that they couldn't do it—[they] had to be able to do this, and they had to be able to do it on a repetitive basis, and we're going to do this on a day-to-day [basis], and then [the interval between countings] would get further and further separated, and so forth. So physically, they had to be able to do these things, as well as mentally.

I talked to them individually. They went back and thought about it. I don't know if they all came back, but most of them came back, got an individual treatment [(personalized care and attention)] all over again. Then they went up to Harriet Hardy and got a treatment from her and anybody else that she might have wanted and so forth. Then they would get a final treatment before they signed the informed consent, but they didn't sign it at that final time: they'd go home and think about it for two or three more days and talk to anybody [they'd] want, [their] physician, lawyer, or anything like that. [We] gave them all the options to be sure that they were right. And then they signed. Now that's about as good as you could get back in those days, and you can't do any better now, except you have internal review boards[77] to see to it that it's done right, [while] we were doing it ourselves.

The approval of the process went all the way up. Harriet Hardy held the proposal a long time; I know she took it to the people in the [MIT] Medical Department. She had one person in the Medical Department who was going to do the injections, [the] fellow that I mentioned previously, who was authorized [by] AEC to do these sort of things for himself, as well as a member of the MIT medical consulting staff. Everything came down [from a higher authority].

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[77] 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.

I had to make my presentation to Harriet Hardy's formal group that was going to do the final formal reviewing, and I was asked questions. It was just like a doctor's exam, for practical purposes. It lasted for hours just to do it well, and everyone wanted to be satisfied about every nitpicking thing; particularly, the dose to the GI [(gastrointestinal)] tract [and] why could it be low because you had so many alpha particles [already] there [passing through the GI tract from ingestion], and all this sort of thing.

This is long before ICRP[78] had ever came out with any standards for [ingested radionuclides and] the gastrointestinal tract. I, in effect, developed for myself all the information to be able to show that [the dosimetry] would work.

FISHER: [You didn't have any knowledge of the fraction absorbed from the gastrointestinal tract into the blood.] But you wanted to determine [it], and you didn't have a GI tract model, so you had to estimate [the absorption factor].

MALETSKOS: That's right, I had to do all that kind of stuff on my own and be able to defend it with references and the whole works. [In addition, there was] the waiver development. I worked with the lawyers and so forth, and it went up and down a number of times. It kept changing, and I said, "You can't do that, you can't get away with that!"

FISHER: What were they trying to do?

MALETSKOS: Well, this is the way lawyers work: When I first went there, I went to Harriet Hardy and said, "[There are no ICRP protocols for GI irradiation;] what do I do?" And, she says "Let me get in touch with the president just to make sure the president knows what's going on." I was told to go see the Institute's lawyer at the law firm, and I explained all this stuff [(the uncertainties that we faced in determining a safe GI dose)], and he said, "We'll take care of it."

After a while, something [(some document)] came trickling down back to me which said, "You're allowed to do any damn thing you"—he didn't say it in words of that nature, but, "You're allowed to any damn thing you want—and MIT is not responsible." I said, "That's not going to work." That's why it took a year, and that was because everybody, except the lawyers, were intelligent enough to know what was going on and, therefore, [to know] what you really needed.

I got what I wanted in the end, that it would be as much [as possible] under [the] control of the subject. If I were to go back [and do it again]—I haven't seen [the records of the way we arrived at those doses] for a long, long time, of course—I would probably do it a thousand times better. But that was it at that time.

So we got that permission, and we were ready to go, and we did the experiment. And we found [what] the absorption [was]. The final writeup on

[78] International Commission on Radiological Protection

this was 100 pages in the progress report and describes all kinds of information that we learned. I told you in the beginning we had to do it once on human beings: we were going to learn everything there was to learn.

We found out that the absorption of radium was around 20 percent, even from paint, which was supposedly nearly insoluble material, and [0.02 percent] from thorium, which was really a soluble material, in terms of thorium sulphate[, a highly insoluble form of thorium, used for comparison to thorium oxide solubility]. The absorption was like about a thousandth of the radium. So as a consequence, even if a little bit of thorium got in, it would not affect the dosimetry by more than five percent and we could use everybody [in the cohort].

FISHER: How many subjects participated in the ingestion studies?

MALETSKOS: The experiment was done, first of all, with injections in about three each, male and female.

FISHER: Three injections [per subject, for both] male and female [subjects, over a span of a few days?

MALETSKOS: Yes. We could look at [the protocol]: there's a table in the report. I'm just guessing right now, but I know [the injections were] by threes. Sometimes we couldn't get three, so we went by two; but they were [done by] threes because [that way,] if two [data points] are together [and] one is off, at least you know [that] it [(rate of the radium or thorium excretion)] wasn't going in one direction, it was going to keep going that way.

Then we did [an experiment]—this is very interesting—that was for [the] radium and thorium [sets of] injections, two injections. Then we did one each of the combination, [using] the mock radium paint, to see if we really could do the measurements we claimed we could do. It was there that we found out that we didn't need to give that much radioactivity for one of them [(radium or thorium) to get a satisfactory set of data]; I've forgotten [which one] that was. Here we made a [dose] correction in a downward direction right in the middle of the experiment because that's the way to go [if you want to protect people] and it's there, and you could see it happening. So the people who followed [(came after)] these first two [subjects] got a lot less of whatever one it was, and we had three of each in the—as far as I could tell.

All these people had to be cleared by the Medical Department from the medical standpoint because we had a string of things [(reactions)] that [would not be medically] allowed, [reactions whose likelihood was] quite astronomical[ly] low but whose causative agencies would nonetheless not be permitted] because you couldn't take any chances.

And we had one person that was off [(low)] by a factor of ten on absorption. We couldn't figure out why; it was not anomalous [(statistically suspect)] because everything checked for that person. All the excretions checked with what was left in the body and what was measured in the body and everything else. And somebody, I was giving a talk and I said

"Anybody knows it is brilliant!" and Roy Albert[79] from New York University said "Have you checked for achlorhydria?" I said, "Oh, no."

Achlorhydria means that you don't have any hydrochloric acid in your stomach, and therefore you don't digest things the way you do if you do have it. So that person didn't get a factor of ten in[to the body]. That person was officially discarded, because we went back and made a test for achlorhydria and [sure enough,] the person had it. Therefore, we were in a position to say, "We will not use this"; otherwise, we would have had to use it [in our results]; you couldn't throw it out. *Now* [given that we knew why the data were off] we could throw it out.

FISHER: In other words, the absorption was about a tenth of what you expected?

MALETSKOS: Yes, that's right, let's say for the radium or something like that. Remember I told you people don't differ by a factor of ten.

FISHER: Interesting. So this involved both oral ingestion and—

MALETSKOS: —intravenous.

FISHER: —intravenous injections.

MALETSKOS: By the way, the radium had to be given in the pill form, because you couldn't give the radium paint, which was in a powdery material, and I didn't want to mix it up with their binder because it would just complicate things even further. You had to make sure that the pill literally got into the stomach. So I made a test with a dummy pill and I was going to do it myself, but the physician that was working for me said, "Don't do it, because you've had enough [imaging substances ingested into your system]. Let me be the guinea pig."

So we went down to the x-ray facility [in the] Medical Department and gave him a drink of water and the paint contained silver sulfide as the activating agent, so that you could see it easily with an x ray. We took the x ray where we thought we could see it, but by golly, we couldn't see it!

What had happened was, the pill—he drank a lot of water but it stuck in his esophagus, the one place where the water wasn't going. We finally found it up over here. *(points to his throat)* So that meant that we had to make the pills slippery in the mouth before they started and make the person drink enough [water] to make sure that it got down inside and it did work out all right.

FISHER: But that [simple pill] experiment was not [with] radioactive [materials].

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[79] Roy Albert, M.D. (born 1924), was a professor of Environmental Medicine at New York University's Medical Center, where he studied radiation and chemical carcinogenesis and cancer from environmental toxins.

MALETSKOS: That was not radioactive, sure, naturally. You don't need the radioactivity to prove that.

FISHER: It's important for the readers to know.

MALETSKOS: This [(the thorium study)] is the experiment that Congressman Edward Markey [(D-Mass)] put in his 1986 report, [an experiment] that he claims that was a bad job because we were doing it on [human] guinea pigs because we gave so much radioactivity. He made these statements, without he or anybody from his team or anyone else ever contacting anybody at the Radioactivity Center that I know of and, especially, not contacting me, the guy that was responsible for the whole experiment.

What they didn't know is that when you give a short-lived material it could be higher [in radiation dose] than the amount that you are allowed annually because it's going to decay away rapidly if you're using the standard for the [short-lived] material. That's the mistake they made. Of course, they never corrected it; and then, when the scandal came out again [in late 1993], he said the same things all over again. We sent him a whole package of information when it [(the Markey report)] came out in 1986, saying exactly what was going on, in effect describing what I just described to you, only by documentation; we never heard from him then and we never heard [from] him till this second.

