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Agency Deed of Gift for Oral History Program Tapes and Transcripts

Deed of Gift for Oral History Interview of Dr. Henry I. Kohn
(name of donor)

1. On September 13, 1994 (date), I Dr. Henry I. Kohn (full name) met with Anna Berge of LBL/ARO (name of interviewers) for the sole purpose of creating an item of personal property known as an oral history interview. My sole intention in creating the oral history interview was to donate it to the United States of America for eventual deposit in the collections of the National Archives and Records Administration. Therefore, in accordance with 5 U.S.C. § 301; authority incorporated by reference in Title III of the Department of Energy Organization Act at 42 U.S.C. § 7151; including 42 U.S.C. § 2201(c) and 42 U.S.C. §§ 5813 and 5817, I Dr. Henry I. Kohn (Name of Donor) do hereby give, donate, and convey to the United States of America for eventual deposit in the collections of the National Archives and Records Administration, all rights and title I have in the tapes, transcripts, and information of this oral history interview subject to the following terms and conditions:

   A. The tapes, transcripts, and all other materials resulting from the interview are to be deposited with Lawrence Berkeley Laboratory Archives and Records Office.

      When active agency use ceases, they are to be offered to the National Archives and Records Administration, to be administered in accordance with Title 44 U.S.C., §§ 2111 and 2114, Public Law 98-497.

B. The transcript will be available for use by researchers as soon as it has been deposited with the Lawrence Berkeley Laboratory Archives and Records Office.

C. The tape recording will be available to researchers after the tape, transcript and any other pertinent oral history materials have been transferred to the custody of the Archivist of the United States.

D. I hereby dedicate to the public all intellectual property rights I have in the interview transcript and tapes.

2. I, (Gary L. Novak of LBL/ARO) xxxxxxxx accept, subject to the above conditions, custody of the tape and transcript of Dr. Henry I. Kohn 's oral history interview conducted on September 13, 1994 at Berkeley, California for deposit in records collections of Lawrence Berkeley Laboratory Archives and Records Office and for eventual transfer to the collections of the National Archives and Records Administration once its current value to my agency's historical program has elapsed.

(Name of Donor) (date) (location of interview)

Henry I. Kohn
Donor

John W. Stone for Gary C. Novak
Recipient (LBL/ARO Representative)

May 5, 1995
Date

May 5, 1995
Date

AUTHORIZATION FOR RELEASE OF INFORMATION TO THE PUBLIC

I, Dr. Henry I. Kohn, authorize the Lawrence Berkeley Laboratory
(name)

to release to the public any and all information, including transcripts and audio copies of, my oral history, taken at Berkeley, California by Anna Berge of LBL/ARO and
(Location) (Interviewer)

_____ on September 13, 1994.
(Interviewer) (Date)

Henry I. Kohn 5-5-95
Signed (Date)

HENRY I. KOHN
Name (printed)

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U.S. Department of Energy OHRE
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Reminder 1

Reminder 2

Reminder 3

Comments Dr. Kohn discusses his career in radiology. His work at Oak Ridge, UCSF under Dr. Stone, and at Harvard.

ORAL HISTORY OF DR. HENRY I. KOHN

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.

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.

Short Biography:

Henry Irving Kohn was born in New York City on . He was married in 1961; they have two children. He received his A.B. from Dartmouth 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 and received his M.D. in 1946. He served as a commissioned officer in United States Public Health Service (USPHS); from 1947 to 1953, he was stationed at Baltimore, Maryland, Oak Ridge National Laboratory in Tennessee, and 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, he was the Gaiser Professor of Radiation Biology. Since 1976, he has been a professor emeritus. During his career, Dr. Kohn has held the following positions:

* From 1957 to 1960: Scientific Secretary, Advisory Committee on Biology and Medicine for the Atomic Energy Commission.

* From 1964 to 1979: Director of the Shields Warren Radiation Laboratory at New England Deaconess Hospital.

* From 1965 to 1969: Member of a radiation study section for the National Institutes of Health.

* From 1971 to 1976: Director of the Center for Human Genetics at Harvard Medical School.

* From 1975 to 1979: National Academy of Science Committee on Nuclear and Alternative Energy Systems.

* From 1982 to 1988: Chairman of the Bikini Atoll Rehabilitation Committee.

* From 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. A few of the topics he has written on are: the biological effectiveness of high energy photons and electrons, the effects of x-ray therapy, nuclear and alternative energy, and the Bikini Atoll rehabilitation.

INTERVIEW WITH HENRY I. KOHN BY MS. BERGE

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. Okay, 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. (a copy of this is inserted at the end of the interview.)

BERGE: Okay.

KOHN: I was born in New York City in . Went to public schools there; then to Dartmouth College. And the rest of my career is stated in its essentials in this curriculum vitae which I have just given you.
[1. 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 in 1949, there was practically no Biology Division left, but a laboratory building of good size was being renovated for it a Y-10. Dr. Hollaender from the 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 transferred to the garret. 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..

[2. San Francisco]

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. 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, a 70 MeV machine. I was to determine the radiobiological effectiveness (RBE) 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. Can you describe, did you only work on the 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 went to Oak Ridge in 1947. That's all on that C.V. there, and then 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 radio biological 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 synchrotron 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 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 or at least what my initial assignment was. I was also interested in age at exposure and the late affects 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 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: Can you talk a little bit about how you felt about the results that you got?

KOHN: I don't understand what you mean.

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 brick-layer 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 fifties 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.

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. 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, Brookhaven, Argon 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 Co. 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 1950-1951. 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, where he became the professor of radiation biology.

BERGE: How do you spell his last name?

KOHN: K-A-L-L-M-A-N. And Dr. Shirley Gunter, who had just taken her degree in Berkeley with a very well known microbiologist, Dr. Stanier.

BERGE: Her name is G-U-N-T-H-E-R?

KOHN: No. G-U-N-T-E-R. Shirley Gunter. Her name, see would be down here somewhere. Let's see. That's it. 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 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. 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.

BERGE: I noticed from reading other bibliographies, for example, Dr. Tobias, that during the 1950's there was a lot of interest in studying radiation affects 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, also, especially for the bio-physicists 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, where 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 and there are some that are diploid. 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.

BERGE: Can you talk about some of the other people that worked in the lab? For example, you mentioned before _____ ..

