DOE/EH-0474

# HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

## *Oral History of Radiologist Earl R. Miller, M.D.*

Conducted August 9 and 17, 1994

**United States Department of Energy**
**Office of Human Radiation Experiments**
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MASTER

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# FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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

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

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

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

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# DISCLAIMER

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

# ORAL HISTORY OF RADIOLOGIST EARL R. MILLER, M.D.

*Dr. Earl R. Miller was selected for the oral history project because of his research at the University of California (UC) Medical School at San Francisco, California. This oral history covers Dr. Miller's service involvement with the Manhattan Engineer District; total body irradiation; Dr. Ernest Lawrence's heavy-ion therapy; the division between UC Medical School and Donner Laboratory; and the receiving of isotopes made on Berkeley's 60-inch cyclotron. Dr. Miller also discusses his research on cleft palates and his infant cardio- and urinary track studies.*

*Two oral interviews were conducted with Dr. Miller at his residence in San Rafael, California. On August 9, 1994, Ms. Anna Berge of the Lawrence Berkeley Laboratory Archives and Records Office and Dr. Gregg Herken of the Advisory Committee on Human Radiation Experiments interviewed Dr. Miller. On August 17, 1994, Ms. Berge met with Dr. Miller for a follow-up interview.*

## Short Biography

Earl R. Miller was [redacted] He was married in 1931 and was widowed in 1978. He has two children. By 1931, he had received both his B.A. and M.A. in Physics from the University of Wisconsin (UW). He attended the UW Medical School from 1932 to 1936. After receiving his medical degree, Dr. Miller interned at Research Hospital in Kansas City, Missouri (1936–37), completed a residency in Radiology at Stanford University (1937–39), and was an instructor in Radiology at Yale University (1939–40).

In 1940, he joined the faculty of the Radiology Department at the University of California Medical School at San Francisco as an instructor and was board certified in Radiology. He served as chairman of the Department of Radiology from 1943 to 1945. In 1949, he was made a full professor in Radiology, a position he held until his retirement in 1974. From 1958 to 1974, he was the Director of the Radiological Research Laboratory in the Department of Radiology. From the mid-1950s until he retired, Dr. Miller spent his mornings in clinical practice and teaching, while his afternoons were spent in clinical and laboratory research. His research included the introduction and use of angiography; the use of radioiodine; the development and use of x-ray diagnostic techniques, sound movies, and ways to reduce patient exposure; and the study of error in radiologic interpretation.

During this same period, Dr. Miller held the following positions:

- 1943 to 1945—Director of Health Physics for the Berkeley Division of the Manhattan Engineer District,
- 1950—Fellow of the American College of Radiology (ACR),
- 1954 to 1957—Member of the Board of Chancellors of ACR and Chairman from 1957 to 1958,
- 1957 to 1965—Member of ACR's Commission on Education and Chairman from 1960 to 1965, and
- 1960s and 1970s—Served three terms on research panels on radiology for the National Institutes of Health.

Among his many honors, Dr. Miller received a Gold Medal award from the ACR in 1972, and the Regents of University of California renamed the Research Radiological Laboratory the Earl R. Miller Radiologic Imaging Laboratory in 1978. Dr. Miller was a pioneer in radiology and has published more than 135 papers.

## Part I (August 9, 1994)

## Wartime Work on Radiation Exposure

**BERGE:** This is an interview with Earl Miller by Anna Berge and Gregg Herken of the Lawrence Berkeley Laboratory Archives and Records Office and of the Smithsonian Air and Space Museum,[1] [respectively,] on August 9, 1994, at Earl Miller's residence.

**HERKEN:** You were just going to tell us about the relationship between the Division of Medical Physics and UCSF[2] and how in fact, you were that contact.

**MILLER:** In 1942, early on, Dr. Stone,[3] who was the head of the Radiology Department at UCSF, was recruited, I think by Dr. [Arthur] Compton,[4] to head up the radiation safety of what became the Manhattan Project.[5] At that time it was called the Metallurgical Lab.[6] At any rate, that left the [UCSF Radiology] Department without a head, and I got called for that. That's when I learned I would never never ever [want to] be a chairman again.

One day, my good friend Staff[ord] Warren appeared. He was a professor of Radiology at [the University of] Rochester in [Rochester,] New York. He came then to UCLA[7] and became the dean there. He appeared on the scene one day and recruited me to head up the Radiation Safety Division of the Manhattan Project over in Berkeley.

**HERKEN:** This would have been what date?

**MILLER:** I think it was in early '43.

**HERKEN:** You worked in radiation safety?

**MILLER:** Yes, two half-days a week, I directed it. I would leave UCSF at noon, and I would go over to Berkeley. Usually the half-day finished at 10 or 11 or midnight. The work consisted primarily of getting blood counts[8] on people, checking out their radiation dosimeters,[9] and making the rounds of the

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1 Dr. Gregg Herken, Ph.D., actually is a Senior Policy and Research Analyst for the presidential Advisory Committee on Human Radiation Experiments.

2 University of California, San Francisco

3 Dr. Robert Stone was an early researcher at the Lawrence Radiation Laboratory and the UCSF and became a major figure in radiobiology research. During the Manhattan Project he served at the University of Chicago as Associate Director for Health under Arthur Compton.

4 Dr. Arthur Compton, University of Chicago, a key member of the scientific team that established the Manhattan Project

5 the U.S. Government's top-secret project to develop an atomic bomb during World War II

6 created at the University of Chicago early in the effort to develop the atomic bomb. "Met Lab" researchers, led by Dr. Enrico Fermi, produced the first sustained nuclear chain reaction on December 2, 1942.

7 University of California, Los Angeles

8 the count of the number of red and white blood cells and platelets in a specific volume of blood

9 Dosimeters, or film badges, were worn routinely to measure accumulated personal exposure to radiation.

various parts of the Berkeley Manhattan District. That got me into seeing the work of the physicists at the cyclotrons[10] and the chemists.

**HERKEN:** You knew Stone beforehand?

**MILLER:** He gave me a job as an instructor at UC in the Department of Radiology in 1940. Now we're going to talk about Stone?

**HERKEN:** Yes.

**MILLER:** Okay. He was an excellent radiologist.[11] His main work and interest, when I got there, was in radiation therapy, and he was a pioneer in that. When I got there, there was a need for some young buck who had just finished his residency as an instructor in Diagnosis. That's what I did. What else [would you like to know] about Stone?

**HERKEN:** You said he was engaged in teletherapy.[12] This would be with total body irradiation [(TBI)] with x rays?

**MILLER:** Not total body irradiation. He treated patients with various kinds of malignancies with modalities that varied from 100 kV[13] up to one of the very early million-volt machines.

**HERKEN:** This was the "Sloan machine"?

**MILLER:** Yes. He built the tanks and whatever.

**HERKEN:** Stone indicated in the official history that he wrote for the MED[14] project that he was doing TBIs in the period of 1940.

**MILLER:** What's that?

**HERKEN:** Total body irradiation, TBIs, from 1942 to 1946 under contract of the Manhattan Project.

**MILLER:** He wrote that?

**HERKEN:** Yes. It was the case, actually, that there were three hospitals involved. There was San Francisco, New York, and Rochester.

**MILLER:** The last was Staff Warren.

**HERKEN:** That's right. It was a MED project, a Manhattan Project. They were interested in the effects of radiation upon the workers, and this was a way of finding out how it affected people. They were getting terminal patients in this case. I think there were a total of 32 or 36 in this case.

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[10] accelerators in which particles move in spiral paths in a constant magnetic field. The resulting beam of high-speed particles can disintegrate atomic nuclei and produce radioactive isotopes.

[11] a physician who diagnoses disease, broken bones, and other physical conditions using x rays or other imaging techniques

[12] radiation treatment in which the radiation source is located outside the body

[13] "kilovolts"—thousand volts

[14] Manhattan Engineer District, the Federal Agency set up to develop the atomic bomb under the top secret Manhattan Project

**MILLER:** Oh, yes. Okay. I presume that this total body irradiation was for a real purpose.

**HERKEN:** It wasn't therapeutic?

**MILLER:** Yes, I suppose it was aimed at being therapeutic. That's important. The idea was to see if there could be an effect, let's say, on the blood system or the lymphatic system[15] that, influenced by the total body radiation, would do something helpful for the treatment of the cancer.

**HERKEN:** Were you aware, and were the people aware, of that part of the project? That would have been a secret project, it would have been classified at that time [by] the MED, the Manhattan Project.

**MILLER:** They classified everything. The important thing at that time in that war was a race for a bomb. Even the fact that radiologists were recruited to work was secret, because somebody over in Nazi Germany would have said, "Radiologists, they're all doing that? They must be doing something which deals with radiation! A radiation bomb!" It was hidden, yes, only from the bad boys over in Germany and from people who would talk out of turn.[16]

**HERKEN:** But pretty much, doctors at UCSF knew about what Stone was doing for the Manhattan Project?

**MILLER:** No, nobody knew what either Dr. Stone or I was doing. I want to go back to that. The secrecy dealt with not giving information to the people we were at war with.

## Remembrances of Joseph Hamilton

**HERKEN:** Hamilton[17] is another figure that we're interested in. Can you tell us a bit about how you met him, when you met him? I assume it's about this time.

**MILLER:** He was running the 60-inch cyclotron in the Crocker Lab.[18] He was doing really fantastically important work checking up with animals on the effect on every organ of every radioactive isotope. As I say, he did a great great job. Ken Scott was involved in some of the chemistry parts of it. Ken was quite an inventor, too.

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15 the system of glands, tissues, and passages involved in generating lymphocytes and circulating them through the body in the medium of lymph; it includes the lymph vessels, lymph nodes, thymus, and spleen.

16 This secrecy was maintained on an unprecedented scale, as described in "The Bomb Goes Public" (p. 54) in F.G. Gosling, *The Manhattan Project: Making the Atomic Bomb* (DOE/HR-0096), September 1994: "The release of the Smyth Report on August 12 [1945], which contained general technical information calculated to satisfy public curiosity without disclosing any atomic secrets, brought the Manhattan Project into fuller view. Americans were astounded to learn of the existence of a far-flung, government-run, top secret operation with a physical plant, payroll, and labor force comparable in size to the American automobile industry. Approximately 130,000 people were employed by the project at its peak, amongst them many of the nation's leading scientists and engineers."

17 Dr. Joseph Hamilton, an M.D., conducted important radioisotope research at University of California, Berkeley.

18 a part of the Lawrence Radiation Laboratory at the University of California, Berkeley

Joe had no fear of radiation. He used to lean up against the D's. You know what a D is in the cyclotron? They were very radioactive. I was always going around with a Geiger counter in my hands in those days. I tried to talk to him about the danger. He ultimately died of radiation.[19] This was massive doses of radiation that he exposed himself to.

**HERKEN:** I've never quite understood that. Hamilton certainly knew the danger. He knew the physics as well as the medicine behind it.

**MILLER:** When you talk about the danger, the so-called danger of some radiation has been so overemphasized and so wrongly overemphasized, that this has done a fantastically bad job on making people understand what radiation does to people. You get to the point where a person might not ever have a chest film because of this tremendous [perceived] danger, that they thought was going to kill them, and of course they die of tuberculosis. Even intelligent people are very very concerned about these minute amounts of radiation.

*(speaking facetiously)* The reason I died young—I'm 86 years old—is because I have a stack of [x-ray] film that's probably a foot and a half high. I made my living during medical school running a radon plant, where you get that bad stuff. That's why I died young. Of course I smoked a lot and drank a lot. The radiation protected me. [(This is meant as sarcasm.)]

**HERKEN:** But Hamilton's case—we're not talking about minor doses.

**MILLER:** This was massive, and they were total body radiation.

**HERKEN:** Why the disregard for the danger?

**MILLER:** I don't know; he thought he was above it.

**HERKEN:** I have the impression that his concern was in pure science, hence he was not really concerned with his own well-being.

## Neutron Therapy Research

**MILLER:** Exactly.

Another thing interested me about it. The people who were really injured by radiation, all worked around neutrons.[20] I don't know whether the gamma rays from the D's were really an important aspect. Whether he was just another one of the neutron guys: Stone, [Bert] Low-Beer,[21] Hamilton.

**BERGE:** Could you tell us a little bit about Low-Beer?

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19 Hamilton died of leukemia. His peers generally agree that Hamilton's illness was brought on by his cavalier disregard of the dangers of radiation.

20 an elementary particle found in the nucleus of most atoms and having no electrical charge

21 Dr. Low-Beer, M.D., a refugee from Czechoslovakia

**MILLER:** Great guy. He was the head of the [Radiation] Therapy Division [of the Department of Radiology] at UCSF when Stone was away. He was a good radiologist. His research work was primarily surface radiation with phosphorus for skin lesions. He got involved with neutrons later, working with Dr. Stone.

**HERKEN:** Was he involved in the early neutron teletherapy with Stone?

**MILLER:** I don't think he was involved with that, early on.

Stone did magnificent work. When Stone took on a project, his records were accepted as the best in the world. When he took on this neutron stuff and his million-volt radiation, all of these patients were followed up faithfully. The records were great.

**BERGE:** I've got a question about the neutron therapy.[22] In the late 1930s John Lawrence was beginning to be discouraged with neutron therapy, and I noticed that there was a revival of it in the later '40s and '50s. What brought about that revival?

**MILLER:** In 1930?

**BERGE:** In the late 1930s, John Lawrence was discouraged with the therapeutic effects of neutrons.

**MILLER:** There were a lot of problems with that. John Lawrence was a physician; he wasn't a radiologist. There were a lot of—I won't say, unpleasantness about it—but a lot of unwillingness to accept the other person's point of view between Stone and John Lawrence. But I didn't know that Lawrence ever got involved. I don't know about the neutron stuff. When Stone took it on, as I remember, there were several people in various parts of the country that were trying to find out what the score was on neutron therapy. I don't know any more than that.

## Relations Between UC Berkeley and UC San Francisco

**HERKEN:** There was apparently tension between the Division of Medical Physics in Berkeley and physicians at UCSF. Can you talk about that a bit?

**MILLER:** They wanted to start a medical school in Berkeley. I think mainly that was it.

**HERKEN:** I know that Raymond Birge was the Physics chairman at Berkeley.

**MILLER:** Who?

**HERKEN:** Raymond Birge. He was the head of the Physics Department at Berkeley, and he wrote a history of the Physics Department. He talks in his history of the creation of the Division of Medical Physics back as early as 1944. This is when John and Ernest Lawrence went to Sproul[23] and

[22] therapy of cancer using an accelerator to produce a neutron beam of radiation

[23] Robert Gordon Sproul, president of the University of California

proposed the creation of it. Apparently there was some cooing and crowing between physicians at UCSF and John Lawrence in particular.

**MILLER:** I was never involved in that; I can't help you.

**HERKEN:** The upshot of it was that if there were any human subjects to be involved in research, that work would be done at UCSF and not at Berkeley, and yet there was a therapeutic unit, as I understand it, at Donner early on.

**MILLER:** But that was run by Stone. At least, the only people that I know that did anything with neutron therapy with patients was Stone, and later Low-Beer.

## Working for the Manhattan Project and UC Medical Center

**HERKEN:** Now, you had a joint appointment with the Division of Medical Physics and with UCSF, or not?

**MILLER:** I think my appointment was with the Manhattan [Engineer] District through Staff Warren.

**HERKEN:** Right. But you were on the medical staff of UC Medical Center.

**MILLER:** Exactly.

**HERKEN:** And no part of your salary came from the Division of Medical Physics.

**MILLER:** No, I never got paid for the work.

**HERKEN:** Hamilton and Stone had joint appointments,[24] as I understand it?

**MILLER:** I don't know.

**BERGE:** In 1946, it looks like you were head of the Health Physics Division for the Radiation Laboratory.

**MILLER:** I thought it was way before that. That's what Staff Warren got me into.

**BERGE:** But the Manhattan District went on until about 1947 or the end of 1946.

**MILLER:** At the end of the war, that was it for me.

**BERGE:** What did you do when you were the head of that?

**MILLER:** I just told you. Remember? The blood counts and all that? That was the thing I did, going around trying to find out if anybody was unduly overexposed to radiation.

