

National Cancer Institute Oral History Project
Interview with Mortimer Elkind
Conducted on July 30, 1998, by Gretchen A. Case
in Dr. Elkind's office at Colorado State University

GC: This is Gretchen Case, talking to Dr. Mortimer Elkind. We are in his office in Fort Collins, Colorado, at Colorado State University. Today is July 30, 1998, and it is about 3:00 p.m. I usually just like to start by asking about your background and about how you came to the National Cancer Institute.

ME: I have a peculiar background. My original training was in engineering. I was a mechanical engineer, a graduate of the Cooper Union School of Engineering. Shortly after that I had a job working as an engineer and then I was drafted into the Navy. I got out of the service and I had the GI Bill of Rights.

I started working as an engineer for the Sloan Kettering Institute for Cancer Research, which was a research arm of the Memorial Cancer Hospital in New York City. While I was there, I was recruited by a person from the NCI in Bethesda. I'll think of his name after a while. In any event, he sent me to MIT to study a particle accelerator that he wanted to get for the National Cancer Institute. I was working in the laboratory where the particle accelerator had been developed, the laboratory of John Trump. While I was there, I decided with my GI Bill of Rights to go to school and I did do just that. So I went to school. At that point, the person whose name I didn't remember is Egon Lorenz, L-o-r-e-n-z. He was a biophysicist in charge of various things connected

with radiation at the National Cancer Institute. He recruited me while I was at Sloan Kettering Institute and at that point I already had one master's degree in mechanical engineering from the Brooklyn Polytechnical Institute. Well, part of the idea that he had was to send me off to John Trump's laboratory to learn about the care and feeding of a particle accelerator called the Van de Graaf accelerator, V-a-n d-e G-r-a-a-f, which I did.

While I was there I decided to apply my school credits and go to school while a guest at the Massachusetts Institute of Technology. I obtained another master's degree while I was there, in physics, and went on to do a Ph.D. in physics. I didn't do any biology while I was there. By the time I finished, which was after a four-year period, Dr. Lorenz suggested that I might want some postdoctoral work, which I thought was a good idea. He gave me a choice of doing physics or doing biology. I chose biology and went out to Berkeley, where I became a postdoc fellow in Tobias's laboratory, Cornelius Tobias's laboratory, at the Donner Lab at the University of California at Berkeley.

GC: That was Cornelius Tobias?

ME: Tobias. T-o-b-i-a-s. I was out there about nine or ten months, when I was called back to the East Coast because Lorenz had died in the interim. He had a rather weak heart. Dr. Burroughs Mider, who was the scientific director, wanted me to get things going in Bethesda. Very nicely, he allowed me to set up my own laboratory with my own

technician, Harriet Sutton. He invited me to do whatever I was interested in. So I started working in biology and worked on yeast biology, actually yeast radiobiology. That was in 1954.

A few years later, Ted Puck, here in Denver, introduced quantitative procedures for working with mammalian cells in culture. I thought that was a great idea to start working on the radiobiology of mammalian cells. So I came out to Puck's laboratory for about a week and had a course that a number of people attended and I was one of them. I think I was here a week and a weekend up in the mountains. I went home and converted my laboratory from yeast to mammalian cells, with the encouragement of Bo Mider, who was the Scientific Director at the NCI.

GC: Bo Mider. Yes, I've heard of him.

ME: And Jay White was my lab chief.

GC: Who was that?

ME: Jay White at the Laboratory of Physiology in the Cancer Institute. I never turned around to look back and just went on from there. Shortly after, about 1959, Harriet Sutton and I published our first paper, on mammalian cells, in *Nature*. That work has been the basis

of, I would think, essentially all the awards . . . wait . . . let me see, I gave you the wrong thing. Essentially all these awards that I've received over the years. The first one is at the bottom and it goes up chronologically. Among them is recognition from the Department of HEW, which at the time was Health, Education and Welfare, the name changed slightly over the years.

GC: It's now Health and Human Services, right?

ME: That's right. Health and Human Services.

GC: One of the reasons I wanted to talk to you was just to know what was going on with radiation and radiobiology at the Cancer Institute over the years. Can you tell me a little bit about what kinds of things you would work on and how radiation was used at the Institute?

ME: I think the general theme of the research was that a few people, myself included, would learn how radiation damages cells. It is a very easy endpoint to measure cell killing, and we were effectively working on the question of how radiation killed cells and, therefore, that made the studies directly applicable to radiation therapy which of course . . . I'm trying to get that reprint, because I want to give this to you. In those days we worked on the easy endpoints, radiation killing cells. More recently the people in the field worked

on mutation and transformation to neoplastic properties, because radiation is quite efficient in doing that as well.

I wanted to show you and give to you, in fact, a manuscript reprint of a paper I gave in Germany in 1997, when I received an award from the Roentgen Museum. It illustrates, for example, because I'm no longer working on the question of cell killing, it illustrates some ideas that have to do with neoplastic transformation or cancer induction. It's written in English and German. If you want to read it in German, you can read the right-hand page; if you want to read in English, the left-hand page.

GC: I don't think I'd be very good at reading it in German, even though my name is Gretchen. I should know more German, but I don't. This is called, just for the record, this is called "Radiation-Induced Breast Cancer: A Conceptual Model," and I have the reprint.

ME: I'm writing a larger paper on that subject right now for a commemorative issue of the *Journal* which will be in memory of a health physicist who died about a year ago, Meryl Eisenbud. This paper is going to be done in a funny way. I have been invited to submit a manuscript to the *Journal*, of course, but at the same time just recently I was asked to give a commemorative lecture in connection with another person, a man by the name of Morton M. Kligerman at the University of Pennsylvania, in Philadelphia. He established a lecture series and has recently become emeritus, and I was asked to give the third

lecture. We finally agreed for me to give it, but in a funny way. I'm going to write the paper and someone else will read it.

