Hertz, Roy 1998

Dr. Roy Hertz Oral History 1998

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National Cancer Institute Oral History Project

Interview with Dr. Roy Hertz conducted on February 2, 1998, by Gretchen A. Case at Dr. Hertz' home in Hollywood, Maryland

RH: I'm Dr. Roy Hertz, and I live in Hollywood, Maryland, at this time. I am scientist emeritus at the National Institutes of Health.

GC: This is Monday, February 2, 1998. It's about 12:30. This is Gretchen Case talking with Dr. Roy Hertz in his home in Hollywood, Maryland

RH: If you would be interested, we might begin with the Clinical Center since this is a history of the Clinical Center that you're involved with, right?

GC: Right, and the whole Institute.

RH: We'll begin with the opening of the Clinical Center.

GC: Okay.

RH: Which was July 6, 1953, now forty-four years ago. Prior to the opening of the Clinical Center, I was given the assignment by the National Cancer Institute (NCI), with which I was then affiliated as chairman of the Endocrinology Branch, to open a research ward at the George Washington University Hospital in Washington, D.C., where we rented the west wing of the fifth floor, consisting of twenty-eight beds. We paid for those beds whether they were occupied or not, and the University was very pleased to have this annual contract with the National Institutes of Health. The dean then was very friendly, Dr. John Parks, who had been a classmate of mine at the University of Wisconsin, who was then also chairman of the Department of Obstetrics and Gynecology, and he welcomed us and gave us all the facilities of the hospital.

We collected specimens there for analysis at the laboratories in Bethesda, although we had a small laboratory on the roof of the hospital. We worked there for four years, training nurses and young clinical associates, essentially interns who had already had their M.D. degrees and some hospital training, to learn something about the clinical treatment of cancer. We admitted to that ward largely breast cancer, prostate cancer, and the design of the program was to treat what we called hormone-related cancers, hormone-producing cancers, and hormone-responsive cancers.

It had been demonstrated by Dr. Charles Huggins, who got a Nobel Prize later, that the use of female hormone was effective in the treatment of prostate cancer, which was dependent in part on the patient's production of male hormone. He introduced the process of removing the testes and putting the patient on female hormone to eliminate the residual male hormone coming from the adrenal gland, which also produces some male hormone. He was recognized throughout the world, and we pursued with him, who was a very dear friend, some of the aspects of his studies.

We then also worked on breast cancer, which at that time had been demonstrated in England by a Dr. Alexander Haddow, that the use of large doses of stilbestrol, a synthetic form of female hormone, would cause regression of advanced breast cancer, inoperable breast cancer, particularly in older women, and it would involve regression of those tissues, which we did a lot of work with. He had also shown that if you used male hormone, that this helped to counteract the estrogen in the breast system and would be particularly effective in the treatment of metastases to the bone. So we treated bone cancer, metastatic from the breast, with androgens and so on.

So we have also people with hormone-producing tumors, particularly of the adrenal gland. The reason why we had that is that in the laboratory, we had developed a compound which was called amphenone, because it was made of two phenol groups, which means two phenols, amphenone, and that later was developed in more detail by the CIBA Corporation, with, instead of the phenol group, they used the pyridil group, and that gave us the now commercially still used compound metyrapone, which is used for the suppression of excess adrenal function. That all came out of our laboratory. So we had that type of hormone-responsive tumor as well. So we had both hormone-producing tumors of the adrenal gland and the pituitary and of the testes, and we had hormone-producing tumors, as well.

GC: *Steroid Hormone Action and Cancer*. I'll hand it to you.

RH: So that we produced all of these various types of chemotherapy, particularly in the endocrine field, and we stayed with that process as really a policy for the development and organization of our efforts. All this we began at the George Washington University Hospital, and on the day the Clinical Center opened, on July 3, 1953, we had been down there four years, we had trained all our nurses, we had trained these young doctors, and we transferred the whole activity to the Clinical Center in Bethesda to the twelfth floor. We opened the hospital on the twelfth floor with those patients, and among them was one of my prostate cancer patients whom I had been treating for four years before with female hormone, actually. His name is Mr. Charles Meredith. I can provide you with a photograph. Attending that day was Dr. John Roderick Heller, Director, NCI; Dr. G. B. Mider, Director, Intramural Programs, NIH; and two nurses.

GC: I'm just going to describe the photograph. There's a man sitting in a wheelchair in front of five people. So the man sitting in the wheelchair is Charles Meredith.

RH: This is a picture of Charles Meredith.

GC: To the far left standing is Dr. Heller.

RH: Dr. Rod Heller. I'm next to him, and you can see that I was a little younger at that time. Next to him is Dr. George B. Mider. Next to him are two nurses, I can't be sure of the names, but they were both very, very helpful and cooperative people.

GC: Did you say her name was Miss Andrews earlier, or did I get that wrong?

RH: I'm not sure. Anyway, I think that picture should be part of your record.

GC: It's a wonderful picture.

RH: You're welcome to it. I can give you a copy. I can give you a copy of this first permission slip, which we admitted that on the day the hospital opened. So much for the opening of the hospital. The next development on the half of the twelfth floor which we were not using was opened by a doctor, whose main interest at that time was in arthritis, and he and I became very close colleagues.

RH: He pioneered the extension of the use of steroid hormones in the treatment of arthritis, which had just been developed at the Mayo Clinic, for which the originators at the Mayo Clinic had just gotten the Nobel Prize, and this doctor followed because his interest in clinical medicine was in arthritis and in vascular diseases as well. He pursued that with great gusto, and the activity of that first year or two was very brisk.

We had a great deal of problems in organizing several departments in the integration of the laboratory services with the clinical services with the X-ray services and the pathology services, and in the pathology services, not only the processing of biopsies, tissues from the patient for diagnosis, but particularly the conduct of autopsies.

I helped with the first autopsy in the autopsy room at the Clinical Center. I helped a young pathologist who was given that assignment, and he and I had some problem in doing this, because the autopsy room had big long open windows, which were open to the public, and they were on the second-floor level, so that anybody who wanted to look in could do so. We had to hang up hospital sheets to cover the windows until we got proper curtains for that autopsy room.

So that was part of some of the things of breaking in a new facility for a new kind of hospital activity, which had not been done before. There had been a similar operation at the Rockefeller Institute in New York. They had a research hospital there, but they did not have the degree of integration of the basic sciences with the clinical activity. But we sort of had to find our own way in bringing these several services together to get the proper cooperation between very self-willed and highly individualist specialists, of which we were one. So it finally settled down after about a couple of years, and we had a very smooth operation.

We had the redeeming feature of having the medical board which served as the governing body of the Clinical Center. The chairman of that medical board later became Surgeon General; his name was Dr. Luther Terry. He was a cardiologist by specialty, and he served as an extremely able judge of what seemed to be appropriate intervention in essentially investigative therapy. What we were commissioned to do was to explore new therapies in previously incurable, untreatable patients, and those patients were admitted to the hospital only by permission of the referring physician who sought our help. That doctor was assured that he would get a complete report of each case, and the case would be referred to him for the continuing therapy. So we set up at that time the proper professional relationships, which were very important.

A very important other function at that time was for the various staff members to go out and talk to the various local medical societies to convey this idea of how we were going to cooperate with local communities, which are referred to as LMD [local medical doctor]. The LMDs responded with enormous enthusiasm, and one of the things that built the Clinical Center, was the response of the great LMDs of this country. We began to get referrals from all over the world, especially when I started working on choriocarcinoma, which was much more prevalent in Africa and other places.

As a matter of fact, I had as one patient that I treated by correspondence with the LMD back in Tokyo, I had the wife of the Director of one of their medical research institutes, Dr. Kurokawa. His wife was cured by our development of our chemotherapy of choriocarcinoma. On one of my subsequent trips to Japan, of which there were several, to talk about chemotherapy, Dr. Kurokawa and his wife called on me, and she was dressed in full Japanese regalia and bowed from across the room sixteen times before she would look up at me. She could not speak a single word of English but she smiled a smile of gratitude, which I'll never forget. She presented me with two almost life-sized dolls, fully equipped Japanese kimonos for my daughter, to take home to my daughter. In addition, they took me to see the Japanese pantomime theater, the name of which slips me right now.

GC: I can't think of it either, but I know what you're talking about.

RH: They took me to see one of those performances, and we sat in the royal box and watched this performance of this pantomime theater, which was tremendous, I understood everything from the pantomime, which is accompanied by a constant flow of Japanese narrative. This was a story about how some woman got killed in the woods and the dozen different stories about the circumstances under which she was killed. The fact was that her husband had killed her. So that was the story. Well, anyway, those are small incidents of that time.

We began to have the type of response which was attributable largely to these referring physicians and their enthusiasm, and we also had another development which was especially helpful to us. That was during the war period, and physicians were brought back and were put through medical school under federal grants. Under those federal grants, they became obligated to serve, either in one of the armed services or in some public institution. We were able to make our selection among those graduates in the post-war period from every medical school in the country, all of whom much preferred to come to the National Institutes of Health for research interests rather than serve in a federal prison, or some other such federal facility, or in one of the armed services. So that we were able to get essentially the cream of the crop of each succeeding graduate class over that period of time, and we set up a clinical associates program. At one time I had a group of about twelve such clinical associates who finally reached a total number over the years of some forty-five, every one of which except one went into academic medicine.

RH: Among them, they are now some of the leaders, some of them now beginning to approach retirement, actually, because what we're talking about is the fifties. They include people like the chairman of the Department of Obstetrics and Gynecology at Duke University, the chairman of the Department of Gynecological Surgery at Memorial Cancer Hospital [Memorial Sloan-Kettering] in New York, great cancer research center, who has just retired from that position. Shall I say names?

GC: Absolutely.

RH: The first name that I should tell you is Dr. Charles Hammond at Duke; and Dr. John Lewis at Memorial Hospital; Dr. Donald Peter Goldstein, professor at Harvard Medical School.

GC: There's forty-five of them.

RH: Yes. Well, anyway, that gives you a sample. If you wish, I could assemble an actual list of them.

GC: That would be amazing.

RH: I do have such a list.

GC: Was Dr. [Vincent T.] DeVita one of the people on your list?