BERGE: No, _____ didn't work in the lab. _____ 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 worked at Manchester for awhile with radioisotopes in the laboratory over there. So he was familiar with say radioactive phosphorus, right, 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 known, past and present are all okay. I think we were quite well aware of the dangers of exposure.

BERGE: Was _____ 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. _____, 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. Another topic I was interested in which raises an interesting question. We worked in the field of radiation genetics with the mouse. There have been only three or four or five laboratories, perhaps, who have done significant amounts of that kind of work in the United States. Now we worked with something call the histocompatibility 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 set up. On the other hand, if you take it from one identical twin to another, they will accept it because they have the same genetic set up. 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 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. 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. We never found any. When I went on later to Harvard and extended these experiments with 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. Yes, a fellow by the name of Melvold, that's M-E-L-V-O-L-D, Roger. He's now professor at Northwestern University, Medical School.

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. His name do you have it down? R-E-Y-

N-O-L-D. He is now, I wish I had a catalog here. He is now retried; he was the chief or the medical examiner for one of the big insurance companies in San Francisco. He lives in Nogales and is in the telephone directory. Anyhow, 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 in, and 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.

[3. Boston]

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 -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 lets say Radiology, consists of the members of radiology of the Massachusetts General, the Beth Israel hospital, the Brigham 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. [tape ended; end of tape 1, side 1]

KOHN: 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 a large enough, in my opinion, a large enough population of radiation therapy patients to make such investigations valid. And 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. And 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 and 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 histocompatibilty 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.

[4. Biology and Physics]

Kohn's Edited Insert: A project that took a great deal of my time end energy during the period of 1975-1979, was the work of the National Academy of Science Committee on Nuclear and Alternative Energy Systems. There were about a half-dozen people on the Committee and this book is the Committee's report, issued in 1980.

BERGE: The Committee on Nuclear and Alternative Energy Systems?

KOHN: Yes. It's a good book because the United States is running into,- 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 fell 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 on-going topics in radiation biology. More recently, I have written a review of *The Nuclear Lion* by John Jagger, 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.

BERGE: 402 pages.

KOHN: That's why I'm saying it. I know the predicaments you're in if you read everything that people threw at you, you'd never get through. But I think that 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.[tape interrupted] What was I saying?

BERGE: You didn't know the people over here in Berkeley very well.

KOHN: Oh yes, right. I have only met Lawrence. Well I saw more of Tobias. But his interests in a way were different than mine, I think, because he was trained as a physicist.

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, 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, 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. Now

that, yeah 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. Just as the biologist couldn't go on and think of the high class physics. The biologist, of course, may tend to think of epidemiology, 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 Lawrence didn't get along. Thinking back now, I do recall that there seemed to be something or other between the two labs. 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 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. I, 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. And 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 MD 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. I've worked on a greater variety of things: plants, animals, man, 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.

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

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 as then. There has been a constant effort to push

back 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 bio-physically, 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 prevent it. Whereas, as you will see from this paper over here, the review of Prof. Jagger's book, that it may turn out in ten or fifteen years that we must use nuclear energy 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, and all through the 1950's 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 Prof. 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.

[5. 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 (1) the type of cancer and (2) 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 a 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 1920-30, 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 the treatment - one brief exposure per day for seven days. I began training in 1949, treatment schedules had gradually been improved and 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 a clinic treats four hundred patients a year, that there are five principle types of cancer, and each may be classified in four stages. On the average, there would be twenty patients in each specific sub-group. 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.

[6. Shields Warren]

BERGE: Next question was, 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 Deaconess and Professor of Pathology at the Harvard Medical School. He became interested

in pathology in the late thirties owning 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 Argon National Laboratory.

BERGE: B-R-E-U-S?

KOHN: No, Brues. 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 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 "non-clinical" salary.

BERGE: What happened to Shield 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 into my plan, I suppose, because he could not face giving back the funds he had raised. I was so naive 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 son little

satisfaction. However, the Deaconess Hospital did name the Lab after him - the Shields Warren Laboratory.

[7. Blood Counts]

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: Did you work with, well what kind of work did you do with that? And did you have any collaboration for some of the hematologists 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 technician who drew the blood and did the counts of patients before, during and after treatment. The results were interested 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 blood count. 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 using, 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...[tape ended; end of tape 1, side 2]

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...

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.

[8. USPHS]

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: The Public Health Service in '49 or '50, the Public Health Service was going to establish a clinical cancer service of their own in Bethesda, 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 as to Oak Ridge, I was sent to Oak Ridge, because 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. He 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, I resigned. I think that's the way it went. Let's see. Yeah, I resigned, it's right here. 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.

[9. The Marshall Islands]

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. You go to your library at the Lawrence...

BERGE: Berkeley Laboratory.

KOHN: No, no, in the, out in your place Livermore.

BERGE: Uh, huh. I'm at Berkeley.

KOHN: Oh, when I call you on the telephone, I'm calling Berkeley?

BERGE: Yes, yes.

KOHN: Oh, I see. Well, Livermore biology library has a complete set of the reports of the, what is it called, the, 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 by Brookhaven, 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.

BERGE: Well, uh.

KOHN: 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. Let's see. I want to look up Reynold Brown. We have Cooper Brown. We have Harry Brown. We don't have Reynold Brown. Nope. Got a Alice Buck here. Well, I'm going to go upstairs and get you that.

Curriculum Vitae

PRIVACY ACT MATERIAL REMOVED

Henry I (rving) Kohn (b.), S.S. No.

Residence and Office: Phone. No. ( )

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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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.

INTERVIEW WITH HENRY I.KOHN BY MS BERGE

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

~~KOHN: By the way that's Henry Kohn?~~

~~BERGE: Kohn, sorry.~~

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. Okay, Dr. Kohn I was wondering if we could start with a little bit on your background, where you were born, where you lived, ~~until~~, where you got your education.

KOHN: Instead of going through all that why don't I just hand you this ~~then you'll have it.~~ brief curriculum vitae. (This is inserted at the end of the interview.)

BERGE: Okay.

KOHN: I was born in New York City in . Went to public schools there, then went to Dartmouth College. The rest of my career is stated in its essentials in the curriculum vita which I have just given you.

1. 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?

(go to page 1A)

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 they reorganized the laboratory, the biology division at Oak Ridge and Arnold became the Associate Director. They wanted somebody there in the biology laboratory with a medical background to do work related to medical subjects; Arnold knew me and I was, therefore, invited to come down. And I did. And through that, spending two years there at Oak Ridge, I became interested in radiation biology.

it?