**BERGE:** You mentioned that you didn't ever want to be chairman again after that.

**MILLER:** That's "the chairman of an x-ray department."

**BERGE:** You didn't like it?

**MILLER:** No. I wanted to do my own research.

**BERGE:** What was your own research contribution?

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[24] were receiving salaries from both the MED and the UC Medical Center

**MILLER:** You can read about it in my bibliography.

## Process for Obtaining Radioactive Isotopes

**HERKEN:** I have a question about how the isotopes were obtained and how they were used in research that Hamilton was doing. Since he was running the 60-inch [cyclotron], did he introduce his own [method for producing isotopes such as iodine-131]?

**MILLER:** There that's where he got it. That's how Ken Scott got me the radioiodine to work on thyroid disease.

**HERKEN:** How would the mechanism of that work? Would you just call over or see Ken Scott and say, "Make up a batch"?

**MILLER:** I think he finally was bringing a supply over once a week.

**HERKEN:** And Scott would get on the ferry over in Berkeley and bring it over to San Francisco and get it to you at the lab.

**MILLER:** I do not know how he got from Berkeley to San Francisco. The radioiodine was brought to my office, which I [had] turned into a lab. We studied the radioiodine for a long time.

**HERKEN:** Starting when?

**MILLER:** When the radioiodine became available. I think that was after the war; I could look up papers. ([The] first paper on radioiodine that I published was in 1948.)

**HERKEN:** I know that there was an approval process for the isotopes after the war. In order to use isotopes, you had to get permission of the Division of Isotope Distribution out at Oak Ridge.[25] Was there anything like that during the war?

## Human Applications Committee and Informed Consent

**MILLER:** I don't know. It seems to me that later on at UCSF there was a committee set up, probably by Stone, dealing with this matter. When I say "set up by Stone," I think it was set up by a lot of other people with this fantastic fear of radiation. Sometime there was one set up at UCSF.

**HERKEN:** Yes, I think this was something called the Human Applications Committee.

**MILLER:** Yes. You know more about it than I do.

*(Dr. Miller inserted the two-page textbox that follows during the editing process.)*

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[25] Originally headquartered in Washington, DC, the MED moved to the new weapons complex in Oak Ridge, Tennessee, in the summer of 1943.

April 11, 1995

ABOUT CONSENT FORMS

Consent forms for radiologic procedures did not even exist when I entered my residency or when I came to UCSF in 1940. Patients were referred to Dr. Newell and to Dr Stone for Radiation therapy and the doctors administered it. People trusted their doctors then and they were not litigation happy. There were fewer lawyers and all the world was a great place.

Even today, I think it is not customary for a patient to sign a consent form for receiving digitalis or other potentially lethal drugs. It is interesting that Radiology as a profession was singled out early to need consent for accepting treatment with the usual modalities and for the radioisotopes.

There was a lot of vigorous discussion about the need for consent forms to be signed by people receiving the new- found radioisotopes during WW II. I think it was the AEC that stirred up the monkeys. I think it was Dr. Shields Warren who opposed the idea vigorously. I agree with him provided that the material was to be administered by someone especially trained to understand and to use properly the material in question.

Let us look at the problem of "Informed Consent" A patient, a layman, enters a Radiologist's office for diagnosis and or treatment with one of he usual modalities or with some radioisotope. Now the doctor explains as well as he can the good and possible side effects that the procedure entails. In order for that person to give a TRULY INFORMED CONSENT, the patient needs only to go to College, to Medical School and to serve an internship and a Residency in Radiology to sign that INFORMED CONSENT. If you do not believe that, try having a patient explain to you his understanding of all the nuances that were involved in his signing that consent form.

Then ask him some questions about something just a little peripheral to the main question, but is still important in the total understanding of the problem, and if you still believe that there is informed consent you are welcome to your beliefs. Consider the problems of a patient signing a consent form for major surgery.

After the War, Dr. Stone returned and took over the chairmanship of the Radiology Department at UCSF. What a load off my shoulders! I had learned that I would never again be chairman of a Radiology Department. At some time about then, Ken Scott came to UC, I think, to work with Dr. Stone in his new Radiologic Laboratory. I believe that by this time Joe Hamilton had died and the Donner Lab sort of folded up. That left Ken Scott with no job.

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Ken Scott's name comes into this because he was involved as chairman of a committee at UCSF, which committee was concerned with consent forms from patients and from doctors who planned to do studies with radioisotopes.

Up to this time, radioisotopes were used for diagnosis and treatment of patients by radiologists who knew what they were doing and had spent a great deal of time knowing about the safety and the dangers of the materials,and of the radiation involved. After the War, a number of doctors, not radiologists, got interested in the use of radioisotopes for their research and, in some cases for the treatment of their patients. Most of these had a 6 months course in diagnostic Radiology during their days in Medical School. This hardly trained them for the use of the new materials. As a result, Scott's committee was set up to examine the protocols of intended research and to determine if the person responsible had sufficient training and knowledge to carry out the work safely. If the committee was satisfied, their gave their blessing and the papers were sent to President Sproul's Office for permission to proceed.

I hope this is a proper time to air one of my beefs about the scare-mongers and the misguided "in-depth" reporters who present in the media a distorted view of the dangers of radiation and cause needless anguish to those who must be exposed to it. The most common distortion involves the failure to differentiate between radiation properly given to a confined area for a purpose important to the patient's health and total body radiation from cosmic rays or from some industrial source. A layman can hardly be expected to detect the slant and they become afraid. What a bummer!!

A classical example of slanted reporting came from the woman in Albuquerque who published a three part series of many pages in the Albuquerque Paper a few years ago. I have happily forgotten her name. If I remember correctly she was given a big prize (Pulitzer?) for the work.These articles were slanted through errors of omission and of commission to present a frightening view of the dangers of radiation. To present an example of the damage that this kind of reporting does let me tell you the story of a grandmother who was coughing her head off and needed a radiologic chest examination. She refused to have the procedure done because the danger to her grandchildren. This is true story that happened to me. Even intelligent people have that kind of fear. It is the fear of the unknown and it is fired up by some, both in and out of the Medical profession, who get on the scare band wagon for the fame that it brings them . Things that scare people always sell.

**HERKEN:** I've not been able to find out very much about it. Ken Scott talks about it in his oral history. He said that at one point he was even the chair of it. He said the way it worked was that some of the physicians would bring names of potential subjects to them, and they would recommend a course of action. I think this ultimately went to the university president. President Sproul was involved in officially approving it before it would come back and experiments would begin. I was going to ask you if you could remember if that sounds true to you.

**MILLER:** I don't know. I think by the time that got started I was way off on something else. I got off on x-ray movies and physiologic[26] studies.

**HERKEN:** As far as the approval process was concerned, that was something that was handled in Berkeley. You would call up Ken Scott and say, "Send me iodine-131 [($^{131}I$)] for use." And the approval process would be handled by the committee.

## Work With Soley to Diagnose and Treat Thyroid Disease With Iodine-131

**MILLER:** No: let's start again. Ken called me up one day and said, "We've got some radioiodine." Mayo Soley and Joe Hamilton had done some real pioneering work on this on the thyroid. He said, "There's a lot of this radioiodine around; do you want some, do you want to try it?" I thought about it for a long time, like 30 seconds, and I said, "Yes. That sounds like a real interesting thing." So radioiodine appeared, and I had to learn about how it was measured, how it was detected, what effects could be expected, etc. Dr. Mayo Soley had worked with Joe Hamilton on the fate of radioiodine in the bodies of animals. Soley was an expert in thyroid disease. He furnished the patients after thorough study and I gave the radioiodine. That's how it developed.

**HERKEN:** Soley was your colleague?

**MILLER:** Yes.

**HERKEN:** He was not a radiologist?

**MILLER:** No, he was an internist. He later became dean at [State University of] Iowa. Became the dean of the medical school at Iowa.

**HERKEN:** Is he still alive?

**MILLER:** No, he shot himself.[27] That's why you should never become a dean.

**HERKEN:** For reasons of ill health, I hope.

**MILLER:** Nobody ever knows.

**HERKEN:** Did he die young?

**MILLER:** Yes.

**HERKEN:** Soley would find the patients who might be good.

**MILLER:** *His* patients. He was an internist with particular interest in thyroid disease. That's how this got together.

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[26] relating to the functions and activities of living organisms and their parts

[27] Dr. Mayo Soley, M.D., took his own life, June 21, 1949, at age 42. He had been Dean of the Medical College of the State University of Iowa since July 1, 1948. The coroner stated that there was no apparent reason for the act. Source: New York *Times*; June 22, 1949; p.16.

**HERKEN:** Then he would contact you as the radiologist.

**MILLER:** Yes. We would work together on this.

**HERKEN:** He would actually administer the iodine, wouldn't he?

**MILLER:** No.

**HERKEN:** *You* would; that was putting the radiologist to work.

**MILLER:** He furnished the patients, he did the history and physical examination of the patient to decide whether the patient did or didn't have thyroid disease. Many of his patients had hyperthyroidism,[28] and some had cancers.

**HERKEN:** This was before he died.

**BERGE:** Did you ever do any of the diagnoses yourself?

**MILLER:** I became good at it, with practice, but the clinical responsibility was Soley's. They were his patients, and he [had] studied and had done work on thyroid disease forever and he knew a lot about it. So it was his. Later on, as time went on, I became pretty good at it, too.

**HERKEN:** Was this only use of radioiodine, not other nuclear materials, phosphorus?

**MILLER:** No, only $^{131}I$.

**BERGE:** I just want to do a little more with the radioiodine studies that you did. When you were working with Low-Beer, he was doing the hematological[29] stuff, and you were doing the radioiodine studies.

**MILLER:** The work of Low-Beer and me was totally separate.

**BERGE:** How did that work? Was he using the blood from patients that you were studying?

**MILLER:** No. He had his own project. I do know that I was much aware of his work on phosphorus, but that was his.

**HERKEN:** Just to confirm something: The iodine that you used had been produced on the 60-inch [cyclotron]?

**MILLER:** Yes.

**HERKEN:** It wasn't from Oak Ridge? It was cyclotron-produced?

**MILLER:** Yes, right. I think that was all we had at that time.

**HERKEN:** I didn't get the date straight on this, but you say this would have been after the war[, in] '46 or '47?

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[28] overactivity of the thyroid gland, resulting in basal metabolic rate and other physiological problems

[29] relating to the study of the nature, function, and diseases of the blood and of blood-forming organs

**MILLER:** That's my remembrance. I could look up my first papers.[30]

**HERKEN:** There are not a lot of documents on this whole period. One of the documents we found here in this case [is] from 1951. But, this is an AEC[31] project.

**MILLER:** "Question on the Thyroid Disease and Radiology," 1951. I was going to say I thought 1950ish was about right. That's the only one you have for me?

**HERKEN:** No, that's the only one I brought with me.

**MILLER:** If it's important I'll get my bibliography out.

**HERKEN:** Is that the first paper with Soley in 1948?

**MILLER:** Yes, January 1948. Low-Beer and I apparently wrote a paper together, "The Biological Effects of Radiation from External and Internal Sources." Oh—that was a UC Radiation Laboratory [(UCRL)] report. (He wrote the section on his work and every UCRL report contained a progress report on $^{131}$I by me.)

**HERKEN:** What's the number on that report?

*(phone rings; tape interrupted)*

**BERGE:** Another question about the radioiodine project: Were you doing this independently?

**MILLER:** Independently.

**BERGE:** So, this was not supported by the AEC?

**MILLER:** The iodine [project] apparently was supported—getting iodine etc., was supported—by AEC. For instance, I wasn't paid to do anything. This was my own research. Does that answer your question?

**BERGE:** So, it's very much like applying for a grant and then doing whatever you want to do?

**MILLER:** There was no grant involved here: We were treating patients.

**BERGE:** Were you working with anyone else on this or was this, again, independent?

**MILLER:** I told you this earlier with Soley. Soley and I worked together for years on this project. When Soley took the deanship at Iowa, Morrie [(Maurice)] Dailey took over Soley's practice and the project, and we worked together.

**HERKEN:** When Soley would find a patient who seemed suitable—

**MILLER:** —They came to him as their primary physician.

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[30] Between 1948 and 1958, Miller authored or coauthored 17 published papers on thyroid disease and its diagnosis and treatment with $^{131}$I.

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

**HERKEN:** But, he would notify you if he found a subject who seemed to be suitable for radioiodine?

**MILLER:** Exactly.

**HERKEN:** Was there a consent procedure here, since you were involved in administering radioiodine?

**MILLER:** Wait a minute—we developed a consent thing. I don't know when; not at the beginning. For instance, a patient who comes to a physician for some heart disease doesn't sign a consent form for digitalis. In this case, this was exactly the same thing. This was a kind of treatment that prevented a lot of people from having operations. *(See Dr. Miller's preceding textbox,* "About Consent Forms," *under "Human Applications Committee and Informed Consent.")*

**HERKEN:** But, this was not tracer amounts. They were in fact—

**MILLER:** —They were tracer amounts for diagnosis of thyroid function and larger amounts for the treatment of hyperthyroidism. In the case of carcinoma[32] patients, we made very clear to patients that they were going to get a big slug. That meant 100 millicuries per dose.

**BERGE:** What were the criteria for having patients undergo radiological treatment? Was it anyone who had hyperthyroidism or cancer of the thyroid, or was it people who hadn't responded to other therapies?

**MILLER:** This was the therapy of choice for hyperthyroidism.[33] In our mind it was the therapy of choice to begin with. Certainly Soley and Hamilton felt that way. What we were doing was to prove this was the case. Not only that, but the job was to see how little we could give and still have the patient get well. We were calibrating the dose and the severity of the disease, and I wrote about this. It depended on who saw the patient.

**BERGE:** How do you mean?

**MILLER:** There is no specific laboratory test that would divide those who were hyperthyroid or hypothyroid[34] or euthyroid.[35] Somebody had to say that the patient had that. This was a part of my interest in the philosophy of medicine, if you will. It turned out that, if for instance, a patient was jittery and kind of stressed, and he went to a physician; this physician said, "You've got hyperthyroidism, you have to have your throat cut." You didn't like that, so you went to another physician, who said, "Well, I'm not sure you have hyperthyroidism." Now in the one case you had it and in the next case you didn't. But, you[r thyroid condition] didn't change. I was very interested in that, because it turned out that we got

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

33 Radiation therapy using iodine-131 is still the treatment of choice for many thyroid disorders, including hyperthyroidism, Grave's disease, and thyroid cancer.

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

35 having a normally functioning thyroid gland

pretty good at separating those with and those without. There were a lot of things to suggest it. But there was no single, objective, critical way to tell the difference.

**BERGE:** What about using iodine as a diagnostic tool?

**MILLER:** Yes, I developed that. That was a corroborative thing. It didn't prove it; it was just another strand of the rope.

## Patient Consent; Contradicting Perceptions

**HERKEN:** Coming back to the consent procedure: Can you tell me about how that worked? *(See Dr. Miller's preceding textbox,* "About Consent Forms," *under "Human Applications Committee and Informed Consent.")*

*(phone rings; tape interrupted)*

**MILLER:** At some time, some forms were developed. I can't remember anything really more than that. I don't remember when it happened. I don't remember what was in the form. There *was* a form. As I remember, that was way after the war.

This, I think, came about because people other than radiologists were using radioisotopes for studying physiology, possibly treating patients; I don't remember that. But the people who got involved in this late had not had any training in this, or very little, and they needed to be monitored by people who knew what it all was about. This, I remember, was how it came to be.

**HERKEN:** I think it was by spring of 1947 [that] the AEC had a regulation that there be consent confirmed in writing by two attending physicians as to the understanding and consent of the patient.

**MILLER:** It could well be. That sounds reasonable.

**HERKEN:** The one thing that's curious is that there is some evidence that the AEC, itself, initially proposed that there be written consent by the subject but that the physician or the intramedical advisory committee, that Stafford Warren headed, actually balked at it and urged that instead of there being written consent upon the patient, there be consent by the two attending physicians. I've never quite understood why that would have been important.

**MILLER:** I don't know. This is my interpretation now. You're a patient and a layman. You don't know from nothing about radiation. You're going to sign this consent form saying it's okay to take this radiation, or it is not okay to take this radiation, without knowing a single thing about it. This didn't make sense to Staff Warren, and it doesn't to me.