RH: Dr. DeVita came in the subsequent group and developed originally as a clinical associate with one of the other services in cancer work, and he was an outstanding contributor to the development of the Clinical Center. Have you interviewed him?

GC: Yes. He's at Yale now. I went up to Yale to visit with him for a while.

RH: Is he all right?

GC: He seems to be doing very well, yes.

RH: I'd say he's in his late seventies.

GC: No, Dr. DeVita is probably in his fifties I think. Really?

RH: Couldn't be.

GC: Well, then he looks very young.

RH: So one of the great satisfactions that I have derived from this total experience has been the training and development and careers of these forty-odd clinical associates, which have been most outstanding and rewarding, and to whom I'm everlastingly grateful for their continuing contact with me. As a matter of fact, Dr. Hammond will attend my lecture next week, very probably at Chapel Hill. A number of people from Duke will come over, because what we did was we set up regional centers financed through the National Cancer Institute for the treatment of choriocarcinoma and related trophoblastic disease.

Those seven centers finally settled down to about five that survived. They're still functioning and are maintained on annual grants from the National Cancer Institute, institutional grants, and have provided consolidated and computerized data from these various centers for the development of newer therapies for the treatment of trophoblastic disease by these respective centers.

That is one of the major contributions, I think, of our group's efforts during that time, and all of these were manned by people who had previously served as clinical associates at Bethesda, with one exception. That was at Northwestern University, which was manned by the Northwestern University medical people, because that had been the center for the study of the morphological or structural aspects of trophoblastic disease under the management of the chairman of obstetrics and gynecology out there, a gentleman by the name of John Brewer, who just deceased at ninety-two years of age a few weeks ago and had been a lifelong friend whose people came and trained with me at Bethesda. He himself spent a couple weeks with us there and then came back and got set up at Northwestern University, which still functions.

GC: Really.

RH: Oh, yes. Yes, indeed. So that's when I think of one of the lasting accomplishments of that effort.

GC: I wanted to go back for a minute. When we talked earlier before I turned on the tape, you said you were very involved of the planning of the Clinical Center and how it was going to be laid out. Was that a committee, or how did that work, the whole planning of how the Clinical Center was going to be built?

RH: Well, the fact of the matter is that where the Clinical Center, the original Building 10 stands, was a big hill, and on top of that hill there stood a guest cottage which was called Top Cottage, because it was at the top of that hill. That guest cottage was used for conferences and seminars and visiting lecturers, and there was actually a room off the lecture room where visiting lecturers could sleep for a couple of days if they came to give a lecture. We had people from all over the world come to discuss various matters of common medical interest and to acquaint them with what we were doing, so on, and that place was really the educational center of the National Institutes of Health.

However, when they decided to build the Clinical Center, they had not only to remove Top Cottage, they had to take it apart and destroy it, and they had to remove that hill. They had to take away enough soil to spread out over the grounds and to help to build up some of the valleys in the total estate. That total estate on which NIH is now situated was originally the Wilson estate, and that was a result of the fortune made by the Wilson Company sports goods, tennis rackets, golf clubs, so on.

GC: I never knew where their money came from.

RH: Wilson. That's right. Mrs. Wilson also had some kind of an interest in the Woodward and Lothrop Department Store, and that was given up sometime fairly early. Mrs. Wilson survived her husband's death, which occurred, I would say, just a little bit prior to the opening of the Clinical Center, and she continued to live in a mansion which is still on the campus, which is almost directly opposite the present Children's Hospital on the campus. That was called, I think, it was called the Wilson House.

GC: Yes, I think you're right.

RH: Yes. That's right. Then Mr. May was the grounds supervisor at that time and was widely heralded as the finest maintenance man for maintaining the gardens and the appearance of the grounds at the National Institutes of Health, and in later years we acquired the mansion of the previous local Bishop of the Diocese of the Episcopal Church, which is Stone House. It sits up on top of a hill. That was the residence of the Bishop of the Episcopal Diocese of this area, Dr. Peters. I used to see him walking around in the evening saying his evening prayers.

GC: Oh, really.

RH: Yes. I first came to NIH on Monday morning, December 8, 1941, the morning after Pearl Harbor on Sunday, December 7, 1941.

GC: How did you hear about the NIH?

RH: Well, it was already recognized through the U.S. Public Health Service as a great research center. The Cancer Institute had been brought down from Harvard where it first was organized as a Cancer Commission. There was also set up a laboratory which was for general biological sciences, and these essentially organized around the extension of the work of Dr. Joseph Goldberger. Joseph Goldberger was the gentleman who had proven that an epidemic disease, pellagra, which was very extensive, was due to a nutritional deficiency. Everyone up to that time had assumed that pellagra, like all other diseases, because of the Koch period, was probably due to a bacterium of some kind, an infection.

When I came to NIH, the predominant emphasis of clinical activity and clinical interest on the campus was infectious diseases, and the rest of the Institutes were just sort of carried along as accessories to infectious diseases where a great deal of important work had been done on polio, on brucellosis, and many other important infectious diseases. So it had been well earned over the centuries, actually beginning in about 1798 when the Laboratory of Hygiene for the U.S. Public Health Service was first organized to have people go out through the marine hospitals to meet boats before they came in and to investigate what diseases any people on the boats might have that they might bring in, and to prepare a quarantine of any of those people and put them in the marine hospitals. That was the origin, actually, of the original research activity of the U.S. Public Health Service.

From that grew the concentration of some of this activity in Bethesda at the National Institutes of Health. Dr. Goldberger had been stationed, prior to the opening of the National Institutes of Health, which originally occupied a property in downtown Washington on Constitution Avenue. Do you know about that?

GC: Right. There was a small laboratory there, right.

RH: Small laboratories there. Still there. That building's still there. It's a white building. It's used by one of the other federal agencies. Anyway, I worked down there for a while. I helped with some work that was going on down there, as a clinical associate when I was in training.

GC: Who hired you? Who brought you to the NIH?

RH: Well, actually, there was a Division of Personnel in the U.S. Public Health Service headquarters downtown, and Dr. J.R. Heller was in charge of that personnel action at the time, and he later became Director of the National Cancer Institute. He helped me to get my original appointment at the National Institutes of Health. I had already had my M.D. and Ph.D. and my M.P.H., so I was pretty well prepared.

GC: I guess so.

RH: In 1941, I was thirty-two years old, and it was really after a period of struggling through many years of the Depression on year-to-year appointments and finally survival through medical school. That was my first really properly compensated opening was with the federal government, because it was essentially a public works project, is what it was.

GC: I hadn't realized that.

RH: Yes, it's true. That's how we came to be. Mr. [Franklin D.] Roosevelt wanted to do that. Yes, indeed. Sure as hell did. I was a thirties' baby, and things were tough, I'm telling you. I remember going to Chicago at the time of the World's Fair in Chicago in '39. That World's Fair was put up on the grounds which, earlier in the Depression, in the early thirties, was known as Hoover City in honor of President [Herbert] Hoover, who was given a lot of the credit for the Depression and refusal to do anything for anybody. Hoover City was occupied by unemployed men who camped out on the banks of the lake.

GC: Lake Michigan.

RH: Lake Michigan, yes. Well, anyway, that's a part of the background of what I'm talking about, so I was really most, most grateful for an opportunity. Thirty-three years of age, I was married. I had not had any children, we deferred having children. I never had any children until 1943. That's right. We married in '34. First marriage. So during all that period, we were deprived. My wife was a social worker, and social workers were employable during the Depression. So she always had enough funding to work me through medical school.

GC: In 1937, President Roosevelt signed the bill that created the National Cancer Institute. Do you remember hearing about that, or was that a part of your world when that happened?

RH: I think so. I think so, yes. Well, I knew things were going on. Oh, yes. I knew about the U.S. Public Health Service. Dr. Parran had been an outstanding Surgeon General who was brave enough to take over from a private organization called the Social Hygiene Association. This was the only group that would deal with venereal disease publicly, and he was brave enough to take it over as a public function. He introduced the study of venereal disease at the schools of public health and to fund them and to set up treatment centers for the treatment of venereal disease. That evolved in great rapidity during the war period when the soldiers were coming home with all kinds of problems.

Anyway, the National Institutes of Health then evolved essentially from infectious diseases through the work of Goldberger into other related areas, because he had shown that something other than a bacterium could cause a disease. So that set up studies in nutrition, which is what I joined originally. When I came to NIH, I worked with a Dr. Sebrell. I was assigned to his laboratory. He had been a junior colleague of Goldberger's and had taken over some of Goldberger's studies. Goldberger, unfortunately, died fairly young and left a widow and a son. In the attic of Building 4, where I first had my laboratory, there was a heap of all of the mementos and records of Dr. Goldberger's original studies. It was subsequently shown that black tongue in dogs, a nutritional artificially induced condition in dogs, was the equivalent of pellagra in the human, and it could be cured by a very small amount of nicotinic acid (niacin) and has nothing to do with tobacco. It's a simple compound, and that was the active principal which was the deficiency which had created this pandemic. You know the clinical features of pellagra were the three Ds. Rash. Diarrhea, dermatitis, and dementia. The place where Goldberger was able to do most of his work was in a mental hospital in Georgia where he was assigned as the Public Health Service officer to help out, and he and his wife undertook these investigations and proved the cause of pellagra. All of those materials were in the attic of that building, and among the various things I found an octagon clock, Seth Thomas eight-day clock, which you wound by your hand. That clock sits in my son's house in California after I preserved it and kept it at home for many years. When he set up his own home, I gave him that clock. It's still there.

RH: I think I've given you the essentials of the background of NIH and its origins and theoretical development, which I think is important for you to have. Now I think the next step is to start to get into some of the major clinical contributions.

GC: That sounds good.

RH: From the Clinical Center, one of the early ones was the development of newer forms of cortisone for the treatment of arthritis by this Dr. B. His first letter of his name is B. It's coming to me. We were such close friends for so many years. Anyway, Dr. B. died quite young. He developed a coronary and died quite young, probably about fifty-five. Anyway, that was one of the first major clinical contributions of the Clinical Center.