KOHN: When I took my Ph.D. there was a fellow student by the name of William Arnold. And at the end of the war they reorganized the laboratory, the biology laboratory at Oak Ridge and Arnold became the associate Director. Since they wanted somebody there in the biology laboratory with a medical background to do work related to medical subjects, he knew me and, therefore, invited me to come down. And 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 the blood of rats following irradiation.

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

KOHN: Well, I was then also in the Public Health Service. I wanted, having a large ??? experience with man, and so I asked the Public Health Service if I could work in some medically or directly oriented service. And they gave me permission to visit in Dr. Stone's department in San Francisco. So I went there and worked there for several years and then left the Public Health Service. Dr. Stone offered me a job in the radiological

Berge: What kinds of things did you do 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 in 1949,. there was practically no Biology Division left, but a laboratory building of good size was being renovated for it at Y-10. Dr. Hollaender from the 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 transferred to the garret. Hollaeder 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.

2. SAN FRANCISCO

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. Stone's division of radiation therapy in San Francisco. I worked there there for several years on detached duty before I resigned from the Service. Dr. Stone had offered me a job in the Radiological Laboratory (go to next page)

page 2

laboratory which I accepted. At that time they had just finished the Radiological Laboratory building which was to house the synchrotron, a 70 MeV machine. I was to determine the radiobiological effectiveness (RBE) 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. Can you describe, 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, and microorganisms.

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 went to Oak Ridge in 1947. That's all on that CV there, and then I stayed at the Radiological Laboratory in San Francisco from 1949 to about 1962 or 1963. We did a variety of experiments, and I did some work

on patients.

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

KOHN: Well, let's see. ~~Well~~, First of all, my primary objective, or Dr. Stone's primary objective was to study the relative biological effectiveness of high energy beams the synchrotron's. ~~And while~~ Since the synchrotron took a long time to get started, we ~~therefore worked where we could, and so I~~ collaborated with other people in ~~Chicago, and~~ in Texas and in New York in doing the RBE on their high-energy machines. My collaborator in the laboratory was ~~a woman by the name of~~ Shirley Gunter, ~~who was~~ a microbiologist. Dr. Gunter went to each one of these laboratories and did her standard microbiological testing ~~etc.~~ We also did standard testing in San Francisco on a million volt machine, which they no longer have, but which was running at that time. ~~Those results, that work was flowing quite well, and~~ In '63 or so, ~~well in 1963~~ 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~~ PHOTONS and electrons. So, we accomplished ~~really~~ what the laboratory or at least what my initial assignment was. ~~We also~~ I was also interested in age at exposure and the late affects of radiation. It makes a difference whether you irradiate the animal when he's young or when he's old, ~~and so on~~. We published a number of papers on that, with Bob KALLMAN and ~~There's in that sheet you will see~~ FOR EXAMPLE, WITH PAUL Guttman; ~~down at the bottom there~~, the last reference, tells about that. Then I did on the sheet

some clinical work on the effects of ~~radiation exposure used for~~ radiation therapy on the blood count, ~~and~~ I did some work with Dr Zippen on breast cancer. ~~I must say I find you know~~ I could talk on and on and on, you know, and I don't want to do that.

BERGE: Oh please do.

KOHN: ~~Well, it's~~ 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 my C.V ~~over there~~ 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 — ~~But anyhow~~ it tells the story. Whoever's ~~really~~ interested can look this over. If he has any background at all he'll immediately see what was going on, ~~we can~~ read one of the papers.

BERGE: Can you talk a little bit about how you felt about the results that you got?

KOHN: ~~Well~~, I don't understand what you mean.

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 other 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 brick-layer building a wall. You lay your bricks, then you drop out. Another fella 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, it's just part of the bricks and mortar that go into the general structure, if I make myself clear. While our work was okay, I don't think any of it deserves the Nobel Prize particularly.

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

KOHN: Conditions in the early fifties were very good. There was money, and if you worked, any reasonably honest, good job, you 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.

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. There was a condition that

When Dr. Stone offered me the job he did make the condition that I would determine the RBE of the synchrotron beam, which in fact I ~~knew~~ what it would be FROM we knew ~~when we did~~ the work on the other HIGH-ENERGY machines before the synchrotron was ready, ~~they pretty much told us what it would be.~~ 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 ~~man, you see, he must have been, let me think for a moment~~ 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. 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 which 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, ~~especially~~ 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 ~~neutron~~ Synchrotron program?

KOHN: That was for whatever I was doing ~~at the laboratory.~~ at the radiological laboratory. Or don't you understand ~~what the radiological was over there?~~ the nature of that laboratory?

BERGE: Not entirely, no.

KOHN: All right. Well, when Dr. Stone ~~left when~~ left the AEC and went back to his post at the UCSF as head of the Department of Radiology, ~~when he left the AEC, they had made a decision to have four in effect.~~

a decision had been made by the AEC to have a number of national laboratories. The A.E.C. was supporting Oak Ridge, Brookhaven, Argonne, and then on a very much smaller scale, they decided that Dr. 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 Co had built a 70 MeV synchrotron, and they would build him a second one for radiation therapy. So a special building was constructed to house the machine behind the main UCSF buildings; it would also contain some laboratory space. Have you seen it?

Berge: No.

Kohn: The Laboratory was completed around 1950-1951. The synchrotron was a very large machine; it had to be installed, made to work reliably, and calibrated. Dr. Stone asked me if I would head the little radiation biology unit, and I accepted. Of course, I was working there some years befpre the machine was treating patients. I had a small group of associates consisting of Bob Kallman, who has just retired at Stanford, where he became

Dr. Gail Adams came to be the physicist in charge of the machine, and he also instructed the radiology residents in radiological physics.

national laboratories, so they were supporting Oak Ridge, Brookhaven, Argon and then in sort of in the same category but on a very much smaller scale, they decided that Stone should have a laboratory here, and he decided that what would interest him was to have as high an energy machine as could be obtained under reasonable planning. They decided that he would, the General Electric Company at that time had built one synchrotron and they were going to build a second one and he was going to get that machine to use for therapy. So a laboratory building was constructed behind UCSF the main buildings there. Have you seen it?

BERGE: No.