**HERKEN:** Do you remember any conversation you might have had to that effect?

**MILLER:** No. This was my own feeling about this. I still feel that way. They say that the doc should explain everything. You take a person with no training and no matter how it's explained, he still doesn't know. These are the things you have to do in order to make a decision. The patient would have to go through medical school and internship and residency [to have enough information to completely understand the risks and benefits of a proposed therapy as well as the prescribing physician] and then they would be able to sign it with truly informed consent.

## Wartime Plutonium Injections

**HERKEN:** Getting back to the case of the plutonium injections: Is that something that Stafford Warren would have authorized from his position?

**MILLER:** I have no idea.

**HERKEN:** We haven't found any documentation one way or the other on that.

**BERGE:** How much interaction did you have with Staff Warren after the war?

**MILLER:** Very little. In fact even during the time I was working presumably for him, I saw him about once every six months. I just got turned loose. He was recruiting.

**BERGE:** That's right at UCLA.

**MILLER:** If he got somebody to do his job, he was off. Are we getting somewhere?

**HERKEN:** Yes, we're going down the list here. Just changing gears again, I had an interest in the strontium therapy work that would have been done.

**MILLER:** I don't know anything about that.

**HERKEN:** You're not involved in that. Somewhat of a bureaucratic question—I don't know if you'll know the answer to this: Initially, the work that was done at UCSF was done under Contract 48, which was the contract to LBL, and later...?

**MILLER:** You mean this. *(holds out a report, cover forward, toward Herken)* Go ahead.

**HERKEN:** *(studies the cover of the document)* This was actually the MED, the Manhattan Project funding. But later, by 1947, there was something known as Contract 10, which is something that Stafford Warren—which is an AEC contract to the University of California for therapeutic work involving the use of radioisotopes. [My question is] if you remember [a] changeover in funding.

**MILLER:** I don't remember anything about that. What was the time of that? Oh, Maurice Dailey was the guy that took Soley's place.

**BERGE:** How do you spell his name?

**MILLER:** Dailey, Maurice.

**HERKEN:** Is he still alive?

**MILLER:** I don't know. Looked just like Abe Lincoln.

**HERKEN:** This is work with radioiodine?

**MILLER:** *(scanning the document)* I was just trying to find the last paper I was involved with. I think '55 might be it.[36]

**BERGE:** After about 1950, did you work with the Radiologic Laboratory that was in a separate building?

**MILLER:** No, that was Dr. Stone's baby.

**HERKEN:** Besides radioiodine, what other radioisotopes would you have done?

**MILLER:** None. (Don't you ever listen?) All of my work was primarily as a diagnostic radiologist.

**BERGE:** Did you conduct experiments per se, rather than just therapeutic work?

**MILLER:** I didn't understand your question.

**BERGE:** You said your primary work was as a diagnostic radiologist. Did you do any experimentation?

**MILLER:** With patients?

**BERGE:** Yes.

**MILLER:** No. My work was primarily as a physician—interpreting radiographs—and a physicist dealing with the physics of machines and of radiation.

**BERGE:** Would now be a good time to ask about—

**HERKEN:** —Let's wait a little bit.

**MILLER:** What were you going to do?

**BERGE:** At some time later on—I'll let him ask his questions first—I understand that you were responsible for a number of inventions and I just wanted to ask.

## Hamilton's Research on Effects of Cyclotron-Produced Radioisotopes

**HERKEN:** Getting back to Hamilton's work, and Hamilton is such a special interest of mine. As part of his work for the Manhattan Project to see how radiation, internal radiation, affected workers, he was proposing by the end of the war to essentially go down the periodic table, the transuranium[37] periodic table, and try different means of how people would be exposed to these elements, either by injection, ingestion, inhalation, were the three methods. He became especially interested in the danger through inhalation, which he determined was the greatest danger. And yet we know about the injections, of course. But, we can only find one case of an inhalation experiment, and that was involving zirconium with Ken Scott back in 1945.[38]

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[36] Miller's final publication with Dailey was, as he recalled, in 1955: Studies with radioiodine. V. Validity of histologic determination of $I^{131}$ radiation changes in the thyroid gland. *Radiology 65:*384–93.

[37] having an atomic number higher than 92, the atomic number of uranium; also called transuranic

[38] In an AEC-funded experiment, Dr. Kenneth Scott inhaled an active smoke containing zirconium-89. The purpose of the experiment was to determine the degree to which the lungs would retain very finely divided active smoke suspended in air. The results showed that almost 100 percent of the inhaled activity (continued...)

*(Dr. Miller inserted the following textbox during the editing process.)*

April 21, 1995
Dr. Joe Hamilton

I first met Joe when I was taken around the Berkeley Projects of the Manhattan District by Dr. Stafford Warren to introduce me to the people whose work I was to watch and monitor for radiation dangers.

I learned that he ran the Donner Laboratory and was responsible for the running the 60 inch Cyclotron there. He collected the resulting radioisotopes for experimental use. His broad plan was to determine the distribution and fate of all the radioisotopes in the periodic table in animals with special.attention to the transuranic elements.

He worked with Dr. Mayo Soley, internist from UCSF, on the distribution and fate of Iodine 131 in the bodies of animals. Their demonstration that this element was concentrated mainly in the thyroid led to the use of Iodine 131 as a tracer for thyroid function and as a means of treatment of hyperthryroidism and selected cases of cancer of the Thyroid.. When Iodine 131 became available to me through the good auspices of Ken Scott, Mayo Soley and I collaborated for many years in the diagnosis of thyroid function and treatment of Hyperthyroidism and Cancer of the thyroid.

Joe had a small staff which included Ken Scott, a roly poly man with sharp mind and inventive bent. He was a great help to Joe Hamilton in carrying out his research and doing his own thing on the side.

I always visited The Donner on the two half days a week that I spent in Berkeley during the War. As director of Radiation Safety for the Manhattan Project mostly I carried a Geiger Counter and tried to point out areas of high radiation dangers to the staff. My other activities consisted of running the film badge programs, the routine blood count program.

The only time I visited Joe at his home was when he was dying. I always liked Joe and had great respect for him as a scientist.

I had my younger daughter with me on this visit and at it we met Leah Hamilton, Joe's wife. She was a fine artist who painted abstract non-representational paintings. Judy, my daughter about 10 years old, on seeing her paintings, asked her what she was doing. Leah's answer, "I fill space with color" I liked that and never forgot it.

**MILLER:** I never heard of it. You remind me of the fact that dimly, I was aware of the fact that Joe's work was part of this fantastically designed long-

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[38] (...continued)
(about 0.5 microcurie of zirconium-89) was retained within the lungs and upper respiratory tract.

term experiment in going through the whole periodic table [of radioisotopes and studying their effects and distribution and excreta in animals.] That's special work.

**BERGE:** Do you know if there was any order? What kind of order was he employing when he was going through the periodic table?

**MILLER:** What was available off the cyclotron. If he got a Z first, like zirconium, and then he got carbon, then it would be Z, [and] C—whatever was available.

**HERKEN:** Wasn't he initially responsible for running the 60-inch [cyclotron]?

**MILLER:** I know, but nature decided what came off of it.

## Research With Patients From Laguna Honda Home

**HERKEN:** I had a question, as well, about the work that Stone and Hamilton were doing up at Laguna Honda Home. Did you have any patients coming from Laguna?

**MILLER:** I did some work at Laguna Honda with Dr. Howard Bierman. What they did was special. I think it could have been phosphorus work for all I know.

**HERKEN:** The only thing we have is really a letter from Ken Scott back to the AEC, I think it's dated 1948, saying that the work that Hamilton and Stone are doing at Laguna Honda is not funded by the AEC. I think it was funded by National Cancer Institute.

**MILLER:** That makes sense. A lot of Stone's stuff was [like] that. Because he was a cancer therapist.

**HERKEN:** I think what Stone was doing was taking patients from Laguna Honda and actually treating them at San Francisco at UCSF. I assume that was it.

**MILLER:** It could well be. I don't know.

**HERKEN:** When you had patients at Laguna Honda, you would take them to UCSF?

**MILLER:** Me? No. We simply used a fluoroscope.[39]

**HERKEN:** At Laguna Honda. Did they have an x-ray machine?

**MILLER:** They had a fluoroscope.

**HERKEN:** There was something known as the Laboratory for Experimental Oncology[40] at that time at Laguna Honda. Michael Shimkin [ran it].

**MILLER:** Shimkin, right.

**HERKEN:** Did you have any work with him?

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[39] a tube or box fitted with a screen coated with a fluorescent substance, used for viewing deep body structures by means of x-ray or other radiation

[40] the branch of medical science dealing with tumors, including the origin, development, diagnosis, and treatment of cancer

**MILLER:** I worked with Dr. Bierman, who was on Shimkin's staff. In fact, one or two of the papers were [written] with Bierman. What we did was to examine [the patient] under the fluoroscope with a catheter[41] and use an opaque material. We studied the arterial supply of every organ in the body, with particular interest of those where there was metastatic[42] cancer.

**HERKEN:** These were not necessarily Laguna Honda patients?

**MILLER:** [Nearly none of them were from there.] It turned out that that's where the work was done, because Bierman and Shimkin were there and a lot of cancer patients were referred there. The only reason it was at Laguna Honda was there wasn't room at UC for a place to [put the Laboratory for Experimental Oncology.]

**HERKEN:** The patients would not all be geriatric patients, is that correct? They would be a range of people.

**MILLER:** They would be primarily people with carcinoma, some kind of malignant disease. That tends to be in the later years, but not limited to that.

**HERKEN:** As I remember it, as I understand, Laguna Honda was a city and county-run home, still is, for the indigent.

**MILLER:** But, again, it's not because they were indigent; it was because they had a cancer. [You must understand that the patients who were referred for treatment to the Laboratory for Experimental Oncology came from everywhere. All regular treatment for their disease had been tried.] These people came from anywhere; [they were referred to the laboratory. Get this in your head!]

**HERKEN:** But the only equipment that you had at Laguna Honda was this antique fluoroscope.

**MILLER:** That didn't have anything to do with that. As a radiologist, I helped in lining up that catheter and then, later on, interpreted the radiographs that we got. [This was work I did with Dr. Bierman.]

**HERKEN:** Again, this was using radioiodine as a tracer?

**MILLER:** No. Don't get it mixed up. We used a [material] called diadrast, which [contains iodine, but] was not radioactive. It was radiopaque.[43] Going, for example, into the cervical artery, we could profuse the whole liver and differentiate the blood supply of the normal liver from [that of] the metastatic cancer in the liver.

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41 a thin, flexible tube inserted into a bodily passage, vessel, or cavity to allow fluids to pass into or out of it, to distend it, or to convey diagnostic or other instruments through it

42 relating to metastatis, the spread of disease-producing organisms or of malignant or cancerous cells to other parts of the body by way of the blood or lymphatic vessels or membranous surfaces; or, the condition so produced

43 opaque to radiation; hence, visible in x-ray photos and under a fluoroscope

This was important work. Because it turned out that any cancer therapeutic agent, given by vein, was diluted through the venous[44] system and the arterial system of the whole body. [In our work,] a catheter was put into the carotid artery, and guided to all the organs under fluoroscopic control. It went directly to the cancer, and the calculation at that time was that the dose to the cancer was 20,000 times greater than a similar dose given by vein. That [was Bierman]'s important work.

**HERKEN:** Let me get this straight in my mind: This was not work you did at the Laboratory for Experimental Oncology. This was not something under Shimkin's direction.

**MILLER:** Bierman worked under or with Shimkin. This was part of the business of understanding the whole phenomenon of neoplasia.[45] Blood supply was part of it. [This was Howard Bierman's project, and I helped him with it.]

*(phone rings; tape interrupted)*

**HERKEN:** *(glancing at the wall)* You have a picture of Stone on the wall [of your house].

**MILLER:** Yes; those people on the wall there were the people that influenced my life.

**HERKEN:** Who was the individual to the left?

**MILLER:** The bald-headed?

**HERKEN:** No, down to the bottom right.

**MILLER:** I don't know. *(Dr. Miller amends:* That was Dr. Ernest Pohle, professor of Radiology at the University of Wisconsin. He gave me a job.*)*

**HERKEN:** We were just talking about Laguna Honda. I think I have it straight now. You in fact did not know very much about Stone's work at Laguna Honda, which I think basically involved taking patients from Laguna Honda to UC Medical Center and using the 70-MeV[46] machine there, the bevatrons.[47]

**MILLER:** The patients came from the Laboratory for Experimental Oncology that was housed at Laguna Honda. They were not patients who were in the hospital. There *was* no bevatron at UC. Wait a minute—yes.

**HERKEN:** Not at Laguna Honda, but at UCSF.

**MILLER:** In what he called the Radiological Laboratory. I've forgotten that even had existed.

**HERKEN:** And the Radiological Laboratory is something that he created himself after the war, is that right?

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[44] of or pertaining to veins

[45] the formation and growth of neoplasms (tumors)

[46] million electron-volts

[47] accelerators in which protons are raised to very high energy levels (currently several billion electron-volts)

**MILLER:** Exactly.

**HERKEN:** About 1947?

**BERGE:** 1951.

**MILLER:** He got funding for that from the National Cancer Institute.

**HERKEN:** Is Bierman still alive?

**MILLER:** I don't know. Great guy. Inventive. He was so inventive that he couldn't get along with Shimkin or anybody else. He finally got to work down in L.A. someplace.

**HERKEN:** Shimkin has written a little history of the Laboratory for Experimental Oncology and calls it [(the history)] *The Lost Colony*. He's fairly open in the history about his disagreements with Stone. I should say he's open that he disagreed, but that it's a little unclear what the disagreement was about.

**MILLER:** I don't know. I remember that it existed. I wouldn't have remembered until you said it. And I don't know what it was about. Whether it was a matter of him getting too big for his britches, or I don't know what. People are human, even doctors.

**HERKEN:** *(turning to Berge)* Do you have any questions on this?

**BERGE:** I'll have some questions later, but not on Laguna Honda.

**HERKEN:** In fact, the laboratory at Laguna Honda was shut down in 1953. I think Shimkin left at that time, although I'm not really sure.

**MILLER:** I think that's [about] right. I don't know the date. I don't even know what happened to him or where he went.

**HERKEN:** Did he do any work with John Lawrence on radioiron?

**MILLER:** I don't know. I wouldn't be surprised if they had done some work together, but I have no solid information about it.

**HERKEN:** But you were not involved in doing work for John Lawrence?

**MILLER:** No, the only involvement I ever had at Laguna Honda was this vascular[48] study, studying the difference between the blood supply of the normal tissues and the cancer.

**BERGE:** Somewhat on the same track, but different hospitals: What kind of involvement would you have had with Langley Porter?[49]

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48 pertaining to vessels that convey blood, such as veins and arteries

49 one of the clinics at University of California, San Francisco Medical Center (a psychiatric unit)

## Radioactive Iodine Uptake in Schizophrenia Patients

**MILLER:** Only when I wrote a paper with Bowman, who was the head of Langley Porter. He was interested in knowing whether people with schizophrenia had any possible thyroid involvement. He sent patients over, and we did the tracer studies on the rate of uptake, which was the most effective way of separating the "normal" from "abnormal" rates of absorption in the thyroid. The study was done during the three-hour uptake period, and we did it at one, two, and three hours. The people who were hypothyroid [had low uptake rates]. The normal group lay in the middle [of the uptake-rate distribution curve], and the hyperthyroids [had high and rapid uptake of the radioiodine]. So you could actually make the corroborative study. Finally it got to the point where the thing was so good that it could be used later on as a diagnostic tool. Then they didn't need it anymore. "Miller's Law"!

**BERGE:** What kind of results did you get from that?

**MILLER:** There was nothing, no correlation. They were all normal, except they were crazy. The thyroid wasn't involved. Soley did the clinical stuff, and I did the radioactive iodine uptake stuff.