Then along came our developments both in the treatment of trophoblastic disease, for which I coined the term, because it is a cancer which is derived from some of the first cells of early pregnancy which are developed nominally to nourish the rest of the development of the organs before any organs are formed. These cells are called trophoblasts. When you're a fetus about eight days in the uterus of your mother, you consist of 107 cells, and of those cells, 90-odd are trophoblast cells. They are cells which are formed by rapid multiplication, very rapid multiplication, to provide the necessary nutrition for the formation of the organs of the fetus. For that reason, they're called nourishing cells. The word "trophoblast" means nourishing. So trophic, chemotherapy of trophoblastic disease.

Now how did that come about? I had become, in my work in nutrition with Dr. Sebrell, very much involved with elements of the B complex, of which one turned to be the treatment for pellagra, and there were other elements of the B complex, which a lot of work was going on at the University of Wisconsin. A big breakthrough was the microbiological tests for these various nutritional materials at the University of Wisconsin under the work of a Dr. Elvejehn, who later became president of the University of Wisconsin. He was then professor in the agriculture school. He and his students found that a certain organism, which was called the Lactobacillus casei, because it was the organism which soured milk, and the Lactobacillus casei was found to require practically all of the vitamins of the B complex and several of the unknown vitamins, because you had to give this bug, to make it grow, all of the known B vitamins, but there was additional material you have to add with a supplement either made out of liver or yeast, which contained a large amount of these naturally occurring substances which had not yet been identified.

So we were able to use the Lactobacillus casei test to try to learn about newer compounds, and I got the idea that not only was this important in terms of the growth and development of the body, but of reproduction as well. I began to study the relationship of nutrition to reproduction. I began my studies first with the baby chick, newborn chick.

RH: This is a picture of the newborn chick, and one of the subtitles to this picture is "They're waiting for the school bus." Anyway, this is one of my prized personal possessions. Unfortunately, it broke, but I can get you a copy. This is a chick which was maintained on all the then-known vitamins, but, in order to grow as big as this one, would have to have a supplement of either liver or yeast, in addition to the then-known B vitamins. So I knew there was something in liver and yeast which did this. I found out by trying to relate this to reproduction. By giving this animal female sex hormone, I could stimulate the growth of the uterus. We knew that what corresponds to the uterus, it's called the oviduct in birds. In this bird, if it didn't have the folic acid, nothing happened. Estrogen wouldn't work.

I knew that there had been some earlier work by a Dr. [Lucy] Wills. She was a medical missionary in a remote village in India around the turn of the century, and she found that when certain of the women there who would not eat any of the abundant vegetables around, but ate some kind of a starch diet, when they got pregnant, they would develop an anemia, but it was a peculiar anemia of pregnancy. That anemia was different from any other anemia in that in most other anemia's the red cell is smaller than normal. In this anemia, the red cell is bigger than normal. It was therefore called the macrocytic anemia of pregnancy. To this day, it's also known as Wills anemia. Her name is Wills. That's as close as I can come right now.

Anyway, Dr. Wills made the observation that if these women did not eat green vegetables then they developed Wills anemia and they aborted. They all lost their babies. If she put them on the green vegetables, made them eat green vegetables, the Wills anemia would go away, and they would go through a normal pregnancy. She delivered them in this small village, so she knew the whole thing from A to Z. This was about the turn of the century. I knew about her work, so on, from having studied all these problems of nutrition and so on, so forth. This is all in my book, incidentally, which I don't have a have a copy here. I should have a copy of my book. I sent you some chapters from it.

She knew there was something in these green vegetables. So we also postulated there was something in these green vegetables. So what did we call it? You know what we called it? Folic acid. Why did we call it folic acid?

GC: From folate or foliage?

RH: Foliage. It was a foliage vitamin, so we called it folic acid.

GC: So you named folic acid.

RH: I am not certain. I think I did. Either I or Dr. Elvejehn, because he knew about this, too, and he worked on it for a while. He called it the grass juice factor, green grass juice factor. He named it folic acid, that's right. I got it from Elvejehn, or one of his boys. He had a whole class of graduate students. He was a great teacher. Conrad Elvejehn.

So, we knew this factor existed, and I had discovered it, that it had this very important relationship to the growth and development of the reproductive tract. I extended these observations from the chick to the frog, to the rabbit, to the rat, and to the monkey. When I got to the monkey and I was able to show this, at that time there was emerging a tremendous amount of interest in what was called the antagonist principle. That is, there was a substance which was essential for the mechanism of action of the metabolism of a cell. If you changed the structure slightly, you could get a substance which would lock into the place where that substance would be used, and the right one couldn't get in. That's the basis of the antagonist principle. It's like taking a key that won't fit that lock and put it in there, and then I can't get my good key in there, until I take that one out, see. That was the principle of the antagonist principle which had been developed by a pair of physicians in Canada. I knew all about that work, and I was the first to extend that to rationalizing the interaction between male and female hormone and estrogen and progesterone, which have a similar antagonism.

I postulated that at a meeting at the Hershey Inn where we had scientific meetings periodically. That would be about 1945, I think, and the chairman of the meeting, Dr. Dr. Heard, was professor of organic chemistry and steroid chemistry, wrote the big textbook on chemistry, said, after I got up and postulated this idea of the antagonism, said he'd never heard such nonsense. Charlie Huggins, who was the professor of Urology from the University of Chicago, who had developed this form of treating prostate cancer with estrogens to oppose the male hormone, and I postulated that's how he got his results, he got up and he said, "You may call it nonsense, but I have never heard a more important generalization in biology." That's all a matter of record of that conference of which no record exists, no written record exists.

Subsequently, about a year later, I wrote a paper along this line and gave it before the American Association of Science, and you'll have a copy of it in those reprints there. So that antagonist principle, together with what we knew about nutrition, operated very strongly, so we got the idea that the way to stop the growth of the female genital tract, if you wanted to do that, was get an antagonist to folic acid. We worked very, very closely with the Lederle Company, which at that time worked very closely with NIH people. All the drug houses did, and still do. As a matter of fact, the Merck Company just built the Children's Hospital for us. Did you know that?

GC: I did. I knew they were involved.

RH: Anyway, we've always worked very closely with the pharmaceutical industry and developed new things. It's been a very fortunate development. The Lederle people synthesized. First of all, we had to learn the structure of folic acid. How did we learn the structure of folic acid? People at Lederle were able to ascertain that this compound, at that time made from spinach. I've still got some of the powdered spinach in my drawer.

GC: Do you really?

RH: Yes. I'll show it to you. Anyway, it was made originally from spinach. That compound, which they finally isolated, was a pterion, which means "wing." You know pteridium, pterodactyl.

GC: Pterodactyl, yes, sure.

RH: The word means "wing." Why was that material called pterion? Because it was similar to the yellow pigment in the yellow butterfly's wing, in the wing of a yellow butterfly, which had been worked out by a British, this is all in my book, and pictures of mine and this whole thing, but pictures of these people we're talking about, Lucy Wills and all these other people. So, antagonist principle and the folic acid thing and these people synthesizing these compounds, and, as they synthesized them, they sent them to me to test in this system.

GC: In the chicks.

RH: In the chicks and also microbiologically, in the L. casei test, which they were running up there, as well. When I tested here, I found some of those which I could effectively stop the growth of the uterus, even in the presence of the maximum effective amount of female hormone. I could flood this baby chick here with female hormone, and I wouldn't get a budge in the uterus unless I gave it some folic acid along with it. Crystalline folic acid. I had the first grams of crystalline folic acid that were ever made, sent to me by the Lederle Company. The colleague with whom I worked most closely there was a Dr. Jukes, Dr. Tom Jukes. He was a biologist. The chemical colleague with whom I worked there was a Dr. Stokstad, and you will read in some of the work there about the folic antagonist. This is all in my book in detail. I've got to get you a copy of that. I'm so sorry. I'm actually having a printer produce fifty new copies for me. I'll have them in a few weeks. I'll send you one.

Anyway, Dr. Stokstad and Dr. Jukes and I worked out the fact that this stuff was active in this system and in the L. casei system, and they then, together with their Research Director, who was a Hindu gentleman at that time, by the name Subbarao, he was the Director of Research at Lederle at that time and was their Director, and he said, "Go full blast on it, boys, and let's get some of this and try to see whether it can be used in gynecological problems to control the growth of the uterus, probably uterine cancer or some other types of cancer." We began to explore the clinical possibilities, and we then began to test it in choriocarcinoma, which was one of the most primitive of the growths of development of the female genital tract, and it worked. The first form of the clinical form of that was methotrexate. Now the history gets complicated by the fact that a Dr. Min Chiu Li, I don't know if you've ever heard anything about Dr. Min Chiu Li.

GC: Yes, he was someone I wanted to ask you about.

RH: Dr. Min Chiu Li was clinical associate in our department. He had been sent to me by a colleague of mine at Memorial Hospital, Dr. Olaf Pearson. I had demonstrated in breast cancer cases that the folic acid antagonist would block the evidence of activity of breast cancer in the bone, which would raise the serum calcium of women and they would get a hypercalcemia. I had demonstrated that calcium was lowered when I gave the folic antagonist to breast cancer patients, showing that it was anti-estrogenic in the human. I had demonstrated that, and that was in the literature.

Just prior to sending Dr. Min Chiu Li up to my lab, Dr. "Olie" Pearson attempted to do this -- to reproduce my work on calcium in a breast cancer patient to see if he could make it work, because it was a possible form of therapy for this disease. Sometimes if this calcium got out of hand, that's what killed the breast cancer patient, hypercalcemia, because they'll die if the calcium gets too high. So we found that this stuff would work, and so he was attempting to reproduce that. He had gotten some of the methotrexate, which was then beginning to be developed more and more by microbiological means, actually developed by making folic acid and then changing it. That's where we first got it.

Min Chiu Li was his lab assistant. The reason he sent me Min Chiu Li is that Li had become a naturalized American citizen, and as soon as he became American he was an M.D. and as soon as he got his citizenship papers, he got drafted, and when he got drafted, he wanted to stay out of the draft. "Olie" got in touch with me and said, could I use this gentleman as a clinical associate, and I took him on. We were very good friends and worked very, very closely, hand in hand, but he had had the experience with Dr. Pearson which not only showed that the calcium in one of those patients would come down, but they didn't use this in the breast cancer patient, they used it in a melanoma case. Certain cases of melanoma, certain cancers produce the same hormone as pregnancy, chorionic hormone. Certain cancers do that, because it's rapidly growing tissue, and any very rapidly growing tissue has this potential. Even certain rapidly growing bacteria will produce this.