KOHN: In which this machine would be put, and in which there would be laboratory space, not expensive but some for animal work. So the building was built and, I forget when we moved in there, I think it was around 1950. Let's see I was there probably around 1950 or 1951 when the laboratory was completed, and they got the machine in. The machine, of course, had to be installed. It was a very large unit. Then it had to be calibrated, but first of all it had to be made to work reliably. So Stone asked me if I would be the Head of Radiation Biology there, and I assented and I went to work. Of course, I was ready to work before the machine was ready to work, as it were. It took a number of years for them to get that machine going. I had a small group of people consisting of Bob Kallman, who has just retired at Stanford, where he became

the professor of radiation biology, eventually.

BERGE: How do you spell his last name?

KOHN: K-A-L-L-M-A-N. And Dr. Shirley Gunter, who had just taken her degree ~~over here~~ in Berkeley with a very well known microbiologist, ~~?~~ is his name? Dr. Stanier.

BERGE: Her name is G-U-N-T-H-E-R?

KOHN: No. G-U-N-T-E-R. Shirley Gunter. Her name, see would be down here somewhere. Let's see. That's it. They were the two people I started with and then they moved on and some ~~of us~~ others came, eventually a fellow by the name of Ludwig ~~came in~~ who ~~went down~~ later ~~and~~ became a professor ~~down~~ at ~~San Diego, what's another place down there?~~ Irvine; Paul Cuttman, and Donald Bailey

~~BERGE: Santa Barbara, I don't know.~~

~~KOHN: Anyhow, well I'll just pass through there. Well, anyhow that's the way that worked.~~ During that time we did a variety of experiments. Dr. Gunter first did this work which is cited over here on page two. Gunter and Kohn, *The Effect of X-rays on the Survival of Bacteria and Yeast*. Because we wanted to use those organisms in ~~calibrating the biological effects of the~~ determining the RBE of the high-energy therapy machines. She did an extensive survey there, and then went on and visited New York, ~~Illinois~~ and Texas, ~~to use her method from her methods~~. Dr. Kallman

has done some very strange thing that I didn't. Strange that Kallman isn't down here. Anyway, Dr. Kallman did a lot of work on mice in the lab.

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

KOHN: Well, bacteria, as you probably know, have only one chromosome. Yeast have pair of chromosomes. But you can grow them in suspension, where they grow freely, you see. They grow like bacteria, and therefore they are very convenient to work with. But you have instead of a bacteria which could be criticized as a subject because it has only one chromosome, so to speak, the yeast are really a diploid cell. I think that's the answer to your question.

BERGE: As opposed, to say, fruit flies?

KOHN: I wouldn't say the yeast are opposed to fruit flies. I just say that I suppose because, no, it is because they are easier to work with. I always come back to that. No, also, especially for the bio-physicists 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, where 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 and there are some that are diploid. So you can compare the one chromosome group with the two chromosome group and see what difference having the second chromosome makes. So that's a virtue.

BERGE: Can you talk about some of the other people that worked in the lab? For example, you mentioned before .

BERGE: No, didn't work in the lab. Dr. 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 worked at Manchester for awhile with radioisotopes in the laboratory over there. So he was familiar with say radioactive phosphorus, right, 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, ~~so obviously he is doing all of his clinical work all day long~~, but he was interested in it. He ~~then~~ contracted leukemia and he died while I was there ~~actually~~. 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. ~~And I think was told to stay out of the way, out of the path of the rays from another source and they did~~. I'm sitting here, eighty-five years old. I've been working with radiation since 1947. ~~And~~ My colleagues who I've known, past and present are all okay. I think we were quite well aware of the dangers of exposure.

BERGE: Was particularly...

KOHN: Careless? No. I don't think so. ~~I think he may have~~, I don't know when he got his overexposure. It may have been when he was in England ~~working in this laboratory~~. 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 of course. He died of leukemia.

BERGE: But he was notoriously careless.

KOHN: Yes, yes.

BERGE: Do you know anything about ?

KOHN: No, I ~~know~~ 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. Another topic I was interested in which raises an interesting question. We worked ~~on~~ in the field of radiation genetics ~~on~~ with the mouse. There have been only three or four or five laboratories, perhaps, who have done significant amounts of that kind of work in the United States. Now we worked with something call the histocompatibility ~~gene~~ SYSTEM. ~~Do you know what those are?~~ 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 set up. On the other hand, if you take it from one identical twin to another, they will accept it because they have the same genetic set up. In the case of animals, say mice, we ~~grow them in~~ breed inbred ~~strengths~~ strains, that is only brothers and sisters are mated ~~bred~~ and they special strain is then established. And they for the most part

will accept the skin ~~one from the other~~ transplant from one another. But occasionally mutations will occur. Then when the skin is transplanted that skin is rejected. So we know that there are 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 ~~sixty~~ such genes ~~is~~ of which perhaps ten or fifteen may be the more important. ~~It~~ We. Not we. I should say a chap by the name of Bailey, Donald Bailey, who was a geneticist. He 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 visited our lab.~~ 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 ~~there~~ in the histocompatibility ~~enzyme group~~ loci. We never found any. When I went on later ~~on~~ to Harvard and extended these experiments with MELVOLD more we still never found any. ~~It has been an interesting finding, but the histocompatibility enzyme I should say that mutations in the histocompatibility enzyme do no show. Now of course they may not show because the mutation, the gene mutates but~~ The mutation may not show because the cells carrying it ~~that~~ do not survive for any great period of time. They may be shunted aside. But in any case, ~~that is an example, and~~ I believe it probably is the only such example. Donald Bailey left UCSF and then went on to ~~a~~ laboratory at Bar Harbor in Maine where he became a senior investigator and ~~I think~~ for awhile ~~was~~ 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, ~~I think~~ we tested something like a million and a half irradiated genes. Much of that work was done with Roger Melvold later on at Harvard ~~and out here~~. Yes, a fellow by the name of Melvold, that's M-E-L-V-O-L-D, Roger. He's now professor at Northwestern University, Medical School.

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. His name, do you have it down? R-E-Y-N-O-L-D. He is now, I wish I had a catalog here. ~~don't know if he's still~~ He is now retired. He was the chief or the medical examiner for one of the big insurance companies, in S.F. ~~Try to get hold of him through UCSF and~~ He lives ~~down your way over there somewhere near the Livermore lab and he has a cabin on that side of the water somewhere.~~ in Nogales and is in the telephone directory. Anyhow, get hold of him, as I say, he was also the Health and Safety Officer for UCSF. Now who else would there be? ~~I don't know whether he's retired yet or not. Anything look in the telephone directory for that area.~~ 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 in, and so in a way there's no point to our discussing it with them. Bob Kallman down in Stanford could

tell you about Henry Kaplan; and 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.