In fact, that's what happened when I wrote this paper as well, because it was in Germany and it was delivered in German by a radiobiologist that I know there. I wrote the paper for him in English and he translated it and delivered it in German. He also has written the German counterpart to it, on the right-hand side of each page.

GC: Oh. So, he wrote the German text in this reprint?

ME: Yes, so that paper is almost ready to go. I'm just waiting for some slide materials to come back.

GC: When is that lecture?

ME: That lecture is scheduled for the eleventh of November. I don't know if I answered your question well enough, but the kinds of things that radiobiologists do these days involves both the therapeutic uses of radiation as well as the harmful effects of radiation. We know, for example, that the survivors at Hiroshima and Nagasaki had relatively small doses of radiation which can be very harmful and they're beginning to learn the same

thing from Chernobyl, and for that matter, even in Kazakhstan, where the Russians have their nuclear test range, in Semipalatinsk.

GC: So, you were in the Laboratory of Physiology for most of the time that you were at the Cancer Institute?

ME: The whole time.

GC: The whole time you were there? Was that the only laboratory that was using radiation or studying radiation?

ME: No. Well, it's an odd thing that they supported radiation biology. A few of us were in that laboratory. Because the Laboratory of Physiology, of course, isn't necessarily connected with radiation. But it came about that Egon Lorenz had some connection with the laboratory and Bo Mider just put me in there to have a place for me to be. But, of course, the whole department of radiotherapy was called the Radiation Branch, and it was actively headed by a radiation therapist. One or two additional radiation therapists were also doing research and we collaborated together. So, there was radiation therapy, being practiced there. In those days, not quite as developed as it is today at the National Cancer Institute. But they also did a certain amount of experimental work and I participated

where it was useful to do so. You can see why I won't be giving this paper in Philadelphia myself. I hope somebody who can speak more clearly than I can.

GC: It must be frustrating.

ME: It is very frustrating. My mouth at times feels like I put marbles in there.

GC: And your mind feels absolutely fine.

ME: My mind is ten yards ahead and my voice is dragging behind.

GC: Well, thank you for doing this.

ME: It's a pleasure.

GC: Your earliest associations, you mentioned that you were working for the NCI but you were on assignment to MIT and on assignment to the Donner Laboratory. Was that a typical thing for people to be loaned out like that?

ME: No, it's very atypical. When Egon Lorenz recruited me at the Sloan Kettering Institute, to make it worth my while and to get me, he offered me an appointment, a civil service

appointment, which was an appointment as an engineer so that I was earning a professional salary all the period of time I was at MIT. My GI Bill of Rights, plus the fact that I was getting everything at half price, fully covered all my expenses, meaning a master's degree and a doctorate. Although, because I was a guest at the University, I could only take courses at half the normal rate. In spite of that, I completed a Ph.D. and a master's degree in four years, which usually would be four years for a Ph.D. alone for a person who was full-time engaged in study. So, it was very atypical. I was earning a good salary. At the same time I was going to school half-time and ended up leaving there with two degrees and going off to Berkeley, being supported all this time by the National Cancer Institute. Very atypical.

GC: Yes, that must have been a pretty stressful, high-pressure time.

ME: Yes. But, you see, it was essentially a good time. It was really a flowering period for science, in my opinion. I've been back to the National Cancer Institute many times since in connection with various assignments and so on and I know people who are there now. I visit their laboratories and I know what kind of conditions they have to work under. The Institute has grown tremendously, but I believe not necessarily in a positive way, because it's so crowded now, so demanding on the people who are there. It wouldn't be a fun place to work at all. It was a fun place when I was there that length of time, in the 1950s and 1960s. It was just great. I left there in 1969. I arrived in 1954. I was hired

in 1949 and sent to MIT, then to Berkeley, and then I went to Bethesda. You can surmise as much from my CV.

GC: So you said it used to be a very fun place to work.

ME: I was a beginner and I had the equivalent of two or three rooms like this for my desk and laboratory. My secretary, my apologies, Meryl, is off to her mother who is having an operation. She's in North Dakota today and I'm sure she would like to meet you.

GC: Oh, I'm sorry I didn't get to meet her.

ME: She'll be in tomorrow, I believe. Well, anyway, I was a beginner, these rooms were turned over to me and money to buy equipment and things like that. These days I find my friends there are lucky if they have as much room as a desk to work in, and that would be a lot.

GC: So you started off with two rooms. This room is about eight by ten.

ME: Eight by twelve, it is prescribed by the State of Colorado. The faculty have to confine themselves to an eight by twelve office.

GC: So you've got two eight by twelve offices now and that's about what you were given when you started at the Cancer Institute?

ME: Yes, that's right.

GC: And what you were saying is?

ME: A little more there, because the rooms were longer. I was chairman of the department at CSU until 1989, when I had another larger office, but now, having come back to the faculty, this is the room for a faculty person.

GC: But you were given about this much space when you started at the Cancer Institute?

ME: I would say in total it probably was at least this much. It may have been a little more, because the rooms were longer.

GC: And then you were given money to buy whatever supplies you needed?

ME: Right, sure, not only supplies but equipment. But those days are gone forever.

GC: I think they are.

ME: I worked in England for a while and there, too, I was lucky, at a time in the development of science when I could be well received and well equipped and so on. I was given an assistant to work with in England from the time I arrived. But then it became in England much the way it is at the National Cancer Institute today. About a quarter of this floor is assigned to me. There are two rooms in this direction across the hall and another laboratory halfway down the hall. That isn't very common, either. We have our own cold room, our own darkroom, our own warm room.

GC: That's very nice to have all that.

ME: We have a room that is equipped with special filters, so it has sterilized air, the same as would be in a surgery suite in a hospital. All the air comes through a HEPA filter. The HEPA filters were the reasons I left the Cancer Institute.

GC: The HEPA filters were designed by the Cancer Institute?