GC: Will produce the chorionic hormone?

RH: Chorionic hormone, yes. So they had demonstrated that this woman with melanoma who had the hypercalcemia also had gonadotrophic hormone in her urine, and when they gave her my methotrexate, not only did the calcium come down, but the gonadotrophin came down. Li had been the clinical assistant on that case, and when he came down, he told me about this. We talked about it, and we said, "Well, let's go ahead with this chorio work and see." I'd never given methotrexate to chorio before. We're talking about the first case. I said, "Let's go ahead and try to see what happens." I said, "We've got assays in the laboratory going all the time for chorionic hormone, and we're all set up to do it. There's a unique possibility following these cases."

By God, the first case worked, the second case worked, the third case worked and brought down the chorionic hormone precipitously within a few days of treatment.

GC: Within days?

RH: Within weeks, within weeks of treatment. At the same time, Li was in touch with another clinical associate who was not in my service, but was a chemist in his own right. This is all in the first paper about the subject. Paul Condit was his name. Paul Condit had reasoned, by studying blood levels of methotrexate in various experimental situations, that the way to use it was to give a hell of a lot to produce toxicity and then let the toxicity subside and get the maximum effect, and he had outlined the details of how to give it to Li and to me.

So that's when Li put the first patient on that five-day intensive therapy. It developed to specifically produced toxicity, maximum toxicity. That toxicity consisted of diarrhea, of sloughing of the mucous membranes, and a dermatitis and loss of hair, that's right, because it hit every cell in the body, but it hit the cancer cell worse, and the other cells in the body would recover, whereas the cancer cell wouldn't, and that established this differential response, quantitative response between the cancer cell and the normal for the first time. Nobody ever had postulated that earlier, and we worked all that out together, Li and I.

I wrote the first papers. Li was very deficient in English. He used the English language very poorly, and he had just been naturalized and so on. He was a thoroughly fine young man, and very intelligent. He had left his wife and two children in mainland China and had been here in the States finishing his medical training for seven years. He had an aquarium in his apartment. He lived in the apartments across from the Clinical Center; some of the clinical associates at that time were given that privilege. He had an aquarium in there full of tropical fish, and he would go over for two hours a day and sit in front of that aquarium and watch the tropical fish and think about his wife and children back in China, every day. He was a very interesting, complicated fellow.

So I wrote the first papers for him, but I said, "Li, you've been doing so much work on this, you're going on as senior author." I put him on as senior author. Those first papers, as you'll see, are Li, Hertz and so on, for the first three cases. So he gets the idea, he leaves us and goes back to Memorial, back to "Olie" Pearson, and when he goes back up there, he gets the idea I was given, because of the then work on chorio got to be very widely known among gynecologists and everywhere around the world. The thing went like a telegram around the world, and I was given a thousand-dollar prize by the American Association for the Advancement of Science. It was called the Rosenthal Award, money given by a gentleman by the name of Rosenthal, a very wealthy philanthropist. I'll tell you more about him later.

But, anyway, Rosenthal gave this award. He had a committee, an awards committee, of noted scientists, and they voted that I should get this award. I received letters and so on, and I accepted the award, and that was published in the newspapers. In that day, when you got an award like that, it was in the newspapers. This is 1956, '58, '57. '57, '58. It was in the newspaper. I get a call from the Director of Memorial Hospital, "Dusty" [Jonathan E.] Rhoads was his name. His name was Rhoads, but we called him "Dusty." I've forgotten his actual first name, "Dusty" Rhoads. He was an M.D. who had gotten into cancer research administration. He lived in a penthouse, which is still there, above Memorial Hospital. The Director of Memorial Hospital lives in a penthouse above the hospital.

RH: I attended the dedication of that hospital, and I was one of the speakers at the dedication, and part of the dedication was a housewarming in the loft. That was 1955, I'd say about then we opened our hospital in '53, about '55, '56, something like that. So "Dusty" and I were friends and so on. So as soon as this award gets in the paper, I get a call from "Dusty." He says, "Where do you get this idea that you did the chorio work? You know who did that. It was Min Chiu Li." I said, "Of course, Li was with me. Of course he was. He was my clinical associate. We published the first papers together. What are you yakking about?" He says, "You know we should have gotten that award instead of you."

I said, "No, it's not true. I earned that award, and I developed the folic acid antagonist, and I have no apologies about taking that award." I said, "Maybe I should have shared it with Li, okay, but I didn't think of it at the time. I was so pleased to get it."

So that started the whole controversy between Li, and from that time on, Li maintained that I had inserted myself into his work in the chemotherapy of trophoblasts. Nobody else ever agreed with him, practically nobody else ever agreed with him, but he maintained this sufficiently that when I got the Lasker Award in the seventies, he was there, we shared it, and so on. Perfectly fine, all right with me to share, but he insisted it was all his, it was 100 percent his; nobody else had anything to do with it. Condit had nothing to do with it, I had nothing to do with it. As a matter of fact, choriochemotherapy had been undertaken by a Japanese, this is all in the first paper by a Japanese fellow by the name of N. Ishizuka, and also by a Memorial Hospital guy who had died, with a Russian name, very good friend of mine. This is all in that first paper. You've got the paper there? Yes, sure, you've got the paper right there. You can get these names out of them.

So, that's the story of the Li controversy. I never said yes or no to him about it, and I never denied his association with it, and I gave him full credit. I don't know what all the yelling was about, but he never stopped yelling until the day he died. He went from job to job after that. He was very irascible, very irritable, pugnacious individual, got into fights with everybody. When he was at NIH, he got into fights with a number of people, and I had to intervene a number of times. I was the only one knew how to get along with Li.

Our neurosurgeon started doing hypophysectomies in breast cancer cases. The idea was to take out the pituitary gland in breast cancer cases to rid them of all hormones and see whether we could get the breast cancer. That had been originated at Memorial Hospital, and our neurosurgeon and I had gone up to Memorial Hospital to review both the medical aspects and the surgical aspects, and we came back and started to do a few of these cases, and Li had been involved in a few of these cases, so I assigned Li to work with this neurosurgeon. The neurosurgeon came to me on the second case. He says, "If that's the only one that's going to work with me, I'm not going to do another one."

So, that was the whole story about Li. He was pugnacious, very difficult, very hard guy, and we attributed [this] to his loneliness and his privations and so on. So that's my side of the story. So, anyway, Li went from job to job, fighting everybody. He went from Memorial, they took him back at Memorial, where he stayed a year. They threw him out. He went to Down State. Do you know the New York hospitals?

GC: I don't know what Down State is.

RH: Down State was the old Long Island Medical School. The hospital which they had was down in lower Manhattan, was called Down State, because they also had a hospital up in Albany, that was Upstate. And down was called Down State. The Director there was a good friend of mine, and he took Li for about a year, and they had to let him go. He went from there to the Long Island, is it called Long Island? The big island off of New York. Yes, Long Island. Long Island Clinical Center in the middle of Long Island, where a Dr. [Arthur C.] Upton is the Director of cancer work there.

GC: Sure.

RH: Yes. Do you know about him?

GC: Yes, I've met Dr. Upton, too.

RH: Dr. Upton can tell you about Li. He was able to handle him for about a year, and he had to let him go, whereupon he emigrated out to the Seventh, he was going from bigger and greater institutions to lesser and lesser institutions and then he went from there to a Seventh Day Adventist school in California. I'm sure you're familiar where it's at in California. In suburban L.A. in California, there's a medical school out there.

GC: Fullerton?

RH: Not Fullerton, no. That's not the name of it. Well, whatever that place is, and a Dr. Longo out there, who is head of obstetrics and gynecology out there, befriended Li. Li died. What he died from, I don't know, but he died within a couple years after he was out there, and I hadn't heard of him since. His two little boys after he left us and had gone back to New York, he brought his wife and two little boys over, and his mother. He brought them all over to the States, and then he took them to California, and then he died. What happened to them and all their future history, I've not been able to find out. I've tried to find out from many sources, from this Dr. Longo, and he doesn't know what he died from. He won't say what he died from.

Anyway, I feel so bad about the whole thing. I've had forty-odd clinical associates. He's the only one who does not honor me. The only one. And that hurts. You know, it hurts that any such controversy about credit should come, it hurts my conscience, I mean, I have a bad conscience. I must have done something not quite exactly right. Well, anyway, that's the truth about that as best I can tell you. So, you can investigate. Maybe you can find his family, find out something. I don't know. If you do, let me know.

GC: Of course.

RH: Anyway, that's the story.

GC: A sad story.

RH: Yes. Then there, of course, developed a worldwide recognition of all this and I undertook all these travels that I've told you about, lecturing and recognition and so on. At the same time, there was going on in our laboratory, over in the basic laboratory I should tell you, I had two wings, three wings in the new Clinical Center building. I had an animal facility on the tenth floor which went out north and south, and then I had two wings on the twelfth floor which went east and west. I had 12 East and what was then called 12 North. We called it 12 North. Then I had 10 I've forgotten what we called it, 10 something. In those, we had our animals. We had to use animals for the chorionic hormone tests. We didn't have the later development. Our laboratory was very influential through a Dr. Vaitukaitis, who is still with the Institute as Director of the Clinical Center's program. You might talk to her, Dr. Judith Vaitukaitis. She's a woman now about sixty years of age. I would say maybe, yes, I would say she's middle fifties maybe. She's a large woman. She has gained enormous amount of weight over the years. She was a very attractive, very good-looking woman, but she never had any male contact from any interest. But, anyway, Judy, a wonderful person, very good friend of ours. Judy developed from the animal test for the chorionic hormone. She was probably the main one who helped develop, with a Dr. Griff Ross, who was one of the other colleagues, first the chemical tests and then the immunological, even more sensitive immunological tests for chorionic gonadotrophic hormone.

GC: We were talking about Griff Ross.