3. BOSTON

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. Harvard and also to become the director of the Shields Warren Radiation Laboratory at Harvard. That laboratory was supposed to promote experimental work in the radiological sciences that is pertaining to diagnosis, to 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 -- 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 the 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 heads of radiology of the Massachusetts General, the Beth Israel hospital, the Brigham so forth. So it's a large establishment. In the case of radiation therapy, there were these five or six departments. In the case of radiation therapy, all the therapy was included in the Department of Radiology. There were not separate departments of radiation therapy [tape ended; end of tape 1, side 1]

KOHN: Furthermore they were all acting independently. Now, to judge the results of treatment by radiation therapy, involves the statistical analysis of large numbers of patients, and none of these institutions has a large enough, in my opinion, a large enough population of radiation therapy patients to make such investigations valid. And what I wanted to do, in going to Harvard, was to establish a department of radiation therapy which would transcend the hospitals individually. They would all be part of a single department which had one head so to speak and would act in concert. And I wanted to establish something which I would call the Joint 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 I think Harvard had maybe altogether, I don't know, three or four hundred patients a year. As I say those were broken up by hospitals treating fifty 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 a lot of political shenanigans, that has come to pass and 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. ~~I don't know whether its larger than New York.~~ 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.

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 with Dr Bailey, that previously done, that there were not any transmissible x-ray induced mutations in the mouse histocompatibilty 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 established department devoted to medical genetics. Instead there was 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.

but I wanted to see it created. To make a long story short, and a lot of political shenanigans, etc., that has come to pass and there now is something called the Joint Center for Radiation Therapy. It may be the largest department of radiation therapy in the United States. I don't know whether its larger than New York. But they treat now something like four thousand patients a year. Certainly more than three thousand. So I regard that as my chief contribution, actually in this field. Then the other thing was, of course, to have a going laboratory of radiological sciences, and I think I did the right thing there by showing responsibility on to the individual departments, rather than trying to establish a single unit under my direction. That's about it. I did more work on this business at Harvard on the business of the mutations of the histocompatibility enzyme. In fact that was the chief thing, the only thing really I worked on there, and with Dr. Melvold. And that sort of finished up that work, it became quite clear that the contention was correct that there weren't any mutations of the histocompatibility enzyme. Now one of the things I was interested in, was this have you ever seen this book? Well, I was a member of the committee that there were about six to seven people.

BERGE: The Committee on Nuclear and Alternative Energy Systems?

KOHN: Yes. It's a good book because the United States is running into, the world not the United States, is going to run short of fuel to produce electricity, and obviously nuclear energy

4. Biology and Physics

A proect that took a great deal of my time and energy during the period1975-79 was the work of the National Academy of Science Committee on Nuclear & Alternative Energy Systems. There were about a half-dozen people on the Committee and this book is the Committee Report, issued in 1980.

Berge: The Committee on Nuclear and Alternative Energy Systems?

Kohn: Yes. It's a good book because the United States is running into, the world, not just the U.S., is going to run into a shoratage 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 the on-going topics in radiation biology.

More recently, I have written a review of The Nuclear Lion by John Jagger, ar 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 book, so that if you read it you almost don't have to read his book.

Berge: 402 pages.

Kohn: That's why I'm saying it. I know the predicament you would be in if you read everything that people threw at you; you'd never get through. But I think 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 or not it is necessary to adopt it. This problem, as you can see, has been a major interest of mine.

is, I mean nuclear substances are one of the fuels for producing electricity. The country, most people some how or other feel that we can find a substitute; we don't have to use nuclear energy to produce electricity, that we can get our energy from the sun or other places, wind, so on. But I think this book here lays a base line for such discussions. I did the radiobiology. I mean, not the radiobiology, I mean I did the radiological aspects of the discussion in this book. That was another interest of mine. And I think a very important one from a practical point of view, in a way it's more important than many of the on going topics in radiation biology, I would say with a smile on my face. And more recently, I have written review of a book published by a man by the name of Yeager, and I was going to give you a copy of it. Which you could say is a continuation of what this book says about radiation. That review is really a very good summary of his book. So that you almost don't have to read his book.

BERGE: 402 pages.

KOHN: That's why I'm saying it. I know the predicaments you're in if you read everything that people threw at you, you'd never get through. But I think that very fairly represents his point of view. 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 a point of view like this so that one can get around it. That has been, well as you can see, this book and that have been a major interest. This book here, we had many meetings

~~and so forth it was a lot of work really~~. What else can I tell you? It's difficult, I never thought of making a summary ~~evaluating~~ of my career. I've enjoyed my career very, very much, and I feel that I've been ~~also~~ 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 (Berkeley) very well. [tape interrupted] What was I saying?

BERGE: You didn't know the people over here (Berkeley) very well.

KOHN: Oh yes, right. I ~~did~~ had only met Lawrence. Well, I ~~would see~~ saw more of Tobias. But his interests in a way were different than mine, I think, because he was trained as a physicist.

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 ~~bodies~~ the obvious. No, there's nothing wrong with that, don't misunderstand me, but he approaches the problem from the application of his knowledge of atoms, neutrons, protons, so

forth, and then when he gets interested more in biology, he sees it as a formal, or somewhat more formal problem. (What's making me hesitate here, 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. Now that, yeah 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, really can't do that~~. Just as the biologist couldn't go on and think of the related high class physics. ~~But the physicist would associate with some~~ ~~thinking~~ The biologist, of course, may also tend to think of epidemiology, 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. ~~I would say that by the~~ It would take more time to think through just what the differences are. ~~In any case, I always found that Tobias was interested in one sort of thing, and while I was interested in hearing him talk about it. It really was not quite the same flavor as some of the other things that would have interested me~~

more, or that more interesting me more. Would you mentioning that Stone and Lawrence didn't get along. Thinking back now, and I do recall that there was something or other which prevented the two labs, we our laboratory from having much to do over here. There was no pressure on me, however, in which I could do whatever I wanted, maybe it was because Lawrence got his Lab. Is Lawrence still alive?

BERGE: No.