**HERKEN:** When did Soley leave San Francisco?

**MILLER:** When he went to Iowa. I don't know when that was; I don't remember dates. It was sometime early.[50]

## Recalling Dr. Joseph Hamilton

**HERKEN:** In thinking of papers, did you have any collection of papers and correspondence with Hamilton or Stone?[51]

**MILLER:** I don't think I ever wrote to Joe, ever. But Joe and I became good friends. *(See textbox,* "Dr. Joe Hamilton," *in "Hamilton's Research on Effects of Cyclotron-Produced Radioisotopes," preceding)* The only time I saw Joe was when I was making my rounds over at Berkeley, and later on, when I visited his home when he was dying; I saw the artwork of his wife. Great artist. My [ten-year-old] daughter was with me and she said, "What are you trying to do?" She said, "I'm breaking up space with color."

**HERKEN:** This is on a personal note, but I noticed in the obituary for Hamilton you mentioned that he nursed his wife through an illness and in the end she acted as his nurse.

**MILLER:** If I remember correctly, she became psychotic; I've forgotten.

**HERKEN:** Was that treatable with drug therapy back then, for psychosis?

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[50] Dr. Soley left UCSF in 1948 to become the Dean of the University of Iowa Medical School. He planned to open a thyroid clinic and an iodine-131 diagnostic and treatment clinic. While helping the university prepare the iodine lab, Soley engaged in clinical work for about six months before he died in 1949.

[51] See Miller's first boxed editing insert, preceding, under "Human Applications Committee and Informed Consent."

**MILLER:** No. He loved his wife very much. He just took care of her, and he endured that time.

**HERKEN:** I read that he diagnosed his own leukemia about 1955.

**MILLER:** It doesn't surprise me at all. I think it was the massive doses of radiation that he had. And they were massive. I think they actually started it [(the leukemia)].

**HERKEN:** Did he ever keep a diary or journal?

**MILLER:** It's just like I wouldn't know about you having a diary.

**HERKEN:** I talked with someone who said that he had a gold-rush cabin, up in Auburn, [California], and he used to go up there and pan gold on weekends. That was his form of relaxation.

**MILLER:** I did hear about that. Who was this that went there?

**HERKEN:** I think it was Bill Douglas who was [with] the [*unintelligible*] lab.

**MILLER:** I don't know anything about that.

**BERGE:** Did you ever go over there [to the Hamilton house] for social occasions?

**MILLER:** No. [The only time I went to the Hamilton house was to visit Joe when he was dying.] My young daughter was with me at the time.

**HERKEN:** They didn't have any children, did they?

**MILLER:** I think not.

**HERKEN:** Did she ever remarry?

**MILLER:** I don't know anything about her.

**HERKEN:** She's no longer alive; I've gone through my list [of historical figures to contact].

## Invention of a Baby Holder (1951)

**BERGE:** I wanted to find out a little bit more about your inventions. We came across a series of pictures in which you were credited with the invention of the "brat board."

**MILLER:** The BRATT BORED [sic].[52] It's [the] one of them I'm most proud of actually. In the department, when this radiation consciousness became acute, we started to use film badges[53] to determine whether our people were overexposed. It was very interesting that the only people whose badges ever got black [from overexposure] were those who took radiographs of young children. That's because they held them [(the children)] when the film was taken.

[52] E.R. Miller. "A Device for Immobilizing Children During Radiographic Examinations." *Radiology 58:*421–23, March 1952.

[53] dosimeters

I gathered the technicians together and I said, "Look, this is a problem. Now, I want you all to think about this and come up with any kind of a baby holder that you can come up with." They came up with the screwballest notions you ever saw.

Finally, I decided maybe I'd take this on. I got to thinking about this [and] remembered how the American Indian women carried their papooses. I went down to the shop and cut out a [flat] piece of one-quarter–inch plywood in the shape [(silhouette)] of a child, with a headpiece, a body with two upstretched arms, and two legs. We used Ace[54] bandages to immobilize the child on the board.

So, then, I offered a prize for a name for this. The prize was a doughnut. I mentioned it to my wife one day and she said, "It's a BRATT BORED." She was a journalist; she *would* come up with something like that! It was spelled BRATT because you never called a child a brat. And it was BORED, because they went to sleep on it.

This device is now still used in certain places in the United States for immobilizing children for intravenous studies, for x-ray studies, for all kinds of stuff. That's an invention, cutting out a piece of plywood. I got a prize for it.

**BERGE:** *(smiling)* I hope it was a good doughnut.

**MILLER:** The prize was: becoming the Gadgeteer of the Year from the Radiological Society.

**HERKEN:** There's actually a Mickey Mouse plaque.

## Technique to Produce Infinite Laminograms

**MILLER:** Actually, from that day on, when we started to use those boards, there was never again an overexposure of a technician.

I thought you were going to come up with the one [(the invention of mine)] that I liked best. It was having an infinite number of laminograms from a finite number of films.

**BERGE:** How do you do that?

**MILLER:** The patient is lying down, and—do you know what a laminograph is?

**BERGE:** No.

**MILLER:** A laminogram is a radiograph, let's say of the chest, in which only one level, or layer, or lamina is shown sharply and everything else is blurred out.[55] That is called a laminogram. The apparatus for making a laminogram consists of a slotted metal bar connecting the x-ray tube holder to

54 an elasticized bandage, usually in a continuous strip, for securely binding an injured joint

55 In the early '70s Dr. Miller, together with E.M. Curry and B.B. Hruska, coauthored two papers on this subject in *Radiology*: "An Infinite Number of Laminograms from a Finite Number of Radiographs," *Radiology 98*:249–56, Feb. 1971; and "A Simplified Procedure for Viewing Multiple Films to Create an Infinite Number of Laminograms," *Radiology 110*, No. 2, Feb. 1973.

the film-holding tray. At the side of the table there is an upright piece to hold a pin, which pierces the bar. The height of the pin is adjustable. It acts as a fulcrum[56] for the opposed motions of the tube and film.

During the filming, the tube stand is moved about 2 to 3 feet along the side of the table in about 3 to 4 seconds. Note that when the tube is moved *up* the table, the film moves *down* the table. *Only* the level of the fulcrum has not moved, and the images at that level have not been blurred. All images above and below the level of the fulcrum are blurred because they moved during the filming. This procedure produces the laminogram.

I then appreciated the fact that during the exposures of the 3 to 4 seconds, with full-wave rectification,[57] you are taking 120 radiographs per second in order to make one laminogram. I said to myself, "Well, how about doing it with 7 unblurred radiographs instead of 3 times 120 or 360 x rays?"

So, we got a skeleton and took seven radiographs of it, each with the tube at equally spaced distances over a total distance of three feet, using the laminographic machine. The seven unblurred radiographs were superimposed in a device that slides the radiographs upon one another. The x rays are lined up so that all the images taken with the tube at one extreme end are properly superimposed to create an unblurred image. A lever with clips to hold the films and a fulcrum in the lever permits the radiographs to slide upon one another so that successive layers of the exposed body are presented sharply from the front to the back of the body. Movement of the lever provides the viewer with an infinite number of laminograms from a finite number of films. Each film was underexposed, so that the total exposure for all of them equaled the amount of radiation required to take only one unblurred ordinary radiograph.

**HERKEN:** [*unintelligible*]

**BERGE:** No, I have only the ones that we have from the pictures. There were a number, such as the image orthicon tube.[58]

**MILLER:** I didn't invent that, I used it.

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[56] the point about which a lever pivots

[57] a technique for changing an alternating current (ac) into a direct current (dc)

[58] a sensitive television-camera tube in which a beam of low-velocity electrons scans a photoemissive mosaic

## Introduction of Stereoscopy to X-ray Film Making

**BERGE:** Or the Bi-plane Simultaneous Therascopic Serialograph Apparatus.

**MILLER:** This apparatus was to be used during the making of carotid angiograms[59] to show the blood vessels of the brain in the diagnosis of brain or vascular disease. [(I never published anything on this.)] When Dr. [Ed] Boldrey and I did the first cerebral[60] angiogram done at UCSF, Dr. Boldrey exposed the carotid artery surgically and injected the contrast medium.[61] I ran a team that took a series of films of the head during the passage of the contrast medium through the vessels of the brain.

Shortly after the first angiogram, I introduced stereoscopy[62] into the film-making. This meant that the x-ray tube had to be moved back-and-forth between exposures. The amount of movement equaled the interocular[63] distance of the final viewer of the films (two-and-a-half inches). Films 1 and 2 made a stereo pair, films 2 and 3 made the next stereo pair, films 3 and 4 made the next pair, etc., etc. The tube was moved by a technician, using a long pole attached to the tube housing. Films were changed by hand.

When the department was moved to Moffitt Hospital, a Special Procedure room was set aside to do cerebral angiography, and the special apparatus was installed. The patient would lie supine[64] on the x-ray table. Above him were two x-ray tubes, called tubes 1 and 2, whose focal spots were two-and-a-half inches apart in the horizontal direction at a distance of 36 inches. The focal spots were aligned toward the head and toward the feet of the patient. Films were housed in automatic film changers.

When all was ready, contrast material was injected into the carotid artery via a catheter. Two seconds later, tube 1 and tube 3 fired simultaneously, taking a frontal and a lateral[65] film of the head from the first positions. The film changers placed new films in position. Two seconds later, tubes 2 and 4 fired, making a frontal and lateral film of the head simultaneously from the second position. The film changers changed films, and two seconds later tubes 1 and 3 fired, making a pair of films from the first position again. This procedure was repeated till the end of the series.

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59 x-ray images produced by x-ray examination of the heart and its blood vessels following intravenous injection of radiopaque fluid

60 of the brain, especially the forward and upper part, which governs voluntary movement and conscious processes

61 a radiopaque substance introduced into a part of the body to provide a contrasting background for the tissues in an x-ray examination

62 the use of a stereoscope, an optical instrument through which two pictures of the same object, taken from slightly different points of view, are viewed, one by each eye, producing the effect of a single picture of the object, with the appearance of depth (like a "3-D" photo)

63 horizontal distance between the two lenses of a stereoscope

64 face-down

65 from the side

Films 1 and 2 from tubes 1 and 2 made a stereo pair in the frontal projection from the first and second tube positions. Films 2 and 3 from tubes 1 and 2 made a second stereo pair two seconds later from the two tube positions. And so on from both the frontal and the lateral views. The stereos provided three-dimensional views as a function of time, which gave the fourth dimension. Two four-dimensional views make an 8-dimensional study with one injection of contrast material.

**HERKEN:** I had a question about Ken Scott. Scott eventually left UCSF, did he not, and Berkeley, and went down to UCLA?

**MILLER:** Did he? If so, I don't know.

**HERKEN:** Do you remember—I was curious why he left.

**MILLER:** Joe [Hamilton] died and the [Donner] Lab kind of folded, didn't it? I think there was no place for Ken.

**BERGE:** At one point he switched from the Radiation Lab to UCSF. Do you have any idea why he did that?

**MILLER:** UCSF?

**BERGE:** He went from Lawrence Radiation Laboratory[66] to UCSF. Do you know why he would do that?

**MILLER:** I don't know when he did it. Unless, did he work with Stone or something?

**BERGE:** He might have. I don't know.

**MILLER:** I think probably the [Donner and the] Crocker [Labs] kind of folded up. Now I'm guessing completely. And if he did come over—Now that you mention it, I think he did spend time at what was called the Radiologic Laboratory, which was Stone's. *(For remembrances about this, see the textbox* "About Consent Forms," *preceding.)*

**HERKEN:** *(scanning a list of UC Radiation Lab reports from the war and postwar era)* Schimpkin is one of the authors here of a paper; that's a name I reckon you've seen before: J.J. Schimpkin.

**MILLER:** What about him?

**HERKEN:** Tell me, was he a colleague at UCSF?

**MILLER:** This was a study of error in interpretation of radiographs. Schimpkin was a chest man. He was a very good one. [Jacob] Yerushalmy was the statistician. The rest of us worked on this project on the study of error interpretation for a long time: in fact, I'm still working on it. *(Dr. Miller later adds:* This is an error; I didn't know a J.J. Schimpkin.*)*

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66 formerly called UC Radiation Laboratory; now Lawrence Berkeley Laboratory, a National Laboratory on the campus of University of California at Berkeley

## Postwar Preference for Unclassified Research

**HERKEN:** There was one paper here, *(scanning the list)* a UCRL report, [about] experience when you were in California when you were treating patients with hyperthyroidism. Did that involve the Langley Porter patients?

**MILLER:** No, I did only diagnostic studies on patients from Langley Porter. That was a Radiation Laboratory report, probably [written] during the war. After the war, I said I would never again be involved with any secrecy project.

**BERGE:** Why?

**MILLER:** [Secrecy] was important during the war, not because I was treating patients with hyperthyroidism, but [because] I was a radiologist involved in a project and somebody [from the Axis][67] is going to figure out that that's somehow related to radiation and that's the bomb.

**BERGE:** Why didn't you want to be involved in [work that was to be classified]?

**MILLER:** That's not my bag. [As a scientist I wanted to share anything I learned with whoever wanted it.]

**HERKEN:** A number of interviews, articles, [were] published in refereed journals. There's also a number of UCRL reports. If it's a UCRL report does that necessarily mean it would have been classified?

**MILLER:** [During the war], it was classified.

**HERKEN:** A lot of these are reports, for example, *(glancing at the list)* in a 1949 *Medical and Health Division Quarterly Report.* Why would that be classified?

**MILLER:** I tell you, they'd classify anything. Part of the answer to your question [is that] I got sick and tired of having to write reports to people. I wanted to do the work.

**HERKEN:** *(scanning the list)* The first date I've seen here is 1948. So, the first biomedical reports done by the Radiation Laboratory would have been in that time frame?

**MILLER:** Was my name on that?

**HERKEN:** Yes.

**MILLER:** I suppose it was one of the first [of the reports on the $^{131}$I studies]. Low-Beer and I, and maybe Stone, all gave separate portions of a report. I'm sure this had to be radioiodine-related in some way.

**HERKEN:** That would probably be receiving AEC funding?

**MILLER:** I don't know.

**HERKEN:** How do you divide first the division of labor as to what portion Low-Beer would write, what portion [you] would write?

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[67] the nations against whom the Allies fought, consisting chiefly of Germany, Italy, and Japan

**MILLER:** Your own work. Our work was separate. I guess the reason, if any, if this was an AEC thing, was because I was being given radioiodine and he was given $^{32}P$.

## Zirconium and Plutonium Injections

**HERKEN:** One report with us—and this is UCRL 68, your name actually isn't on here, but Low-Beer, Scott, Hamilton, Stone [are][68]—gives [an account of] an injection that was done January of 1948.[69]

**MILLER:** I don't know about that. One would think that all these injections, whatever were done, were with therapeutic [or diagnostic] intent.

**BERGE:** Do you know how they selected the patients that they were selecting with, say, zirconium or plutonium?

**MILLER:** [I don't know how they made the selection. My guess would be that they would select a patient to be injected because they could acquire some very important information from the test or that it was injected with therapeutic intent.]

**BERGE:** I've noticed that there seems to be a general pattern that they would inject, say, plutonium or zirconium into a patient a day or so [before] they would actually amputate or take something out.

**MILLER:** How else would you do it?

**BERGE:** I guess my question would be, then why would you need the radioactive substance?[70]

**MILLER:** [Because you use the radioactivity of the isotope to tell where the isotope is.[71] If the isotope emits gamma rays, these can be detected by a Geiger counter aimed at the body from the outside. Gamma rays penetrate tissue very well. Alpha and beta particles emitted by a radioactive isotope travel only a short way through the tissue and in general do not get out of the body. Then, to detect the amount and the location of the isotope in the tissue, it is necessary to remove some of the tissue, make a microscopic slide out of it, and expose a film in contact with the slide. The radioactivity blackens the film where it lies and shows its exact

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68 Low-Beer, B.V.A., K.G. Scott, J.G. Hamilton, and R.S. Stone. "Comparative Deposition of $Zr^{95}$ in a Reticulo Endothelial Tumor to Normal Tissues in a Human Patient." Berkeley, CA: University of California Radiation Laboratory, UCRL-68.

69 In 1946, at UCSF and the Crocker Radiation Laboratory at Berkeley, AEC-sponsored research was carried out to study the uptake and deposition of zirconium. The subject, a 55-year-old female patient with a reticulo endothelial tumor that had arisen in the spleen and then metastasized to the liver and left leg, was given a test dose of zirconium-95. She was administered 1.76 millicuries of zirconium-95 in saline by intravenous injection 24 hours prior to a scheduled midthigh amputation of the left leg. Samples of the tumor, as well as normal tissue, were later obtained from the limb for zirconium-95 assay.