ME: I'll tell you a little story.

GC: I would like to hear that.

ME: My laboratory was on the B1 floor of the Clinical Center. At one end of the floor there was an exit to the outside because the shape of the ground went down right there and it was shaped like this. So at the end of the B1 floor you could walk out of the building and at the other end you could take the elevator up to different places in the Clinical Center, if you've ever been there. Well, for ten years or more it was fine, but then there became so much traffic down that hallway that contamination was getting bad. About that time, NASA was getting going. One of the things that NASA did was to develop something called a HEPA filter. Do you know what that is?

GC: Yes. I have a HEPA filter on my air filter at home.

ME: Good, then you know what they are about. There were various designs that became available and I ordered some filters for my laboratory because of contamination from the hallway and the particular air distribution from the room to the corridor. Well, the units arrived and the man who was in charge at the time, the man who had to give the okay to have the devices installed, kept stalling me. I wasn't important enough for him. So eventually I just decided to leave. I left the NCI over filters. Now we have about five or six of those units, large units, in this laboratory, and it's much improved. Including a room which is equipped with air that comes through the HEPA filters, so it is basically a surgical suite room. We have a laminar flow hood inside that room as well.

GC: Let me just go back. So, you designed the HEPA filters that are up here. Is that what you said?

ME: No. But there are a number of them. They came from catalogs. But equipping of a room for this purpose in a facility outside a hospital is not very common. I don't know of any other laboratory where that's been done.

GC: But you also said you left the Cancer Institute because they were stalling so long on installing the filters?

ME: That's right. I just couldn't get anywhere with the people involved at the time. And, of course, when I announced that I was leaving, then right away there was a great hue and cry about we're going to install the HEPA filters. Well, I think they may have done that, but that was a little late for me.

GC: So, without the filters, because of the contamination, there was no way you could really do your work, was there?

ME: Well, I was just more or less treading water. I was young and impatient. That wasn't good enough. I went off to the Brookhaven National Laboratory and I was there for a couple...and I designed my own laboratory there as well, as a matter of fact. I was there

for a couple of years and then I went off to England and worked there for two years. The people who invited me over wanted me to stay and I considered it, but I finally decided not to. I think for my boys, as well as my wife, it was probably best that we did not stay, although Nikki certainly enjoyed it over there, too. I shouldn't say that. Let's put it this way: mainly for the education of my sons, it was probably best that we came back. I have one son who on Monday will start working for, let's see, the right name, the National Security Council. Right now he's working for Vice President Al Gore. He's a specialist on Russian affairs, my youngest son.

GC: How many children do you have? Two?

ME: I have three.

GC: Three sons?

ME: Three boys.

GC: That's nice.

ME: And four grand-boys and one young lady who has a great burden to bear among all her boy cousins. Well, anyway, that made me move, and it was probably just as well that I

did. From England I came back to the U.S. and went to the Argonne National Laboratory, which was another lab, like Brookhaven, both of them run by the Department of Energy. From Argonne we went through this period in the 1970s when the government was cutting back on facilities and science in general and people out here invited me out to take over the department. I was chairman until 1989, when this building was completed, and that's one reason why I have such nice laboratory facilities. I designed them myself, I assigned them, I divided up the space.

GC: So you decided how much space you got?

ME: Yes. The state dictated the size of the room, but they didn't dictate how many I could have. But CSU has been very kind to me. This isn't a very big, high-powered place, like NCI or Harvard or Princeton and so on. We're kind of middle-of-the-road type people here. But there are some very good people. I happened to be chairman of an organization called the University Distinguished Professors. There are twelve of us, which represents less than 1 percent of the faculty.

GC: So when you came back to Bethesda, did you get to design your own projects right from the beginning?

ME: Yes.

GC: Or were you brought back for a specific project?

ME: Well, in a certain sense I was, but let me explain it a little bit. I was hired originally as an engineer—I told you that, the reason was that Egon Lorenz had the idea of having in his department a number of accelerators for generating x-rays and electrons for treating cancer. Now, what was the question again, please?

GC: We were talking about why you came back to Bethesda and whether you came back for a specific project, and you said kind of you did.

ME: Well, the reason why I was hired so early was that I was to learn how to wet nurse these machines and come back and run them and be in charge of running the machine for the department. Well, that was nice. I did learn how to wet nurse the machine and my Ph.D. thesis was done working with the machine that we had at MIT at the time.

GC: With the Van de Graaf?

ME: Yes, but being a high-class technician didn't appeal very much. So, that's why I went off to do biology and Bo Mider, in credit to him, did not insist that I be a technician on the

machine. He got somebody else for that purpose. He encouraged me to set up my own laboratory and do my own thing.

GC: So then, once you set up your own laboratory and you were able to do your own thing, what did you start doing? What did you choose to start with?

ME: Well, first, since I had learned yeast technology at the Donner Laboratory in Berkeley, I worked on yeast, but then, as I mentioned to you, the field of mammalian cells matured so that by about 1958-1959 the field was ripe to do good quantitative work with mammalian cells and that's why I changed over to it. There were several people at the National Cancer Institute who were at the forefront of the development of the technology. One group was Wilton Earle, Katherine Sanford, and Virginia Evans. I've collaborated with Katherine Sanford for quite a bit recently in connection with this model of breast cancer. Anyway, it was Ted Puck who developed the initial technology that led us to work with mammalian cells in the same way we had been working with bacteria and yeast cells. So, that's why I came out here to learn and changed my laboratory and changed everything after that.

GC: Can you tell me a little bit about Wilton Earle? That's a name I've heard a lot but I don't know much about him.

ME: Well, he was a very large man, quite stout. He died shortly after that. And Virginia Evans, I think, was the second person who was in charge in the laboratory. I can't tell you anything more personal about him, but he was housed in the old National Cancer Institute building, where he developed facilities for culturing mammalian cells and had many things named after him, like Earle's medium of various types.