KOHN: No. So I can say anything I want. It may have been that Lawrence's brother exerted a certain amount of pressure to establish that lab and see to it that his brother was the professor. 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. I, the thought passes through my

mind 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 we started at something and this developing into some great thing which has flowered and benefited mankind. And perhaps you'll get that from some of the other people you work with. My career has been probably more varied than most people. You know I have an MD 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, I've worked on a greater variety of things — plants, animals, man, so forth. So it's harder to make it coherent. It would have been much easier to get somebody specialized say in the nervous system and stayed with all of the time.

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

KOHN: Well, what would you like to know? I'll tell you, one of the [illegible] I feel that we really don't know very much more now, basically, than we did 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 for radiation epidemiology as there then. There has been a constant to push back the levels of acceptable

exposure to radiation. And so they are being pushed back and back and back until they are getting to the stage now where they can't be measured, where I would say they can't be measured, and that pushing back has largely been one matter of what can we measure and control. It has not been radiation biology that has changed or invented something new. It's simply been what I would call a practical problem of management. And that in many ways is the most important aspect of this whole problem. What can man tolerate? What can be set as a standard? I think the problem has been pushed back to levels that can't be measured, either epidemiologically or physically, in a practical way. I don't think anything has changed in a way except from a general point of view, I do think that there is a danger of pushing this too far. Because the further back you push the standard without being able to prove, in fact, that you are absolutely correct, the more difficult, the more you reinforce the idea that any radiation is dangerous to man, and therefore, we must do away with it. Whereas, as you will see from this paper over here, the review of Dr. Zacharias's book, that it may turn out that in ten or fifteen years we must use nuclear energy to provide much electricity, and perhaps we are not paying enough attention to the engineering of electricity production through nuclear energy. To make that process more efficient and less dangerous. Because of man's psychological attitude toward the danger of radiation. 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, and all through the 1950's 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 Prof. JAGGER's ~~Mr. Yeager'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. ~~fast~~. Well, I think that sort of delivers my message ~~which isn't very much. Well,~~ I ~~think it's a very important message, but maybe~~ ~~more important would be if I were to tell you that we've discovered that, you know, radiation affects~~ ~~death of the chromosome~~? 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 thorough 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. Now these people now have an office in Vienna, ~~in Vienna, or just outside of Vienna~~. This is really a monumental work, ~~thing to put something like this together as a source book.~~

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.

5, ~~[illegible]~~ 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 were talking about the problem of the statistical analysis of radiation therapy treatments. Was the treatments, 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: How you pull yourself up by your bootstrap. You see, how do you establish what's the right therapy when you treat, we have the standard for today and you treat people. And then that formal treatment protocol will be changed gradually, you might say to yourself, well let's say we given a certain dose over five weeks. We treat the patient five times a day, I mean five times a week. Then you ask yourself the question, well perhaps I could give the same dose, instead of treating him five days a week I could treat him three days a week. Would that work? And you just have to try it. There is no other way of establishing.

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

KOHN: There was a Frenchman by the name of Regaud. Have you ever heard of him? Well he became professor at the in Paris.

Kohn: ~~In the case of treament in clinical medicine, you must pull yourself up by your bootstrap.~~

Kohn: Cure rates vary with (1) the type of cancer and (2) with 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 per cent 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 aweek.

Kohn: In the early days of radiation therapy it was generally held, especially by the German schoo that the biggest dose (tolerated) given as fast as possible was the best treatment. During the period, roughly 1920-30, 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 the treatment - one brief exposure per day for seven days. When I began training in 1949, treatment schedules had gradually been improved and become more or less standardized, for example, treating over the course of five weeks in the case of many types of cancer.

~~The need for relatively large numbers of patients is readily appreciated when one considers treatment analysis must consider the type of~~

I'm talking now back in the period of say, 1919. If you had a cancer here, they would make up a container for radium They would strap it on there and you would wear that, let's say for two days, three days, take it off put it away. It was made of course, first to prevent radiation from going out here, but only going down there, and there were certain results. Now, the German school believed that the more radiation you could give and the faster you could give it the better. But this chap, Claude Regaud, believed that perhaps you ought to take it easier. So he gave his dose of radiation over a week and then he extended it out to two weeks. And he got very much better results in the healing. The skin would heal, and the cancer would also go. So in effect, Claude Regaud invented the fractionation or the protraction of dosage. Now, starting with that, some chap came down from, I've forgotten which one it was, well they then applied it to the cancer of the larynx. There they found two weeks and somebody said let's do three weeks. Four weeks, five weeks and by this practice but always making a gentle step they finally decided that probably five weeks was okay. Then they also found that in order to get a good result they would have to raise the dosage a bit, on the individual treatment, so that the total treatment, say, might go up 50% or something like that. But giving it was an improvement. That's the way that all was invented, so when I came, so when I was trained, it was known that as a result of these fellas, that five weeks and I would say that I've forgotten maybe 3500 maybe 4000 rads was about it. Now, you could try and give the treatments at five weeks, seven weeks if you wanted to. Now, in

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 a clinic treats 400 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 20 patients in each specific group. If you wished to change the treatment, you copuld have 10 in the new group and 10 in the standard one. If you expect an survival to be improved from 5 out of 10 (50%) to 7 of 10 (70%),it would be difficult to establish. Obviously, much larger numbers of patients and good statistical analysis would be required.

6. Shields Warren

Berge: Next question was, 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 Deaconess and Professor of Pathology at the Harvard Medical School. He became interested in [redacted] in the late thirties owing to his interest in cancer. In 1939 he bacme 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 tunred into the [redacted] THE Atomic Bomb Casualty Commission. When the AEC was established in 1947, he became the first director of the Division of Biology & Medicine, a post which heheld 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

order to establish that this was better, you obviously had to accumulate enough cases, because while you make your certain fraction. Let, me start again, the cure was not an all known proposition. You cured 50% of the cases, 60% you didn't cure them all of any one of them. So in order to establish that you have to have a hundred or two hundred here, and then a hundred or two hundred over here and then you got kind of an improvement you're looking for, especially now, because the chances are you are perhaps curing almost all those who can be cured, you see. Of course, in the early days, when it was the French versus the Germans, so to speak, the difference was tremendous, you could see it. The Germans caused damage to the skin, the French didn't. Make it that simple. It didn't take any brains to see that. Now, where you may get say treating a certain kind of cancer with a certain regime you get 40% cure. Now you want to go on and improve that, and what kind of improvement are you looking at? Well, if you get only 50% or 60% well in order to get the difference between 40% and 60% on a firm basis you have to start with a large number of patients. That is perhaps a 100, 200 patients here and a 100 200 patients there. That was one reason why I felt that pooling or establishing a joint center for radiation therapy at Harvard would give you these large groups who had been treated in a relatively uniform way, so that comparisons could be made. You can't, just think in radiation therapy there might be, I'll say there are ten kinds of cancer to be treated. You only have 300 patients a year, you might only have plenty of patients, each one of these kinds of distribution is a little different with them.

to join to it a laboratory building that would house a high voltage machine and a small research radiobiological laboratory, similar to Dr. Stone's.