70 (if injected before the amputation) if the body part was going to be removed anyway; or (if injected afterward) if the body part was already removed

71 that is, to tell you whether blood or lymph circulation was [or had been] reaching that body part

position of the isotope in the tissue. Luckily, $^{131}$I emits both gamma rays and beta particles. Does that clear it up?]

**BERGE:** I think so.

**HERKEN:** Though in 1948 there was less sensitivity [about] the bomb, people knew the bomb existed. I'm surprised that this work would have been apparently classified back in January 1948.

**MILLER:** I don't know anything about that. The original work on radioiodine was, number one, to find out about it, and also to calibrate the doses and see that you get the greatest good with the least harm.

## Research With Healthy Volunteers

**HERKEN:** I know that Hamilton in 1950 was proposing what he identified [to become] his early experiments with healthy subjects. He wrote to Shields Warren using [radiation] in healthy subjects, in prisoners, for example. Hamilton himself was considering doing experiments on human subjects. Experiments with prospective therapeutic benefits.

**MILLER:** I think you've left something out: These were volunteers.

**HERKEN:** Yes, he specified they would be volunteers.

**MILLER:** That's mighty important. In other words, they weren't guinea pigs, in that sense. They were willing to accept the risk for what good they could do humanity.

**HERKEN:** Did you do any similar such work with volunteers?

**MILLER:** Unless you talked about the [Langley Porter Clinic director] Bowman ones. These people, if I remember correctly, signed a consent form. Because there was no clinical evidence of active thyroid involvement one way or another, these studies were to see in what range their iodine uptake would fall. The patients didn't have any evident thyroid disease, but they might have. Bowman's concern was that if the thyroid was involved and if the radioiodine could be involved, it could help treat their schizophrenia.

**BERGE:** Earlier you said that there wasn't at that time a good way of diagnosing thyroid disorders.

**MILLER:** There still isn't, [but there is an excellent correlation between the thyroid function and the rate at which radioiodine is absorbed in the thyroid.]

**BERGE:** What led Bowman to believe that there might be a link?

**MILLER:** I don't know what was in his mind. One thing about the schizophrenia [patients], they're pretty hyper; so is an active [hyper]thyroid [patient]. So, they had this in common. Was the thyroid in some hidden way involved to create this jitterness? [That] was the question we tried to answer.

## Tracing the Records of Patient Consent

**HERKEN:** [Did he see to it that] some form of the consent form went into the patients' records?

**MILLER:** I don't know. It was bound to, I'm sure.

**HERKEN:** I'd be interested in finding out who the *[unintelligible]* was, because the Committee[72] is interested in finding the course of consent and are not quite sure where to start looking. I suppose that would be in the patients' charts. Are there enough records at UCSF kept under the doctor's name, or were they always in the patient's name?

**MILLER:** There were no computers at that time, so if you wanted to find a patient, [or to] get a patient's chart, you got it by *his* name, not the doctor's name. The doctor, undoubtedly, had a record of which-patient-is-who.

**HERKEN:** It would be very helpful to us if we could find the doctors' records and went through the doctors' records and then patients' records.

**MILLER:** I thought these people made pretty good records of follow-up on patients, didn't they? I know the Stone stuff was magnificent, and the records we kept on our patients were really complete.

**HERKEN:** The problem is, they're not under Stone's name at UCSF.

**BERGE:** Do you have any idea where either Stone's records, or even earlier records might be?

**MILLER:** No; I think that stuff all went down the drain.

**BERGE:** You didn't turn them over to the archives[73] or to the medical school?

**MILLER:** No. I turned my bound reprints of articles I wrote over to the archives, but that's the only relation I ever had with the archives. None of that would go. They would go with the charts. With time, the charts occupy so much space that they either throw them all away or try to pick them; so if the patient doesn't come in again for five years, then they go.

**HERKEN:** Did you do work in your house [*unintelligible*]

**MILLER:** No.

**HERKEN:** I know Pat Durbin[74] has a painting by Leah [Hamilton] in her office.

**MILLER:** Probably very "far out" and beautiful. Very abstract, and that's why I loved that. I never would have remembered that, but you brought it up: "I break space with color."

**HERKEN:** What was your work with Pat Durbin? How do you happen to know her?

**MILLER:** I've never heard of her. Who's Pat Durbin?

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72 The President's Advisory Committee on Human Radiation Experiments

73 Lawrence Berkeley Laboratory Archives and Records Office, where Berge works

74 For the transcript of the interview with Durbin, see DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (July 1995).

**HERKEN:** Pat Durbin worked for Joe Hamilton.

**MILLER:** What, as a secretary or something?

**HERKEN:** She was initially a glass washer.

**MILLER:** I never dated her or anything.

**HERKEN:** She got her degree in Radiobiology later on. She's the one in 1974, who turned up [the fact] that some of the plutonium subjects were still alive and alerted the AEC to that fact, and [that] resulted in the subsequent interest of the AEC in reconstructing the case.

## A Career in Research

**BERGE:** I just have two finishing questions I'd like to wrap up with it. If I understand correctly—actually, I don't understand correctly! Was your primary interest when you started your studies in medicine to do research or to diagnosis?

**MILLER:** I might tell you a little story about that. Ed Chamberlain, who was a professor of Radiology at Temple [University in Philadelphia], came to visit Dr. Pohle at [the University of] Wisconsin. I never saw Pohle quite so mad, because Chamberlain came down and spent the whole time with me in my lab instead of with Pohle. As he was going out the door he said, "I can tell what your life is going to be like: You're going to work yourself out of this laboratory and get into medicine, and then you're going to work yourself right back into the laboratory for the rest of your life."

And he was so right! [I was always involved in research.] All these memories. For a guy my age, that's all you have left.

**BERGE:** I don't know; this *house* is nice.

## Professional Contribution

**BERGE:** One more question for me to sort of bring it all back together: What do you see as the results of your work over the past forty, fifty years, in terms of what you were able to produce with your research?

**HERKEN:** Did you finish with that document? Can I make a copy of this?

**MILLER:** You may have that.

*(The three-page response that follows is a scanned image of a printed document provided by Dr. Miller after the interview. The page numbers in the first paragraph refer to an earlier, double-spaced draft of this transcript.)*

April 11 1995

This is a rewrite of pages 52,53,and 54 of report on an interview of Dr Miller by Ann Berge and Gregg Herken for the Archives of the Lawrence Radiation Laboratory

Berge: What do you see as the results of your work over the past 40, 50 years, in terms of what you were able to produce with your research?

Miller: I introduced body section radiography (laminography) to the UC Department of Radiology in 1937. My interest in this kind of radiography lasted till 1973 when I developed a method for obtaining an infinite number of laminagrams from a finite number of films.

Dr. Mary Olney and I introduced angiocardiography for the study of congenital heart disease in babies in 1944. Dr. Ed Boldrey and I introduced cerebral angiography in the study of Brain disease and cerebro-vascular disease in 1946. These interests lasted till 1953.

Dr. Soley and I introduced the use of Radioiodine at UC for the diagnosis and treatment of thyroid disease in 1948. This was a major interest until 1959

Jacob Yerushalmy (statistician) and L Henry Garland led a group of us that started a *study of error in interpretation of chest radiographs in 1952.* My own interest in error of any kind continues to the present day. It led to my final studies of seeing in the interpretation of a scene. The word "seeing" has two definitions both of which must be met in order properly to know the meaning in the scene. The first is to gather information through the eyes and the second is to "see" or to understand the meaning of the scene. The interest embraced not only error but also how one made a correct decision. Search patterns, viewing distance, training, the quality and size of the image, the amount of contrast and contrast gradient, the center of interest, the size and shape of the zones of confusion of the every part of the imaging system. the amount of movement and its rate were only some of things that entered into consideration. All of these aspects enter into the need for survival.

The Bratt Bored was made in 1952.

A NEW UNIT FOR MEASUREMENT OF PATIENT EXPOSURE.

I would like particularly to call attention to reprint # 85 entitled "Patient exposure during fluoroscopy" published in Radiology Vol 80 No 3. Pages 477 to 485 March 1963.

In this paper I invented a new unit for the measurement of radiation exposure of patients.

The unit was $cm^2 R$ and is pronounced **square centimeter roentgens.** The unit was nicknamed SCIMMERS. To my knowledge this is the first unit which takes into account the flux of radiation and the area to which the patient is exposed simultaneously.

It is reasonable to suppose that the injury to a patient caused by exposure to ionizing radiation is proportional to both the area exposed and to the flux of radiation. This unit can be expressed also in Gram rads of absorbed radiation.

It was shown that experienced examiners using a 5" image intensifier subjected patients to only one fourth to one fifth as much radiation as did less experienced examiners using a conventional Fluoroscopic screen during examinations of the viscera in the abdomen.

Dr. Frank Hinman first awakened my interest in the physiology of voiding in 1954. My interest in X-ray cinematography began about the same time. It was apparent that if one was to understand the physiology of an organ system, one needed to know what happened to it as a function of time. The best way to do it was to use X-ray movies to display the anatomical changes continuously and to display, simultaneously, those physiological parameters of importance to the working of a system.

Developing the apparatus and the procedures to provide such information about the lower urinary tract at rest,. during normal voiding, during strain, and during incontinence; the mouth and pharynx during speech and swallowing; and the esophagus during swallowing occupied the major effort of the Radiological Research Laboratory until 1973.

I was given the Laboratory in 1958 by President Robert Gordon Sproul and directed its activities until I retired in 1974. With the help of Ed McCurry EE, (inventive guy), and Bernard Hruska (super technician) we developed the most sophisticated means of making these studies in use at that time. The final apparatus employed a Vidicon TV camera to record the physiologic data, and an image orthicon TV camera to record the x-ray image. The images from these cameras were presented side by side on a video screen and on every frame of the movie film.

For example, in the study of the lower urinary tract in women, on every frame of an xray movie, and on every frame of a video tape showing the contrast filled urinary bladder and later, the urethra, there were 8 channels of physiological data. The latter showed absolute pressures in the bladder and urethra., the absolute pressures in the colon (abdomen) and the rectum and the differences in these pressures with respect to one another, plus the amount and rate of voiding, during rest, during strain, and during "holding" urine. The patients had either incontinence, had difficulty in initiating voiding or had recurring infections in the bladder due to incomplete voiding or reflux of the urine into the ureter. The sound track recorded the commands to the patient and the movie and the video image showed the response of the patient while all this was happening. Study of these data allowed us to understand the abnormal physiology in these patients.

In studies of the esophagus, the x-ray image showed the passage of Barium and the data showed the pressure gradients throughout the esophagus during swallowing.

The late Dr. Lucie Lawson got me interested in the studies of patients with abnormal speech due mainly to cleft palates. The x-ray movie showed a lateral view of the mouth and pharynx during speech and swallowing. The sound track recorded the sound of the speech. The data from a sound spectrograph recorded the frequency components of the sound on each frame of the movie. Dr. John Q. Owsley was the Plastic Surgeon most interested in these patients. He and we developed a very clear picture of the way the soft palate worked in patients with nasal sounding speech. Through this understanding, Dr. Owsley was able to develop the most effective surgical techniques for the alleviation of these patient's abnormality.

All these studies were made in line with the fundamental drive of acquiring the greatest amount of data about a patient with the least radiation exposure.

We became interested in image manipulation in 1970. We used one vidicon TV camera and sometimes two simultaneously to look at x-ray and other images .Any single line of the TV raster could be isolated and the variation in its brightness presented on the viewing screen of an oscilloscope. The brightness and contrast gradients were presented instantaneously across the width of the image. The optical quality of the image could thus be determined instantaneously and be recorded objectively.

This type of analysis was applied to images that were subtracted from one another, both isotropically and anisotropically and in those in which edge enhancements were introduced. The effects of defocusing the image on these parameters became a major aspect of the studies of pattern recognition.

## Remembrances of Personalities

**HERKEN:** When was the last time you saw Joe Hamilton?

**MILLER:** I have no idea. Actually if you know when he died, I can say that it was shortly before that. I went up to visit him one time. —Oh, I think that was when I saw Leah's work. I think that was it: I went to visit Joe.

**BERGE:** Was she still psychotic at that time?

**MILLER:** I don't know.

**HERKEN:** He was home. He wasn't in the hospital.

**MILLER:** He was at his home.

**BERGE:** What was he like in terms of personality?

**MILLER:** Good question. He was a nice guy. Number one. He was a dedicated scientist, dedicated scientist. He became sad when he was dying. I guess everybody would be. You don't fear death, you fear the dying. It was sad.

**HERKEN:** How would this manifest itself?

**MILLER:** Sad. I like the Spanish word *tristé*. That has the sound of sadness.

**HERKEN:** How would you characterize Stone when you talk about him?

**MILLER:** Stone was a Scotsman. That was part of his makeup. He was very chintzy about spending money on the department, for instance. He was an excellent clinician. He was an excellent radiologist. He was a dedicated and exciting research guy. Everybody recognized his work on patient analysis, and you have the best in the world.

**HERKEN:** He was somewhat of a prickly character, I had the impression, just in correspondence I've seen. He made some enemies.

**MILLER:** Yes, he could. It never came into my ken. Like Shimkin, for example, or even Bierman. He had a reason. He felt that those people were getting out of line. With John Lawrence, he thought he was getting out of line.

**HERKEN:** How so?

## Tension Between John Lawrence and Stone

**MILLER:** I don't think I'm going to answer that. I don't know; he just disagreed with what [they were] trying to do.

**HERKEN:** With what Lawrence was going to do?

**MILLER:** Yes, trying to develop a medical school in competition, for example.

**HERKEN:** That would have been one cause of tension between Stone and Lawrence, certainly. John Lawrence was trying to create a medical school at Berkeley and Stone was at UC San Francisco. I can see how that could be a cause of tension.

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## ROBERT SPENCER STONE, M.D., L.L.D.

by

Earl R. Miller, M.D.

The world has lost a great and wise man with the death of Dr. Robert Stone on December 18, 1966. He died at the age of 71 of cancer, the disease with which he was concerned for most of his life.

Dr. Stone [redacted] He received his B.S., M.A., M.B., and M.D. all from the University of Toronto between 1919 and 1928. In Peking, China, he served as an Assistant in anatomy at the Peking Union Medical School from 1919 to 1921. His internship was served at Grace Hospital from 1924 to 1925 and he practiced Radiology at the Grace Hospital in Detroit with Dr. Roland H. Stevens from 1925 to 1928.

He came to the University of California as an Instructor in Radiology in 1928 and rose to Professor there in 1938. In 1939, he became Chairman of the Department of Radiology and served in this capacity from 1939 to 1943 and from 1946 to 1962, the time of his retirement.

From 1942 to 1946 he was on leave of absence from the University as Associate Project Director of Health of the Metallurgical Project and as visiting Professor of Roentgenology at the University of Chicago. Dr. Stone was the first Chief of Staff at the University of California Hospital from 1954 to 1958. He was instrumental in bringing the Radiological Laboratory to the University in 1951 and served as its Director from 1951 to 1964. It was in this Laboratory that the work on the 70 million volt Synchrotron was done. After his retirement in 1962, he was recalled as Professor Emeritus with the University of California School of Medicine. Until his death, he continued working in the Laboratory

During Dr. Stone's long, productive life, he was author or co-author of 60 scientific publications. In his quiet way, he had a profound influence on the field of radiation therapy. He, with J. T. Hamilton, was the first to administer artificial radioactive substances with therapeutic intent. He was the first to do million volt therapy, neutron therapy, and therapy with 70 million volt Synchrotron. Each of these accomplishments was a highly significant advance in the field.

Dr. Stone was highly honored during his lifetime. He received these justly bestowed marks of distinction with humility. He held honorary memberships in the Cancer Society of Guadalajara, College of Physicians in Philadelphia, Philadelphia Roentgen Society, Royal Society of Medicine in London, the Canadian Association of Radiologists, and Alpha Omega Alpha. He was given the highest United States civilian award, the Medal of Merit for wartime work in the Metallurgical Project in 1946.

