DOE/EH-0471

HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Radiologist
Henry I. Kohn, M.D., Ph.D.

Conducted September 13, 1994

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

FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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

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

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

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

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DISCLAIMER

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

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Interview with Dr. Henry I. Kohn, M.D., Ph.D.
Setting: September 13, 1994, Berkeley, California
Interviewer: Anna Berge (Lawrence Berkeley Laboratory Archives and Records Office)

ORAL HISTORY OF RADIOLOGIST HENRY I. KOHN, M.D., Ph.D.

Dr. Henry I. Kohn was selected for the oral history project because of the positions he held at Oak Ridge National Laboratory; University of California, San Francisco (UCSF); and Harvard Medical School. This oral interview covers Dr. Kohn's career as a radiologist; he also offers his perspective on the people he worked with and the era he worked in.

On September 13, 1994, Ms. Anna Berge of the Lawrence Berkeley Laboratory Archives and Records Office interviewed Dr. Kohn at his residence in Berkeley, California.

Short Biography

Henry Irving Kohn was [redacted] He was married in 1961; he and his wife have two children. He received his A.B. from Dartmouth College in 1930 and his Ph.D. in Physiology from Harvard in 1935. From 1935 to 1937, Dr. Kohn was a Traveling Fellow, General Education Board in both Stockholm, Sweden, and Cambridge, England. From 1937 to 1943, he was an Instructor–Assistant Professor of Physiology and Pharmacology at Duke University in North Carolina.

In 1943 he entered Harvard Medical School, receiving his M.D. in 1946. He served as a commissioned officer in the United States Public Health Service (USPHS). In that capacity, from 1947 to 1953 he was stationed in Baltimore, Maryland; at Oak Ridge National Laboratory in Tennessee; and at UCSF. He remained at UCSF for ten years and joined the Research Laboratory as a clinical professor of Experimental Radiology and research radiologist. He left UCSF in 1963 to take the position of Fuller–American Professor of Radiology at Harvard Medical School and, from 1968 to 1976, served as the Gaiser Professor of Radiation Biology. Since 1976, he has been a professor emeritus.

During his career, Dr. Kohn has held the following positions:

- 1957 to 1960—Scientific Secretary, Advisory Committee on Biology and Medicine for the Atomic Energy Commission,
- 1964 to 1979—Director of the Shields Warren Radiation Laboratory at New England Deaconess Hospital (Boston),
- 1965 to 1969—Member of a radiation study section for the National Institutes of Health,
- 1971 to 1976—Director of the Center for Human Genetics at Harvard Medical School,
- 1975 to 1979—National Academy of Science Committee on Nuclear and Alternative Energy Systems,
- 1982 to 1988—Chairman of the Bikini Atoll Rehabilitation Committee, and
- 1987 to 1990—Referee for the Rongelap Reassessment Project for the Republic of the Marshall Islands.

Dr. Kohn has published more than 150 scientific papers on such topics as the biological effectiveness of high-energy photons and electrons; the effects of x-ray therapy; nuclear and alternative energy sources; and the Bikini Atoll rehabilitation.

BERGE: This is an interview with Dr. Henry Kohn by Anna Berge.

KOHN: Pronounced like "ice cream cone."

BERGE: With Dr. Henry Kohn by Anna Berge of the Lawrence Berkeley Laboratory Archives and Records Office on the 13th of September, 1994, at his residence in Berkeley. Dr. Kohn, I was wondering if we could start with a little bit on your background—where you were born, where you lived, where you got your education.

KOHN: Instead of going through all that, why don't I just hand you this brief *curriculum vitae*.[1]

BERGE: Okay.

KOHN: I was [redacted]. Went to public schools there; then to Dartmouth College. The rest of my career is stated in its essentials in this *curriculum vitae*, which I have just given you.

Studying Effects of X Rays on Animal Blood Chemistry at Oak Ridge

BERGE: Okay. Can you tell me a little about what interested you in your particular field to begin with? How you got interested in it?

KOHN: When I took my Ph.D., there was a fellow student by the name of William Arnold. During the war, Arnold worked at Oak Ridge. At the end of the war, Oak Ridge reorganized the Biology Division and Arnold became the Associate Director. They wanted somebody in the Biology Laboratory with a medical background to do work related to medical subjects; Arnold knew me and I was invited to come down. I did. And through that, spending two years there at Oak Ridge, I became interested in radiation biology.

BERGE: What kinds of things did you do while you were at Oak Ridge?

KOHN: I worked with rats and investigated the changes in their blood chemistry following single doses of x rays. I understand that people subsequently have had difficulty in confirming our results.

BERGE: Anything else you want to say about that time period?

KOHN: When I arrived [at Oak Ridge] in 1949, there was practically no Biology Division left, but a laboratory building of good size was being renovated for it. Dr. Hollaender from the U.S. Public Health Service [(USPHS)] was the director of the division. He spoke with a German accent and was not an impressive person, scientifically. However, he knew the value of money and he used his position to build up a good lab. But, as a person, he was, in my opinion, not to be admired. He was amazed when I told him that I would be transferred to San Francisco. I'm sure he had planned to surprise me by telling me that Dr. Jacob Furth, a pathologist, would be taking over my quarters and I would be

[1] A copy of Dr. Kohn's *curriculum vitae* can be found at the end of the interview.

transferred to the garret.[2] Hollaender hated people with medical training. When I tried to tell Furth, on his arrival, that he might have trouble with Hollaender, he looked down his nose at me. But, some years later, when we met in Boston, he remarked in a somewhat apologetic tone that I had been right.

Work at UCSF's Radiological Laboratory

KOHN: Well, I was then in the Public Health Service. I wanted to have some experience with man, so I asked for permission to visit Dr. [Robert] Stone's Division of Radiation Therapy at UC San Francisco. I worked there for several years on detached duty before I resigned from the Service. Dr. Stone had offered me a job in the Radiological Laboratory, which I accepted. At that time, they had just finished the Radiological Laboratory building, which was to house the synchrotron,[3] a 70-MeV[4] machine. I was to determine the radiobiological effectiveness (RBE)[5] of its x-ray beam. That's about all I can say about that. I hope your other interviewees talk more than I do.