GC: Right.

ME: Then there was another man who came along who was even more well known and that was Harry Eagle. Harry Eagle was responsible for a medium called Eagle's medium and so on that was used around the world. Harry was in charge of the laboratory and then became director of the Cancer Institute. I never was in Harry's group, but I used to collaborate and co-mingle with the people in his laboratory as much as I could. They were very useful to me and useful to some of the others, as well.

GC: Is it pretty typical for people between laboratories to collaborate with each other on projects?

ME: Not then.

GC: Not then? So, people would work mainly within their own labs?

ME: They did. If only because they were busy with what they were doing. But it was a lot looser. Everything favored interaction between people at the time as opposed to how it is today. Today there may be some of that, but it's on a very personal level. Competition is very keen at the National Cancer Institute. Of all the institutes now currently, the doors get closed off to interchange between people that otherwise used to be open to them. NIH had a very collegial atmosphere for many years, as long as I was there. After all, I had no biology or training when I went there in biology. There was only one formal course that I took in biology and that was bacterial technology. I went out to the coast, to Cold Spring Harbor, for two weeks and took a course in the summer on how to grow a type of virus called a bacteriophage, and how to grow bacteria, and experiments that could be done with them. But I learned biology by being at NIH. I went to seminars and so on.

GC: So, at the time you were there, you are saying NIH was very collegial, people taught each other?

ME: That's right. It was a very open place for a young lad who didn't have a family and was burning up the environment there. I used to live across the street from the Clinical Center, in the nurses' quarters. I don't know what the building is called now, but there is a building across the street that has six stories and which was nearby and was mainly for the idea of having places for the nursing staff to live there who had to be on site. It's

usually like that at hospitals. I used to live right there and I could run across the street into my laboratory if I had to. I had things going on twenty-four hours a day.

GC: You were there twenty-four hours a day?

ME: I had things I did twenty-four hours a day, because I could just step into my apartment if I had to and have my meals there and go back to the laboratory. Many experiments designed had a time schedule which was twenty-four hours or more which might require me to be there every two hours to record a point of data or something. So, it was very convenient. And, as I say, I was single and was interested in my work. Nobody was holding me back. I was getting all the encouragement I needed and all the support I needed. I had a technician, Harriet Sutton, who was a fine person.

GC: What was the name?

ME: Harriet Sutton, S-u-t-t-o-n. My first papers are Elkind and Sutton.

GC: So, the nurses' quarters across the street, you said it was six stories and all kinds of people from the Cancer Institute lived there?

ME: No, usually just nurses or a cadre of MD's. They had a program at the time—it may not be true today, where a young physician could do his medical service by signing up in the Public Health Service for a couple of years. And if he was real clever and was willing to work for a little bit less money, he would take a job at the Clinical Center and do research at the same time. I'm not sure how popular that is today, but at that time it was really a golden opportunity. So, some of those fellows also were there. And we had our own little community.

GC: Now, were those the clinical associates?

ME: That's right.

GC: That must have been kind of fun to live in the same place where all these other people that you worked with were.

ME: It was great.

GC: The Clinical Center, you returned to Bethesda in 1954, and the Clinical Center had just opened in 1953. Can you tell me what it was like: What it looked like? What the atmosphere was like?

ME: I remember the distinct impression of concrete floors and that kind of thing, as the finishing touches were put in place. People were busy getting going and would not just sit around and wait for something to be polished up and finished up. The new building had in it several very spacious rooms for the types of equipment that Egon Lorenz had wanted me to wet nurse. They were big vaults surrounded by reinforced concrete which filtered the radiation and so on. Those rooms are being used today in a very much redesigned facility, but at that time it was mostly just empty spaces. Because there were some changes made in the plans for what kind of equipment to put into them, Congress did not come through with all the money that they would need for the purpose and, besides, Egon Lorenz made the mistake of dying in the interim.

GC: While you were there, how did the Clinical Center change? Did it grow quickly?

ME: Well, it was feeling its way. The whole field of research was changing at the time. This was a very formative period in the history of NIH altogether. The Director was a man who originated the idea of study sections to review applications for research support and so on. That was just getting going. Actually, I was one of the early members of the Radiation Study Section, in 1961, I believe it was. I think it was started about 1958. There were a lot of things that were just developing. You know, this whole idea of peer review was something that developed in the United States and now lots of other countries are trying to copy it, but not very successfully. It's part and parcel of how we do things

here. It was a very unique thing at the time and still is very unique. I mentioned to you that I was in England for a couple of years. Well, having been there for a while, I was asked to review some applications from time to time, and more of them were sent to me when I came home. I was asked to offer some comments on the Patterson Laboratory in Manchester.

GC: Patterson? P-a-t-t-e-r-s-o-n?

ME: Yes, that's probably right. Well, the application was less than a quarter of an inch thick. This was probably a dozen people involved, their salaries and so on, and that's how they did business in England. It was kind of "you know them or you don't know them." I knew the people and I could make comments about them, but peer review was different. Your ideas had to shine forth as well as your background and training and so on. It wasn't a perfect way and today I think the review system has its problems, but it's still pretty good. The fact is that I was supported as a member of the Intramural staff for many years and supported very well, and I was supported well at the National Laboratory in Argonne. After I came here, for the first time in my life essentially I applied directly for support from the National Institutes of Health and the National Cancer Institute. Right now I'm in the last year of a very special grant called an outstanding investigative grant from the National Cancer Institute which is in its eleventh year, and I plan to retire when it finishes at the end of March, the thirty-first of March of next year. So, as I was the

department chairman, I applied for some support before my stint as chairman ended and I've had this for eleven years. That was really remarkable, it doesn't happen anyplace else.

GC: So, are you still doing cancer research here then?

ME: Sorry?

GC: You're still doing cancer-related research here?