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He also received the Gold Medal of the Radiological Society of North America, the Janeway Medal of the American Radium Society, the Medal of the American Cancer Society for cancer control, the Gold Medal of the American College of Radiology, the Gold Medal of Citation from the U. S. Atomic Energy Commission, and an honorary L. L. D. conferred by the University of California in 1956. He was the Pancoast lecturer in 1946, the Carmen Lecturer in 1951, and the Gordon Richard Memorial Lecturer in 1960. Dr. Stone served with distinction on many committees on the local, state, national, and international levels.

Dr. Stone was survived by his wife Willemina, his son Robert, and daughter Margaret.

Throughout his life, Dr. Stone was characterized by his thoughtfulness, compassion, tolerance, kindness, and honesty. His great wisdom and profound understanding of basic problems made him sought by many for his advice. He was partisan only to the truth and because of it, he received respect of all. He was the "Dean" of Radiology because of his sound judgment. As a man, he inspired great loyalty among those with whom he worked. He work his halos modestly.

**MILLER:** Stone felt strongly that there wasn't room for two medical schools twelve miles apart.

**HERKEN:** Although it seems somewhat curious that Stone would have been one of the liaison people between UC San Francisco and UC Berkeley.

**MILLER:** It was his work in both places.

**HERKEN:** I'm just thinking out loud here: I'm a little surprised that Stone wouldn't want to create his own center at San Francisco at the medical center and not have much to do with Berkeley.

**MILLER:** You see, timing is what this is all about. Early on, Stone did not have a bevatron, he didn't have a laboratory. The Berkeley thing had the big equipment for neutron therapy and the whole bit. That is why he would go to Berkeley. You understand?

**HERKEN:** Yes. Let me just mention a question Anna had asked earlier. Stone was one of the first to use neutron therapy. I think around 1947 he writes a paper and they abandoned this.

**MILLER:** He also was one of the first that did million-volt radiation therapy. He was the first one who ever used radioactive material in an attempt to treat a patient.

**HERKEN:** Sodium or phosphorus?

**BERGE:** Sodium.

**MILLER:** Sodium, exactly. It's funny recalling this stuff out of thin air.

**HERKEN:** A couple of letters I've seen from Stone suggested that at one point there was a rivalry. I don't understand this, because I need a radiologist to enlighten me on this: a rivalry between Stone and his Sloan machine, the bevatron, and new cobalt machines were being developed and used on the east coast that used 360-degree rotation of the radiation beam. Is that correct?

**MILLER:** I have no idea.

**HERKEN:** The case I saw involved Loran Gatter *(phonetic)*. Gatter was diagnosed as having a brain tumor, I believe, and he was initially treated by Stone in San Francisco, but then he went on to Mass[achusetts] General [Hospital to be treated] by one of the new machines.

**MILLER:** Normally, radiation therapy is given to the patient from two or more directions in order to give the tumor the most radiation with the least radiation to the normal tissue around it. With the 360-degree rotation the ratio of tumor to normal tissue dose is increased, and therefore there is less damage to the normal tissue.

**HERKEN:** This is actually a document that comes from the archive.

*(Herken hands Miller a document)*

**MILLER:** Is it in relation to large doses of radioiodine used for treatment of carcinoma or thyroid? A twelve-year-old boy was riddled with metastases from cancer of the thyroid. We treated him with 100 millicuries for five times. Today he is well. His father and I are dear friends, for good reason. This boy, now fifty years old or getting on to sixty, can break me in half. That was the one really shining example.

**BERGE:** Did you have any interactions with Dr. Paul Aebersold[75] at all?

**MILLER:** I knew him. He was working with the Atomic Energy Commission and that was the relationship.

**BERGE:** Are you aware that, I believe, it was him that committed suicide later on? Do you know anything about that?

**MILLER:** No.

**BERGE:** You don't know anything about that.

**MILLER:** I know he was a magnificent dancer. All of the women loved him.

**HERKEN:** You didn't happen to know Shields Warren by any chance?

**MILLER:** I knew him, but I never really was close to him.

**HERKEN:** I was just wondering. From what we're seeing here, this sounds like research.

*(Herken shows Dr. Miller a document.)*

**MILLER:** This had to do, I'm sure because it was large doses of iodine, it was with carcinoma of the thyroid. It had to be, because we were attempting to get the tracer dose down farther, farther, and farther. Of course, when you're treating a carcinoma, you have to hit it hard; otherwise, forget it.

**HERKEN:** This is for the Project [(Contract)] 48, so that answers that question. So, this would have been AEC-funded at that point.

**MILLER:** '47, okay. I didn't know anything about that. You were asking before why I felt I didn't want to be conducting this, because you have to do this kind of thing all the time.

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

## Part II: August 17, 1994

**BERGE:** This is an interview with Dr. Earl Miller by Anna Berge of the Lawrence Berkeley Laboratory Archives and Records Office on the 17th of August, 1994, at his residence in San Rafael, California.

**MILLER:** I haven't had this sense of paranoia about you, as I did about that guy.[76]

**BERGE:** The following interview is a continuation of an interview with Dr. Earl Miller on his work as a researcher and radiologist at the University of California in San Francisco.

Many of the following questions will be based on the history of radiology as seen by Dr. Earl Miller, which is a transcription of a recording on video of the history of the [UCSF] Department [of Radiology] as he lived it.

Dr. Miller, you made a number of references to different hospitals that you worked with. I was reading this [transcription of your video] and you mentioned, in the section on the growth of the department, a number of different other laboratories and hospitals. I was wondering if you could tell us a little bit about how your collaboration or the department's collaboration with other hospitals and other institutions worked—for example, San Francisco General Hospital, or any number of them.

## Use of Tomography to Diagnose Tuberculosis Patients

**MILLER:** Let's stay with the San Francisco General to start with. The San Francisco General was a hospital for indigent people. It's still, I think, called San Francisco General. It's quite a different place now. At the time I was there, I was there as a [Radiology] resident from Stanford [University's medical school, 1937–39]. [The] San Francisco General Radiology setup was run by both Stanford and UC. I had nothing to do with UC at that time; in fact, I didn't know much about it. That's where I met Henry L. Garland.

Anything else you want to know about it?

**BERGE:** I guess I don't understand how you worked with them.

**MILLER:** I was a resident in training at Stanford, then at SF General, and then back at Stanford, in the old Stanford Hospital at Clay and Webster.

**BERGE:** Did you ever do any work after you were a resident there?

**MILLER:** There, no. Although, somehow I got interested in tomography.[77] A Dr. Petrillo, I think—he was a radiologist—had one in San Francisco. Neither UC nor Stanford had one [(a tomograph)]. I went to the shop and

[76] referring to Dr. Gregg Herken, Ph.D., who had participated with Berge in the August 9, 1994 interview of Dr. Miller

[77] a method of making x-ray photographs of a selected plane of the body

built one. That was the time when tuberculosis[78] was a rampant disease. A whole hospital wing was devoted to the care of these unfortunate patients. [To test the laminograph,] I asked the doctors on that ward to send me their six toughest patients, those in which there was persistent positive[79] sputum[80] and no demonstrable cavities. All six were shown to have cavities in the lungs with the new gadget.

**BERGE:** What's laminography?

**MILLER:** It's body-section radiography. By means of an arm, *(pointing to different parts of his outstretched arm)* here's the x-ray tube, here's the patient, and here's the film.[81]

**BERGE:** Any other hospitals you worked with?

**MILLER:** No. Then I went back to Stanford in San Francisco. There was a dual arrangement where both Stanford and UC sent residents to SF General Hospital for study.

**BERGE:** You mentioned, for example, Moffitt Hospital.

**MILLER:** That's UC. There's Moffitt and now there is another one on the UCSF campus called Long.

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[78] an infectious disease that usually affects the lungs, causing small, firm, rounded nodules or swelling

[79] showing or indicating the presence of disease

[80] saliva mixed with mucus or pus, coughed or spat from the lungs or respiratory passages

[81] Dr. Miller gives a full explanation of laminography earlier, in "Technique to Produce Infinite Laminograms."

*(The six pages that follow were scanned from a printed document provided by Dr. Miller after the interview.)*

HISTORY OF RADIOLOGY .
UNIVERSITY OF CALIFORNIA AT SAN FRANCISCO

AS SEEN BY
EARL R. MILLER, M.D.
IN THE MID 1980'S

This document is a copy of the text used by Dr Miller
in recording a video-taped oral history of the Department
as he has lived it.

OPENING REMARKS

on page 3 of Beryl Markham's book
WEST WITH THE NIGHT
I found a quote that best describes this morning's activity:

"This is a remembrance, a revisitation;
and names are keys that open corridors
no longer fresh in the mind,
but nevertheless familiar in the heart."

## HISTORY OF RADIOLOGY UCSF

### EARLY HISTORY

Even in the early 1940's many doctors made their living in practices off campus. They either donated their time or were paid a pittance by UC. They worked only part time. Gradually, UC became peopled by full time employees with additional Clinical Faculty on a part time unpaid basis. There were no full time paid Faculty at the San Francisco General Hospital. The changeover to a full time faculty was gradual.

### HOWARD RUGGLES

He was the first Radiologist at UC. He came in daily in the morning and read the films which were ordered by the clinicians and taken by the technicians. He would read even a single film of an extremity for example. He wrote with Holmes of the MGH in Boston, the first major textbook of Radiology. It was THE textbook.... five eights of an inch thick and covered all of Radiological diagnosis and therapy.

Dr. Ruggles loved his greenhouse. During his time at UC, glass plates were used to record the x-ray images. He took the obsolete ones and built his greenhouse with them.. Look down, see a flower. Look up and see duodenal ulcer, Paget's Disease, Sarcoma of bone etc. Scary!

### DR, ROBERT SPENSER STONE MD LLD

Dr. Stone was the first full time Radiologist at UC. The Department was a branch of Surgery until about 1941. He was first chairman of the department after it became a department of the University in its own right. No one else in the department had a title other than his academic rank. Only the head of department could become a full professor. This was a common practice in many Universities at that time.

Before I came to UC, Dr Stone did both diagnosis and therapy as did most of the Radiologists of that time. He was the first to administer 1000 KV radiation and the first to use radioactive isotopes with therapeutic intent. He was proud of his work in diagnosis as can be seen from the encephalogram in his portrait, but his real forte was in therapy and radiation safety.

In 1942, Dr. Arthur Compton invited Dr. Stone to Chicago to set up and head the Radiation Health Division of the Metallurgical Laboratory. Later, this became the Manhattan Project, Later he went to Oak Ridge where he lived with unpaved roads in an an unfinished town and spent the War years there.

After the War he undertook neutron therapy for the treatment of malignancy using the 60" cyclotron in Donner Lab and later using the BIG machines on the Hill.
The AEC funded his Radiological Laboratory on the San Francisco Campus where he directed research with the sychroncyclotron and did patient therapy with this modality.

With the coming of Moffitt Hospital, he got funds to finance the 13th floor as a Cancer Research Institute. He got $1,000,000 for this ; a huge amount in those days. When the committees were formed to get the funds for the Moffitt Hospital the estimate for the whole project was $2,000,000.

He had a lovely home in Forest Hills in San Francisco. His garden was a showcase. He did all of his own gardening. He spent his vacations in the high Sierras, hiking and fishing, and doing photography. He enjoyed his weekly bridge game with friends.

THE DEPARTMENT IN 1940

The department occupied the east end of third floor of UC Hospital. Therapy had a 200 KV, a 1000 KV, and 1 low voltage machine along one corridor. There was one office and a small shop.

Diagnosis consisted of 3 radiographic rooms, 1 fluoro, 1 darkroom in 1 corridor, 1 reading room with 4 reading areas on the "poop-deck" (area above surgery) 1 small consultation room, 1 typist, 3 technicians, small file room, and Dr. Stone's office and place for his secretary. Dr. Capp and I were the staff. There were 2 residents. We all lived and worked on the "poop deck." The #1 chest room had a modern 3-phase generator. The #2 room had a mechanical rectifier and open bowl tube holder with exposed high voltage wires. Fluoro had discarded therapy tubes for rectifiers and no facilities for spot filming. Dr. Jim Irwin (resident) and I blew out so many of the rectifiers doing hand-held spots in fluoro that we finally got some decent equipment for spot filming in 1940.

GROWTH OF THE DEPARTMENT

I came to the department ion July 1, 1940. In 1941, I got an office in the old radon plant room (shades of old times.) I made my living during medical school years running a radon plant in the Department of Radiology at the University of Wisconsin.) Research with $I^{131}$ done here.

In 1950, Dr. Mike Shimkin brought a Cancer Research Lab to the Laguna Honda Hospital. In it was an obsolete fluoroscopic-radiographic room. Here, Dr. Howard Bierman and I did arterial catheterization and visualization of all of the arteries of all of the viscera of the whole body by introduction of catheters and opaque material in the brachial, carotid, and femoral arteries and directly into the arterial supply of the organ. Chemotherapy was carried out via catheter in the hepatic artery to metastases in the liver.

In 1951, Dr. Stone brought the Radiological Laboratory to the campus. The 70 million

volt Synchrocyclotron was housed here and was used for patient therapy and basic physics experiments. Radiobiology experiments were done here.
In 1953, radiographic equipment was installed in the Thoracic Clinic in the old Clinic Building. Here the first x-ray movies were carried out first using 16mm format and later the 35mm format. These were done with a lens manufactured specially by Eastman Kodak. Nine were made. Aperture F0.65. They weighed about ten pounds. Special glass had to be manufactured to be able to achieve this high aperture. Pediatric patients were studied because of their small size. The early studies of congenital cardiac anomalies were done here.

In 1958, President Robert Gordon Sproul was instrumental in getting me space for the Radiological Research Laboratory. It was first housed in the basement of the medical school building. 3200 square feet. When the Moffitt Hospital and the East and West Health Sciences Towers were built, this lab was relocated in the basement of the East Tower. The Lab consisted of a large well equipped shop, electronics lab, x-ray room, dark room, conference room, and office.

In 1960, H. C. Moffitt Hospital opened. Radiology occupied the third floor. Therapy occupied the East end with north-south and east -west corridors. Diagnosis occupied the big square block on the south side. The east corridor was used for in-patients and the west corridor for out-patients. Each had 4 radiographic rooms. The south side of the department had 3 fluoro rooms. All the films moved to the central double dark room area. They were wet processed by automatic machines now totally obsolete. The films were taken north to the file room, north again to the reading rooms and then north to the consultation room. Originally, four separate reading rooms were provided. These provided privacy but also provided isolation. They were replaced by a large single reading room. Offices occupied the north side of the main Hospital corridor. The Cancer Research Institute occupied the 13th floor of Moffitt. This was brought to the UC by Dr. Stone with funds from the Cancer Institute of the NIH. In it was an experimental x-ray room. Dr. David Wood was the first director. This was a large fluoro-radiographic set up. It was a patient service room used only for experimental studies. This room was originally designed to be used by Dr. Howard Bierman and me for continued work on cancer chemotherapy using intraarterial administration of drugs. Dr. Bierman left the University about this time and the room fell to me for my use. Here I had the first image intensifier-image-orthocon TV equipment. The rest of the equipment for this room was designed and built in the Radiological Research Lab. It was in this area that the physiology of the lower urinary tract was worked out using highly sophisticated equipment. We also studied the action of the mouth and pharynx during speech and swallowing in patients with speech abnormalities, mainly cleft palates.

In 1962, Dr. Margulis became chairman of the department. He developed the department as you see it now. This was the best thing that ever happened to the Department of Radiology at UC. Dr. Margulis brought a whole new attitude about what

(Page 4 was not provided; Dr. Miller could not be reached.)

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Radioisotopes: The only radioisotopes used in medicine when I started in radiology were radium and radon for the treatment of Cancer. Artificially produced radioisotopes became available with the advent of the Manhattan District and thus created a whole new sub specialty in Radiology.

Ultra sound: This was unheard of in 1940. Now, it too has become a sub specialty in its own right.

Image Intensification: made the use of television possible in Radiology and it in turn allowed x-ray movies to be done with acceptable patient exposure. This, in turn opened the way to the introduction of combined anatomical physiological studies.

Angiography: in all of its ramifications and the use of catheters for minute anatomical studies and interventional Radiology for therapy were unheard of in 1940 and now are a most important part of Radiology.