BERGE: Some do, some don't. Did you only work on the RBE?

KOHN: No, I had an x-ray machine at my disposal, and I therefore did a variety of experiments with the x-ray beam on rats, but especially on mice.

BERGE: What was the advantage of mice over rats?

KOHN: They're smaller. You could have more of them in the laboratory.

BERGE: And what happened after you were there for a couple of years and then left?

KOHN: Well, I was there from, let's see, I stayed at the Radiological Laboratory in San Francisco from 1949 to about 1962 or 1963. Well, we did a variety of experiments, and I did some work on patients.

BERGE: Okay. What kind of radiobiological experiments did you do on mice and rats and other living beings?

KOHN: Well, let's see. First of all, my primary objective, or Dr. Stone's primary objective, was to study the relative biological effectiveness of the synchrotron's high-energy beam. Since [the project to install] the synchrotron [at the UCSF Radiological Laboratory] took a long time to get started, we collaborated with other people in Texas and in New York in doing the RBE on *their* high-energy machines. My collaborator in the laboratory was Shirley Gunter, a microbiologist. Dr. Gunter went to each one of these laboratories and did her standard microbiological

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2 a small, cramped attic

3 a machine that accelerates charged particles to very high speed by combined application of a low-frequency magnetic field and a high-frequency electric field

4 million electron-volts

5 relative biological effectiveness—ratio of the damaged caused by that radiation (i.e., the synchrotron) to the damage of the same absorbed dose of reference radiation, usually cobalt-60 gamma-rays

testing. We did standard testing in San Francisco on a million-volt machine, which they no longer have, but which was running at that time.

In 1963 or so, Warren Sinclair, who was the Texas collaborator, and I wrote a review of the international literature on the relative biological effectiveness of high-energy photons and electrons. So, we accomplished what the Laboratory [wanted], or at least what my initial assignment was.

I was also interested in age at exposure and the late effects of radiation. It makes a difference whether you irradiate the animal when he's young or when he's old. We published a number of papers on that, with Bob Kallman and, for example, with Paul Guttman; the last reference, on the [*curriculum vitae*] sheet, tells about that. Then, I did some clinical work on the effects of radiation therapy on the blood count. I did some work with Dr. Zippen and Mrs. Lum on breast cancer. I could talk on and on and on, you know, and I don't want to do that.

BERGE: Oh, please do.

KOHN: From my point of view, it isn't that important. But, I think anyone with any training could look over these selected papers, which I list in the *C.V.*, and see the sorts of thing I was interested in. I'm using *I*, but of course, my collaborators were equally interested. They showed—better than I can in my stuttering way here—what went on then. I can give you a list of all the papers I've published if that's desirable, but I think this selection here (I don't know quite why I wrote this brief *curriculum vitae*) tells the story. Whoever's interested can look this over. If he has any background at all, he'll immediately see what's going on: you can read one of the papers.

BERGE: Do you feel that your years at the Radiological Lab were productive in the way that you had hoped they were going to be?

KOHN: I didn't hope; I just automatically assumed that they would be productive if I worked hard. And they were. But the thing about science is that science moves on. So, each chap who is working hopes his work will be great. While it may be great for the moment, he is more like a bricklayer building a wall. You lay your bricks, then you drop out. Another fellow comes and lays some bricks on top of yours and so it goes. Unless you make some really important discovery, most scientific work is just part of the bricks and mortar that go into the general structure, if I make myself clear. While the work was okay, I don't think any of it deserves the Nobel prize.

BERGE: Can you talk a little bit about what conditions were like to work under during those years?

KOHN: Conditions in the early '50s were very good. There was money, and if you worked, any reasonably honest, good job could get support. That probably isn't true today. I have no complaints whatsoever. I feel I was quite well treated by the Atomic Energy Commission [(AEC)].

BERGE: Did you mostly follow your own research, or were you able to choose your topics of research and then proceed, or did you follow the program that the AEC had intended?

KOHN: I had complete authority. When Dr. Stone offered me the job, he did make the condition that I would determine the RBE of the synchrotron beam. In fact, we knew what it would be from the work on the other high-energy machines, done before the synchrotron was ready. But, other than that, all the work was of my own design and choosing, and my junior collaborators were selected by me on the basis that such topics would be congenial to them and that they were technically able to pursue them.

BERGE: Can you tell me a little bit about Dr. Stone? How it was working for him, what type of personality he had?

KOHN: Dr. Stone, from my point of view, was quite senior in 1950. I was about forty years old and Dr. Stone, I suppose, was about sixty; I don't know, exactly. So, I rather looked up to him, first, on the basis of age, and then because he was a very well-known figure. He was a short man, gray-haired at that time. He spoke in a gentle, low voice. He was very much of a gentleman, but not a pretentious gentleman. He was very easy to talk to, but we did not have very many conversations, actually.

Dr. [Gail] Adams, the physicist, finally got the synchrotron going. They did not have anyone available to treat the patients. Dr. Stone wanted to have a particular person assigned to that. He offered me the job of treating the patients, since I had qualified to be a radiation therapist, but I declined because I felt that if I treated the patients and was doing experimental work, it would be intellectually unsatisfactory. I didn't do it. And, I'm glad I didn't. I liked Dr. Stone is about all I can say.

BERGE: How do you mean it would have been intellectually unsatisfying? Meaning, do you prefer research?

KOHN: No: what I mean is, if you're going to be a good therapist, you've got to devote a lot of time to it. (If you're going to be a good experimentalist, you have to devote a lot of time to it.) And, I didn't want the responsibility of treating patients every morning and then going to a lab every afternoon. Because I was much more seriously interested in experimental science than that would allow. This is not to say that clinicians shouldn't do laboratory work; I don't mean that. But for me, I couldn't make that time division. So, I didn't.