By the way, you saw the calculations that Bernie Cohen[80] made on that report and in the *ANS* [(American Nuclear Society)] *Newsletters,* I think it was, where he calculates the dose for all the ones [(experiments)] that Congressman Markey discusses in there. I have a copy that I could show you. They're all, I believe, acceptable, except one is just a little bit over, and ours is acceptable by today's standards, let alone the standards back then. [They're] really acceptable, not just marginally acceptable.

GOURLEY: The records on this stuff—you had the formal proposal that you did, you had the presentation, this whole approval process that you developed over the years with the lawyers and such. Where are these records now?

MALETSKOS: They're buried in the archives, as far as I know. That's how I got a lot of these things when I made the package for Congressman Markey, [from] the archives of the Radioactivity Center, or the archives of Professor Evans himself, if they're kept separate. That's how we found them. At the time, if I needed something [it] would have been in Professor Evans's domain, I probably called him—matter of fact, one of the things says, here's a note from him that says, "We have *copies,* but the originals are in such-and-such place." He sent me a copy, and I just used the copy. It took long enough to dig out from the archives all the information for him anyway; [the archivist] wasn't going to spend any more time on it than necessary.

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[80] Bernard L. Cohen, Ph.D. (born 1924), a nuclear physicist, served as a group leader at Oak Ridge National Laboratory (1950–58). In 1961 he became a professor of Physics at the University of Pittsburgh. Cohen is known for his research on nuclear reactions, health effects of radiation, and risk analysis.

GOURLEY: So you have copies of most of these things.

MALETSKOS: Yes—well, everything [that] is in the archives.

GOURLEY: Yes, because if you had sent the originals, that would be something that we would be very interested in getting copies of for our records to just ensure that we're complete.

When you went to the Age Center of New England, what was the original size of the [group of] elderly folks that [you] presented this to [and said,] "We'd like some volunteers?"

MALETSKOS: I don't remember; I'm guessing twice as many as we would have needed for the experiment, just to be prepared to not have to go through that whole procedure all over again. They knew that we were going to select from them, first of all, those that really wanted to participate once they heard [what we wanted to do]; secondly, ones that fitted in best with what we were *able* to do. For example, we wanted to have equal numbers of male and female [control subjects]. If all the males volunteered and the females didn't, we would not have been in good shape; there is no point to the experiment. You've got to have both sexes in there to do the job correctly.

GOURLEY: What was the average age?

MALETSKOS: They were at least 60 [years old] and they went up to 80, I believe. I could tell you something that gives you an idea of what their attitude was like. I don't know if you remember, but at that time, the Russians had put [the] Sputnik [earth-orbiting satellite] up in the air [in 1957] and there were people [(cosmonauts)] flying around, somebody up in that capsule [in space] up there flying around. When they [(the elderly subjects)] went into the whole-body counter that was their capsule, their attitude was, "Boy, we're participating in an [important] experiment, we're actively involved in it, and they trust us and we trust them, and this experiment is being done by both of us." You can't get a better mental attitude than that.

GOURLEY: We want to be sure and take a look at that *ANS Newsletter* on the dosimetry, because that's something really—

MALETSKOS: That's an independent calculation.

GOURLEY: Right, that is really interesting. We certainly want copies of that.

The other thing you mentioned [was the] the pill, and you wanted to make sure that it went down into the stomach, but they had said not to self-experiment; we have seen with other researchers that they self-experimented. Was that [(self-experimentation)] something that you did?

MALETSKOS: Not in terms of taking radioactivity. I made that decision because otherwise I'd be doing it maybe more that I should and I would be making my own decisions. But in terms of just swallowing an *empty* pill, it wasn't any particular problem. I also participated in experiments, every step that those people had to go through, I went through myself, just to be sure that when I was telling them it was "doable" and they would not

get hurt. We were doing things that nobody had ever done in measurements, in body configurations [(for whole-body counting)] that nobody had used before, and I had to be sure that we could do it. So I became the guinea pig for that, but I don't consider that anything serious.

GOURLEY: Why did one person drop out?

MALETSKOS: The person decided that he didn't want to do it [(the experiment)]; we didn't ask why. We told him that you have that right and all you have to say is "I want to drop out."

FISHER: You "sham-exposed" them, without taking either an injection or—

MALETSKOS: Yes, I went in and sat in all the positions.

FISHER: And tested all the configurations.

MALETSKOS: Yes, if I remember correctly I probably sat there like the [subjects] just to be sure over a period of time. You may feel fine for the first ten minutes, but if it's a 30-minute run in an awkward position, it may turn out to be not comfortable, and therefore, you should be prepared to modify.

FISHER: Was there a funding sponsor for this work?

MALETSKOS: This was the progeny of the AEC. What was it at the time? It was ERDA.[81]

FISHER: Well ERDA was in the early '70s, so it had to be one before.

MALETSKOS: Then it's the AEC.

FISHER: So it would have been the Atomic Energy Commission in the 1960s?

MALETSKOS: Yes, they knew about it because we were talking about it; they knew we had to do it since way back.

FISHER: Do you remember who the authorizing officials were at AEC during that time?

MALETSKOS: No, I don't remember. I don't remember whether there was a formality for it, because they [(radium-228, thorium-228, or its derivative, thorium-234)] were naturally occurring material. It was a formality in the sense that they knew we were going to do it, but not in terms of getting permission to do it, in a formal fashion, as if it were radioactive material from the Atomic Energy Commission itself.

FISHER: So you don't remember who the [AEC] contacts were?

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

MALETSKOS: I don't know, right offhand. I think by that time Paul Aebersold,[82] who had run the Human Use Committee [and] would probably run it, had already left. I remember S. Allan Lough[83] being involved in some earlier things, so that he may have been still involved. I don't think he was the final authority, if there was one. I just don't remember that.

FISHER: Was there any thought as to the suitability of these elderly subjects, because they were older? Was there a slight risk that the latent period[84] was long enough that it would not affect them? Did that consideration ever come up?

MALETSKOS: Sure, because the people that you'd *like* to do [the experiment] with would be 19-year-old girls. They were the ones that were getting the bulk of [the radium ingestion, in the watch-dial paint booths]; obviously, you're not going to do the experiment on them.

Well, then, do you ask the next question, "To whom do you do it?" Well, in an absorption experiment, you know, as people get [older] the absorption changes a little bit, so it's not best to use the elderly people. We felt there was no justification for using anybody younger than these elderly people [in case the exposure might lead to cancer decades later]. So that was the way that we went. In terms of any potential health effects, it had to be near zilch: they were getting such low amounts of material.

But on the other hand, if there were going to be any problems, you brought up the subject that hopefully, the radiation they were going to get, if they were going to get—if all these agents acted the same way, it would be a bone cancer, or a cancer of the sinuses. The latent period is so long that they weren't going to ever get it [because as elderly people, they were already nearing the end of their natural life span].

FISHER: When you talked to these subjects, did you explain to them there were these late effects?

MALETSKOS: Sure, absolutely, that was the purpose. They were not only told what they were going to do and why and all this sort of thing, they were told what we thought the risks were at the time. Absolutely, that was the main reason, that was more the reason to bring them in; you still had to let them know what the rest of it was. Even though I was the one that

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[82] Dr. Paul Aebersold established the administrative system for distribution of radioactive isotopes. After working on the Manhattan Project at Los Alamos and Oak Ridge from 1942 to 1946, he served as director of the Atomic Energy Commission's Isotopes Division at Oak Ridge from 1947 to 1957. He retired as the Director of the AEC's Office of Isotopes Development in 1965. Two-and-a-half years later, he committed suicide. For additional information on Dr. Aebersold, see "Safety of the Nuclear Industry" in the interview with Merril Eisenbud (DOE/EH-0456, May 1995); "Remembrances of Personalities" in the interview with Earl Miller (DOE/EH-0474, June 1995); and "Oak Ridge Committees (Isotope Distribution, Human Use, et al.)" and "Vanderbilt University Study of Pregnant Women and Iron-59" in the interview with Karl Morgan (DOE/EH-0475, June 1995).

[83] S. Allan Lough, Ph.D., was a chemist by training who worked in the Division of Biology and Medicine, Atomic Energy Commission, in Washington, D.C. Lough was responsible for reviewing and approving applications for the use of radioactive materials. Upon his retirement, he joined the staff of the National Council on Radiation Protection and Measurement, in Bethesda, Maryland. Lough is now deceased.

[84] After exposure to a carcinogen, it takes 5 to 15 years or longer before evidence of cancer is apparent.

was responsible, I think that was done about as well as you could ever do. [Because the subjects had to maintain uncomfortable body postures for long periods while being counted,] it was a very tough experiment, as you've heard; it wasn't done in five minutes; it was an awful lot of things that had to work, and could have failed at any step, because we just wouldn't be able to do it. It was real tight all the way.

FISHER: Incidentally, did you attend the Alta Conference[85] that was put on by the University of Utah on radium and thorium?

MALETSKOS: I've attended some of them, but I don't remember the Alta one.

FISHER: Maybe 1972, '73, or '74?

MALETSKOS: Was it in '74? Because I gave a paper on it. There is a paper in there, a short version of the metabolism of radium and thorium.