ME: Well, I've done work related to this [breast cancer] model. And I have at the moment three graduate students who are working on various aspects of radiation transformation and damage repair and so on. I have one student from China, one student from Russia, one student from Thailand.

GC: Very international.

ME: All women.

GC: All women, that's interesting. How many women were working at the Institute when you were there?

ME: A fair number. For example, in the laboratory next to me, just nearby, there was Willie W. Smith, who is a very well-known radiobiologist, somewhat older than I was. There was Millar, I forgot her first name, M-i-l-l-a-r, was the name of a person. I think it probably was about evenly male and female, as far as the investigators were concerned.

GC: Really?

ME: About, I don't think the view of things has changed very much since then. I think the composition of the staff has all along been kind of mixed.

GC: Was it also pretty evenly split on the level of technicians?

ME: In those days we didn't have some of these notions that we have today about women's rights and civil rights and so on. It just came about because the people in charge were open-minded and they pursued excellence rather than gender or race and so on. I had one black fellow, two black people, who worked for me as technicians. There were not many black people that I can remember who were scientists. In fact, I can't remember any.

Here at CSU I had a black student at one time from Nigeria. He turned out to be a great problem. He sued me for \$6.2 million because he wasn't getting his degree. To make a long story short, with the University's backing and so on, and legal help from the state, the whole thing was thrown right out of court and he skipped the country. Didn't even pay his lawyers. If you come across the Nigerians, they are kind of special people. Hopefully they will get their country straightened out. They're going through a re-organization right now, but it's just terrible, their attitude about a number of other things.

GC: In terms of the entire Cancer Institute, how large was the Laboratory of Physiology or how important was it? What part did it play in the Institute?

ME: I think there were ten or twelve investigators.

GC: Ten or twelve investigators?

ME: A small department, about the same size as the department I was in charge of here. We have a few more, because we are related to the veterinary medical school down the road and there are a few down there who are also in this department. But be that as it may, there were about ten or twelve that I can remember, and there were technicians, quite a few assistants, and so on. The department in total was about fifty people.

GC: And so that was fairly small.

ME: I think it was a modest size. Things have grown in different directions since then. As different things have been emphasized since then. But there were some very well-known people who were at that time either clinical associates or just researchers. That's how it was. But those were formative stages for the Cancer Institute. They had at one time an operation on the West Coast, in San Francisco. There was a laboratory in San Francisco and later on a smaller laboratory in Baltimore, and both were closed eventually and consolidated in Bethesda.

GC: Did you do any work in either Baltimore or San Francisco?

ME: No.

GC: You just knew of these?

ME: No, we had communication with those people and a certain amount of interaction, but I didn't do any work there. At that time we also had tennis courts where the National Library of Medicine now is located. I was chairman of the tennis club, so I remember it very well.

GC: There was kind of an on-campus tennis club?

ME: Yes, we had a very good group. One of the nurses that I played with, Janet Fitzwater, who turned out to be my wife's boss. She was the chief operating room nurse and Nikki was her secretary.

GC: So your wife worked at National Cancer Institute, too?

ME: Yes. I met her there.

GC: You met her there? How did you meet?

ME: Janet introduced me to her. We were very good friends over the years and still are, but she quickly shuffled me off to Nikki.

GC: She shuttled you off to Nikki?

ME: Introduced me to her secretary. Janet is still living, although she is somewhat older than I am, with a person who was, I'm forgetting her name, but was the chief administrator for the Clinical Center. They lived together for years.

GC: It sounds like there was quite an active social life on campus at the Institutes of Health.

ME: A lot of it just went by me. I was too earnest.

GC: Too earnest? Was it pretty typical for you to work very long hours?

ME: Yes.

GC: What time would you start? What would a typical day be like for you?

ME: Well, the average day didn't involve working through the night, to be sure, but nonetheless I wouldn't shy away from things when some work was required on the weekend or evening and so on. We just did it. If we convinced ourselves it had to be done, we did it.

GC: Do you think most people in your lab were like that, too?

ME: Harriet was the second most dedicated person. But the others were, too. Bebe Moses was that way. She's another technician. Katherine Volz, V-o-l-z. There were other people who did the unsophisticated work, dish washing and so on. Those were early days

of the development of the technology. Now today you can just open up laboratory supply books and order everything out of a standard manual. But in those days, many of those things had to be done personally and somehow devices that weren't designed for the purpose were put together and made to work that way. For example, at that time only glass dishes were available. Puck developed a way of cleaning glass dishes which we all copied. One device which is very common, a kind of a special rough cloth that people use in dish washing, mirror cloth. It's made out of a plastic, but the weave of it puts the fibers together in a very rough way. Well, one of the steps in cleaning these dishes was that after they were washed, they had to be scratched with this cloth to get any stuff off the surface and then washed again. Those were all things that we knew because we developed the technology ourselves.

GC: So you had to think on your feet?

ME: Yes.

GC: And put things together.

ME: Yes. Let me mention something else: The United States was terribly much in the forefront here. For example, we have many companies that make and supply median today. In addition, one of the things that was needed was serum. In those days, the best

serum, human serum, was available because there was a good price available to a person who was willing to donate a pint of blood so that human serum could be made. Usually these chaps who lived on welfare on the Bowery in New York or elsewhere would do this and get a little money to buy alcohol or whatever else they lived on. So one source of serum was human serum, another source was horse serum. Harry Eagle had his own horse out on a farm in Rockville, where he used his horse's blood periodically to produce serum for his work. A third source was fetal calf serum which Ted Puck originated. There was a company called the Colorado Serum Company here in Colorado which developed the first source of fetal calf serum. Fetal calf serum became very popular ever since. We had to learn how to collect the material, sterilize the material, store the material. This was not part of the technology at the time. All of that is very simple today. You just go to a catalog and order anything out of the book. The United States has been way ahead of all other countries in these developments, and it was ten or more years later before the same kind of sera were available in England, Germany, or France.