BERGE: Was that for the synchrotron program?

KOHN: That was for whatever I was doing at the Radiological Laboratory. Or, don't you understand the nature of that laboratory?

BERGE: Not entirely, no.

KOHN: All right. Well, when Dr. Stone left the AEC and went back to his post at the UCSF as head of the Department of Radiology, a decision had been made by the AEC to have a number of National Laboratories. The

AEC was supporting Oak Ridge,[6] Brookhaven,[7] Argonne,[8] and then, on a very much smaller scale, they decided that Stone should have a high-energy unit for therapy in San Francisco. Dr. Stone chose the highest-energy machine possible with reasonable planning. The General Electric Company had built a 70-MeV synchrotron, and they would build him a second one. So, a special building was constructed to house the machine behind the main USCF buildings; it would also contain some laboratory space. Have you seen it?

BERGE: No.

KOHN: The laboratory was completed around 1950 to 1951. The synchrotron was a very large machine; it had to be installed, made to work reliably, and calibrated. Dr. Gail Adams came to be the physicist in charge of the machine, and he also instructed the radiology residents in Radiological Physics. Dr. Stone asked me if I would head the little Radiation Biology Unit, and I accepted. Of course, I was working there some years before the machine was treating patients. I had a small group of associates consisting of Bob Kallman—who has just retired at Stanford [University], where he became the professor of Radiation Biology.

And Dr. Shirley Gunter, who had just taken her degree in Berkeley with a very well-known microbiologist, Dr. Stanier. They were the two people I started with and, then, they moved on and some others came; eventually, a fellow by the name of Ludwig, who later became a professor at [University of California at] Irvine. Paul Guttman and Donald Baily, during that time, we did a variety of experiments. Dr. Gunter first did this work which is cited over here on page two [of my *C.V.*]: Gunter and Kohn, *The Effect of X-rays on the Survival of Bacteria and Yeast.* Because we wanted to use those organisms in determining the RBE of the high-energy machines. She did an extensive survey there, and then went on and visited New York and Texas. Dr. Kallman did a lot of work on mice in the lab.

Advantages of Yeast Cells for Studying Radiation Effects

BERGE: I noticed from reading other bibliographies—for example, Dr. [Cornelius] Tobias—that during the 1950s there was a lot of interest in studying radiation effects on yeast cells. Why?

KOHN: Well, bacteria, as you probably know, have only one chromosome. Yeast have pairs of chromosomes. You can grow them in suspension. They grow like bacteria, and are very convenient to work with.

BERGE: As opposed to, say, fruit flies?

KOHN: I wouldn't say that yeast are opposed to fruit flies. I just say that it is because they are easier to work with. I always come back to that. No,

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[6] Oak Ridge National Laboratory (Oak Ridge, Tennessee)

[7] Brookhaven National Laboratory (Upton, New York)

[8] Argonne National Laboratory (Argonne, Illinois)

also, especially for the biophysicists[9] who came in from physics or who are not so familiar with different kinds of biological material. To a biologist, perhaps, working with the fruit fly may not pose any great problem. Whereas, I think to the physicist it may appear to be a little complex. But, working with yeast, [means that] all you have to do is inoculate the liquid culture, put it in an incubator, and look at it the next day: "Has it got yeast in there or not?"—and so forth. Those are the technical advantages. I think that's the answer.

I don't see that yeast has any great virtue. Well, perhaps it has one. There are two kinds of yeast cells. There are some that are haploid[10] and there are some that are diploid.[11] So you can compare the "one chromosome" group with the "paired chromosome" group and see what difference having the second chromosomes makes. So that's a virtue.

Reflections on Bert Low-Beer and Joseph Hamilton

BERGE: Can you talk about some of the other people that worked in the Lab? For example, you mentioned before[12] [Bert] Low-Beer.

BERGE: No, Low-Beer didn't work in the Lab. Dr. Low-Beer was the head of the Radiation Therapy Division of the Department of Radiology at the University of California San Francisco. He was from Czechoslovakia; he had escaped, I believe, from Prague. Very interestingly, I think his father had been the chief Rabbi, and he went to England. I believe he [(Dr. Low-Beer)] worked at Manchester[, England] for a while with radioisotopes in the laboratory over there. So, he was familiar with radioactive phosphorus, which was a well-known tool in those days. But he had been trained as a radiologist originally. He simply got that job when he entered England, and he then came to the United States.

I don't know quite how he got to Dr. Stone's department. I think he may have worked with the Lawrence group for some brief period of time and then came over to Stone's department, where he became Assistant Professor of Radiation Therapy and then Associate Professor of Radiation Therapy. He did no experimental work when he was with Stone, but he was interested in it. He contracted leukemia, and he died while I was there. And, no doubt, his leukemia resulted from his exposure either in England—well, during his career, whenever, but not in San Francisco.

BERGE: Can you talk a little bit about the exposure of most people who worked with radiation at that time? What was the awareness level of the dangers of exposure?

KOHN: I think it was very good. I'm sitting here, eighty-five years old. I've been working with radiation since 1947. My colleagues, whom I've

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9 practitioners of *biophysics,* the branch of biology that applies the methods of physics to the study of biological structures and processes

10 having only one complete set of chromosomes, as opposed to two (diploid)

11 having double the basic number of chromosomes (as possessed by haploid organisms)

12 in advance of the interview

known, past and present, are all okay. I think we were quite well aware of the dangers of exposure.

BERGE: Was Dr. Low-Beer particularly—

KOHN: —careless? No. I don't think so. I don't know when he got his overexposure. It may have been when he was in England. Or, it may have been when he started out in radiology and was being trained as a radiologist in Prague; they may not have been so careful taking x rays. So, he was repeatedly exposed, albeit to small amounts of radiation. You know about Dr. [Joseph] Hamilton,[13] of course. He died of leukemia.