FISHER: Did you go to Salt Lake and up to Alta [Ski Resort] to attend that conference?

MALETSKOS: Yes, I would have gone.

FISHER: *(smiling)* Do you remember a young projectionist who ran the slide projector that day?

MALETSKOS: By the name of Darrell Fisher? No, I don't.

(laughter)

FISHER: He was there and he heard your paper and [young Fisher] probably didn't realize until many years after[ward] the quality of scientist you were.

MALETSKOS: You haven't said whether I'm good or bad, though.

Cesium-132 Research on Humans (Mid '60s)

FISHER: The high quality of scientist that you are, but I was certainly there.

In this same time period, we see some publications on the retention of cesium by humans after intravenous injections, and it's interesting that the isotope cesium-132 was used, and secondly, that it was administered intravenously. Can you remember some of the details of this experiment?

MALETSKOS: Yes, [but] I'm not remembering very much at all, and I'm surprised that I am [not,] because I was the honcho again on that one; I don't know why. I've almost got a total mental block; I cannot even remember the actual people who were involved or anything like that. I took a quick glance at the report and it still doesn't trigger anything. I may have mentioned to you that I thought the injections were done some place else. They were done at MIT by the physician that I talked to you about who was licensed to do this—

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[85] See the proceedings of the International Symposium on Biological Effects of ^{224}Ra and Thorotrast, Alta, Utah, July 21–23, 1994, published as *Health Physics,* Vol. 35(1), July 1978; edited by C.W. Mays.

FISHER: By the name of?

MALETSKOS: [Martin] Lubin.[86] He's not on the paper, [but] I think there is a footnote. Do you have a copy of the paper?

FISHER: I don't have a copy of the paper.

MALETSKOS: It was all done during this period, when I was developing this [absorption study] according to Hoyle. What I can't remember are the steps that we took. My thinking wasn't a step function [(a smooth progression)] from that experiment to the next, to the absorption experiment. My [moral] conscience was the same, as far as I know. So all the things I described [(all the precautions I followed during the radium experiments)] should have gone through that whole thing, but I don't remember a thing about it.

FISHER: I came across this by reading a report from Los Alamos and where your worked is referenced. *(reading from the report)* It says here, "Maletskos et al., report a two-exponential retention for five elderly subjects given cesium-132 intravenously."[87]

MALETSKOS: Yes, it probably came from the same place. I don't remember the routine of what I just described to you; I'm drawing a blank and I'm sorry, right at the moment. Keep in mind, you asked if it was intravenous instead of giving it orally, for example. Again, you're going to do an experiment, you want to extract as much information as you can; nobody had any information on the early retention. That's why we gave it intravenously: so we could use it not only as a calibrating device, but as a means of measuring fallout.[88] In the early phases because you never were getting anybody immediately, except in an accident situation or from fallout.

FISHER: It also says that the BHL, which I assume was biological half-life, was 80 to 90 days.

MALETSKOS: Right, we could see that even in the short period of [measuring] that short-lived radionuclide.

FISHER: When only about two-tenths to three-tenths of a microcurie was administered.

MALETSKOS: That's low enough, isn't it? Again, remember, when you asked me the philosophy of "How much do you give," [well,] you give the least you could get away with, and do the experiment well.

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[86] Martin Lubin, M.D., Ph.D. (born 1923), served on the staff of the MIT Medical Department and was a professor of Biophysics at Harvard Medical School (1953–68). He was later a professor of Microbiology at Dartmouth Medical School (1968 until retirement). Lubin conducted studies on the regulation of synthesis of biomolecules. He was familiar with the use of radioactive materials and their biological effects.

[87] C.R. Richmond, J.E. London, and J.E. Furchner; "Retention of Intravenously Administered Cesium-132 by Man"; in *Biomedical and Medical Research Group of the Health Division Annual Report, July 1962 to June 1963;* Los Alamos, New Mexico, LAMS-3034, 1963; pp. 21–33. See LANL-22 ("Cesium-132 Metabolism in Humans") in the *Experiment List.*

[88] radioactive debris from a nuclear detonation or other source. Fallout is usually deposited from airborne particles.

FISHER: These subjects would have probably also come from the Age Center and they would have volunteered?

MALETSKOS: I'm sure that's where they would have come from. This is why I'm saying I'm drawing a blank. I'm sure we went through the same motions. Now whether we documented it as well as I described, I don't know, I cannot remember, I'm sorry. Matter of fact, I even missed starring it [(putting an asterisk by it)] when you asked to see my bibliography. That's how much of a blank it was for me; that's the first time I've gone through my bibliography in a half of century, practically.

FISHER: It was interesting to read of that first cited in the literature, and then secondly to come across it in your list of publications.

GOURLEY: I was looking through the materials and I guess we're up into the '60s now. I notice that you were chairman of the Ad Hoc Committee on intracomparison of human, I guess it was cesium-137 body burdens for the AEC Division of Biology and Medicine.

MALETSKOS: That's correct.

GOURLEY: How did that come about? How did you get to that position?

MALETSKOS: The AEC people were tracking to see what was in the body [with respect to] cesium, and they were getting all kinds of numbers; some of them didn't jive. It looked like it was a calibration problem. It turned out to be a calibration problem; I've forgotten what the test was. I didn't do the test; Ernie Anderson[89] at Los Alamos did the test. As a matter of fact, he was the chairman first, but then he had to do something else, and got off sometime during the first year of this committee.

And so, when the committee was put together by the headquarters, and said "Look, we got to find out where we stand, because we're getting all these data on cesium body burden, and how fallout is going around the world, and everything else. We don't know that we're comparing apples and oranges between the different measurements." The questions were, "What is the problem, what do you do to correct it, and how to find out what the situation was?"

We decided that what we would do, is that we would literally—and it wasn't always clear that the people had correct [calibration] sources or good sources that they were using [as] standards. We determined what was going on by making a special set of radioactive sources that were under the jurisdiction of one person, so that they would be done right and correctly. They'd all be calibrated [and] each person involved in these measurements would get a set of three sources that would give a

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

range of radioactivity, to see how well they could do with something that was close to background, something that was [higher than the background radiation and thus] easier to measure, and something that was intermediate. Then find out what was going on.

Eventually, the real test would be, two people would come around with cesium deliberately in them, and be tested by everybody. This is what the committee [planned], and I became the chairman. I oversaw the production [of these sources] up over here [by] what is called the New England Nuclear Company.

FISHER: Let's go to that just a little bit more.

MALETSKOS: I wanted to mention that we ended up here because, it turned out that they could make little beads containing not only radium, which is what I wanted—that's how I found out about it—but beads of cesium-137. There would be little baby BBs as a source, in a plastic rod, at a particular point in the rod. The rods had to be manufactured well; the cap after sealing had to be sealable, so that it could never pull apart after you sealed it. Even if the glue failed, and that sort of thing, because otherwise you would have [radioactive] beads [bouncing] all over the place.

This was done, these were sent around, and it took a little bit longer than we had intended, because people knew this was coming. They were working hard to get themselves up to snuff. So the pressure was on them to see if they could do a good job; they were building up to do a good job on their own. By the time we did the experiment, we found very few that could not do it well (it was a two-year period), and it was only from then on that you could say all the data were comparable.

FISHER: What you're saying is that there were some subjects, normal subjects, who ingested solution containing cesium-137 beads?

MALETSKOS: No, I'm not saying that at all. I'm saying these beads were used to make radioactive standards for calibrating, for measuring the device and calibrating the device and determining, by their methods, how much radioactivity was in each bead.

FISHER: Were they taken in vivo?[90]

MALETSKOS: No, no: these were in a plastic rod! Each of these laboratories was monitoring their own group of people because the AEC asked to follow this. We had at the Radioactivity Center six or seven people that we monitored on a monthly basis, and so did all the various laboratories. [The question was,] "Was everybody was on a common baseline or not?" The answer was that we [were] not.

FISHER: Cesium was from fallout?

MALETSKOS: This was the cesium-137 from fallout. And, so we went through this whole routine. Then two people at Los Alamos, put a little extra cesium-137

[90] inside the body

above and beyond what they had, so it would be a little bit higher [in radioactivity]. Then they went around to the laboratories to measure in actual human beings. I don't [know] whether it was two of them or it ended up as one; I know one person came around. That was the final test on the comparison, and then I put together a report of what the status was and the committee members reviewed it and submitted it to the Atomic Energy Commission.

GOURLEY: So there were one or two persons at Los Alamos?

MALETSKOS: Whatever they did down at Los Alamos, I don't know what the routine [was] for getting the radioactivity.

FISHER: Do you remember who the subjects were that went around to the different laboratories?

MALETSKOS: I remember one, Phil Dean,[91] but I don't know anybody else. I think that there were going to be two, but I think they ended up as one, but I can't verify that right now.

FISHER: From Los Alamos?

MALETSKOS: Yes. He's no longer there; he left there and went some place else quite a while ago. I don't know where he is right now, if he's still alive, he should be my age.