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

Kohn: I suppose I was the outstanding middle-aged fellow available (sm,iling). No, I won't be that bold. Oh, there was Austin Brues, head of the Bioligy Division, Argonne National Laboratory.

Berge: B-r-e-u-s?

Kohn: No, B-r-u-e-s. 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 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 "non-clinical"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 believe now 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

Well I mean you can see, you can't do anything, unless you can give them a magic pill, which will automatically cure. So that is why that is a very important thing to get the numbers. What's you're next question?

BERGE: Next question was, 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: You know Shields Warren was, he was a consultant with the AEC at the end, after the war ended, I forget what his official job was during the war, but at the end of the war he became the director of the division of biology and medicine for the AEC to help them out. He really didn't want the job, but he did it because they needed somebody. Then they finally got another man in. He was professor of pathology for Harvard Medical School he was at the New England Deaconess hospital. Shields, I think, I don't quite know how he got interested in radiation toxicity. I think the AEC, at one point, asked him about it, and he then looked it up and found there wasn't any comprehensive review, and he set about to write and wrote in the 40's. He wrote a series of papers reviewing what was known about the affects of radiation toxicity and radiation biology. Then Shields was primarily interested in cancer, and came to radiation through that path. I don't know what do you want me to say about him.

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

lab to a central location in the Longwood Avenue area of the School, and including in it facilties for research controlled by diagnosis, therapy and nuclear medicine as well as radiobiology.

Warren's son-in-law subsequently told me that Warren hated me. I can see why. He had written hundreds of letters to raise the two million plus 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 naive that when I put forward my plan, I did not know that I was ~~in a powerful position. I had acted entirely on the basis of what I considered to be realistic clinical and scientific goals.~~ 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.

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KOHN: Well I suppose I would say that I was the outstanding and middle-aged fella who could do it. I don't know. No, I won't be that bold. Well, I was probably the only one who was thoroughly trained. And who also was in the physical end. Oh there was the fellow who was the head of the biology division at the Argon National Laboratory. There was a fellow by the name of Austin Breus.

BERGE: B-R-E-U-S?

KOHN: Yes, Have you ever heard of him? He's a little older than I am. I don't know whether he's still alive. Anyhow, he was a good friend of Shields, and in my work with the AEC and etc., I knew Breus when I visited the lab and so forth. I believe it was Breus who recommended me to Warren. I think Warren more or less made some inquiries and then offered me the job. I think they had offered the job first to Henry Kaplan at Stanford, and he made some remarkable requests, shall we say, which they couldn't meet. Then they came down to me. I never discussed it with Henry, so I don't quite know. But I do know, but they didn't offer him quite the same job that they offered me. I was duly examined by a committee and so forth. I don't think there was a great deal, to be frank, I don't think there was much competition for this job because it was a job which presumably required some knowledge of both biology and medicine, so forth. I think the man who was practicing medicine, they didn't pay him enough money there having

7. BLOOD COUNTS

Berge: I'm just about finished. You mentioned something about radiation effects on the blood count during radiation therapy.

Kohn: Yes.

Berge: Did you work with, well what kind of work did you do with that? And did you have any collaboration with some of the hematologists 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 technician 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, "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 blood count. 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

professorship, you see. I think the chief thing was that he asked Austin Breus. Breus probably said, recommend Henry Kohn. What else do you want to know?

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

KOHN: He was still there. No, you see he was responsible for raising the money to build the building, the laboratory. In fact, he told me one time. I think he said that he wrote something like a thousand letters to raise the funds for that money. The guy ran from the Public Health Service and then he had to match that, I think. Which he did. It's sort of funny. I got them to change the location of the building, because I wanted it to be an integral part of the medical school. They had originally, he had originally planned to have it as really a part as Deaconess Hospital. Furthermore, he had planned to have it underground, the whole thing, and really built around a high voltage machine. When I saw the plans I told him that I would never go there for such a lab because, again, there would be no joint nothing. It didn't even fulfill the requirements that he had stated in the Public Health Service contract, where they said that they would support the various radiological sciences. So he then abandoned that plan, had the architects draw up another set of plans for the building then had to find the land on which to put this. And putting the lab in the medical school center. That was quite a job. I think he was not altogether pleased by this, if I may make

me; the effect had not been reported before.

Berge: Was this X radiation?

Kohn: Yes, in the clinic. Another surprising thing. During the course of treatmen the count fell, and then it would take 5-6 months for recovery to take place. But recovery did take place.

Berge: Because I think during that period a number ofpeople on this side of the Bay were doing blood count studies, but I think after injections of either iron or phosphorus. I think a lot of it was iron. So I was wondering.... [tape ended[

Kohn: What next? ....GO TO PAGE 360F THE ORIGINAL MS.

Berge:

(no page 35)

a conservative statement. But it was either that or loose the grant. I was unaware of the power of my position, and he probably thought I knew it. I was too innocent of politics to realize that I had pushed him to the wall on this thing. It was funny. I won't expose everything until I write my biography. But I want to be 95 before I write it.

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: Did you work with, well what kind of work did you do with that? And did you have any collaboration for some of the hematologists at UCSF?

KOHN: No. I was no great shakes, but, where is that? That paper was published in 1955, but it changes the human lipocite count during X-ray therapy for cancer ???. Well I had noticed when I came in doing some clinical work, I noticed the literature on the changes in the blood count was very small, amazingly so, and so I did these, I had an assistant, a technician who would withdraw blood from the patients following being treated, and did these blood counts and then I published this paper. I thought it was interesting results, but what I meant was, it's a funny thing, I still am not quite sure what it means, but anyhow, it makes you

see if you, you don't understand what radiation doses do you?

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

KOHN: Well, radiation dose means the amount of radiation you give to each cubic centimeter of tissue that's being exposed. What I found was, if you irradiated this part of my neck, we'll say, you got one affect. If you radiated down to here, you got another bigger affect. If you radiate down to here you get a still bigger affect. So it's proportional to the amount of body mass that was radiated. Now you think that if the dose was, now let's see. Well it was surprising to me that's all I can say, that it should turn it out that way. Nobody, it hadn't been observed before.