BERGE: But he was notoriously careless.

KOHN: Yes, yes.

BERGE: Do you know anything about Dr. Hamilton?

KOHN: No, I was acquainted with him. But I didn't know him intimately.

BERGE: Did he come over to UCSF or did you ever come over to Donner Laboratory?

KOHN: Rarely, rarely.

Radiation Genetics Experiments on Mice

KOHN: Another topic I was interested in, which raises an interesting question, [is this]: We worked in the field of radiation genetics with the mouse. There have been only three or four or five laboratories, perhaps, that have done significant amounts of that kind of work in the United States. Now, we worked with something called the histocompatibility[14] system. If you take a piece of skin and transplant it, from me to you, you will reject it because we don't have the same genetic setup. On the other hand, if you take it from one identical twin to another, they will accept it, because they have the same genetic setup.

In the case of animals—say, mice—we breed inbred strains. That is, only brothers and sisters are mated, and the special strain is thus established. And they, for the most part, accept skin transplanted from one another. But occasionally, mutations will occur. Then, when the skin is transplanted, that skin is rejected.

So, we know that there are [a] number of genes which control what is called histocompatibility. In other words, tissue compatibility. There may be as many as, I don't know, forty such genes, of which perhaps ten or fifteen may be the more important ones.

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[13] Dr. Hamilton, an M.D., worked at Crocker Laboratory, then the site of a 60-inch cyclotron. Crocker was a part of Lawrence Radiation Laboratory, later renamed Lawrence Berkeley Laboratory. Hamilton, who died of leukemia, is discussed in several transcripts of this series, notably in the John Gofman interview (DOE/EH-0457).

[14] the condition of being similar antigenic types such that cells or tissues transplanted from a donor to a recipient are not rejected

I should say, a chap by the name of Bailey, Donald Bailey, who was a geneticist, devised a technique for transplanting the skin of mice on their tails. That was nice, because it was very easy to read the results of the experiment. He was a worker supported by the Radiological Lab for a number of years. He and I—I did the radiation and he did the genetics—looked for mutations in the histocompatibility loci.[15] We never found any. When I went on, later, to Harvard and extended these experiments with [Roger] Melvold, we still never found any.

The mutations may not show because the cells carrying it do not survive for any great period of time, or they may be shunted aside. But in any case, I believe it probably is the only such example. Donald Bailey left UCSF and then went on to a famous laboratory at Bar Harbor in Maine, where he became a senior investigator, and for a while the director of the laboratory.

BERGE: What part did radiation play in that? I understood the genetics part. I didn't understand—

KOHN: —We tried to induce mutation with x rays, and we couldn't do it. I believe we—it's been estimated, we tested something like a million and a half irradiated genes. Much of that work was done with Roger Melvold later on at Harvard. He's now professor at Northwestern University Medical School [in Evanston, Illinois].

Reflections on Reynold Brown and Henry Kaplan

BERGE: Do you have any other colleagues from that time at UCSF that you want to talk about?

KOHN: There was a chap by the name of Reynold Brown. And he's an important one for you to get hold of. He is now retired; he was the chief or the medical examiner for one of the big insurance companies in San Francisco. Get hold of him; he was also the Health and Safety Officer for UCSF.

Now, who else would there be? Of course, you could always go over and speak to the head of radiation therapy. The trouble is that the people who are in there now played no role in the era in which you are interested, so there's no point to our discussing it with them. Bob Kallman down in Stanford could tell you about Henry Kaplan; Henry Kaplan was the Professor of Radiology at Stanford. But I don't think that Henry really had any connection with the Lawrence Livermore people; I don't know. If there is something to be learned, Bob could tell you about that or not. That's really about all that I can—

BERGE: —Maybe we could round it out with some information on your work after you left, when you went to Harvard. You can tell me a little bit about those years there.

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[15] sites

Establishment of Harvard's Joint Center for Radiation Therapy (Mid '60s)

KOHN: It was owing to a man by the name of Shields Warren. Have you ever heard of him? Shields Warren obtained money for a radiological laboratory at the New England Deaconess Hospital in Boston. I left this place to go to Harvard, and also to become the director of the Shields Warren Radiation Laboratory. That laboratory was supposed to promote experimental work in the radiological sciences that pertains to diagnosis, therapy, and to isotopes. So, each department—the way I eventually organized that part, each department had its own space in the laboratory. In other words, the Department of Diagnostic Radiology had a laboratory, which it was responsible for operating, and I did nothing. The same with Therapy, and the same thing with Nuclear Medicine. I had my own lab for Radiation Biology. So, I considered myself a rental agent, unlike most directors of laboratories, who operate quite differently.

The thing I was most interested in going to Harvard for—well, there were two things. First, you must understand that the Harvard Medical School has a number of hospitals in its community, so to speak, where the heads of department—say, Radiology or Medicine or whatever—are all members of the Harvard faculty. So, the Harvard Department of, let's say, Radiology consists of the members of Radiology of the Massachusetts General [Hospital], the Beth Israel Hospital, the Brigham [and Women's Hospital, and] so forth. So, it's a large establishment. In the case of radiation therapy, there were these five or six units and all of them were included under the rubric of Department of Radiology. There were not separate Departments of Radiation Therapy and of Diagnostic Radiology.

Furthermore, all the units were acting independently. Now, to judge the results of treatment by radiation therapy, requires the statistical analysis of large numbers of patients, and none of these institutions had, in my opinion, a large enough population of radiation therapy patients to make such investigations valid.

What I wanted to do in going to Harvard, was to establish a Department of Radiation Therapy which would transcend the individual hospital. All would be part of a single department, which had one head and would act in concert. I wanted to call it the Conjoint Center for Radiation Therapy. The treatment of the patients could be in the various hospitals; there's no problem about that. It was the coordination of the results. When I started this movement I think Harvard had maybe, altogether, I don't know, perhaps four hundred patients a year. As I say, they were distributed among hospitals treating fifty to a hundred patients a year. So, that was my first objective in going. I didn't want to be the head of this, but I wanted to see it created.