RH: Two of the most important clinical colleagues I had were Dr. Griff Ross, who had come to me from the Mayo Clinic, very interesting man. He already had his M.D. and Ph.D. degree when he came. He had his boards in internal medicine, was a highly trained specialist in endocrinology. He was the son of a practitioner of general medicine in a remote small town in Texas near the Mexican border, and his brother was a practitioner, took over from their father as a practitioner of medicine in that small town, and he was a small-town Texas boy. He said "Tex Mex," he always called it. He was a wonderful, wonderful person, storyteller. The patients just adored him, and he could do anything, talk anybody into anything. Griff and I were very close. Then there was also Dr. Mortimer Lipsett.

GC: Yes, that name has come up, too.

RH: There's an auditorium, because Mort then had a tremendous administrative career after he finished with me. I'll tell you that story, too. Mort was probably the sharpest bedside doctor I've ever worked with. He was so keen and so observant and so knowledgeable, that whenever you were stuck about a case, you'd call Mort.

GC: Really.

RH: Oh, yes. Yes. Now, the way we worked is we regarded ourselves as three senior doctors who were pretty much on the same level, although I was chief. That was always understood, I was chief. We decided that the clinical obligations of the ward would take up so much time that what we would do, we would each take four months on the ward of the year, and that would make up the twelve months of the year. I would take four months, they would take four months, and during those four months, I would be available for any clinical problem that the clinical associates had, any patient-care problem. They would be available for consultation on any of those cases, and I was available when they were on, and so on, and any night calls or extra duty, the guy who was on for those four months was responsible. If you wanted to leave town for a few days, go give a lecture someplace, you'd sign out to the other one, see.

We went along like that, it started about '58, and we went along unperturbed until the year I became Scientific Director of the Child Health Institute, 1965. 1965, I became Scientific Director of the Child Health Institute with the understanding with Dr. Shannon, in the Director's office, that I could take my whole operation from the Cancer Institute and transfer to the Child Health Institute since we'd become so much involved in reproductive medicine. That seemed to make sense, and everybody in my group decided to come with me, except Mort. Mort decided, he was a very competitive person, and he decided that he wanted his own setup, he should have his own setup, and I thought he was right.

He went to see Dr. Shannon. Shannon said, "You stay in the Cancer Institute. Let the rest of them go." That's what they did. Griff came with me, and everybody else except Mort and a few people attached to Mort's laboratory stayed with the Cancer Institute, and he then developed his own, he was given part of my beds, and the rest remained with us for reproductive research and so on. Griff came over as my Clinical Director, and I was the Scientific Director of the Child Health Institute. Biggest mistake I ever made to leave the Cancer Institute.

GC: Really.

RH: I was miserable as a full-time administrator. I wanted to be back in the lab and back in the hospital. I didn't want this office. I had an office as big as this house. I had a budget of $600 million and so on to look after, and I had all these people who wanted this and wanted that. I just was miserable, and I lasted about a year. I said, "This is not for me."

I resigned, and I took an appointment at George Washington University, where all my old friends were down in gynecology. They made me professor of medicine and obstetrics and gynecology, and gave me a beautiful salary, more than I was earning, and so on.

I got down there, and I stayed with them for about a year and a half, and I got so homesick for NIH that I wanted to come back to NIH, and I got back. Dr. Shannon was very happy to have me back in the Child Health Institute, not the Cancer Institute, because Mort had taken that. When I left the directorship of the Child Health Institute, Mort started pushing for that, and he got it a little later. He became first a Scientific Director and then the Director. The Scientific Director took care of the intramural part, and the Director took care of the whole thing including extramural. Then he became Director, and he had a very, very distinguished administrative career. He and Griff, jointly, he and Griff remained like that [crosses fingers], no matter what. He and Griff then became, respectively, Director and Associate Director of the Clinical Center, and they invited me back, actually, at one point. I was down at G.W., and I didn't want to get back at that particular juncture.

About three years later, then the guy who had taken over in child health and my own personal laboratory--his name will come to me in a minute--Dr. Lynn Loriaux, invited me to come back. I was meanwhile serving at George Washington again as professor of pharmacology, actually. That's right, pharmacology department, with a joint appointment in obstetrics and gynecology, which I never was. I was never an obstetrician/gynecologist, never was. I'm an honorary member of all the obstetric and gynecological societies.

GC: I saw that.

RH: I've never been a gynecologist. I'm an honorary member. I'm a mascot. Well, whatever.

Anyway, Dr. Loriaux had me back. He got me appointed as scientist emeritus, which is a lifelong appointment. There's only three or four scientist emeritus on the campus, or anywhere. One was Julius Axelrod, who was a Nobel Prize winner, and two or three others. One of them was also a Nobel Prize winner, and he died, Dr. Chris Anfinsen. Anyway, so there's only about two or three of us around, and it's a lifelong appointment, and you have all the privileges of the place. You can have a laboratory and whatever, and so on, and I did until, I'd say, about maybe two years ago. The trip going in became more and more complicated with the traffic, and I was getting older, and I just begged off. I'd go in and see my colleagues there and my friends maybe about once a month, still do. So, that's the present association.

Griff left NIH after a period of duty as Associate Director of the Clinical Center, and he went to Houston, Texas, where he was invited to come down as dean of the school of medicine, because he was originally from Texas. His father and his brother were quite well known in Texas medicine and so on and somehow or other it worked out they wanted him to come down as dean, and he was by that time quite a well-known person, you know, and he went down there. Just after he got down there, he developed cancer of the prostate within about a year, and he died about a year later. He was down there for two years. He was vice dean, or associate dean, for basic science work, he was essentially dean for the clinical science, and he had a boy by the name of Knobil, Ernst Knobil. Ernie Knobil is still there. He took over as dean after Griff died from very rapidly advancing prostate cancer.

Griff's wife, when he was at Mayo Clinic, he had married the woman who was in charge of the babies' nursery at Mayo Clinic, and she was a lovely person. They have a memorial lecture every year for Griff, annual memorial lecture, which I helped set up. She would come the first several years after he died. She doesn't come anymore; she's pretty aged by now. I'd say she's probably touching eighty now. She had been a nurse. "Pinky" was what we called her. Everybody called her "Pinky." She had red hair and freckled face. She was such a nice woman. They had two children. I don't know what happened to them. But, anyway, lovely people. So, that was Griff's, or Mort and Griff's, that's part of the history.

Lipsett remained, and about a year after Griff died, he developed a malignant lymphoma for which there was no therapy. There have been a few moderately effective therapies developed since that time. I would say that was about 1990, probably about twelve years ago. Mort died. I saw him the day he died. He was jaundiced, in terrible condition. He died that day, and his widow, Mort had been very unhappy in his first marriage. His first wife was a biochemist who worked very closely with some of the very outstanding biochemists at NIH, and I don't know whatever happened to her. She drifted off. They had two boys, and the two boys stayed pretty much with Mort. She didn't seem to want to have much to do with them or something, I don't know. Anyway, they stayed with Mort, and then Mort passed away quite young.

Meanwhile, he had remarried, and his present widow still comes to the annual memorial lecture they have for Mort in the Lipsett Auditorium. They named the auditorium for him, because of all his contributions to the Clinical Center and so on. He was really a remarkable person. He was top in everything he tried to do. He would play squash. People played squash with him. He had to win every squash game. He was the Eastern Seaboard champion in four-partner bridge, and that's how he became acquainted with the gentleman who wrote the bridge column for the *Washington Post*. There was a column about bridge in the *Washington Post* in the "Style" section. This gentleman, Karten, and his wife then became a very good friend of the Lipsetts, with Mr. Karten, through the bridge thing, and he appointed Mrs. Karten as the executive officer for the Endocrine Society, which Mort had, in the meantime, become president of the Endocrine Society, of which I myself was at one time vice president of that society, had the meeting at NIH. We used to have meetings in various universities. Now they have meetings in hotels and big conference centers, thousands of people.

The growth of the so-called learned societies through the grants programs have been inconceivable. I mean thousands. Neurosciences have a meeting with 40,000 people all descending on New Orleans. My stepson, Toby's [Hertz' wife] son, is with the Eye Institute. He's a neuroscientist, a Ph.D. in neuroscience by training. Michael Oberdorfer. He goes to all those meetings, and there were 40,000 at the neuroscience meetings in New Orleans, the last meeting. We used to have a meeting of maybe a hundred, two, three hundred, maybe five hundred. Five hundred got to be a big meeting. This is all through the grants program, because there's so much money around for research that didn't exist before, not in those amounts. So that's part of the evolution of the NIH that you should know about, the tremendous impact of the expansion of the extramural program. That's right. I mean the tail began to wag the dog.

GC: Right. Grants and contracts were a big controversy.

RH: Contracts. Yes.

GC: How did you feel about contracts?

RH: Actually, I had a part in the development of the first contracts. Almost one of the first contracts was for the preparation of cortisone for the treatment of arthritis and the distribution of that through a committee we had at NIH that distributed to people who had come to apply for cortisone for their respective research projects. So that was probably the first one of the so-called reagents. Then we began to develop various kinds of reagents for different kinds of tests, and those were put out through contracts developed because of the mass production of them, and then put out and distributed through various granting agencies.

We set up what was called the National Pituitary Agency, which still exists. The National Pituitary Agency developed the first standardized preparations for the bioassays of the various pituitary hormones and then later for the development of the immunoassays and for the radioimmunoassays. It's been that progression of tests that evolved in the development of endocrinology during this time.

GC: Did you feel that the quality of science was still good with the contracts, as opposed to giving grants to individual scientists to do the work?

RH: Well, some yes, and some no. Sometimes they got pretty poor junk. We originally set up the Pituitary Agency to collect human pituitaries, because we found that any of the animal pituitary preparations were very ineffective in a human. We really needed to use human pituitary hormones, like growth hormone and FSH for the follicle stimulating and luteinizing hormone for infertility cases and so on. We felt that we had to develop human source of pituitary. We entered into an agreement with the Veterans Administration. This was one of the first I arranged. I arranged this in collaboration with a Dr. [Delbert M.] Bergenstal, whom I haven't mentioned. Have you heard his name?

GC: Yes.