GC: This is all just born out of necessity?

ME: That's right. These companies were the forerunners of the biotech industry in this country. There was an outfit in Bethesda called Microbiological Associated and they may still be somewhere around in that area. It was partly owned by Harry Eagle. He was involved in developing the company. It was one of the major sources of biologics

for cell cultures and cell cultivation. Wilton Earle was involved in the development, as well as Virginia Evans and Katherine Sanford.

GC: Katherine Sanford is someone I'm hoping to talk to as well. She lives in Delaware.

ME: She's retired. She's living on the Eastern Shore. Her married name is Miflin, M-i-f-l-i-n. If you would like her address, her retirement address, I could get that for you.

GC: Oh, that would be great. I have one address for her, but it's in Delaware.

ME: That's probably it.

GC: Dover, Delaware?

ME: I think it is Delaware. Delaware has a shore that is contiguous with the Eastern Shore of Maryland. I think that's it.

GC: Right. Good. Yes, I'm hoping to talk to her soon, too. What was, we talked about this a little bit, but what was the general working atmosphere at the NCI? What kind of working relationships did people build? How was work done?

ME: Very collegial. We had a seminar series consisting of a group of us who would go out to dinner once every three or four nights, which would be followed by a speaker who might be some visiting dignitary passing through or one of the people in the group. Christian B. Anfinsen was a guiding spirit. Chris Anfinsen died a few years ago, but he was one of our early Nobel Prize winners at NIH. It was very relaxed and very open. If I had a problem trying to do something and didn't know how to do it, I could go talk to one of these older people who might be very experienced in that particular aspect of the work, and they would offer their advice. Maybe that goes on there today, but I doubt that it has the same openness about it.

GC: So, if there was a particular piece of equipment you didn't know how to operate or a particular procedure you didn't know, you would just ask someone else who had perhaps done that before?

ME: Yes.

GC: Since you were in the Clinical Center—I know you were mainly on the research side—but did you work with any patients at any time, or did you work with the MDs on any of the cases?

ME: It would have been in connection with radiotherapy, but I had very little interaction with them. I could have had much more if I had wanted to, but I didn't have the time. I was so busy doing my own thing.

GC: Was there a pretty solid division between clinical work and research work at the time?

ME: There was a division, to be sure, but it wasn't a barrier you couldn't cross. If you had a collaborator who was in clinical work, you could be effectively doing clinical things through that person. But my background was not in medicine, so I didn't have a license, and that would certainly be a deterrent to doing anything individually on my own. But if I wanted to collaborate with some of the other people, I could have been doing it, medical things, clinical things. If we had an idea we wanted to try out on a patient, the normal procedure would be for me to find an opposite [staff] member to be the interface between the laboratory and the patient work.

GC: Did any of the clinical associates ever work in your laboratory or in your field?

ME: Yes.

GC: Do you remember who?

ME: Bill Shipley was a radiotherapist at MGH in Boston.

GC: What was the last name?

ME: William U. Shipley. U was for Upjohn.

GC: Like the Upjohns?

ME: He was a very well-known and highly respected radiotherapist. He also did a residency in surgery, so he was qualified in surgery as well as radiation therapy. Another person who is a personal friend even today is William Mohler, Bill Mohler, and his boss at the time was a man called Scotty Pratt. At the time they were in charge of the computer facility at the National Institutes of Health. Unfortunately I don't have a photograph here to show you. Perhaps the principal person in that regard was the postdoctoral student you might say that I had who, in fact, received the Fermi Award with me last year. His name is Rod Withers, W-i-t-h-e-r-s. He did his medical qualification in Australia and then went to Great Britain, where he worked for Hal Gray, who was a very famous radiation biophysicist. On the way back to Australia from Great Britain, he stopped over at my laboratory in Bethesda and hung around for some days. Went back to Australia and came back and worked with me for two years. It was a period in which he worked with me that, based upon that work, collaboration, I feel quite sure that he also then

received recognition as a Fermi prizewinner, and since then he also received another thing that I had received. In 1989 I was awarded the Kettering prize from the General Motors Cancer Research Foundation. That was the second time they had awarded a prize to someone who came from the field of radiation science. Rod Withers this past year received the Kettering prize also.

GC: Must have been quite a student.

ME: That's right. He was the best student I ever had.

GC: Really?

ME: I have had good ones since then. Not many, but the ones that I have had have been pretty good. Rod happened to get the prize at just the right time, because General Motors upped the ante this year and instead of a hundred thousand dollars and a gold medal for the prize, which is what I received, they raised it to two hundred fifty thousand.

GC: That's quite a raise.

ME: General Motors likes to refer to these prizes as being to the Nobel Laureates of cancer research. There are two or three prizes given, one is the Sloan prize, one the Kettering

prize, and the third one is named after another GM fellow—I've forgotten his name, though.

GC: Well, just judging from the people who have gotten the GM awards in the past, it's pretty impressive.

ME: It is a pretty impressive group, that's right. It should be.

GC: You worked under two of the directors, John Heller and Kenneth Endicott. Did you have any contact with the NCI directors?

ME: Not really. There's a picture of me receiving the department citation and I think it's Endicott that may have been the chief, we were shaking hands, at the time. But interaction with them was minor. It was greater with Bo Mider when he was scientific director, because he brought me there and set me up to work and so on.

GC: How was he to work for? Dr. Mider.

ME: Well, fine. He was fine. Because while he seemed to be a very stern fellow and a demanding kind of person, it turns out he gave me everything I wanted and supported me in everything I did. So, you know, without his support, I wouldn't have been able to do

things I had done initially. So, he was just fine. This is going on tape, so maybe I shouldn't say any more. I think that was a reflection of the times. He was in a position to do this for a young person easily and he did it with good intuition, but things have changed since then. Very much so.

GC: It's just harder to get materials and resources.