To make a long story short and to pass over a lot of political shenanigans that has come to pass, there now is something called the Joint Center for Radiation Therapy in the Longwood Avenue area. It may be the largest department of radiation therapy in the United States. They treat

something like four thousand patients a year—certainly, far more than three thousand. I regard that as my chief contribution in this field.

Then, the other thing was to create a Laboratory of Radiological Sciences, and I think I did the right thing there by establishing groups for diagnosis, for therapy, for nuclear medicine, and for my own work in biology. Each group was responsible for itself; they were not under me as a laboratory director.

My own experimental work, in collaboration with Dr. Roger Melvold, confirmed that previously done with Dr. Bailey, that there were not any transmissible x-ray–induced mutations in the mouse histocompatibility system. I should also add that this work led me to have a general interest in genetics work at the medical school. Harvard at that time did not have a department devoted to medical genetics. Instead there was established at that time a loose confederation of workers from several departments in several hospitals, called the Center for Human Genetics, of which I served as the initial director for some years.

Radiological Assessment for the National Academy of Science Survey of Nuclear and Alternative Energy (1975–79)

KOHN: A project that took a great deal of my time and energy during the period of 1975 to 1979, was the work of the National Academy of Science's Committee on Nuclear and Alternative Energy Systems. There were about a half-dozen people on the committee, and this book *(hands Berge a book)* is the committee's report, issued in 1980.

BERGE: *The Committee on Nuclear and Alternative Energy Systems*?[16]

KOHN: Yes. It's a good book because the world, not the United States—is going to run short of fuel to produce electricity. Obviously, nuclear substances are one of the fuels to produce electricity. But most people somehow or other feel that we can find a substitute for them, that we don't have to use nuclear energy to produce electricity, that we can get our energy from the sun, the wind, etc. This report lays a baseline for such discussion.

I did the radiological aspects of the discussion in this book. That was a great interest of mine, and a very important one from a practical point of view, and in its way more important than many of the ongoing topics in radiation biology. More recently, I have written a review of *The Nuclear Lion*[17] by John Jagger, *(hands her Jagger's book)* and I am going to give you a copy of it. Jagger's book is what you might call a continuation of what the report says about radiation. My review is really a very good summary of his, so that if you read it you almost don't have to read his book.

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[16] National Research Council. *The Committee on Nuclear and Alternative Energy Systems* (series). Washington, DC: National Academy of Sciences (1978).

[17] Jagger, John. *The Nuclear Lion: What Every Citizen Should Know About Nuclear Power and Nuclear War.* New York: Plenum Press (1991).

BERGE: *(flipping to the final page)* 402 pages.

KOHN: That's why I'm saying it. I know the predicament you're in: if you read everything that people threw at you, you'd never get through. But, I think [that] that [(my review)] very fairly represents Jagger. Now, that point of view is not a popular one in the United States today, but time will tell as to whether it is necessary to adopt it. This problem, as you can see, has been a major interest of mine.

What else can I tell you? It's difficult; I never thought of making a summary evaluation of my career. I've enjoyed my career very, very much, and I feel that I've been quite lucky in the way that it has gone. I somehow or other always managed to have a good job, and that's very important, I might add: to be quite interested in what I was doing. I've never had a period in which I've said, "Why am I doing this? This is awful work! These people are preventing me from doing what I want!" I've had some constraints, but nothing I could dream of complaining about. I never knew the people over here in Berkeley very well.

I have only met Lawrence. Well, I saw more of [Cornelius] Tobias.[18] But, his interests in a way were different than mine, I think, because he was trained as a physicist.

Biologist and Physicist Perspectives on Radiological Effects

BERGE: Can you explain a little bit about the difference in perspectives from a physicist or a radiation physicist and a radiation biologist's point of view?

KOHN: I'm not sure that I would want to generalize too much, but I think the physicist would tend to think in terms of his knowledge of atoms, neutrons, protons, and so forth, and then when he gets interested in biology, he sees it as a formal, or somewhat more formal, problem. What's making me hesitate here [is that] I'm thinking of another physicist of about the same age as Tobias, perhaps even younger. Well, I'd say the physicist's associations would be all with physical and chemical phenomena. The biologist, who may know a good deal about physics and chemistry, nonetheless has another set of associations dealing with the functioning of the organism as a whole. I think that's the way I would put it.

So, for certain kinds of problems it doesn't make any difference, because if the problem is very closely defined and if the problem deals with a particular physical aspect that underlies a biological end point, they'll come to it in the same sort of way. But then, the biologist can go off and think about the functioning of the whole animal, and the physicist by and large doesn't quite [do so], just as the biologist couldn't go

[18] For the transcript of the interview with Tobias, see DOE/EH-0480, *Human Radiation Studies: Remembering the Early Years; Oral History of Biophysicist Cornelius A. Tobias, Ph.D.* (July 1995).

on and think of the high-order physics. The biologist, of course, may tend to think of epidemiology,[19] which is of considerable importance.

On the other hand, the other physicist who came to my mind, a man by the name of Warren Sinclair, ultimately became quite interested in the effects of radiation on populations. It would take more time to think through just what the differences are.

You mentioned that Stone and [John] Lawrence didn't get along. Thinking back now, I do recall that there seemed to be something or other between the two labs [(Donner and Crocker)].[20] There was no pressure on me. I could do whatever I wanted. Is Lawrence still alive?

BERGE: No.

KOHN: No. So I can say anything I want. It may have been that Lawrence's brother [(Ernest)] exerted a certain amount of pressure to establish that lab and see to it that his brother was the chief. I really don't know. On the other hand, my general impression was, "Lawrence was okay as a professor; nothing wrong, you know, with his being one." Have you interviewed Tobias yet?