RH: Dr. Bergenstal was one of my senior colleagues prior to the arrival of Lipsett and Ross. Dr. Bergenstal had been an associate professor of medicine at the University of Chicago, very widely recognized, very closely associated on the medical side with the prostate work of Dr. Huggins, and Huggins called me and asked me to help him get out of military service. He was married and had two boys, and they still wanted to draft him. Usually a married man with children was omitted, but he was a university professor, and they said they were going to get him, and they got him. So Huggins got in touch with me, and I brought him to NIH. I would say it was a good four years before Griff and Mort. That's right. Actually, I brought Mort originally to assist Bergenstal. His wife was a nurse, beautiful lady, and she nursed on one of the other services at NIH, not on my service, and was very highly regarded as one of the nurses. She was a nice, good woman, mother of two children. She still came and never missed a day nursing and so on. But, anyway, Del and I were very close, very close. He developed cirrhosis of the liver, and the way that happened was that he developed an infection right here.

GC: Under his eye?

RH: In the angle of the nose, it's a very common place for an infection. The sebaceous glands there get infected. That thing swelled up, his face swelled up like that. He had a fever and chills, and I admitted him to the hospital on the infectious disease service, and a Dr. Norman McCullough, who was then in charge of that work, took him over. He took the blood tests. The blood tests showed that his liver wasn't working, and that was probably the basis for this rapidly advancing infection which responded to penicillin treatment, intravenous penicillin. So we made a big liver workup, which at that time included biopsy of the liver, and it turned out to be cirrhosis of the liver. At that time, the prognosis for cirrhosis of the liver, with the tests that he had, he had tests which indicated advanced cirrhosis of the liver. He was a fairly obese man, moderately obese man, but his belly had swelled way out like that, and he was full of fluid, full of fluid. He continued to be my medical associate for about a year after the diagnosis was made. Mort lived next door to him. Mort, who came to NIH as his assistant, took over looking after Del like he was a brother, and brought him to work every day, took him home. He could only work a couple hours a day, he was so weak and so sick. Then he became terribly jaundiced, and he developed infections of the skin, which happens with cirrhosis.

One day I had to take Del Bergenstal aside and say, "Del, I don't think you can see the patients anymore, because you're just too sick." He got so angry, which I would have, too. He got so mad and so angry, and Mort and I just quietly developed a kind of a private conspiracy that we would baby him along, because we knew he was going to die. So we babied him along, and Mort just took care of him until the day he died. He died, I'd say, within about a month after I did that. Yes, that's right. That was heartbreaking.

My first wife meanwhile was terribly ill at the same time, and she died about six months after him. That was one hard year, '62, that's right.

GC: With everything you've been telling me, it sounds like the people at NCI and in the Clinical Center were very close. Would you say that was true?

RH: Oh, yes, yes, yes. The clinical people were very close. All the clinical colleagues were very close. We had these clinical rounds, you know, with discussions of cases. They were brilliant, they were wonderful, they were such nice exchanges. We had a postmortem conference on every patient. At that time the evaluation of a hospital depended on what percentage of the deaths in a hospital were autopsied. Nobody bothers about that anymore. Autopsies are not regarded as important anymore, because they have so many chemical tests in the laboratory and diagnoses and X-rays, all kinds of X-rays, you know, and so on. Other kinds of studies, electrocardiograms and so on, they feel the autopsy is not that important. I still think it's fundamental to the whole. At NIH, they still do a large proportion. It's part of the understanding with which a sick patient goes in, and always was. We talk with the relatives about that.

GC: When you are asking for permission.

RH: Yes. I've been over, four o'clock in the morning, to get an autopsy carried out under sterile conditions, and we could take the tissue and put it in tissue culture. That's what we did, we developed our own tissue cultures. See, Dr. Earle, who was one of the pioneers, you know, Wilton Earle, he was a patient of mine. Enormously obese person, with hyperphagia, couldn't stop eating.

Anyway, Wilton Earle and I were very close, and he helped me set up a little tissue culture lab in my own place, and he and I actually made the first hormone-producing tissue culture. We thought so little of it, we took the trophoblastic tumor, the less malignant form called the hydatid mole, and we took the hydatid mole and grew that in tissue culture and got the hormone off the top of the fluid. I tested the hormone in my lab and it showed that we were getting hormone produced.

The Director of the Cancer Institute at that time, Dr. Scheele and Dr. Dyer, he was Director of NIH, went to the news but I have newspaper clippings there, and it's shown in the *New York Times*, they went to the Congress, to the Budget Committee, and said, "We're going to produce hormones in bottles." That's the whole basis for the Genentech Corporation.

GC: Really.

RH: The whole basis for the Genentech Corporation is on the basis of we could have patented it, but you know what we did with it? The technician working in our lab, her name was Helen Walzl. I don't know if I gave you a reprint of that.

GC: I don't know. I'm not sure.

RH: I can give you one right here. I can show you Helen Walzl. Helen Walzl was working for her master's, a technician in the lab with Earle. Walzl, nice young girl, and she needed laboratory stuff for her master's thesis, and she said, "Can I use this?" I said, "Sure." So she did, and Earle said, "Sure." She took it, and then later we decided we were going to publish it, and we published that with Helen Walzl as the senior person, the lead person in the article. The article includes Earle, myself, and my laboratory associate, William Tullner, whose name you should also have, William W. Tullner. He was my laboratory associate for twenty-eight years, ran my laboratory, and he and I alternated senior person on the publications. He took senior one article, I took senior on the next article. We published probably thirty papers together.

GC: What was he like?

RH: Bill Tullner is still alive. He's eighty-two years old. He came to me, he had been a graduate student at Temple University in Philadelphia, where they studied protozoa, single-celled animals, and he did his master's thesis on the type of single cell that rotates, called a rotifera. You know rotifera?

GC: Yes.

RH: How do you know all this stuff?

GC: I was a biology major for a while.

RH: Oh, I didn't know that. I'm a Ph.D. in zoology, you know. I don't know if you know that.

GC: Yes, I do know that.

RH: My Ph.D. was in zoology, in zoology department. He put his master's thesis on the rotifera, some of which rotate this way [right], and some of them rotate that way [left]. They were working on the genetics of breeding. They divide by cell division, and they took the right ones and had them breed, and they took the left ones to see if this would breed true, and they were finding out that it did almost entirely breed true, that this was a genetically determined thing whether they went this way or went that way. Anyway, that's kind of the basic thing he was involved in. He came to me as a young man, had just gotten married, and he had just gotten out of the Navy. He was about four years in the Navy where he had gone after he did his master's thesis. He had just gotten out of the Navy as a fairly high rank in the Navy.

I hired him to be my right-hand man in the laboratory, which he turned out to be such a nice person, and he raised four kids during the time we were together in twenty-eight years. They lived in Bethesda. His wife died fairly young, about fifteen years ago, fairly young. She had ovarian cancer, and he's been widowed ever since. He was born and raised down on Cape May. Do you know Cape May?

GC: In New Jersey?

RH: New Jersey.

GC: Sure.

RH: He, as a boy, was an avid bird watcher, which he still is. He's one of the country's most accomplished ornithologists, which he did entirely separate from our work in the lab, and he helped the guy who wrote the book on ornithology. Do you know that guy that wrote the textbook on birds of the United States?

GC: Oh, sure. The book everyone has.

RH: The book everybody has.

GC: Right.

RH: That guy. He just died recently. As a boy, Bill Tullner took him out in a rowboat and showed him the shore birds of the Jersey Cape May shore, which he knows like the back of his hand. There's a huge bird sanctuary down there where he lives, and he goes out there about three times a week with his binoculars, and he studies those birds still to this day.

GC: Amazing.

RH: Oh, yes. He's a remarkable fellow. He's eighty-two years old. My wife and I have gone down there to visit him about four or five times. The last time was about a year ago. We had a wonderful visit. We stayed two or three days, and we go out for meals and had just a grand time. One of my closest personal friends.

My other very close personal friend is a Mrs. Olga Collier. She was my secretary for the same period of time. She came on just about when Bill came on. She and Bill are like family, and we're all three of us just like family.

GC: Does she still live in this area?

RH: She lives in Rockville [Maryland], right near, you know where that wooden bridge goes across? There's a wooden bridge in Rockville that leaves the main drag and goes out across some streets. She lives at the foot of that bridge to the far side, where she bought a house in the thirties for $3,400.

GC: Thirty-four hundred dollars?

RH: Thirty-four hundred dollars. Included a year's fuel, electric fuel. It was during the Depression they built those houses. They couldn't sell them, and they were just giving them away. They couldn't sell them. She's lived there ever since, and she won't move. Her husband was a bus driver. She was my secretary for twenty-eight years, never missed a day. During that time, they raised two children, which she now has about maybe ten, twelve great-grandchildren. She has great-grandchildren. She's a woman now in her middle seventies. She stands about that tall. She was originally from Honduras. She was born and raised in Tegulcigalpa, Honduras, and speaks Spanish. She's bilingual, totally bilingual, and whenever we had Spanish people as patients, we had many South Americans, sometimes flown up, in moribund condition, to our service, and she was their social worker, essentially, and their interpreter and everything. To this day, she still visits two of those families, one in Argentina, one in Buenos Aires, the Villar family. The Villars came a different way. I'll tell you about the Villars. But she still visits them. There's another family in Venezuela where it was a patient who died, and she visits that family after all these years.

GC: Amazing.

RH: That's right.

GC: So these were people who'd come us patients, came up to the service, and she acted as kind of a liaison.

RH: This one family was, but the other one, he came as a postgraduate student. He was a worldwide-renowned pathologist, and during the Argentina trouble with Perón, you know, the universities were closed. He [Perón] closed them. He didn't want people to know anything, and Villar was out of a job. So he packed up. He was not married at the time. He married his technician. His name is Oscar. Anyway, we've known these people. The papers are here, which we did together, on testes, rat testes. He was a world authority on testicular structure, and so on. Oscar and his wife come up at least once a year, and he's now almost totally blind with diabetes. He always was quite an obese man. They had their first baby while they were here on that fellowship. He was on a Guggenheim Fellowship, that's how he came here. Do you know the Guggenheim? He came on a Guggenheim. I had a lot of them.

GC: Were you very close to your patients? Did you get to know your patients very well?

RH: Too much.

GC: Too much?