FISHER: You published a paper in 1963 on cesium-137 measurements in normal human control subjects. These would be people from the general population and the cesium would be fallout cesium from normal ingestion of food and water.

MALETSKOS: That is correct. One or two people might have been from the laboratory at MIT, and the other would have been people at MIT or something like that.

FISHER: This was also at the height of the nuclear testing era, prior to the Test Ban [Treaty of 1963].[92]

MALETSKOS: That's why the AEC was interested in this information, and wanted to have it be on [a] good common basis. And why we were doing it. It was an extra measurement, as far as we were concerned. We did it and we followed them and watched it do whatever it did over the years.

FISHER: Was there any additional experimentation on human subjects where cesium was administered either orally or by injection?

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[91] Dean was in charge of operating and refining the HUMCO II whole body counter during its early development.

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

MALETSKOS: No, the cesium-132 experiment that we talked about was done to make sure that we were calibrated correctly. We used cesium-132 because it's a short-lived nuclide [(6.2 days, vs. 30 years for cesium-137)] of the cesium nuclides.

FISHER: Where was the cesium-132 made?

MALETSKOS: I don't remember; I know the paper describes where it was done. It was done as a special radiation with some [bombarding] particle—it was done in an accelerator of some sort.

FISHER: But you don't know where?

MALETSKOS: No, I'm sure it says it in there, but I don't know what it is now. It's a radionuclide you come by easily.

Radium Burden Examination of Radium Dial Painters (Mid '50s to 1985)

FISHER: You were also involved, I think during this time, and maybe also before and afterwards, in the measurement of radium burdens in body tissues of former dial painters.

MALETSKOS: That's correct, and still have been, up to last measurement at MIT, which was about 1985 or something like that. I mentioned earlier [that] I started working by measuring people with radium out in an open room with a good old-fashion Geiger-Müller counter. Then we ended up building the whole-body counter with a huge sodium iodide crystal, which made life an awful easier.

We had a major set up for measuring their breath thoron [(radon-220)] and breath radon [(^{222}Rn)] because we made gamma ray measurements, and we made breath measurements of both radon[-222], which comes from radon-226, and radon-220, which was called thoron in the old days, which comes from the thorium series. We always did that, so [that] we always knew what the ratio [of the exhaled to the retained radon] was. We could average out a ratio that we could use [as] an average ratio for those people we could only measure one way. However, [it] turned out to be [that] a lot of people were measured only by breath radon using flasks,[93] for example.

FISHER: How was the work done in conjunction with the radiobiology center at Argonne?[94] The human radiobiology study?

MALETSKOS: This work was being done at MIT long before Argonne ever got involved. The source of a lot of these subjects was New Jersey; that was where the bulk of the radium work was being done. The people who had been treated medically [with radium] were all over the country. Then

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[93] This means that samples of exhaled air were collected in flasks and then radiologically counted. When this is done, the sample is allowed to sit for a while, while ^{220}Rn decays away, leaving only ^{222}Rn. The ^{222}Rn is then measured.

[94] Center for Human Radiobiology, Argonne National Laboratory

there were people we called chemists and physicists, who happened to work with radium; they were eligible candidates to be in the [experiment] cohort [(sample)] because those people work with the open vials or material, [which] would allow [potential] ingestion or inhalation. [For] a physician who used a radium needle—for example, for therapy—[we] wouldn't bother with that person; there would be no access to them [of] the radium to the body.[95]

Argonne found some people that had been working with radium in Illinois, and so they decided to get into those measurements, [which] they did, and proceeded to follow their ideas and so forth. This was good, because it became a competition of how to do the job well. Eventually, we had to calibrate with each other, in terms of what we thought were the right [measurements to use].

Then we actually sent the same subjects to both places, so that we could see if we could get the same value for the[se] subjects, using somewhat different techniques, and so forth. This went on as two separate entities funded by the AEC, and as a matter of fact, there was a[n additional] small group in New Jersey that acted more as a source of finding new people, as opposed to making any measurements. The [people] came to MIT for measurements because they were closer to us, and this parallel study went on for quite a while. Then, when Robley Evans's time came to retire, he wanted to be sure that this whole study would continue, with the hope that you would catch every last person. You [wanted to] catch the last person, because it was possible to do that before the end of the century.

He set up the concept of the human research center at Argonne.[96] Its main function would be to study the radium cases, and anything else that involved radioactivity in people. It would be something that would be directed by Argonne [because] Argonne was going to be a continuing organization [and] it would have a Congressional line item [for] its own budget. People couldn't interfere with it, and therefore, all the information would go over to Argonne, and all the data would be collected by them. We had started doing that anyway, because that was going to be a good way to handle [the data] in the first place. That's how the center started at Argonne. The work started at Argonne, and the Center came to life.

FISHER: So Robley Evans had a big part in this.

MALETSKOS: It was his idea, he [(Evans)] went and talked the AEC into doing this; he presented it to Congress, I believe, with the AEC, to make sure that everybody understood what the purpose of this was.

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[95] Radium needles were not hypodermic needles, which could accidentally prick the physician, but slivers of radium implanted in a subject's body surgically to destroy a tumor. Such needles could not be accidentally introduced into the surgeon's body.

[96] At a 1967 symposium, he proposed that the AEC establish a National Center for Human Radiobiology so the AEC could follow up and combine all the radium cases being studied at MIT, Argonne National Laboratory, and elsewhere. On September 1, 1969, the center opened at Argonne, headed by Robert E. Rowland; Evans maintained a satellite office at MIT.

Here was a very select group of people of internal exposure to radiation that we had studied for quite a number of decades already. We had learned an awful lot: the standard [for internal radium] was set back in 1940 [by Evans], and there was no reason [to] change [the] need [for] all this information. We were learning new things; now we could do dose–response relationships, which we couldn't do before.

There was a chance to finish this in a decade with not too much of an expenditure to complete it. It was a great idea, [and] it went on for a while and then Congress decided they didn't want it in their line budget. [The] AEC or DOE eventually decided they didn't want to support it anymore, even though there was still less to do. I personally [am], and I'm sure Evans is, disappointed that it's not coming to its proper completion.

The reason for that is, when Robley Evans retired, we decided we wanted to do one last analysis, [but] before we would do the analysis we would review every piece of information on every subject, and see if we got it right. We would leave the data in as best shape as we could by the people who were involved—that was Robley Evans; Mary Margaret, his secretary and deputy director of the Radioactivity Center (eventually); and I. We did, we took two years to go through all those pieces of paper [and] we found errors, not only in our own reports, but in the reports of—presumably the same data [in]—the Argonne reports, for different subjects. This was peculiar; we found errors in different ways.

FISHER: Mary Margaret is Mary Margaret Evans.[97]

MALETSKOS: Yes, right, Evans. We had our set of data that we had to reanalyze in a new fashion. It shows a [safety] threshold of around 1,100 rads, which is big when you show a straight-line relationship to the abscissa [(the x-axis)]. I've given a talk on it [but] I haven't finished writing the paper, which I'm now making first priority.

FISHER: 1,100 rads dose to bone surfaces or bone volume—

MALETSKOS: —to average bone volume—

FISHER: —as a threshold for induction of late effects?

MALETSKOS: For sarcoma[98] in the skeleton and for carcinoma [99] in the sinuses. It's unusual that they both come out the same way. We can talk about that later. We've done that [reanalysis], but the whole operation at Argonne has now ceased, and the official data still are those [previous data of Argonne] rather than Argonne correcting their half as well, to get them all on a common new basis.

So I don't even know now how to handle the data; DOE has left us in a very bad situation. I don't know what could be done about it, because it

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[97] After his wife died, Evans married Mary Margaret McClanahan, who had served as his secretary for 45 years.

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

[99] a malignant tumor composed of epithelial tissue—the tissue layer covering body surfaces or lining the internal surfaces of body cavities, tubes, and hollow organs

took the three of us, with the help of two technicians, to do this on a semi–full-time basis because we were making measurements of radium subjects at the same time; this [reanalysis] was done on the side. It kept on going, and it would take you a long time. You wouldn't want to do [it] all the time, because it's nerve-racking to do. You have to go through every piece of paper and make sure the information you need is there in this great big table to make the dose calculations, and everything else, in some correct form.

A couple of years ago, we had a workshop at Argonne as to what the retention function [should be]. What do we know now that we should modify the retention function from the *ICRP-20*.[100] We all agreed that lambda,[101] which is [a] factor [in the] retention function, should be changed from 1.5 percent to 2.5 percent, as a function of radium content. You find that people with a higher radium content excrete more rapidly than those who have a lower radium content. The bone apparently is getting either some damage or whatever is happening; in effect, you have greater osteopetrosis[102] is what it amounts to.

The [Argonne] dosimetry has been corrected, but our data [are] based on the old dosimetry. [The MIT cohort has] the best set of information on the data, but we have to convert our analyses to the new dosimetry. Argonne has the new dosimetry, but they haven't reviewed their data for the last time.