BERGE: No, I believe several interviews were made of him in 1979. And right now he's living in Oregon.

KOHN: Oh, is he?

BERGE: So, it'll be a little bit more difficult for us. I called him up, and he was willing but he wasn't willing to come down.

KOHN: No, of course not. The thought passes through my mind that this has not been a very satisfactory interview. Sort of haphazard, jumping around. I ought to be able to give you a straightforward, coherent story which shows how I started at something and this developed into some great thing which has flowered and benefited mankind. Perhaps you'll get that from some of the other people you work with.

My career has been more varied than that of most people. You know I have an M.D. and a Ph.D. I'm a licensed, so to speak, radiation therapist: I've passed the boards. I wrote my Ph.D. thesis on photosynthesis.[21] I've worked on a greater variety of things (plants, animals, man, [and] so forth) than most. So, it's harder to make it coherent. It would have been much easier for you with somebody who specialized, say, in the central nervous system and stayed with that all of the time.

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

[20] The rivalry between these two branches of the Lawrence Radiation Laboratory is discussed by Dr. John Gofman in the transcript of his interview (DOE/EH-0457, May 1995).

[21] the process by which plants convert carbon dioxide, water, and inorganic salts into complex organic materials, especially carbohydrates, using sunlight as the source of energy and with the aid of chlorophyll and associated pigments

BERGE: But, maybe you can give me an idea of some of the different work and developments pertaining to plants, animals, and men.

State of Knowledge in Radiation Epidemiology

KOHN: Well, what would you like to know? I'll tell you, one of the problems—I feel that we really don't know very much more now, basically, than we did formerly. It's a funny thing to say. Oh, we know much more about details, and the details can be interesting. I'm speaking now of radiation biology and radiation epidemiology. But, the same problems face us for radiation epidemiology [now] as then.

There has been a constant effort to push back [(push down)] the levels of acceptable exposure to radiation. And so, they are being pushed back and back until they are reaching the stage now where their effects can't be measured either epidemiologically or biophysically, in a practical way. I think that there is a danger of pushing this too far. Because the further back you push the acceptable limits of exposure without being able to prove, in fact, that you are absolutely correct in doing so, the more you reinforce the idea that any radiation exposure is dangerous to man; and therefore, we should prevent it.

Whereas, as you will see from this paper over here, the review of Professor Jagger's book, that it may turn out in ten or fifteen years that we must use nuclear energy [to generate] electricity, and that we are not paying enough attention, now, to the engineering of electricity production through nuclear energy. That, it seems to me, is the major problem here.

BERGE: What do you see as the changing attitude? How do you suppose it came about from when the first bomb was dropped [in August 1945], and all through the 1950s people were generally supportive of research on radiation? What happened to change that?

KOHN: I didn't mean that they weren't supportive; they still are supportive of the investigation of the effects of radiation. What I mean is that they have become so frightened of the effects of radiation, that this will impede the engineering studies to make more efficient use of nuclear energy. This doesn't mean that I'm all in favor of nuclear energy being scattered throughout society. But, it does mean that there is a real problem, as defined by Professor Jagger's book, and that problem will have to be faced. I think, from the point of view of radiation toxicology—that's what I would call it—we know probably enough. Possibly somebody is going to discover an antidote for radiation toxicity, which would make me wrong now, but I don't think that's going to come very soon.

Well, I think that sort of delivers my message, which isn't very much.

Incidentally, if you want a comprehensive book—I don't think you have the technical background to deal with this. Are you familiar with these books?

BERGE: I've seen various other types of book that deal with the same.

KOHN: There is no other one that is as comprehensive and as though as this one: *Sources, Effects, and Risks of Ionizing Radiation.* United Nations Scientific Committee on the Effects of Atomic Radiation, 1988 Report to the General Assembly, with annexes.

BERGE: Did you also work on this committee?

KOHN: I worked, I helped the first committee that produced one of these. I think that was back in 1957 or something like that. I helped as a consultant to the staff of the book but I haven't done anything with it since. These people now have an office in Vienna. This is really a monumental work.

BERGE: What kind of consulting did you do for them? Were they asking you about facts, about—

KOHN: —No, I came into the office and I did the chapter on radiation biology.

Radiation Therapy

BERGE: I see; I see. Okay. Well, while you were talking, I took some notes and I was wondering if I could go back and ask you about those.

KOHN: Sure.

BERGE: When you were talking about your time at the Shields Warren Radiation Lab, you mentioned the statistical analysis of radiation therapy treatment. Were the treatments in the various hospitals experimental, or were they by that time already established therapies?

KOHN: They were not experiments, in the conventional sense.

BERGE: So, what was the necessity at that time of doing the statistical analysis?

KOHN: Cure rates vary with, one, the type of cancer and, two, the stage of the disease at which treatment is initiated. To make comparisons between different cancers, or different stages of cancer, it is necessary to make statistical comparisons. Likewise, when seeking to improve the cure rate by some change in the treatment schedule—for example, by increasing the dose by ten percent or by giving five treatments a week instead of three.

BERGE: How did they establish a dose, originally—for example, like five times a week? Let's say it was five times a week.

KOHN: In the early days of radiation therapy, it was generally held, especially by the German school, that the biggest dose (tolerated), given as fast as possible, was the best treatment. During the period, roughly 1920 to '30, Claude Regaud of Paris argued that the differential effect of x rays on cancer and normal tissues could be best obtained by giving the treatment slowly. For example, healing was very much better when skin cancer was treated over a period of a week than in one day.

Originally, they used a radium applicator, strapped, say, to the arm. When the x-ray machine was introduced in about 1920, they

fractionated[22] the treatment: one brief exposure per day for seven days. I began training in 1949; treatment schedules had gradually been improved and [had] become more or less standardized—for example, treating over the course of five weeks in the case of many types of cancers.