RH: Yes, yes. Like Mr. Meredith, I mean, people like that, that I took care of them for years, how could you not be close to your patients?

GC: Was Mr. Meredith a special one?

RH: Oh. And to see a woman come in moribund and walk out.

GC: Do you remember all those first patients that you gave the treatment to for the chorio?

RH: Yes, yes. The first one was Longoria, the second one was Connelongo. Longoria, Connelongo. The other one was another Italian name. Right now, I can't speak it. Their initials, the way we used to give clinical reports, we'd put the patient's initial. We never gave a patient's name for publication.

GC: For privacy.

RH: But in that first paper, with Li, Li and the technician who was doing the bioassays, put him on the paper, too. That's how much we tried to get everybody acknowledged. Sometimes we put the nurses on the paper, and so on, give them acknowledgment in the footnote or something. You can read it all there.

GC: You've mentioned nurses in several of the papers.

RH: Yes. I'm keeping you too long.

GC: That's okay. I can wrap it up.

RH: Mort and I went to his funeral.

GC: To Bergenstal's funeral.

RH: He was buried in the area here someplace in a regular funeral service. We fed the Pumphrey Funeral Home. You know the Pumphrey Funeral Home in Bethesda?

GC: Yes.

RH: We fed them for years. In fact, I think they were about to go out of business until the Clinical Center opened, and we started feeding them. We fed the other one farther down on Wisconsin [Avenue]. There's another one.

GC: Do you think that doctors at the Clinical Center were closer to patients than at a different hospital?

RH: Yes, because we had them for much longer periods of time. A month's stay, two-month stay, three-month stay was not uncommon. I estimated one time the average cost of taking care of one of those. We ended up with a series of 105 chorio cases, and the average cost of each one of those cases was a minimum of $100,000 at that time. In current dollars, it would be seven, eight times as much. The National Cancer Institute never said aye, yes, or no. They have never refused me anything. NIH never refused me. Anything we needed, microscopes, equipment, incubators, anything that we ever wanted, just order it. I was running out of money, I got a budget for every year. I was running out of money. The fiscal year at that time ran from July 1 to July 1, and if I was running out of money in May or June and so on, in came some more money.

GC: Amazing. What about in terms of the research that you did? Were you ever told what to do?

RH: Never. That was one of the operating principles of NIH, and that's what you should emphasize: that was freedom, freedom to investigate. No subject was barred, even in the most outlandish things. I don't think I told you anything about the biotin work. Did I tell you anything about biotin?

GC: No.

RH: You have reprints about biotin?

GC: I think so.

RH: Well, I can tell you how that came about. The day after Pearl Harbor, which was December 8th, 1941, I reported to the Clinical Center, to the NIH, not to the Clinical Center; Clinical Center didn't exist.

GC: Right.

RH: Day after Pearl Harbor. First, any officer at that time who reported had to go and see the Director. The place was that small. There were 400 people on the campus.

RH: You went in, and the Director at that time was a gentleman by the name of L.R. Thompson, who was an infectious-disease man, fairly undistinguished, but had become the Director of NIH. Dr. Thompson sits me down. He says, "Welcome to NIH. Anything I can do for you? Have you found a nice house to live in?" and all this and that. "Have you got a family, any children? Oh, this is nice," everything and so on. "Anytime we can ever do anything for you, you come in here and tell me." Stands up, shakes hands, and out I go to report to Dr. Sebrell.

I go over to Building 4. There were only four buildings on the campus at that time. The Cancer Institute building is called Building 6, there's actually six, and there was the main building, and then at each corner of the main building was Building 1, 2, 3, 4. I go over to Building 4. The far end of the first corridor is Dr. Sebrell's office. He's got the whole laboratories of the whole first floor for nutrition work, animal things, sterilizers, all that kind of stuff. I walk in. He says, "Sit down." He says, "I hear you had a lot of experience in laboratory research." He says, "Have you ever heard of biotin?" I said, "No." He said, "After this, you're working on it." I said, "Yeah?" He says, "We have a project down in the same hospital where Goldberger did his job on pellagra, and me and a Dr. Strickland," who is a resident doctor, it's a mental hospital in a small town in Georgia, in remote, remote rural Georgia. I've never been there. Anyway, I had the name Breckenridge in mind. I think that might be the name of the town, Breckenridge. He says, "We have patients down there that we're putting on what we hope will prove to be a biotin-deficient diet," because they had already been able to demonstrate biotin deficiency in rats.

The way that had come about was, again, at the University of Wisconsin, women working in home economics, where they still taught home economics, and they had a course in cooking and nutrition in the home economics school, and a woman by the name of Parsons out there had fed nothing but egg yolks to one set of rats, and egg albumen to the other set of rats, see. The ones on the egg yolks did quite well, got along fine. The ones on the egg albumen, egg white, suffered what she called egg-white injury. They lost their hair, they crumpled up, they developed a diarrhea, their eyes became blurred, they developed red exudates around the whiskers and around the mouth, and they died. They hunched up and died.

She worked out that what was happening was that something in the egg white pulled out a substance that was needed in the diet and made a complex with it and caused it to be excreted in the stool. She postulated that the substance might be biotin, and she named the substance in the egg white, because of its avidity. It was avid. She named it avidin.

So he [Sebrell] says, "We've got this urine coming from these patients that are on this experimental diet of feeding them massive amounts of dried egg white and very little else, and they are developing a little diarrhea. You will get their stool specimens and their urine specimens, and you will test them for biotin by the microbiological assay, the L. casei test," for which biotin was very useful.

There was another test, a yeast test, a yeast called Saccharomyces cerevisiae, which also used biotin and was completely arrested from growth by the presence of a minute amount of avidin, and you could use that as an assay for the presence of avidin or for the presence of biotin, either one. If you developed a very sensitive strain, you could have one that was biotin-efficient by natural selection and have that responding. My first publication from NIH is "The Improvement of the Bioassay for Avidin in Saccharomyces Cerevisiae." I'll show you that.

GC: You said that you talked to the Director of NIH. Did you also talk to the Directors of NCI a lot? Did you have a lot of contact with Cancer Institute Directors later on?

RH: Yes, because I was a Branch Chief. When I went to the Cancer Institute, I became a branch chief. We were without a Director for a while. Our Director had retired, Roscoe [R.] Spencer. Spencer had been the previous Director of the [PHS] Rocky Mountain Laboratories where he had worked on tick fever. He had helped to develop what they thought was a possible vaccine, but it turned out not to be useful.

GC: What was Dr. Spencer like?

RH: "Spenny" was a fussy old lady. He was a rather thin man, quite elderly, little thin mustache, very precise. He was a fussy old lady, that's all we ever thought of "Spenny," we called him.

GC: Did you get along with him?

RH: He got along with everybody. "Spenny" was lovely, and he was helpful, and he had some crazy theories, microbiological theories, too, about some killer substances in certain foods that would kill bacteria. He was on the track of something, but he never quite got it pinned down. Anyway, he was a snob, too, I mean, he would not associate with anybody below the rank of fellow Director. He was the top man, and that was it, and so on. A commissioned officer was obliged, when he reported to any particular place, to have cards, such as the name and title Assistant Surgeon General, and go by your Director's house and leave a card on a Sunday in his vestibule.

GC: A calling card.

RH: A calling card, on a tray in his vestibule to pay respects to the fact that that was your Director. Now, he might be home. If he happened to be home, he might invite you in. Most often, he would not even know that you had come. You'd just leave it in the vestibule and go away. Well, "Spenny" was like that, and you had to do everything just according to Hoyle. We were in uniform during the war period. When I was with him we had to. We entered the war the next day, sure, and we were in uniform. When you saw an officer on the street or walked, you had to salute him.

GC: You had to salute him when you saw him?

RH: Oh, yes, sure. You salute the uniform. Funny thing, when the girls started showing up at the Navy hospital across the street, the WAVEs, you know, I first saw the WAVEs, and they had to salute us. The understanding was that the WAVE would salute first. A gentleman would not salute a WAVE first, because it was then also general etiquette among the civilian population that a woman always said hello to a man first. A man did not address a woman unless she was ready to speak to him. Yes, I don't know if you remember that.

GC: No.

RH: Of course you wouldn't remember. My wife, she'd remember. Also, at that time, you wouldn't use a swear word, what do you call a swear word, a cuss word. A male would not use a cuss word in the presence of a woman. Women used them privately between themselves on occasion, and I knew that, but a man would not use a swear word. When I met Toby, it was still forbidden at that time to use a swear word. It wasn't until, I'd say, way, way after the Second World War, it was during the "female uprising," the NOW [National Organization for Women] period.

GC: So they started using swear words?

RH: Yes, that's right. They started using swear words, and we started using swear words. The word "shit" you never used in the presence of a woman. Even "goddamn."

GC: Just wasn't done.

RH: It just wasn't done. It was discourteous. If you were wearing a hat, you tipped your hat when you passed a lady. I never wore a hat, except in the military.

Well, anyway, so, getting back to avidin and biotin. So these cooks out there had described this egg-white injury. This was called egg-white injury. They had pretty well proved it was due to a vitamin deficiency which could be cured by feeding the yellow of the egg along with it [the white]. If you fed them whole eggs, it was a beautiful diet, nice and nutritional diet, because you gave them enough biotin to overcome the action of the avidin.

The first thing that occurred to me when Sebrell says, "You're going to work on avidin, egg-white injury," I said, "That's from an egg." To him, an egg was a nutritional item. To me, an egg was a reproductive item. He didn't know that. He didn't understand that. So I said to him, "We've got to study what all this has got to do with reproduction."

Well, anyway, it ended up that I brought in some frogs in the spring, and the frog, you know, has a jelly around the egg, frog albumen, and I proved that there was avidin in there by the microbiological test, the yeast test. I proved that every species, it's in the literature, I proved that every species of bird that I had to pick from, I got pigeons' eggs. I went to the zoo, I became a very good friend of the zookeeper at that time. We became very good friends. I got all the birds in the zoo that we could collect, peacock eggs and it's listed in the literature there, and I published that I could show avidin in all of them. I could show very high levels of biotin in the yolk of all birds. So it had something to do with the whole reproductive process.