So what do we leave for posterity? I'm disturbed with this. Evans and I hoped that everything would be done well. We rechecked in the workshop what the dosimetry would be: we would make one new dose calculation and [so would] Argonne separately[; anyway, each of us] wanted our own data first. Then [the two sets] collectively, with everybody's data, would be available to anybody else to analyze anyway they wanted. [Neither of us] can do that now. I find that highly disturbing when we spent six decades on it already, and a few more years would do the trick [to finish up the scientific analysis of the data]. I think that's really bad policy. The importance of it [(this data) is that the problem of radium ingestion] is chronic, it's not only internal, but it's chronic[, taken into the body a little at a time].

FISHER: We were talking about why the data should be analyzed to completion. I think we concluded that radium toxicity is still a concern, because we ingest, some more than others, radium in our drinking supplies, on a chronic basis, and some people drink more radium than others.

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[100] International Commission on Radiological Protection, Publication 20, *Alkaline Earth Metabolism in Adult Man*, Oxford: Pergamon Press, 1973. Produced at Argonne National Laboratory by a committee headed by John Marshall, *ICRP-20* addressed the retention and dosimetry of the alkaline earth elements in humans.

[101] the ratio of the natural logarithm of 2 divided by the effective retention half-time or $^{ln}2/T_{eff}$. It is the fraction of the radium exiting or "clearing" per unit time.

[102] a condition in which there are bandlike areas of condensed bone at the epiphyseal lines of long bones and condensation of the edges of smaller bones; also called *marble bones, ivory bones, Albers–Schönberg disease*

MALETSKOS: That's correct, it's one reason, but there [are] other, [even] more important reasons. One of the most important ones is that this is a long-term study of chronic irradiation over a long period time, because the radium stays in you for so long. Even though it's constantly excreted, but slowly, and this is the way you get exposed whether you are drinking the water like you just described. Whether you're getting radiation on a continuing basis from natural background or as a worker—if you're in a position to get any radiation at all as a worker, everything is done on a chronic basis. It's not done [on] an acute basis, except for an occasional accident or unusual situation. In the case of nuclear medicine, you get a single jolt of radioactivity for the test purpose, but you're not expecting any difficulty there, because presumably, the amount that is being administered to you is low enough to do the experiment, but not [high] enough to produce a health effect. Certainly it is not enough to produce health effects that compare with the seriousness of the health effects that are being checked for.

This is an important comparison. Something like the chronic irradiation of radium studies are the only thing we have that allows us to do a complete study of dose–response for people who have been studied very well. This is from the standpoint of the health status, including the x rays of their bones, and so forth, and from the standpoint of the fact that you can measure what is in them. The dosimetry is not based on the guess [of] what might have happened, like you do [for] the Atomic Bomb studies.[103] *There* it's a calculated number and you hope you are in the right ballpark, never mind coming close to the right value. You don't know that you're going to be very high for one person, very low for another person, and hope that these things will cancel out and maybe make things come out right. But you don't know that [(the actual dose spread)], and you'll always be [suspicious of] that, and you'll always be [suspicious of] what the real dose was.

Here we have a situation where we may have a problem of deciding how to calculate the actual dose, but we were awful close to the right value. We are closer to the right value in these studies, where you can make the measurements of the content of radium in these people externally without taking them apart. That is a fundamental importance in the quality of the result.

GOURLEY: How did it come about, that—for some of the radium painters, exhumations were done?

MALETSKOS: That's correct.

GOURLEY: How did that come about?

MALETSKOS: Once you start getting your cohort and [it's] getting big now, you're maybe getting these people because they have a symptom, or you found

[103] Maletskos is referring broadly to studies in which health physicists try to determine how much ionizing radiation was received by persons who were living near atomic bomb tests conducted in the Pacific or, in the case of Hiroshima and Nagasaki, by persons who were irradiated by the A-bombs that were dropped on those cities by American bombers.

them by some specific method. The two methods are different, and therefore you can prejudice your result. What you want to do is get as much of the cohort as you can so that you can say, "I can treat everybody equally" without saying, "These people are biased, and those people are biased in another way."

The next step, you work at finding people who are not alive, as well as those that *are* alive. Remember, the radium is in the bones; bones take a long time to disintegrate, if they are going to disintegrate at all. Measure those bones and know what that person has for a content of radium, and then you have all the diagnosis prior to that, if you can get it; you certainly have a death certificate and whatever else is available. You can also relate what the final findings on that person [were] from the health status to the amount of radioactivity found in that person. And that becomes a perfectly good subject in your study.

There were disinterments, [but] there's a lot of rigmarole finding people and getting the permission. There are major problems with handling dead bodies that are deteriorating. Especially at a place like MIT, where you don't even have a mortuary and [it was] rather gruesome sometimes, I might say.

FISHER: But in handling these tissues [obtained from graves] you went through some procedures to honor them, to pay due respect to the tissues, to watch over them, and to interact with the families?

MALETSKOS: Absolutely, everything was done in as nice a way as you can imagine. I mean that "nice" in all ways, and everything was returned after analysis.

FISHER: For proper internment?

MALETSKOS: For proper internment, this was all done ahead of time. Matter-of-fact, people could come in and look to see what we were going to do. I don't know if anybody actually did, but it was meticulously well-done. There would be an exception—for example, obviously you wanted to have a clean skeleton that you measure, because you couldn't afford to have decomposing tissues. In a laboratory where your doing the measurements, there would be occasions where you want to get rid of that decaying tissue; the permission was obtained that we would digest the skeleton in a caustic [solution, or] something like [that]. That material would have to be disposed-of correctly, but not go back to the original place.

GOURLEY: What percentage of [survivors] gave permission [to exhume the bones of their deceased family members]?

MALETSKOS: I don't remember. We've done I would guess we done on the order of a hundred bodies, but that's guessing, right now.

FISHER: Were most of these radium dial painters?

MALETSKOS: I'm sure there were [also] some people who had been medically treated. They would be harder to find, because they are scattered all over the place; so I'm sure that the bulk of them, if not all of them, are dial painters.

Other Radionuclide Research

FISHER: I've noticed in your bibliography there are some papers on the metabolism and retention of activated products, such as sodium-24, potassium-42, and potassium-43. Do you remember any of these studies? One in particular by Tang and Maletskos, 1970?

MALETSKOS: That didn't involve human beings.

FISHER: It was just an analysis of sodium-24 and potassium in tissues?

MALETSKOS: Those were neutron activation studies.

FISHER: That was ex-vivo [(outside the body)].[104] There is one, however, though that says, "Exchangeable Potassium in Man Using a New Radioisotope, Potassium-43." Zollinger, Van DeWater, Maletskos, and Moore. Do you remember that one?[105]

MALETSKOS: Sure, that's an experiment that got into being because I'd happened to have an appointment [at the Harvard Medical School]. This was when I was working with Shields Warren,[106] who was [one of] the great pathologist in this country, and also the first to head in the Division of Biology of Medicine for the Atomic Energy Commission. I had a double appointment when I was working for him at the Cancer Research Institute at the [New England] Deaconess Hospital [in Boston] and also at the Harvard Medical School.

Jim Johnson,[107] who use to be (and still is) at Colorado State, had a sabbatical, and he came over here to do some work with Dr. Francis Moore,[108] who was the Chief of Surgery at the Peter Bent Brigham Hospital [in Boston]. Dr. Moore was always interested in body composition, and that's why Jim came along [so] they decided to do this experiment. I knew Jim Johnson, and he knew me, and we got together to collaborate on this sort of thing. It was all done under the auspices of the Peter Bent

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[104] Tissue, hair, or excreta samples may be put in a reactor, irradiated, and analyzed to determine elemental composition and amounts.

[105] See R.M. Zollinger, J.M. Van DeWater, C.J. Maletskos, and S.D. Moore, "Exchangeable Potassium in Man Using a New Radioisotope, K-43," in *Surgical Forum* 21: 213–215, 1970.

[106] 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.

[107] James E. Johnson, Ph.D. (born 1936), is a retired professor of Animal Science and Radiological Health Sciences at Colorado State University, and once served as campus Radiation Safety Officer. He conducted research on alkali metal metabolism, whole-body counting, and environmental radioactivity.

[108] Francis Moore, M.D., of Peter Bent Brigham Hospital (now Brigham and Women's Hospital) in Boston, was interested in body composition studies and the relationship of body composition to surgery. He made a number of detailed anatomical separations and analyses of the constituents of different body tissues in man.

Brigham Hospital. I just happened to be at the Harvard Medical School at the time; I had nothing to do with the actual execution, except participate in the work.

FISHER: Was there an injection?

MALETSKOS: I had all the equipment to make the measurements you see.

FISHER: Was there an injection of potassium or ingestion of potassium?

MALETSKOS: I'm sure it was injected.

FISHER: Do you remember, did you do the counting?

MALETSKOS: All the measurements were done in my laboratory at the Harvard Medical School.

FISHER: Do you remember who the sponsor of the work was in any way? Do we have to look at the paper?