The need for relatively large numbers of patients in order to gauge the effectiveness of treatment is readily appreciated when you consider the following hypothetical example. Suppose [that] a clinic treats four hundred patients a year, that there are five principal types of cancer, and each may be classified in four stages. On the average, there would be twenty patients in each specific subgroup. If you wished to change the treatment, you could have ten in the new group and ten in the standard one. If you expected survival to be improved from five out of ten (fifty percent) to seven out of ten (seventy percent), it would be difficult to establish. Obviously, much larger numbers of patients and good statistical analysis would be required.

Recollections of Shields Warren

BERGE: You said that you had been brought to Harvard by Shields Warren, and I was wondering if you could talk a little bit about him.

KOHN: Shields Warren was Chief Pathologist at the [New England] Deaconess [Hospital] and Professor of Pathology at the Harvard Medical School. He became interested in pathology in the late '30s, owing to his interest in cancer. In 1939, he became an officer in the Navy's Medical Department and, circa 1942, with Dunlap, Gates, and Friedman, wrote a series of papers summarizing what was known about radiation pathology. He was in the first team to visit Nagasaki and Hiroshima after the bombing and, I believe, he was the primary instigator of what later turned into the Atomic Bomb Casualty Commission.

When the AEC was established in 1947, he became the first director of the Division of Biology and Medicine, a post which he held until they could find a permanent appointee. Dr. Warren had established his own Cancer Research Institute at the New England Deaconess Hospital, and looking back upon it now, I suppose he wanted to join to it a laboratory building that would house a high-voltage therapy machine and a small research radiobiological laboratory, similar to Dr. Stone's.

BERGE: What made him invite you? Do you know?

KOHN: Well, I suppose I would say that I was the outstanding and middle-aged fellow available. *(smiling)* No, I won't be that bold. Oh, there was Austin Brues, head of the Biology Division at the Argonne National Laboratory. A little older than myself, but no longer alive. He was a good friend of Warren, and in my work we had frequently met. I believe it was he who recommended me to Warren. I think they had first offered

[22] separated or divided into component parts

the job to Henry Kaplan of Stanford, but he made some remarkable requests, which they did not meet.

Then they came down to me. The appointment involved a tenured Harvard professorship as well as the directorship of the Deaconess Lab, so I was examined by a medical school committee, too. Frankly, I don't think there was much competition for the job; there were not many who were qualified in medicine and biology, and who wanted to work for a "nonclinical" salary.

BERGE: What happened to Shields Warren after you moved to Harvard?

KOHN: He was still at his post at the Deaconess, but I believe the project was a great disappointment to him. I now believe that he had hoped for a tight connection to his Institute. But, I had made it clear to him—and everybody else—that I hoped to make the lab a medical school facility and to establish a Conjoint Center for Radiation Therapy, to which it would be attached. My acceptance involved drawing up another set of plans, moving the lab to a central location in the Longwood Avenue area of the School, and including in it facilities for research controlled by diagnosis, therapy, and nuclear medicine as well as radiobiology.

Warren's son-in-law told me subsequently that Warren hated me. I can see why. He had written hundreds of letters to raise the two million-plus dollars needed for the lab building, and he had been the primary person responsible for getting an American Cancer Society professorship for the lab's director. He gave in to my plan, I suppose, because he could not face giving back the funds he had raised. I was so naïve when I put forward my plan that I did not know how powerful my position was.

His plan didn't even fulfill the requirements that he stated in his contract with the Public Health Service (raising money for the building). God knows how many hours Warren spent on that project, which yielded him so little satisfaction. However, the Deaconess Hospital did name the Lab after him: the Shields Warren Laboratory.

Research Into the Effects of Radiation Therapy on Blood Count

BERGE: I'm just about finished. You mentioned something about doing radiation therapy work in UCSF, specifically on the effects on blood count.

KOHN: Yes.

BERGE: What kind of work did you do with that? Did you have any collaboration for some of the hematologists[23] at UCSF?

KOHN: No, I was no great shakes. But where is that mentioned? That paper was published in 1955, *Changes in the Human Leukocyte Count During X-ray Therapy for Cancer and Their Dependence Upon the Integral Dose*. I had noticed when doing some clinical work that the literature on changes in the blood count was very small—amazingly so. I had a tech-

[23] medical specialists who study the nature, function, and diseases of the blood and of blood-forming organs

nician who drew the blood and did the counts of patients before, during, and after treatment. The results were interesting, though I still am not sure as to what they mean.

Do you understand what is meant by dose?

BERGE: What do you mean by "I don't understand"?

KOHN: Well, radiation dose means the amount of radiation given *per gram* of exposed tissue. What I found was that if you irradiated a small part of the neck, you got a certain depression of the [leukocytes[24] in the] blood count; [but,] if you irradiated the whole neck, giving the same dose as before, you got a much larger effect. So, the effect was proportional to the body mass irradiated. Well, it was surprising to me; the effect had not been reported before.

BERGE: Now, was this x radiation?

KOHN: Yes. It was the clinic—Another surprising thing: During the course of treatment, the count fell, and then it would take five to six months for recovery to take place. But recovery did take place.

BERGE: Because I think during that period of time was also when a number of people on our side of the Bay were doing blood count studies, but I think they were doing blood count studies after injections of either phosphorus or iron. I think a lot of it was iron. So I was wondering—

KOHN: What next?

BERGE: Next: Were the patients that you were taking the blood from, were they, was this considered part of their therapy to withdraw the blood, or was that just by the side? You were doing an experiment on the side of, whatever?

KOHN: You can consider this part of their treatment. In other words, I had not introduced any hazard to these patients by taking a small amount of blood and determining how well their white cell count was doing. It was a little more information, actually, for the therapist who was treating them.

BERGE: Right, right. And, did the therapist use that information afterward?

KOHN: Perhaps. But, I believe the count went into the patient record.

BERGE: Were they informed, so to speak, of that—

KOHN: —I don't remember, but it's a trivial thing.