BERGE: Now was this X-radiation?

KOHN: Yes. It was the clinic...Another surprising thing was it took, maybe not so surprising, but anyhow, during the course of treatment the dose, the count fell, and then it would take five or 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 using, 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...[tape ended; end of tape

1, side 2]

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 because. 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 counts went into the patient records.

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...

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.

8. USPHS

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: The Public Health Service in '49 or '50, the Public Health Service was going to establish a clinical cancer service of their own in Bethesda and they were going to have a, well, they thought they might have, perhaps I should say, a large radio biological research unit associated with it, and also for public health work. So I'd been recruited, ~~I'm trying to think~~ Well as a person who might serve in that large unit when it came into action. ~~And so,~~ Meanwhile I was assigned ~~as a radiation therapist by the head of the unit in Baltimore by a fellow by the name of ??? Then he sent me off to~~ to Oak Ridge. I was sent to Oak Ridge, because Arnold requested, the Oak Ridge people requested, that I work there. They needed somebody to do ~~who had a radiobiology lab that dealt with~~ animal work ~~research~~ that might have some application to ~~direct connection with~~ man. ~~So it was through that~~. So I was on extended duty as they said in the Public Health Service, Assigned to Oak Ridge and I stayed there for two years ~~and~~ Then I felt to stay in this business I had to see what happens to man. So I asked to go ~~to~~ to San Francisco,

to Dr. Stone's place. He said he was willing to accept me, to Dr. Stone's place 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. Which the Public Health Service consented to because their facilities were not yet ready. So I did that. And when Dr. Stone opened his laboratory, he then asked me to move over there when my residency was finished, and do the radiation biology for him. Because the Public Health Service, which was still not ready, and then I resigned from the Public Health Service. I think that's the way it went. Let's see. Yeah, I resigned, it's right here. In `53, I resigned from the Public Health Service and joined the University of California at SF.

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.

9. The Marshall Islands

BERGE: Otherwise, I noticed on your vita and also I got some

information from Who's Who in American Men in Science, that you were, this is quite beside the point, but 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. You go to your library at the Lawrence...

BERGE: Berkeley laboratory.

KOHN: No, no, in the, out in your place Livermore.

BERGE: Uh, huh. I'm at Berkeley.

KOHN: Oh, when I call you on the telephone, I'm calling Berkeley?

BERGE: Yes, yes.

KOHN: Oh, I see. Well, Livermore biology library has a complete set of the reports of the, what is it called, the, that committee.

BERGE: The Bikini Atoll Rehabilitation.

KOHN: That's it. The Reports are also in the Library of Congress. Well, the Bikini Atoll were asking for money. Millions of dollars to repair their island and make it

suitable for them. And the congress which give the money, and so they had a committee 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 they were doing it. Brookhaven, was doing it, and what we thought about things in general. So they are a series of reports. I suppose I should have mentioned that. And then I was the referee of the Roundalot Reassessment Project. It's the last thing down on this. And that, the same thing there, that the Roundalot people were requesting money. And so in my reports to Congress I would summarize the work which had been done, or were being done, and then what more needed to be done.

BERGE: Well, uh.

KOHN: Those are finished by the way. I see the, probably the Bikini people signed off somewhere around `88. Roundalot 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. Let's see. I want to look up Reynold Brown. We have Cooper Brown. We have Harry Brown. We don't have Reynold Brown. Nope. Got a Alice Buck here. Well, I'm going to

go upstairs and get you that.

[redacted]

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Curriculum vitae

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Henry I(rving) Kohn (b.), S.S. No.

Residence and office:

AB(Dartmouth 1930); PhD (Harvard 1935); MD (Harvard 1946); Diplomate (Therapy), American Board of Radiology (1951).

Travelling Fellow, General Education Board (Stockholm and Cambridge 1935-37); Instructor-assistant professor of physiology & pharmacology, Duke Medical School (1937-43); Commissioned 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 Systems (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 as asfollows.

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

end

INTERVIEW WITH HENRY I. KOHN BY MS. BERGE

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 in 1949, there was practically no Biology Division left, but a laboratory building of good size was being renovated for it at Y-10. Dr. Hollaender from the 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 transferred to the garret. 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..

[2. San Francisco]

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. Stone's Division

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. 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, Brookhaven, Argonne 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 Co. 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 1950-1951. 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, where he became the professor of radiation biology.

BERGE: How do you spell his last name?

it is because they are easier to work with. I always come back to that. No, also, especially for the bio-physicists 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, where 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 and there are some that are diploid. 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.

BERGE: Can you talk about some of the other people that worked in the lab? For example, you mentioned before 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 worked at Manchester for awhile with radioisotopes in the laboratory over there. So he was familiar with say radioactive phosphorus, right, 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

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 and 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 histocompatibilty 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.

[4. Biology and Physics]

77 Kohn's Edited Insert: A project that took a great deal of my time end energy during the period of 1975-1979, was the work of the National Academy of Science Committee on Nuclear and Alternative Energy Systems. There were about a half-dozen people on the Committee and this book is the Committee's report, issued in 1980.

BERGE: The Committee on Nuclear and Alternative Energy Systems?

KOHN: Yes. It's a good book because the United States is running into,- 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 on-going topics in radiation biology. More recently, I have written a review of *The Nuclear Lion* by John Jagger, 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.

BERGE: 402 pages.

KOHN: That's why I'm saying it. I know the predicaments you're in if you read everything that people threw at you, you'd never get through. But I think that 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,

back 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 bio-physically, 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 Prof. Jagger's book, that it may turn out in ten or fifteen years that we must use nuclear energy 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, and all through the 1950's 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 Prof. 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

in pathology in the late thirties 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 Argon National Laboratory.

BERGE: B-R-E-U-S?

KOHN: No, Brues. 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 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 "non-clinical" salary.

BERGE: What happened to Shield 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 into my plan, I suppose, because he could not face giving back the funds he had raised. I was so naive 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.

[7. Blood Counts]

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: Did you work with, well what kind of work did you do with that? And did you have any collaboration for some of the hematologists 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 technician who drew the blood and did the counts of patients before, during and after treatment. The results were interested 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 blood count. If you irradiated the whole neck, giving the same dose as before, you got a much larger effect.