ME: Oh, yes.

GC: It sounds like it was, what you needed you were given or you were able to procure.

ME: Right. In fact, I didn't even fully appreciate what was going on, I just wanted to work. I was washing out cast-off glassware and so on and using it again instead of buying new stuff. My background and my experience is pretty atypical, I'm aware of that. But it benefitted from some people who didn't realize at the time what they were doing for me and I didn't realize what they were doing as well. One of them was Bo Mider and another was Jay White.

GC: Jay White. Could you talk a little bit about Jay White?

ME: Jay White was my laboratory chief. He more or less implemented what Bo Mider was trying to do. He made Harriet Sutton available to work for me and otherwise never turned me down for anything I

wanted. The limitation in those days, and even today, much more so today, than what you could buy, because money was essentially unlimited, was do you have the space for it? Yes, I think there were rules against putting equipment out in the hallway there, a fire hazard and so on. I myself should know about that, because I have some stuff out in the hallway and he'd be saying, well, I'll not go into that. Anyway, that was the basic limitation on what a department could buy. The money was always there. And if it wasn't there one year, it would be there the next year. I think the National Cancer Institute, the National Institutes of Health, are too large.

GC: They are too large now?

ME: I think so, right.

GC: Why's that?

ME: It's too big. It's going to be an unmanageable organization. There's too much bureaucracy and it's getting bigger, not smaller.

GC: Do you think that affects the research, the very basic research?

ME: I think it has a bearing on it, but it's a complicated picture, of course. There is no single thing that you can point to. But recently there was a study performed by the Institute of Medicine for the Director of the National Institutes of Health. They made a number of proposals and suggestions, particularly about public participation in the programs at NIH.

Well, that's fine, but I think one fundamental thing they didn't touch on is the size of NIH. There ought to be a separate NIH, they ought to have another campus and another separate...It's unwieldy now. Too many layers of bureaucracy and so on. As it must be as it becomes more highly organized and bigger. So, while I'm a great supporter of NIH, I feel today the peer review system needs improvement and the size of NIH needs attention. They're building another Clinical Center, or adding to the current one, and they need it, to be sure. That place is tremendous. Every Institute has its own building project. Space in the Clinical Center is limited.

GC: It sounds like when you were there, space was at a premium.

ME: When I was there?

GC: When you were there, was space hard to come by?

ME: No, not for the most part. I don't know what the relative sizes of the staff may be; it must be much larger today than it was earlier. How much larger, I don't know, but it must be quite a bit larger.

GC: When you started at the National Cancer Institute, did you have goals in mind for where you wanted to go, what you wanted to do while you were there?

ME: Well, early on I devised, I asked the question, which we would say, scientifically and devised a way of answering the question. It had to do this with the ability of cells to repair damage, radiation damage, and I've been doing that ever since. Why get off a good thing if you have something going for you that way?

GC: Right.

ME: And this model that I gave to you about breast cancer research, I feel is relevant because of the lack of ability to repair damage in breast cancer target cells. So, it's the absence of repair in this case.

GC: So this was kind of an ongoing goal?

ME: Yes, because it was very basic cell biology involved.

GC: Was this the question you began asking after you came to the Cancer Institute or did you have that in mind before you came?

ME: In Berkeley, in Tobias's group, the issue of damage and repair was something we faced all the time, but the particular experiment and the particular way that I phrased the question didn't occur to me until I was back in Bethesda.

GC: Did that come to you all at once or was it something that developed slowly?

ME: It developed over a short period of time. There were various reasons associated with the fact that radiation therapy was spread out over time and effective as a treatment and the various suggestions of what might be going on. Much the same way, I've had suggestions about breast cancer from pieces of information in the literature, which I refer to in that paper you have, but I feel quite certain about particular things that other people might not see in that article, or might disagree with. The fact that there were women who, as was true with TB, were monitored once a week by fluorography, as was done in the 1950s here and the 1960s and then would have the same induction of cancer per unit dose as a person in Hiroshima and Nagasaki radiation exposure. This from instantaneous exposure of the A bomb to me is a fundamental point.

GC: I didn't realize that was that.

ME: These things have all been published for many years.

GC: What do you feel was your greatest contribution while you were at the NCI? What do you remember the most about your career at NCI?

ME: At that point there was one repair process that had been discovered by Ruth Hill, working with bacteria. Ruth is now dead. She died shortly afterwards. But we came along and with mammalian cells demonstrated repair of sublethal damage. We have been studying the cell biology and the molecular biology of that ever since.

GC: What do you think the greatest benefits of doing research at the NCI were for you or have been for you? What was the best thing about doing research there?

ME: To me personally or to the field at large?

GC: Both. You can answer that both ways.

ME: Personally, it opened up doors that would not have opened up for me ten years later or twenty years later anywhere else in the country and it was certainly true working for a U.S. government laboratory at that time. I was able to do things I wouldn't have been able to do otherwise or elsewhere. To the field as a whole, discovering this repair process which at one time was commonly known as Elkind repair. It's been very important to the

field. There is a generation of scientists, radiotherapists included, who don't really know who I am and don't ever cite my work because it's just part of the fabric of things.

GC: Was there anything that was exceptionally difficult about doing research at the Cancer Institute?

ME: It was very good. Very helpful. Supplies were very generous. They had then, and probably still do now, have their own store rooms, from which you could order supplies and we ordered glassware and everything was cleaned and sterilized, and it was like walking into a supermarket: You just helped yourself to things off the shelf.

GC: Really?

ME: Seriously. It was much the same way. I'm not sure we had carts, but we had baskets that we would carry around and put things into baskets. Test tubes were plugged up and ready to go, everything sterile and so on. Then there was a media production department, the department where they produced all the kinds of media you might need. You would design your own medium and have them produce it for you, sterilize it for you. That may have lost out in usefulness when these companies became so prominent and they made all these materials so cheaply, very cheaply. There were two things: the

media supply department and the glassware department, because in those days they didn't have plastic ware yet. But they were terribly helpful.