MALETSKOS: Yes, you have to look at the paper, and now, it could have been the Atomic Energy Commission. I don't remember what potassium it was; may have been from Oak Ridge, I'm not positive; I think that's a cyclotron-produced material, though[, so it probably didn't].

FISHER: It was potassium-43.

Personal Anecdotes

GOURLEY: So, how well did you know Shields Warren?

MALETSKOS: Very well. I knew him from the time I came to the Radioactivity Center. There was always interaction between Robley Evans and Shields Warren. I had enough interaction with him, so that he invited me to come over [to Harvard] and join his group and be the head of biophysics. He was interested, that's why I went over, because it was a good challenge and he wanted to see to what extent neutron activation analysis could play in research medicine and in forensic medicine. That's why it was in the Harvard Medical School. He put me in what was then the Department of Legal Medicine; that was a whole new experience, to be in a Pathology Department that was devoted to death by non-natural means.

FISHER: One of the questions I've been dying to ask you—it doesn't relate as much to human experimentation, but here we are interviewing you overlooking a beautiful lake, and a forest surrounding on a hill near the ocean. Did you commute every day from Gloucester down to Boston to work, and then come home again?

MALETSKOS: Absolutely, I commuted by train. In the early days, I commuted by train, there were quite a number of trains. In many respects, it was very good, because [during the long commute] I kept up with my references very well. I learned how to write reasonably well on the train so the secretaries could read my writing. So I got a lot of writing done and it was a

good time not be bothered by anybody because I knew I didn't want chit-chat with the other people on the train.

FISHER: I wanted to mention that, because where you live now is where you've lived for how many years?

MALETSKOS: 47 [or] 48 years.

FISHER: So you've made this commute quite a few times.

MALETSKOS: Yes, until it got so bad because I had to have meetings in Boston or Cambridge in the evenings, and I started getting home way too late. I decided that I'd better start driving, which I did. I did drive while I was at MIT for a while, and then I drove all the time when I was over with Shields Warren at the New England Deaconess Hospital and the Harvard Medical School. Finally, I went out on my own, as you know, back in 1972, when my research funds went down the drain because it was all "soft money." I decided that I would to go out on my own and see what would happen, and luckily I could do half-time research, even as a consultant, so I had my cake and ate it with no administration.

Research as a Private Consultant and Additional Publications (1972–95)

FISHER: That's a remarkable aspect, also, of you career. I think we should mention for this public history that in 1972 you left MIT and became a private consultant; [for] the last 23 years you've been self-employed. You also remained very active in professional society activities, and you belong to numerous professional societies, and you served on numerous scientific committees that are all listed on your résumé.

MALETSKOS: A one-man operation, that's correct.

FISHER: I'm curious about another experiment mentioned in your list of publications, a paper by Reeve, Green, Maletskos, and Neer. The first author is Reeve, "Skeletal Retention of Calcium-45 and Strontium-85 Compared: Further Studies on Intravenously Injected Strontium-85 as a Tracer for Skeletal Calcium."[109] What do you remember about this work?

MALETSKOS: Reeve[110] was a fellow that came to work with Bob Neer [111] at the Massachusetts General Hospital; he came from the United Kingdom. One of the things that they wanted to pursue was what other tracer could they use for long-term studies of calcium metabolism, as opposed to the calcium-47. It was easy to measure, but for the long-term studies you had to use

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[109] See J. Reeve, J.R. Green, C.J. Maletskos, and R.M. Neer, "Skeletal Retention of Calcium-45 and Strontium-85 Compared: Further Studies on Intravenously Injected Strontium-85 as a Tracer for Skeletal Calcium," in *Calcified Tissue Int.* 35:9–15, 1983.

[110] J. Reeve, a physician, was interested in bone metabolism and the physiology and function of the parathyroid gland. He worked for one or two years with Bob Neer at Massachusetts General Hospital.

[111] Robert M. Neer, M.D., an endocrinologist at Massachusetts General Hospital who conducted research on osteoporosis, bone metabolism, and the physiology and function of the parathyroid gland

calcium-45, which meant an awful lot of analytical measurements: you can't measure [it] any other way because it's a beta emitter only. So, they decided to do this experiment, and this experiment was essentially [a] Massachusetts General Hospital [study]. I participated on a combination service and part of the team basis, because I could do the measurements on the whole-body counter on these people at the Radioactivity Center.

FISHER: Do you remember who the subjects were? You counted them?

MALETSKOS: No, I don't remember now, I just don't remember. I don't know whether they were patients of Dr. Neer, who [were] okay from the standpoint of the calcium metabolism, because Dr. Neer was an endocrinologist[112] and he was also head of the endocrinology group. So there could be something else wrong with you, but it wouldn't interfere with the skeleton measurements; [or] they could have been just regular normal people, [obtained in the] usual way.

That was all done through the Massachusetts General [Hospital, and samples were] then brought over to MIT for counting. Then all of us on the team, sat down and analyzed the data, and calculated everything else, and put the paper together. There were other experiments in there, like in the Krohn's disease [(a congenital disease of the GI tract)], where you are using natural [radioactivity], namely the potassium-40 that's in the people, to study the disease.

FISHER: There's another paper listed, a little later on by Shipp, Maletskos, Dawson-Hughes, "Measurement of Calcium-47 Retention with a Whole-Body Counter."[113]

MALETSKOS: That was done at the Tufts Human Nutrition Research Center [at Tufts University in Boston], where they do a tremendous amount of work on calcium metabolism; being a nutrition center, that's one of their interests. One of the interests of Dawson-Hughes was osteoporosis, and they had just received the whole-body counter from MIT. It had been set up, because MIT didn't need it anymore, and they wanted it, and it was transported to them and [set back] up. They needed to get somebody to set it up and make sure it was going to run well, and also show them how to do the experiments. So they called me in, and I did that. I did a big development job on knowing how to measure [the calcium-47] using a scanning technique, so that they could use even smaller amounts of radioactivity. [I] developed the whole technique and then [we did] a little study on it.

It turned out to be a really good way of developing the technique. I finally solved the problem, and I guess everybody start[ed] to copy them now, [that is,] those who still want to do those sort of things. Again, that was all the permissions and that sort of things; they have a very good

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[112] a medical professional who studies endocrine glands and their secretions, especially in relation to their processes or functions

[113] See C.C. Shipp, C.J. Maletskos, and B. Dawson-Hughes, "Measurement of Calcium-47 Retention With a Whole-Body Counter," in *Calcified Tissue Int.* 41:307–312, 1987.

setup over there [at Tufts] for doing all the requirements of the Independent Review Board. They do it very meticulously well, and all that was done on their side.

Comments on Human Radiation Experimentation Controversy

GOURLEY: There has been all this publicity, [and] one of the things that we are hearing about is that these radiation—there were secret radiation experiments, and [that they were] Government-sponsored. I was wondering what your experiences were with Government secrecy, and was it ever a problem for you? Did you have trouble getting information? How secret was it?

MALETSKOS: The words "secrecy" and "secret" were never mentioned until the [so-called] scandal came up. Everything was out in the open; we talked about it with everybody. Everything is in public form, either as a peer-reviewed paper, or a progress report. Soon after the experiment was done, you had a chance to write it up, so there hasn't been anything [secret] that I've been involved [in] or anything the Radioactivity Center has been involved [in], as far as I know.

Even the original iron studies that I mentioned on blood preservation came out right after the war. As a matter of fact, dated in 1946, in the *Journal of Clinical Investigation*, everything was out in the open. So it's false, as far as I'm personally concerned.

GOURLEY: Okay. Well I wanted to get that on the record. More on this strain of secrecy: One of the experiments that the press has really been very concerned about were the plutonium injections. At what point did you know about those, and were they available amongst academia?

MALETSKOS: I haven't been able to recall when I first knew, I tried, [but,] I knew before. The first thing that came out publicly, was Pat Durbin's[114] 1970 report, if I remember correctly. I don't know why hers came out before Wright Langham's report came out, when it was in 1980, I believe, about ten years later.[115]

I was aware of it before that; what I don't know is [if] I was aware of it nearly from the beginning, because after I finished my Ph.D. thesis, there was a lot of information that was adaptable to Wright Langham['s studies] and that bunch hadn't thought much about how you [analyze] these metabolic experiments, and everything else. [So], they wanted to know how you analyze them. He asked me to talk about my thesis to provide a background for his people to know. By inference, I am suspicious that I may

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

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

have been told that, but I don't remember at all that they had these experiments going, and they needed to know how to analyze it.

FISHER: So you went down to Los Alamos and lectured on the how to conduct a metabolic study?

MALETSKOS: Right, I talked about mine, because it was in great detail, and you could look at it if you want. This is the first computer model of a dynamic study for any [calcium] radionuclide. There were no digital computers in those days. This was an analog computer that Gordon Brownell [built]; does that ring a bell to you? He developed [it] over at Massachusetts General, because he wanted to study the metabolism of iodine: the first detailed metabolic [study was conducted] at Massachusetts General, using his computer. Fortunately, he designed it just in time for him to do his work, and then for me to come over, and apply my data to it. Otherwise, I wouldn't have this detailed analysis that I have in here.