A Review of my Own Career in Radiology and some of the lesson I learned from it. My undergraduate and early postgraduate training was in physics and math. Because of this I was able to work in the Department of Radiology at the University of Wisconsin as physicist and earned my way through medical school running a radon plant.

#1: I am the luckiest man in the world. I often wondered why I shouldn't pay the University for the privilege of working in it and particularly in the field of Radiology where every case is a challenge and every day was fun.

#2: I learned that history is for the old. The old have only the past to think about, whereas the young have the present the the future to think about.

#3: One should prepare for retirement at age 13 so that you will be ready for it when it comes. Choose hobbies that do not require physical effort.

#4: Retirement is a time for review of one's career and that will be of interest only to the one who does the review since it does not matter to anyone else. Nevertheless, here goes: the only part that counts is the legacy of papers that one writes. Lectures disappear into thin air. Committee work gives a chance to know about one's colleges, but in general is a waste of time. Choose only the committees that further your own goals: in my case, education and research.

## Working in the Radiological Research Laboratory

**BERGE:** What kind of work did you do with the Radiological Research Lab?

**MILLER:** That was my life.

**BERGE:** It just had a fancy name.

**MILLER:** That's what I called it. The lab occupies 3,200 square feet, had a [machine] shop, electronics lab, x-ray room, dark room, conference room, and office space. In 1978, the regents renamed the lab the "Earl R. Miller Radiologic Imaging Laboratory."

**BERGE:** Are you feeling okay?

**MILLER:** Yes.

**BERGE:** Can you elaborate on what your research was like, in terms of how a general day was like?

**MILLER:** I did clinical work and teaching in the mornings, and I worked in the labs in the afternoons. There were two labs. The Radiological Research Lab was in the basement of the West Wing, [room] number 207. The Clinical Radiologic Research Lab was in the Cancer Research Institute on the 13th floor of H.C. Moffitt. Two half-days a week I worked up in the Clinical Laboratory. The rest of the time I spent down in the other laboratory.

So, the work up on the 13th floor was primarily the study of physiology of the lower urinary tract and the study of speech. We examined patients who had lower-urinary-tract problems, patients who had swallowing problems, and who had abnormal speech, usually due to cleft palates. These patients required the use of the sophisticated equipment which presented anatomic and physiologic data simultaneously on each frame of an x-ray movie, and on each frame of a videotape.

The basement lab provided the instruments and the know-how to make and use such equipment. Data from the Clinical Lab was analyzed in the basement lab because it [(the Radiological Research Lab in the basement)] contained sophisticated TV and electronic equipment for the analysis and dissection of images. Data was usually presented on the face of the CRT[82] tube of an oscilloscope.

[82] cathode ray tube, as used in televisions and computer monitors, consisting of a vacuum tube generating a focused beam of electrons, which illuminate phosphors on a screen to form a visible image

## Investigating How Radiologists See Images

**MILLER:** I was concerned with, and interested in, interpreting the data from the 13th-floor lab. We would run the movies over and over again and interpret the information on the lower urinary tract and the speech path. I was particularly interested in how a radiologist saw things.

To "see" has two definitions. One is "to perceive" and the other is "to understand." My interest in this grew out of a study in error interpretation: "What is it that got in your way?"

In the process of this, we set up a television arrangement looking at x rays or any kind of image. We were able to isolate a single line of this information [on an oscilloscope]. Ed McCurry did the engineering work. You could see what line was being studied. This showed the density across this line. Not only that, but [we] were able to differentiate the rate of change. For example, one of the things that I beat my head about forever was the realization that the contrast gradient[83] was most important.

Also, [I sought to investigate,] "What do you mean by 'an image of something'?" An image of something has to have an edge, and you have to understand what you mean by an edge, in order to be able to talk about an object. An edge can be sharp or not sharp.

Suppose, for instance, you're looking at an infinite[ly wide] wall, and to the left it's bright and to the right it's actually black and it's uniformly changing. There's contrast there, but no edge. This is due to the fact that the contrast gradient across the image remained constant; therefore, no edge. An edge appears when the rate of change of illumination differs from that of the surround. The sharpness of the edge is defined by the rate of change of luminescence per unit distance in the scene and ultimately, in the eye.

The Zones of Confusion of all parts of an image-producing system have a profound effect of the sharpness of edges and the ability to define an object. Defocusing images enlarges the Zones of Confusion and has a profound effect on [the radiologist's] ability to define an object. Viewing distance plays an equally important part in scene interpretation.

These studies were the life of the lab.[84]

I don't know if there's anything more to say.

**BERGE:** I was just trying to look at what years. That must have been the 1950s, is that right?

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[83] the rate at which the contrast of the image changed per unit of distance across an x-ray image. See Dr. Miller's elaboration, which appears two paragraphs later.

[84] For more on the role of contrast in interpreting an x-ray image, see Dr. Miller's paper, "A Multiple-Film Technique for Contrast Enhancement and/or Reduction of Patient Exposure," in *Radiology 110:2* (February 1973).

**MILLER:** Actually it was the last years until I retired [in 1974]. A lot of this stuff was never published, because I felt that either I could understand it all or I couldn't understand any of it. I may still write some of this stuff.

We can look at a thing that's in the reprints. What I talked about at a postgraduate course is the most important. It has more information about what the whole thing was about, more than any published paper. What I just told you about the second derivative—that is, the rate of change per unit distance across an image, across an edge—was, I think, new. At least, I had never heard anybody talk about it. Nor had they considered this idea of this huge image that doesn't have any edges.

Another aspect of this was particularly interesting. One of the people that worked in the lab had a poster with two pictures on it. The pictures were identical. The text was in English on one and in Russian on the other. Now the images of the pictures and of the text were identical in sharpness. The fact that I didn't understand the Russian meant that I could never understand the picture. This illustrates that even though you have an excellent, sharp picture, you may not know what it means.

The latter is what a layman does [when] looking at an x ray. It's all there, and he can "see" it with his eyes, but he can't "see" it with his brain. That's what separates the layman from the expert in any field at all. It turns out that this rate of change that I was talking about applies to all senses—touch, taste, smell, sight, and hearing. Maybe [it applies as well to] ESP[85] for all I know. I don't know what else to say about it. I'll show you the one thing on there.

**BERGE:** Are you okay?

**MILLER:** Yes.

**BERGE:** Thanks. *(sips from a drink Miller has provided her)*

**MILLER:** Is that good?

## Establishment of the UCSF Radiation Laboratory

**BERGE:** Yes. Question: You mention here that the University of California San Francisco Hospital started out as a clinical facility, and somehow it gravitated towards research. I was wondering if you could say more about that. It's very sketchy here [in the transcript of your video].

**MILLER:** Originally, before 1940, UCSF was a proprietary medical school with unpaid volunteer teachers. With the coming of a full-time paid staff, research grew. Personally, I guess all my life I was interested in research. I was characterized by a curiosity. In other words I wanted to know *how* and *why* a thing worked. When I was given an office I got some radioiodine. I turned it into a lab, and then I tell the story of how I happened to get the lab.

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[85] extrasensory perception

Every young going guy was offered chairmanships of Departments of Radiology all over the country. I don't know how many I was offered; it doesn't matter. Finally Columbia [University], in New York came through. They offered me a three-story research building. I have lived in New York and also I had been chairman of the department. I didn't want either one again.

Through Dr. Stone, I had lunch with the president of the university and I said I wouldn't take that offer if I could get a research lab [elsewhere]. I wasn't going to take any offer [at Columbia] anyway. At any rate, they thought I was worthy of having around. Dr. Sproul said, "I'll get you a lab." Later he said, "We've got money for a lab, design it." I had a ball trying to design that.

Anything else?

**BERGE:** What kinds of considerations were you looking at when you designed the lab?

**MILLER:** I wanted a first-class shop, because we needed to make things. I needed an electronics lab, x-ray room, dark room, conference room, and office [space]. I needed good TV equipment and a complete electronics shop. The conference room was used to interpret data from the 13th-floor lab. The shop was used to make instruments and gadgets. The electronic lab was used with the TV and electronics to study the characteristics of images and edges and the effects of defocusing of the ability to identify an object.

The basement lab developed the equipment for use in the 13th-floor lab. I had the support of Ed McCurry, electrical engineer, and Bernie Hruska, super tech[nician], without whose dedicated help these studies would never have been made.

## Remembrances of University Presidents Sproul and Kerr

**BERGE:** You mentioned that you were able to sit down with President Sproul. Was [the University] small enough in those days that you could set up an appointment and speak with him, or did you have a prior appointment?

**MILLER:** Dr. Robert Gordon Sproul was probably the most amazing person that anybody ever met. He never forgot anything. He knew intimate details of people in the San Francisco campus that he almost never visited. He knew things about it.

It was through Dr. Stone that I was able to have lunch with Dr. Sproul. Because Dr. Stone apparently would like to have me around, and there was a chance that I might leave, I needed to talk with somebody that had enough power to get enough money to set up the lab I wanted. It was through Dr. Stone and the dean that I got the appointment with President Sproul.

I remember that Leon Goldman—that's Dianne Feinstein's[86] father—needed an electric typewriter. Those were the days when people were poor. Dr. Sproul got him an electric typewriter. And one day they met somehow and [Sproul] said, "Did you get your typewriter?" He said, "Yes." An amazing guy.

Did you ever know of Clark Kerr, who was also president of the University after Sproul?

**BERGE:** I don't know much about him.

**MILLER:** Maybe this is off the subject.

**BERGE:** If you want me to keep it on, I don't mind.

**MILLER:** This has to do with Clark Kerr. We went down to a UC conference and seminar. It was an open session: faculty were allowed to ask any question. Some guy got up and was going to embarrass Clark Kerr. He gave him something like a thirteen-part question. Kerr listened, and this guy sat down with a smirk on his face, and Clark Kerr repeated each of his questions and gave the answer right down the line! There was a standing ovation. We couldn't believe it.

## Early Career

**BERGE:** That was lovely.

I thought you might talk a little bit about some of the people you've mentioned in here that you are grateful to. For example, let's start off with Stone, because he's earlier, and then maybe move on to Dr. Margulis.

**MILLER:** I remember when he gave me the job.

**BERGE:** He must have thought you were worthy of keeping around if he went to President Sproul for you. Why do you think that is?

**MILLER:** When I came to UC, I had just finished my instructorship under [Hugh] Wilson at Yale. I had just passed the boards and I was an AOA.

**BERGE:** AOA?

**MILLER:** Alpha Omega Alpha, an honor society composed of the top ten percent of medical school classes. And Dr. Stone needed an instructor. He knew I had been over at Stanford with Dr. Newell, so he offered me this job.

In those days I was hot. When you're just out of your residency, you know more than you ever do again in your life. I had a good time. I did a lot of work on developing a teaching file, and a lot of work doing teaching, after the usual workday, from 5:00 to 7:00 every night. He pushed me, as far as promotions, very hard. He gave me an office. Nobody else had an office. Whatever it was, we got along well.

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[86] Senator Dianne Feinstein (D-CA)

**BERGE:** I noticed you mention, later on [in your video transcript], Dr. Margulis. Can you talk a little bit about him? I know nothing about him, by the way.

**MILLER:** I met Dr. Margulis one time when I was a part of the residency review committee of the AMA.[87] It was in St. Louis. When Stone was going to retire, consideration was given to a lot of people. There were some that had made me shake in my boots. When Margulis's name came up, I remember Maurice Sokoloff called me and said, "What do you think about this guy?" I said, "You can't get a better one."

Then, I described in this thing, too, how he developed the department. I think there's little doubt about the fact that it was due to Dr. Margulis that he developed a residency program, a postgraduate program, a post-doctoral program that was as good as any in the world, and I think maybe better than many of them in the world. Tremendous guy.

We disagreed on some things; that is, I thought things ought to be done this way and he thought they ought to be done his way. He was right in every case. He made a few mistakes. They were beauties! But he made very few of them.

I learned one thing about him. One thing to do is to make a decision, go ahead with it, [and] if it's wrong, change it. And he did that. He was a master at that. I don't know what his IQ was but I think it was over 1,000 [sic].[88]

**BERGE:** What was his specialty?

**MILLER:** He was a diagnostic radiologist with particular interest in the gastrointestinal tract. He did fluoroscopy[89] one day a week.

## Work Through the AMA to Improve Radiology Training

**BERGE:** You make a statement here, let me read it to you: "Many residencies in Radiology were established in private hospitals and in private practices of radiology. A number were used as slave pens for the trainees." I wonder if you could talk a little bit about that.

**MILLER:** That was a common thing. You got a resident in Radiology, like an apprenticeship. He would take all of the night calls; he would take all of the weekend calls, and they would use him as a "gofer." With that, he had a chance to watch the master at work. In watching the master at work he became "a radiologist." Then he took his boards, and then he did the same thing to the next person.

**BERGE:** What were some of the common places they did their residencies?

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[87] American Medical Association

[88] Dr. Miller is exaggerating, of course; the highest intelligence quotient ever recorded on the Stanford-Binet test was below 230; only one person in 50,000 has an IQ of 170 or higher.

[89] the use of a fluoroscope (a tube or box fitted with a screen coated with a fluorescent substance, used for viewing deep body structures by means of x-ray or other radiation)

**MILLER:** There wasn't any place that didn't have them. Many of them were in the private hospitals, just all over the country.

There were only two aspects of service to the NIH,[90] to the AMA, and to the College of Radiology that interested me. One was research and one was teaching and education.

I got involved with this matter of the evaluation of [Radiology] residency programs across the country. In the process of this, we, as a committee, visited almost every residency program in the country. We did just what the Flexner Report did with the medical school program. It revolutionized medical practice and teaching. We did exactly the same thing with the residency program. We would write up an evaluation.

That's where I met Dr. Margulis. We would write up an evaluation, and either it was approved or it wasn't approved. If they lost their accreditation it was a very severe blow. We were the power that could get a Radiology department upgraded in a way that the head of the department couldn't. He would go fight with the superintendent of the hospital; he needed equipment etc., [for example]. "You got plenty," [might be the superintendent's reply]. We'd come in and say, "You don't have enough equipment." The superintendent would come running down [to the department head]: "What kind of equipment do you want?" It changed the face of radiology. We were a group of academic radiologists, chosen for this particular work.

## Rise of Radiology Specialization

**BERGE:** Soon after that, you said, the first beginnings of specialization were in the separation of diagnosis from therapy. Before we get into that, what period of time was that that you were talking about there?

**MILLER:** I would think in the late '40s and early '50s.

**BERGE:** Like I was saying, you said the first beginnings of specialization.

**MILLER:** I think I also mentioned that most Radiology departments in the early '50s were run by people who were therapists. That was the one thing that distinguished the radiologist from the general practitioner or urologists or other practitioners of medicine. At that time, the people interested in orthopedics,[91] urology,[92] heart disease, etc., all had x-ray machines, and they did their own interpretations. Then there developed a group of young people who began to specialize in their studies. For whatever reason, and I think part of it was these residency review committees, the Board of Radiology established diagnostics as well as therapy as separate. You could become a therapist or you could become "a

[90] National Institutes of Health, Bethesda, Maryland

[91] the medical specialty concerned with correcting deformities or functional impairments of the skeletal system, especially the arms, legs, hands, feet, and spine, and their associated structures, such as muscles and ligaments

[92] the scientific, clinical, and surgical aspects of the study of the urinary and genitourinary tract

radiologist [(a diagnostician)]." For instance, I'm a radiologist. When I had my exam in 1940, radioisotopes, therapy, and diagnosis were on the [licensing exam] questions. All phases. Today, forget it.

**BERGE:** A lot of that was a function of all the discoveries they made during that time. You couldn't be a radiologist and know about all of what you just described.

**MILLER:** Yes, we could! The amount was small then. There was more than that. What happened was that there came a time when the American College of Radiology (or the AMA) examined and visited private doctors' offices that had x-ray equipment. I was on these committees. If the machines were substandard, they couldn't have x-ray equipment. The radiologists became so good at the business of diagnosis that this private-practice part of the thing just died off. But it's not dead yet. I went to an orthopedist recently. He sent me to his own technicians for x rays and he interpreted them. I gave him some help.