GC: So was this a central supply?

ME: Central supply.

GC: For all the NIH?

ME: That's right.

GC: Or just for the NCI?

ME: All of NIH, they were located in the basement somewhere, I recall. We would just go down there and walk through with our basket and just pick things up. Now, when we finished, we had to fill out a very cursory form and give that to the person in charge so that they would know we had been there.

GC: It's not like that anymore.

ME: Right. It took about as long to fill out the form as it did to make a copy of the form.

GC: At the time you were there, did you feel that people felt very loyal to the NCI or proud of where they were?

ME: I think so. Certainly. If you get to visit with Katherine Sanford, she's had some rough times from the National Cancer Institute, because she stayed on there up until her retirement. I had left and she was there when the place was imploding on itself. But, in spite of that, I think she would also be very supportive of it, of the organization and the people who were there.

GC: How was the Institute perceived in the field outside.

ME: Oh, there was no place like it. The NIH as a whole is so far ahead of any place else in the world. I'm not saying that improvements couldn't be made, or that they're just perfect in what they do. The size of the organization worries me some, but it's just amazing as a scientific organization. Congress is breathing down their necks all the time. It's highly politicized, very difficult, but in a way they bring it on themselves, because with such a large budget, it's natural that Congress is going to want to know what's going on.

GC: Do you think politics was always a part of the research there?

ME: Well, it seemed to me there was always some committee of Congress who was examining us, investigating what was going on, asking us to justify our existence. There was one committee after another committee. I thought we did rather well. In fact, we had an office that was involved with just communication with congressional committees and giving testimony down there and so on and so on. I don't know how often you watch the radio, but I watch the News Hour pretty often and, not infrequently, somebody from... well, I have two sources of information. I've been active in the Radiation Research Society, which is a scientific society that supports the work I've been describing. I was president of the Society in 1981. Right now I'm just chairman of a committee for research support. My organization is a member of a certain coalition of organizations in Washington called the National Coalition for Cancer Research, NCCR, through them and through information that one normally just receives as available on the radio and TV, I have a pretty good idea of what's going on involved with the support for science and medicine in Congress. Both sorts of information give me the ingredients that I use for a news story I write every couple of months for the newsletter of the Radiation Research Society. I have an idea what the budget is, how the budget negotiation is going in Washington, what it's doing, what it's being used for, and the testimony that's being asked for regularly from the Director of the Cancer Institute, Director of the National Institutes of Health, and so on. Anyway, through the National Coalition for Cancer Research, which is a lobbying organization and lobbies for budgets for cancer research and cancer treatment, I am more or less aware of the building up of the monies in the last ten years

and as it is currently. It is that kind of experience that makes me aware of the fact that there is so much money going into the field, and too much concentration all in one place, in Bethesda. The research budget of the Colorado State University is about \$80 or \$90 million, something of that order; the total budget is about twice that. The budget of NCI is about \$2 billion.

GC: I know, it's pretty unbelievable. Did you ever, were you ever involved in having to justify your work?

ME: Oh, surely.

GC: What kinds of things would you have to do?

ME: Well, we would have to discuss it before committees or write papers about it. Not only at NCI but elsewhere as well, in the Department of Energy and so on.

GC: Any government agency.

ME: I don't think there's anything that was a free ride, or a free lunch. I was quite happy to describe and to justify what we were trying to do.

GC: You mentioned earlier that you left the Institute because you felt like the filters weren't going in fast enough and it was difficult to do work.

ME: I wanted to carry on my work. I wasn't getting anywhere. So I waited maybe six months. It took a number of months for the devices to arrive altogether and still, the Scientific Director wouldn't approve the time and the money to have them installed, so I decided I'd had enough.

GC: Do you feel like you left on good terms with the Institute though?

ME: Oh, yes. Plus the man whose name I'm not mentioning, because I don't want you to give him a black eye, was a good friend for many years. But there comes a time when one has to start to do something in your own life, make your own decisions, and he understood that.

GC: Then you moved on and you've ended up here at Colorado State, where you're still doing cancer research and it sounds like you're still associated with the NCI because of the grant you've gotten.

ME: Yes. There was one or two before this particular grant also. I served on Study Sections and so on and made site visits for them and still do.

GC: Oh, you do?

ME: I haven't been in about a year, but about a year ago I made a visit to Johns Hopkins.

GC: And that's for the Study Section on Radiation?

ME: It had to do with a large program project application that was submitted from the Radiation Therapy Department.

GC: So do you still work with people from the Extramural Branch?

ME: I did up until that time.

GC: Well, I'm coming to the end of my questions. I don't know if there was anything I haven't touched on that you wanted to talk about?

ME: I want to say it again, but I said it before, my background is very atypical.

GC: Very atypical.

ME: And for which I'm very grateful that people kept opening the doors, so to speak, to let me do what I wanted to do. I didn't always know exactly what should be done, but it worked out for the best anyway. I think what I'm working on now is a serious business and it is a combination of fundamental science and some very important practical applications.

GC: It sounds like you had an amazing career and a very fruitful one.

ME: Well, I'm hopeful that I can get something done about my ataxia, but it's holding me up at the moment. One of my sons and his family is coming on Saturday and we're off to Winter Park, which is really over the mountains. There is a Y camp there where we will spend the week. We've been doing this ordinarily with two of the sons, but then Johnny got tied up with trips to Moscow and so on. So, we had to go ahead with just Sam. Johnny is the son who works for Gore still, although on Monday he starts working for

Samuel Berger. But my other son, Sam, who is coming on Saturday, works for United Parcel Service. He's a middle manager in the air delivery of hazardous goods.

GC: So he probably deals with radiation once in a while, too.

ME: He does.

GC: Thank you very much. This ends the interview.