First Knowledge of Plutonium Injections

FISHER: Do you recall how you learned of the plutonium?

MALETSKOS: No, I cannot recall. I'm sure I learned it between whenever they started and Pat [Durbin]'s [review], but I can't remember where, and it might have been only a little bit before Pat's for all I know, but I don't remember.

It always struck me as being an unusual thing to do an experiment where you couldn't tell a subject what you were giving [them], if in fact, they were truly doing that. I've asked myself the questions since this situation has developed, "What would I do in a case like that? What would I, personally, do?"

Well, I don't know what I would do. Remember the conditions, you had a war on, you were developing something that was absolutely brand new and you knew that there were potential problems, from an occupational health standpoint.

GOURLEY: The name itself was classified. [Plutonium was referred to as "product."]

MALETSKOS: Yes. I know that, that's the problem. You had to find out how this material operated in the body, so that you could deduce from excretion measurements or whatever what the person might have gotten. The other alternative was to use the whole workforce as a guinea pig, if you want to use the word guinea pig. That's not good, either.

So you had to do some kind of an experiment. It had to be on human beings, because they had already done a few [animal] experiments, I think it was on mice and a few rats; you knew what was going on [in terms of plutonium's fundamental behavior]. These were done at Berkeley, if I remember. Something like this is really important; [it] lasts in the body a very long time, [so] you really want to do it on human beings. And [there is] nothing better for knowing something about [the metabo-

lism in] a human being than doing [an experiment] on a human being. You can't use animal data all the time. If you could do it, you do it well, and you could do it safely.

My thinking is, "Yes, if there is a benefit to mankind, or a benefit to them or a benefit to a group of people like them, or something like that[, do the experiment]." So I don't know how I would have handled myself under those conditions.

GOURLEY: When did you met up with Langham's group, to talk to them?

MALETSKOS: That was way, way back, its got to be before 1955, I think. This [(my metabolism research)] came out in 1954, so it was shortly after.

Thoughts on the Use of "Disadvantaged" Populations in Human Radiation Experimentation

FISHER: It may be the most difficult thing for future generations to understand, [why] what may be considered "disadvantaged" populations were used for tracer studies. You've already told us today that they were institutionalized and so they were on carefully controlled diets, samples could be obtained, and they were a manageable population. Was there any concern about the fact that maybe they didn't quite understand what they were participating in, because of their mental retardation, and that this was a humanitarian concern at the time?

MALETSKOS: I've always worried about that, and it turns out that the Fernald School, if you look at the Massachusetts Task Force Report, the superintendent allowed a lot of people in his population to be used for a variety of experiments. I don't know whether he personally himself felt that he could wholesale his subjects in this particular fashion.

Until I knew about that, I thought there were only be a few isolated situations, like iron experiments, and that was it. Apparently there were an awful lot of experiments that were going on like that, where these people were being used; that sounds like a wholesale misuse to me. You always have that problem when you get a[n ideal control] population. I worr[ied] about the same thing when I went to the Age Center of New England: "Is this the right thing to do? Would I volunteer if I were in the same position?" If you have any kind of a conscience, you ask yourself these questions.

Remember I said, "We went to them two or three different times, because they were the only source. Without going down the street, and grabbing people off the street." What do you do now? You have advertisements in the newspapers; have you seen them? They're in there. They're little blurbs and they say, "We're doing this experiment and we'd like to have people of this type come in, call this number," and [they indicate] if there is a stipend, or [state what] the stipend is or whatever the [dollar] number is. We didn't do that in those days because we didn't think that was necessarily the way to go; this is the evolution of thinking.

Why is that okay now, and it might not have been okay back then, I don't know. What is the difference, going to an organization that has a large population from which you could draw. They are themselves volunteering, for example, to study aging, even though you may not be using them for aging all the time, as opposed to putting out a notice. Isn't that enticement, that notice? I don't know where that fine line is.

FISHER: But you're overriding. The thinking probably was [that] the levels of [radio]activity were negligibly low, [not] sufficient that there would be any physical harm.

MALETSKOS: That's correct, but you see, you wouldn't have gone to them in the first place if you thought that the activity was too high—at least *I* wouldn't have, let me put it that way. Remember I told you the effort was to strive to keep it down low; can you do an experiment today where only ten millirem are ever going to be given [to obtain metabolic information]? That's a pretty good target that we achieved back then, that you can't [improve on,] even today for the same type of experiment.

FISHER: There was probably also the consideration that we live in a natural radiation environment. We have radioactive material naturally in our bodies, we have internal dose on an annual basis of at least 30 to 40 millirem from [natural] internal sources[, mainly from carbon-14, potassium-40 and tritium]; this [radiation exposure] would be small compared to that? Was that thinking involved?

MALETSKOS: I don't know that we broke it down in that detail, but certainly background was a very important consideration. This [experimental exposure] was [a] fraction of background. That initial paper that I'm talking about, the paper on the preservation of blood, which describes all the basic thinking that we're talking about, is there. Whether it's spelled out or not, it's there. It's a classic paper; I think people ought to read that and see how good it is.

Career Highlights

FISHER: One thing that we would like to ask, in conclusion, is, as you look back on your career, which is a very distinguished career, and a very honorable career, what have been the highlights and what are you most proud of?

MALETSKOS: I really don't know. The reason I say that is the thing that I like about my career is, that I keep growing. I've never had a case of saying, "Well, I've done all I can or my gray cells [(brain cells)] are starting to deteriorate so quickly that I'd better stop." That sort of thing has always been a new type of challenge; this is one of the nice things; there was no way to appreciate [it] in the early days. As I mentioned to you, my background is highly multidisciplinary, even in getting my Ph.D. in three different fields. That has made me open up my avenues [to] things that I would be doing, compared to only one, and I could float between these fields without any difficulty. I can change my thinking automatically and it's intrinsic in me [to] do it, but it's also gotten me into things that would be highly unusual, if you look at the bibliography.

FISHER: Are there any particular aspects of your research that you are particularly proud of that you would like remembered?

MALETSKOS: There are several things. One, being near the beginning of an era has been tremendous. I probably never would have looked back, unless we had this episode of what I keep calling the "scandal," and will continue calling it "the scandal." I never would have looked back in the same way; I would never have gone back and looked at some of those papers, that sort of thing. I don't go back through my bibliography to just reread the titles, and that sort of thing, so that part is great.

The interaction with different types of people, from different fields, has been fantastic. That has been great; I've enjoyed that. The challenge of working in different fields has been great. The challenge of doing things that involved things that are important in terms of peoples' health, has been good, because a lot of it directly involved human health, one way or another, or the effects of agents on health, and that sort of thing. The absorption experiment was a major experiment in my whole career. I probably will never do anything as difficult as that again [and] I don't want to do anything as difficult as that was. At one point, I had the whole Radioactivity Center working for me. There was so much going on because of the short half-lives, and everything else, and the logistics for them were unbelievable, never mind the good science and the good measurements and all this sort of thing.

By the way, you asked about, we were talking about people being confined when you do these experiments. Every experiment that we talked about that involved me, [also] involved people being confined. Those people in that absorption experiment, were put in a little, small hospital that was adjacent to MIT, and that became our clinical center. The clinical center was actually being built at the time, and we couldn't use it. MIT now has a clinical center. It can do any kind of experiments on human beings, anytime it wants. That capability was not available [then].

You never do an experiment like that without putting them in a clinical center, because that's the only way you could control the total experiment; you don't lose samples, you don't lose measurements at the right time, and everything else. So those people stayed in there, they stayed in there for a week and ten days, whatever it was, based on the half-life of the material. You have to have them confined if you want to do a good experiment. If you don't, you're not going to have control. Why bother to do it? So confinement is an important thing, and it's fundamental to that experiment. Anything that involves metabolism has to have confinement, certainly in the early stages. And then later on you don't necessarily need it. So that's fundamental.

It's been a lot of fun with two people, who are two pretty good scientists and physicians, namely Robley Evans, and Shields Warren. Not many people get a chance to have two good people to work for, and that's been fun. It's been even just as much great fun being on my own, even though there [are] problems with being on your own: you don't have a secretary

available to you in the instant you need somebody. Or, until you get all the fancy equipment, you don't photocopy when you want to photocopy. There [is] a lot of little housework that you have to do that somebody else normally does, but the independence is tremendous.

I'm very fortunate: how many people can do consulting and do real research as a consultant? That's unusual; it's been unusual, and I guess it speaks well for whatever I've done prior to that. I've only advertised once, and that was when I first decided [to do consulting work, when] my funds stopped. And I still had proposals out, supposedly to be approved, and if they had been approved, I probably never would have done what I did do [(consulting)]. But I let everybody know that I was available, and if they needed help I'd be available for a few months anyway, and in about two months, I was working at a 100 percent plus; it [has] never ceased since then. That's been remarkable.

FISHER: That's amazing.

Work With Manomet Bird Observatory (1975–95)

MALETSKOS: There have been some nice, challenging things that you may have noticed in there [(my résumé)]—that [I] worked with the birds, for example. Why would I ever get involved with a bird observatory? They even ended up making me a trustee; I've been a trustee for 20 years, and on the executive committee, and everything else. Well, that was a challenging thing.