So the next thing I worked out was that I went over to Beltsville [Maryland], the [USDA] agricultural farm station, and I published a series of papers with a Dr. Frapps from over there. Dr. Frapps was one of the world authorities on egg-laying in adult hens. We demonstrated that the avidin is present in the oviduct, in the albumen secreting portion. The way a bird's egg is formed is the yolk is formed in the ovary above, and then that yolk is ejected, and when the bird is about to ovulate, so called, and that goes into the infundibulum of the oviduct, which is a long structure like an intestine in an adult animal, and it takes about forty-eight to seventy-two hours for that yolk to pass down through to where it reaches an area near the cloaca, it was called the shell gland. That shell gland secretes the shell, and in the shell there are dyes. It can be a blue shell or a red shell. Those dyes are called porphyrins, chemically, and the porphyrins are deposited and the calcium is mobilized in the bloodstream to produce the shell, all during this whole period when the animal is carrying a blood calcium level higher than any other animal, three times, four times the level that you could survive with. If you got that blood calcium, you'd be dead. Anyway, they do that. It doesn't affect their heart. It can stop your heart, and so on. They handle this very well, and that egg is finally passed out through the cloaca, which is a common opening between the gastrointestinal tract and the reproductive tract.

The oviduct in the adult animal is on the right-hand side. The left hand forms embryologically, but it involutes, and nobody knows what causes that, we have some leads now on what makes that go away, because we at one time thought that would be of importance for cancer, to make tissue go away, you know. I'll go through that a little further with you, if you've got time. Have you got time?

GC: Yes. We need to probably wrap up in the next few minutes, though, the next fifteen minutes.

RH: Whatever you say, and you can call me anytime you want to.

GC: Okay.

RH: Well, anyway, this oviduct then we found contained, by bioassay, by the yeast test, contained enormous amounts of avidin where the albumen is being secreted, and as the egg passes, the places where it has passed during its transit, above where the yolk is, the avidin has reduced substantially, so it has given that egg a certain amount of the avidin, and as it goes on further, the egg is finally deposited with much more avidin than there is biotin in the yolk. Then I determined that as you incubate the egg to develop, if you fertilize the hen with a rooster, the sperm stay in the mammalian reproductive tract about forty-eight hours. You know how long they stay in a hen after one contact with a rooster?

GC: No.

RH: Forty-two days. That rooster has really got power. Anyway, so if you get yourself some fertilized eggs, which we had delivered to the laboratory from a place up in Connecticut, we had what you call Rhode Island red chicks, and I must have used, over the years for the various studies on folic acid and all this other stuff, I probably have used a million chicks, baby chicks. We would get as many as three, four hundred every Monday; put ten in an experimental group for statistical analysis, for dose phenomenon, so on. What we ended up showing is that we could induce avidin formation in the baby chick, where the oviduct is just a tiny little thread, weighs about ten milligrams. I should have some. I have some in there in plastic, and weigh about ten milligrams, and you can make that tissue grow to about two grams in six days under estrogen, maximum dose of estrogen. But estrogen does not induce avidin formation. You know what it takes to induce avidin formation?

GC: No.

RH: Progesterone following the estrogen. I worked all that out. So we had a bioassay for avidin induction in a baby chick, and all those papers are in there.

GC: And this was your first project coming into NCI?

RH: Well, that's what I was assigned to. No, I was assigned to work on those urines from those patients. I got that out of the way in the daytime. I'd come back at night and do this other work.

GC: This is all at night, on your own time?

RH: Well, it was different odd times. Saturdays. At that time, we worked five and a half days a week. Until the end of the war, we worked five and a half days a week. During the war, we worked six days a week, on the war schedule. During the war, my laboratory with Dr. Sebrell was assigned to assay the rations at Walter Reed Hospital, which two lieutenants brought over by truck every day in garbage cans, aliquots from the table of the Army, and we would homogenize that and assay for all the vitamins to determine the nutritional value of the diet being given the Army. Then we got the assignment to assay the K-rations. You know what the K-rations were?

GC: That the troops carried.

RH: The emergency ration, yes, that the troops carry, and we got that job, too, and we did that. We learned what the vitamin content was, and made a recommendation that a certain amount of various B vitamins be added to the wafers in the K-ration. Dr. Sebrell got some kind of a decoration from the Department of War for contributing to the war effort, on my work.

GC: On your work.

RH: That's right. Well, anyway, he was very good to me. He was a nice man. But then an opening came in the Cancer Institute, and I decided to go over there and became Branch Chief. I was promoted. I was with Sebrell about three years.

GC: So when you came over as Branch Chief, was Dr. [John R.] Heller in place at that time? It was still Dr. Spencer?

RH: Spencer, and then after Spencer, we were without a Director for a period of time.

GC: Who ran the place while there was no Director?

RH: The Branch Chiefs. Five of us constituted an executive team, and it was the most productive time that Institute ever had.

GC: Is that right.

RH: That's right.

GC: Why is that?

RH: Because we weren't bugged by a Director. That's right. Well, anyway, then we got a Director who bugged the hell out of us, a man by the name of Harry Eagle. Do you know that name? You'd better know that name, because Harry Eagle had been the author of one of the immunological tests, a variation of the Wasserman test for syphilis, and it was known as the Eagle test. He was world-famous for the development of the syphilis test. So somehow or other, they decided he could serve, he was commissioned officer at [Johns] Hopkins, assigned to Hopkins for that work, and he was given the assignment to become Director of the Cancer Institute. He came in, and we had one hell of a time with him. He meddled in everybody's laboratory. He meddled and meddled. He didn't know what he was talking about. He had never done any work in cancer, and so on. He was totally unqualified. Well, anyway, I think he lasted about three years, and during that time, we had a hell of a time, and it got to be a joke. We called it, you know, the presenting feature of syphilis, which he had worked on, is the chancre. It's a big boil on the penis of the man; it's called a chancre. We changed the name of the Cancer Institute to the Chancre Institute. That's right. That's what we called it, the Chancre Institute. Well, anyway, we finally got rid of Eagle, but, during that time, he was a thief, too.

GC: He was a thief?

RH: He was a thief. He moseyed in on Earle's tissue culture, and he got some idea of supplementing some of which Earle and I were working on, to supplement his tissue culture with a mixture of the vitamins to make it more nutritious for the cells, and to see if that would enrich the thing. Eagle caught on to that, and he stole it. He stole it.

He decided he was going to run a tissue culture lab. He took over part of Earle's lab for about a year, and he started futzing around there, and he comes out with a mixture of fluid which he calls Eagle's fluid, Eagle's medium. We had previously decided to call ours Earle's medium, you know, which had already been recognized and which was growing cells, but not optimally. We were trying to get better growth and more complete growth, and this guy comes in and he steals it, just steals it right off and patents it.

RH: Harry Eagle, the most obnoxious, disagreeable, nasty person I've ever had to deal with in my life. That's right. He's dead, so I can talk. That's right. He lived a long life after that. From the Cancer Institute he went, I can't remember, but it was to somewhere that gave him a lot of money. I've forgotten what it was, some school or something, I've forgotten. I can't tell you where Eagle went. You might look up in *American Men of Science* for the later years of Harry Eagle. That was a very important period.

GC: Heller was next, right?

RH: Rod Heller. No, no. We got a guy who later became Surgeon General [Scheele]. His wife was a dentist, and she was a dental officer in the Dental Institute. Became Surgeon General, began with an S., Scheele.

GC: Oh, I'm in trouble. I should know this one.

RH: I've got newspaper articles with pictures of him in it. I'll get it.

GC: I've got it somewhere.

RH: Well, anyway, that's the guy who became the Director. Then, after him, we got Rod Heller, and Rod Heller was very intelligent, very understanding, knew what he was talking about, and what he didn't know, he would ask. That was what was important.

Well, we better wind up then, if you have to. I'll go and get that for you, and you can take it with you.

GC: Actually, I have one more question for you. I just wanted to know, when did you first hear about cancer, and how did you think about cancer when you first heard about it? I guess my bigger question is, how do you conceive of cancer?

RH: You can't possibly go through medical school and be trained in pathology and attend autopsies and do any amount, the most minimal amount, of clinical exposure without getting involved with cancer. Forty percent of the deaths at that time, well, I would say at that time people weren't living so long, so cancer was not like heart disease which was still the most frequent form of death. But enough people were living long enough that cancer of the breast was always widely prevalent during all my medical-school exposure, and cancer of the uterus, prostatic cancer.

When I trained as an intern, I treated a whole ward of cranky old men who had had prostatic surgery, and we had all these fluids running into them to keep them irrigated and so on and so forth. There'd be many in the state hospital where I trained, in a state university hospital, poor people from all over the state were sent in, and we would have as many as fifteen, twenty prostatic cancer patients on the ward at any one time, many of them dying, many of them just begun to be treated by attempts to resect this or to treat that metastasis, so on, until Huggins came up with this orchiectomy and estrogen. Nothing really significant could be done for those people. We all knew this. We all saw it every day. Breast cancer was very prevalent. The other thing that was very obvious was skin cancer. We'd see people come in with horrible cutaneous developments, melanomas and all kinds. That state hospital was one hell of a place for pathology, I'll tell you, the University Hospital for the state of Wisconsin, the whole state. There was no other referral center in the state, and we would get people of all extractions. Up in a certain part of the state, you know, a lot of Finns settled around the Lakes, and you know what we would get from them? Fish tapeworm, because they always ate raw fish. They would eat the worms, eat the fish with worms, eat the whole thing, and they would come in with a certain type of anemia associated with the fish tapeworm and so on, which later turned out to be a relative folic-acid deficiency, many, many years later. That's a macrocytic illness. We had many Norwegians, of course, many Swedes. We had Finns, Norwegians, Swedes, and we had a large number of Indians, Indians with tuberculosis. The TB sanitarium, every county had a tuberculosis sanitarium, and the ones throughout the whole state of Wisconsin, in each of the counties, half to two-thirds of the people being held there to wait until they died, were Indians. I tended as a resident, or as an intern, at a state hospital outside Milwaukee devoted to tuberculosis, which was about half Indians, from all over the state. They still got Indian troubles out there, as you know.

GC: That's right. Okay. I'm going to stop this tape now.

RH: Okay.

End of interview