BERGE: I know; I think, actually, quite a number of people did the same kind of thing with the blood counts. That's why I was asking, because we've got such a large number of people here who did blood count studies. I was wondering how much—

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[24] white blood cells

KOHN: —I did not put anything into a patient: I just took a small sample of blood and did an ordinary blood count. The amount of blood withdrawn was no hazard.

Time With the U.S. Public Health Service (Early '50s)

BERGE: Well, no. One more question: You mentioned that you first came to UCSF as a member of the Public Health Service. Can you describe what you did during those times?

KOHN: In '49 or '50, the Public Health Service was going to establish a clinical cancer service of their own in Bethesda, [Maryland,] and they were going to have a—well, they thought they might have, perhaps I should say, a large radiobiological research unit associated with it, and also for public health work. So, I'd been recruited, as a person who might serve in that large unit when it came into action.

Meanwhile, I was assigned to Oak Ridge, I was sent to Oak Ridge, because [Dr. William] Arnold requested—the Oak Ridge people requested—that I work there. They needed somebody to do animal work that might have some application to man. So, I was on extended duty, as they said in the Public Health Service, assigned to Oak Ridge. I stayed there for two years; then I felt, to stay in this business, I had to see what happens to man. So, I asked to go to San Francisco, to Dr. Stone's place.

[Dr. Stone] said he was willing to accept me to observe what happened in therapy. And after I was there for a very short time, Dr. Stone suggested that I ought to go through a residency in radiation therapy while I was there to become, in fact, a therapist. That would be the most efficient way of doing it. The Public Health Service consented, because their facilities were not yet ready. So, I did that. And when Dr. Stone opened his laboratory in '51 or '52, he asked me to move over there when my residency was finished, and do the radiation biology for him. Because the Public Health Service was still not ready, in '53 I resigned from the Public Health Service and went into the University of California at San Francisco.

BERGE: Can you describe a little bit what kind of work you were doing with animals in Oak Ridge?

KOHN: I told you: We were making a study of the chemical changes in the blood of the rats following radiation.

BERGE: Oh, so it was still the same thing.

KOHN: Those with single large doses of radiation.

Chairing the Bikini Atoll Rehabilitation Committee

BERGE: Otherwise, I noticed on your *vitae*, and also I got some information from *Who's Who in American Men in Science*, that you were on various committees, including, which I found rather interesting, the Bikini Atoll Rehabilitation Committee. What was that about?

KOHN: Well, the Bikini Atoll people were asking for money. I suppose I ought to mention that. That was quite a large project.

Livermore Biology Library has a complete set of the reports of that committee.

BERGE: The Bikini Atoll Rehabilitation?

KOHN: That's it. The reports are also in the Library of Congress. Well, the Bikini Atoll people were asking for money—millions of dollars—to repair their island and make it suitable for them. To inform the U.S. Congress for this purpose, a committee was appointed, the Bikini Atoll Rehabilitation Committee, of which I was the chairman. And, we for a number of years reported to the Congress on the progress made by Lawrence Livermore and Brookhaven [National Laboratories], and what we thought about things in general. So, there are a series of reports. I suppose I should have mentioned that.

And, then, I was the referee of the Rongelap Reassessment Project. It's the last thing down on this. And that, the same thing there, that the Rongelap people were requesting money. And so, in my reports to Congress, I would summarize the work which had been done, or was being done and, then, what more needed to be done. Those are finished by the way. I see the Bikini people signed off somewhere around '88. The Rongelap people, I should think, signed off around '91. Something like that.

BERGE: Well, I think I've asked you about everything that I can think of. If you can think of anything else you want to let me know about, or let posterity know about, please feel free to.

KOHN: All right, I will. ❑

Curriculum Vitae

Curriculum Vitae

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A.B. (Dartmouth 1930); Ph.D. (Harvard 1935); M.D. (Harvard 1946); Diplomate (Therapy), American Board of Radiology (1951).

Traveling Fellow, General Education Board (Stockholm and Cambridge 1935-37); Instructor-Assistant Professor of Physiology & Pharmacology, Duke Medical School (1937-43); Commission Officer USPHS at Baltimore, Oak Ridge National Laboratory & U. of California, San Francisco (1947-53); Clinical Professor of Experimental Radiology & Research Radiologist in Radiological Laboratory, U.C. Medical School, San Francisco (1953-63); Fuller-American Cancer Society Professor of Radiology, Harvard Medical School (1963-68); Gaiser Professor of Radiation Biology (1968-76); Professor Emeritus (1976).

Director, Shields Warren Radiation Laboratory, New England Deaconess Hospital (1964-79); Director, Center for Human Genetics, Harvard Medical School (1971-76).

Scientific Secretary, Advisory Committee on Biology & Medicine, Atomic Energy Commission (1956-60); Associate Editor, Radiation Research (1957-61); Member, Radiation Study Section NIH (1965-69); National Academy of Science Committee on Nuclear & Alternative Energy System (1975-79; chairman, Bikini Atoll Rehabilitation Committee (1982-88); Referee, Rongelap reassessment Project (1987?-91?).

Dr., Kohn has published more than 150 scientific papers. some examples are as follows:

Melvold, R.W., Kohn, H.I. and Bailey, D.W. "Interaction of H-2B^{b} and Mutant Histocompatibilty Gene H(KH-11) in the Mouse." *Immunogenetics* 11:597-603, 1980.

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H.I. Kohn
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Kohn, H.I.: "Tyramine Oxidase." *Biochemistry Journal*. 31: 1693-1704, 1937.

Kohn, H.I.: "Number of Chlorophyll Molecules Acting as an Absorbing Unit in Photosynthesis. *Nature*. 137: 606, 1936.

Reports of the Bikini Atoll Rehabilitation Committee and the Rongelap Reassessment Project may be found in the biology libraries of the Lawrence Livermore and Brookhaven National Laboratories, the Library of Congress, or the library of the Marshall Islands government.