They started to do that, they got funded, they wanted to use birds as a measure of what's in the environment. And it's an ideal situation, because the bird metabolism is so high that they first pick up [(absorb)] quickly, but then they get rid of it [(excrete)] quickly. You are, in fact, following it on the basis every time you catch them. Here is an experiment that birds were recapturable, you're using the same birds over and over again, you're doing a laboratory experiment in the field. The question was, they needed more funding, [so,] they went to the AEC after the money the local nuclear power station gave them ran out, and they were interviewed by a review team that came to Wood's Hole that didn't know that the Manomet Bird Observatory had never been reviewed by anybody. The observatory didn't know what constituted a review, so it was a disaster, as far as the review was concerned.

They wanted to know what to do [to pass the next review]. And how they found my name, I don't know, but they found my name, and I went down there and told them that they had a brilliant idea, but they were going about the execution incorrectly, and they didn't have the right equipment to boot, and that sort of thing. So [I] immediately made a report and they turned around, and said "Well, why don't you take over?" I said, "You're not going to get any money with me as a consultant, because the AEC wants to know that I'm on somebody's staff, and I'm not going to be on anybody's staff. But if you want to do it, I'll get somebody and we'll work [it] out."

So I called Charles Levy[116] over at Boston University, who became a joint [investigator]. And then I designed a good bird counter and everything else. People kept saying, "Well, what did you do to make the birds go to sleep? What did you give them to go to sleep?" I said, "Nature takes care of that." All you do is turn the lights off, and they go to sleep. Put them in the birdy counter and they are asleep in ten seconds, so they're not fighting. So you could measure them for 100 minutes and they don't know that they have been there for 100 minutes.

GOURLEY: And that's just natural.

MALETSKOS: Yes, it's natural; I'm not hurting the birds. The birds would be marked, they were tagged, [and then] they were recaptured; [so] you knew what they were doing time and time again. And it's amazing how sensitive it was.

We described one experiment where there was no release [(venting of radioactive gas to the atmosphere)] ever from the operating plant, but there was a release when they went into a refueling stage. They opened up the lid for the first time; all the monitors on the tower were not working because you're not running, and the building monitors weren't sensitive enough to pick up this little oozing of radioactive iodine. But it came out and dropped on the ground.

The birds picked it up and ate it, and we saw it inside them [by means of the bird counter]. Snow came, and there was no more iodine in the birds, it was only a little bit. [Then] it melted, and the iodine was back again [because the birds were once again eating it]. Then I proved that it was [iodine by] holding a few [birds to determine if] the [biological] half-life was the right thing, because one of the radium gamma rays was in the right place [to interfere]. That was very sensitive, and then we did an experiment. Where we practiced that, there would be a real incident. We went up to Maine Yankee[, the nation's oldest operating commercial nuclear power plant] at Wiscasset, [Massachusetts] and just set up to do the whole experiment, and we offered this service when TMI [(Three Mile Island)][117] took place, and nobody wanted to have it. We would have known the whole business within about two days, and [there] wouldn't have been all this argument [about] which way did the plume go, and everything else.

GOURLEY: So the little bit of iodine that was at Pilgrim[, Massachusetts]?

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[116] Charles K. Levy, Ph.D., a general biologist at Boston University, previously with Massachusetts General Hospital, is still a professor or emeritus professor of biology at Boston University. He is interested in radiation biology and has conducted research in classical biology and multitracer tagging of wolves in the field to track the young and their behavior with respect to parents.

[117] 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.

MALETSKOS: Yes, but that was trivial; it didn't even ring a monitor. We're talking small amounts, that's the beauty of it.

FISHER: So you're now 73?

MALETSKOS: Yes.

FISHER: You'll be 74 this year?

MALETSKOS: Later on.

FISHER: You're the Dean of the [Health Physics Society] Summer School this year, near Boston.

MALETSKOS: It's going to be in Beverly[, Massachusetts] at Endicott College. Out of the city, but close.

FISHER: Still active and working hard.

MALETSKOS: Yes, absolutely, working too hard. I've got to take advantage of being old and have a little fun.

FISHER: We've really enjoyed having this opportunity to help you prepare an oral history interview.

Additional Comments on Human Radiation Experimentation Controversy and Closing Comments

MALETSKOS: I wanted to say something, if I may.

GOURLEY: Sure. Go right ahead.

MALETSKOS: I'm sorry this whole episode took place. I think it was the wrong thing to do, and it's created fear and anxiety in a tremendous number of people in the population; certainly, more [people] than ever got involved with the TMI incident.

FISHER: By "episode," you mean the review?

MALETSKOS: The announcement by Hazel O'Leary of what was supposedly taking place, what she was going to do about it, and how she was going to compensate, and all this kind of thing. I have no objection to the investigation as such, but it should have been done first, and find out the bad ones [(researchers)]. *Then* talk about the bad ones, and put it in perspective that everybody else [(every decent researcher)] was okay, and not frighten people. I get calls from frightened people, and that's not nice.

FISHER: It's taken a lot of your time?

MALETSKOS: There were five weeks' solid of news media on my back from 7:00 a.m. to 10:00 p.m., all over the U.S., all over the world, and no one wanted to listen to the story correctly. It was a feeding-frenzy situation that was completely out of control and never would have stopped unless Tonya

Harding and Nancy Kerrigan hadn't come along.[118] So the news media continued [the] frenzy, but they went into something else.

The other thing is that I feel even more badly about [is] Congressman Markey doing what he did, and not investigating anything, and continuing to say that things were bad and making personal "hay," if you want to call it, for no good reason, without actually finding out what the facts are. So you got another bunch of people who have been frightened back [from the time his report was written] in 1986. The report never [had a public impact in 1986] because it was wrong, I suspect. *He continued* to talk about his report during *this* whole episode, as being, "Look how smart he was, he checked into this long ago, and he was right." He was not right, as far as I know, on any of the ones [(experiments)] that he reported in there, and I know for sure he was not right in the one that I was in charge of. I don't like that kind of—

FISHER: In other words, the details were misinterpreted, misreported?

MALETSKOS: Yes, but why report without talking to people who did it? I mean, why not really find out? And then, don't report it if there's nothing there, or at least want to learn. I don't know who made this investigation, whether they just read the report that was in the—it's a public document—the report from MIT. It's a normal progress report to the Atomic Energy Commission; it's in all repository libraries, as well as various other places, and then in the AEC Archives and everything else. Nobody's hiding those things.

And that's not good, and I don't enjoy that kind of thing, and I don't know that there has been any real correction made.

There are still frightened people out there, and I don't know how you're still handling it [at DOE]. And, it's not going to come out well now, as an after-effect. People are going to remember what they now know, and not going to remember if everything was probably okay [and] the amount of things that may not have been okay. I don't know any of them personally myself, so I can't speak in general, but I'll bet that we'll find out it's not going to be very many in comparison to all the experiments that were done and all the people that were involved. So if you've done it [(your investigation)] correctly, very few researchers would have been found to have done it incorrectly, and therefore, there might have been a gripe. As you all know there are people that are being sued now, probably unnecessarily, because of all this when there wouldn't have been a case for any suing. I don't think that's good at all.

FISHER: This has also happened to you?

[118] Harding and Kerrigan were ice skaters vying for a position on the 1994 U.S. Olympic Figure Skating Team. After it was discovered that Harding's boyfriend had hired a "hit man" to break Kerrigan's leg, Federal investigators sought to determine whether Harding had been a coconspirator. For many months, nearly every evening news report contained coverage of the Harding–Kerrigan rivalry or the investigation. In the United States, the episode is widely regarded as the leading news story of 1994.

MALETSKOS: I haven't been sued yet, except I'm waiting for the shoe to drop. [Though] it may *not* drop, now that the task force [report] came out from Massachusetts. But that doesn't totally exonerate people like it should have; it's not a clean-cut job. Partly, I suppose, because of the composition of the task force; putting people who were subjects [of radiation experiments] on the task force is not correct. Not that they shouldn't be talked to or anything like that, but what you need on a task force is totally objective people that weren't involved with [the experiments], that are knowledgeable, even if they are people that represent the public. That doesn't bother me. But they have to be totally objective. You can't have that, what they did.

FISHER: Well, we came here so that your perspectives would be aired, and that perhaps the record can be corrected by giving you this opportunity.

MALETSKOS: My oral history is probably correct, but I don't [think] that it's going to correct anything. That's my problem, and I don't know what's going to happen at DOE's level with Secretary O'Leary. I don't know how she could retract in any way now [information] that would make anybody feel comfortable. That's what I call the irresponsible pattern in her path for having done that, and originally making big hay. I don't know the circumstances of why she did that, or anything like that and it doesn't make any difference. The fact that it did occur when it should not have occurred is what is important.

GOURLEY: Well, thank you very much.

MALETSKOS: You're welcome; sorry it's not a good, sunny day. ❑