Orthopedists, chest people, urologists, still do have some of their own stuff, but what happened was with the research; I think that is what you're referring to. It got to the point where there were things to do in every phase. For example, we fooled around a little bit with ultrasound,[93] and I didn't think we'd ever have teachers [in that field]. Now, it's whole departments. We fooled around with radioisotopes, and now there are whole departments of Nuclear Medicine and there's Neuroradiology, Intervention Radiology, etc. There's CAT-scan[94] radiology. There's MRIs.[95] Pediatrics, neuro[96] specialists. Somehow it just grew like Topsy.

We can take a look at a department at UC and other good universities and see the same thing. You see now a list of specialized activities; it's a mile long. When I started, there were four people in the department. When I checked it up a couple of years ago, there were over 200 people there. I don't know how many people there are now; it grows exponentially. There are whole fields of this clinical practice, and we have a research arm of every one of these specialties.

## Study of Pediatric Patients With Congenital Heart Disease

**BERGE:** I'll be done in just a second. *(flipping through the video transcript)* There was one interesting—if I can find the page—one interesting little sentence in here that I thought you could tell about. Maybe there's a funny story behind it. It's about the pediatric patients and small size.

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[93] the practice of reflecting ultrasonic waves off interior body structures to produce a visual image, or sonogram, for diagnostics

[94] computer aided tomography; or, an x-ray image obtained by examination with a CAT scanner

[95] magnetic resonance imaging (a process of producing images of the body regardless of the presence of bone by means of a strong magnetic field and low-energy radio waves)

[96] relating to nerves or the nervous system

**MILLER:** This was the work that was done on the study of the x-ray movies on the patients with congenital heart disease.

**BERGE:** What was his name?

**MILLER:** Dr. Mary Olney. The only ones that were chosen for study, were those whose life expectancy was in terms of weeks—at most, months—and they were tiny.

**BERGE:** I'm sorry?

**MILLER:** Such serious congenital disease that they couldn't live.

**BERGE:** What were they being studied [for]?

**MILLER:** These were the ones that were sent for study. Angiocardiography[97] was in its absolute infancy. We could inject the opaque material and take serial films. You'd get maybe five films or a few more. From this you had to try to interpret just how the flow occurred. In order to do this correctly or efficiently and effectively, you had to see all the phases, which meant movies. We didn't have image intensification. We didn't have any television at that time. We got special lenses to try to get the films.

The whole thing had to do with the reduction of dose. If we had taken adults and tried to do this with movies, we would have burned holes in them [from the high x-ray dose required]. In the case of the children, being infants and newborns, they were so tiny that one could use much much smaller dose, because they were so small.

It was to get the dose down, and to be able to study all the phases of the heart motion, you had to have movies, and this had to be done with main strength and awkwardness. We tried to get better lenses or better cameras and all this sort of thing. None of this did any good until image intensification and television. *Now* you could take movies of even adults through the belly and show the results without overexposing the patient.

**BERGE:** The way it was worded here, I was [mis]understanding it. I thought you needed them for the small size because you couldn't get a bigger-size glass [focusing lens] or something.

**MILLER:** That played a part. It meant that you could use a very small field.

**BERGE:** I don't understand why you had to use terminal patients.

**MILLER:** Because, number one, nobody knew much about congenital heart disease at that time and they were going to be exposed with high doses of x ray.

They had physical examinations and they could listen to the child; they could take a history and see how blue they were, etc., but they didn't really know what was going on. They didn't realize how many kinds of congenital heart disease there were. They understood a few, such as

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97 x-ray examination of the heart and its blood vessels following intravenous injection of radiopaque fluid

atrioseptal[98] defects, ventriculoseptal[99] defects, etc. There are hundreds now. This was the beginning of trying to understand some of the kinds of congenital heart disease in live patients.

Now it's hard to realize how little was known at that time. This was at the cutting edge. This type of thing was going on in a number of universities, [University of] Pennsylvania and Harvard, the good ones.

**BERGE:** Was this going to be diagnostic?

**MILLER:** Yes.

**BERGE:** [I get] a little confused about how everything is used. I may ask some silly questions some times.

**MILLER:** Therapy and diagnosis. In therapy you use the radiation to treat patients; in diagnosis, you want to give the least amount of radiation in order to understand what's wrong with the person. That's the difference.

**BERGE:** I'm interested in how your interest developed from the radioiodine studies to, later, the urine studies.

## Physiologic Studies

**MILLER:** These were totally separate. The urinary studies were really physiologic studies of patients. The work with radioiodine was both diagnosis and therapeutic. It dealt with reaction of the thyroid to $^{131}I$.

The first part of the work dealt with the problems of handing the stuff, measuring the radiation from it, developing means of recording its accumulation and excretion from the thyroid, and developing units to express the dose. We used a radium standard for this.[100]

My primary job was x-ray diagnosis. As such, I directed examinations and interpreted the results. The diagnostic radiologist's job was looking at the x-ray films and together with the history, and [based on] what he saw, to come out with a diagnosis. It was apparent to me that this was always incomplete, because you had information about the anatomy; you did not have information about the physiology of the organs under study.

In order to do that, you needed to have movies, so that you could watch the anatomical thing, and you needed simultaneous information about pressures, size, shape, differences of pressure, and how one affected the other, etc. I became interested in the physiology of the lower urinary tract, and in the mouth and pharynx during speech and swallowing. These were ideal areas for the development of the combined anatomic-physiological studies. My interest was in trying to get the most informa-

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98 pertaining to the division between the two thin-walled upper chambers (atria) of the heart

99 pertaining to a partition between the two ventricles (lower chambers) of the heart

100 At that time, no radioisotope had been more thoroughly characterized for its biomedical effects than radium.

tion for the least exposure to the patient. This meant that you needed all of the anatomical, radiologic anatomical, and all of the physiological things simultaneously, over a period of time.

The patients with urinary problems were women with incontinence,[101] inability to start urination, or recurrent infections due to incomplete voiding. The speech patients were those with nasal speech due to cleft palates or unknown causes.

**BERGE:** Why women?

**MILLER:** The woman has a short urethra; the man has a long urethra. We just gave up trying to study men; we couldn't do it. We *could* do women. Women are more prone to urinary problems.

For the study of the urinary tract, we developed a system, including TV transduction of the fluoroscopic image of the bladder and urethra, and the output of eight channels of strip-chart[102] recordings of the physiological data simultaneously as a function of time. Sound recorded commands and responses. The strip charts provided material for further study. Movies were taken of a separate monitor. The whole thing was recorded on videotape for immediate replay and study.

The bladder was filled with opaque material. Catheters were placed in the bladder, urethra, colon, and anus. Squeeze pressures of the urethra, centimeter by centimeter, were made at rest. The patients were asked to strain, to "hold" urine and, ultimately, to void.

From these data we gained a complete understanding of the relationship between the various forces involved in continence, incontinence, and difficulty in starting urination. These studies opened the way for rational treatment of the various ills.

For the speech studies, we used [a] lateral view of the mouth and pharynx during speech and swallowing. Sound and sound spectrography were shown on each frame of the movie and videotape. As a result of these studies, rational surgical procedures for the cure of the nasality[103] were developed.

**BERGE:** Thank you. ❑

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[101] inability to restrain natural discharges of urine or feces

[102] a printed stream of paper, usually perforated for subsequent separation into sheets, that plots variables against time and emerges continuously from an instrument

[103] the pronouncing of oral sounds as nasal sounds

# Appendix

BRIEF HISTORY EARL R. MILLER, MD.

[redacted]

1925-1931: BA and MA (physics) University of Wisconsin, Madison, WIS

1931: Lab Tech in Biophysics Lab; Rockefeller Research Institute New York City, NY

1932-36: MD from Medical School University of Wisconsin (AOA). Physicist, Department of Radiology; and ran the Radon Plant; Madison, WIS

1936-39 : Residency in Radiology, Stanford University Hospital, San Francisco, CA

1939-40: Instructor, Radiology, Yale University, New Haven, Ct.

1940: Board Certification in Radiology

1940-49: Instructor to full Professor, University of California Medical Center, San Francisco, CA

1943-45: Chairman, Department of Radiology, UC Medical Center, SF, CA

1943-45: Director, Health Physics, Manhattan Project, Berkeley, CA

1950- : Fellow of the American College of Radiology

1954-57: Member, Board of Chancellors, American College of Radiology (AOR)

1957-58: Chairman, Board of Chancellors

1958-74: Director, Radiological Research Laboratory, UC Medical School, Department of Radiology, San Francisco, CA

1957-65: Commission on Education, ACR

1960-65: Chairman, Commission of Education , ACR

1960's...1970's: Three terms on Research Panels on Radiology, NIH

1972: Gold Medal, American College of Radiology

1974: Professor Emeritus, UC Medical Center, San Francisco, CA

1978: Regents named Earl R. Miller Radiologic Imaging Laboratory at UC Medical Center in my honor, San Francisco, CA

E R Miller's residency and career at UC

This is worth discussing, I think, because things are so different now.

I graduated from the University of Wisconsin Medical School in 1936 (AOA)
I worked my through School running a radon plant and acting as Physicist for the department of Radiology under Dr. Pohle, who did the therapy for the Department .I met Dr Lester Paul there. Bill Middleton, Professor of Medicine, pointed to Dr. Paul one day and said "There goes 80% of the diagnoses made in this Hospital" It was then that I decided that I would be a Radiologist. I spent one year in New York working as a technician in a Biophysics Lab at the Rockefeller Research Center. New York was a wonderful, safe, interesting place in 1931.

I interned in Research Hospital in Kansas City Missouri ( a town run the BOSS Pendergrass. Interesting) I had only two full nights of sleep during that year.

I spent two years at the old Stanford Hospital and SF General for residency. They offered only 2 year training courses. I spent 6 months in therapy under Dr. Robert Newell, and 6 months in Pathology under Dr. Bill Dock and Dr Dave Wood.

My medical research started at this time. At every autopsy, the coronary arteries of the deceased were injected with an opaque material. Dissection was made ala Schlesinger technique and radiographs of the specimen were obtained. It was shown that arteriosclerosis was a ubiquitous disease of the arteries and that a single blockage was rare.

I spent 6 months at SF General under the great Harry Garland who taught by scarification (Scare and Scar). I learned a lot from him. The last 6 months were spent at Stanford simply watching the practice of Radiology in a relaxed fashion.

I applied to Yale for a third year residency and was given an instructorship and a living wage. (WOW) By this time I was married and had a small daughter. New Haven was beautiful and we were lucky to have a great house there.

Hugh Wilson was Boss at Yale and he opened my eyes to what the residency and the practice of Radiology was all about. He was the sharpest radiologist I ever met. The morning started with a conference at which the cases of the day before were presented by the residents who had checked the films and had rendered a report.
Dr, Wilson never missed that conference and he never missed a diagnosis that I know of. He too taught by scarification. Woe unto any resident who flubbed a case or didn't get the proper films for the examination.

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Watching these residents at work made me aware of the fact that I really knew nothing about Radiologic diagnosis and that I had better get to work and learn something.

The Journal of the American Roentgen Ray Society and the Journal , Radiology, were THE Journals in which "all" radiologic literature was published. I decided that if I read one half of an an annual bound volume each night from one of the Societies, published during the past ten years, I probably would be ready for the Board exams and I wouldn't be such a dunce. Frequently I was reading at 3 AM but I did get through them. The Board exam was a breeze.

Dr. Robert Stone had offered me an instructorship at UCSF in July 1940.at $3500 a year. He was tight with a buck but at least I had a job. Dr. Stone was a therapist and he needed a hot-shot kid to fill in in diagnosis. We got along well. I had a ball working there. It was so much fun that I always thought that I should pay the University for the privilege of working there but I always cashed their checks. Dr. Stone supported me strongly and through him I became the youngest full Professor on that campus. He also made it possible for me to get the Radiologic Research Laboratory in 1958. He made it possible for me to work there half days. That became the joy of my life.

3/16/78

## RECOLLECTIONS OF AN OLD CROCK
by
Earl R. Miller , M.D.

I was [redacted], the year of the Great Panic. I presume that there is a connection, but I prefer to ignore it. My elementary school days were passed learning how to get beaten up. I remember using the word "wafted" in a sentence one day in the eighth grade. I hurt for days after that.

My mother died when I was thirteen years old, and Dad was broke after her seven-year illness. During High School, I became chief cook and bottle washer. Cordon Bleu I was not, but we survived. During the summers, I worked in a foundry, in an ice house, and a pea canning factory as a weigher. I was given the weigher job because I could count.

About this time, I met Eddie Krumbiegle who lived on 7th Avenue near Greenfield Avenue. That was about 4 blocks from where I lived. We were about the same age. Except for my Dad, he became one of the most influential persons in my life in boating, scholarship, and music.

During high school days, we started to see a lot of each other, mainly at my house. We built a sailboat together, a 17' flat bottomed, center boarded, sloop rigged sailboat with a bow sprit forward and a boomkin aft. We even made the wooden spars with draw knife and spoke shave. We launched it one summer day in Lake Michigan on the North Side beside the beautiful yacht Christina owned by the Pabst Brewing company family. On the day of the maiden voyage, the Captain of the Christina, recognizing that we were novices, asked if he could go along for the ride. We jumped at that, and he took the helm.. I will never forget my sensation when we raised the sails. The boat became alive and I was hooked on boating. We had an offshore breeze and sailed South on a broad reach along the shore. What a thrill! About a half hour out, the starboard turnbuckle let go and the mast tipped to port. The Captain flipped the boat around on the port tack and sailed us home, luffing all the way. We replaced the turnbuckle with one that would had worked well on the Queen Mary if she had had sails.

I became so involved in boating that I owned 13 boats in my lifetime. They were mostly sail boats: monohull, catamaran, trimarans; some had inboard, outboard, and inboard-outboard engines on motor boats. Years later I designed and built a catamaran in my garage (yes, she did come out of the door, with minor reconstruction of the garage door.) Built along the lines of a Tiger Cat, she was seventeen feet long and, at one time we got eighteen measured knots out of her. What a thrill! That boat almost cost me my life twice. The first time happened when I was pouring flotation into

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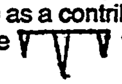
the bows. The directions say that the material is dangerous and must always be used in the open air. No one believes directions, and it was cold outside. I began to cough a little, and then the phone rang. The phone was at the top of the stairs in a closed off space. There the coughing became really severe and I suddenly realized the the smell was that of cyanide, and I could see the green room in San Quentin. I dropped the phone, ran outside, and survived, but now I read directions. Later, under the Richmond Bridge, north of San Francisco, in a following sea, the aft hatch covers floated up enough to let the aft end of the boat swamp. I was sunk up to my chest and the boat floated at that canted level. A tug from the Long Wharf (Chevron) came by and picked up my passenger and me. We got aboard and I was given a cup of hot coffee. I remember trying to hold it still enough to drink. I was shaking so much that I had to hold it in both hands. It was practice for the time that I might become a drunk. The flotation almost killed me, but then it saved our lives. Even Steven!

One day, I was up in Pt. Reyes, California, duck hunting. I went into town for a bottle to give to the club. On the way, there was a Lion's Club bazaar, and on the field, I noticed what seemed to be a boat on top of a boat. I went over to examine this strange structure and found that it was a foldable 24 foot trimaran with the sponsons folded on top of the main hull. The hull was full of water, so I figured that if it would hold the water in, it would hold the water out. Only the hulls were there. The price, after $40 had been crossed out, was $15. I decided then and there that I had to have that boat. I went back to the club and one of the men, Toby Giacomini, agreed that he would deliver it to Sausalito if I bought it. I immediately made out a check for the $15, and then wrote a check for the Lion's Club as a contribution...But I wanted that boat for $15, for the following reason: I named it the  which is the appearance of a trimaran from the aft end but also was the price of the boat....in Latin. How erudite can you get?

Later, I fell in love with a beautifully built 28 foot trimaran equipped with an El Toro dinghy. I couldn't resist it. I still had the catamaran and the 24 foot trimaran so, I had four boats with nine hulls. There are two solutions to that one.

The bigger tri was a great joy. The kids used to dance on the deck. In the summertime, the sun does not go down till about 9pm. I would leave the office, take the twenty minutes to get to the boat, go out on San Francisco Bay, watch the sun go down, and then see the lights come up in the City. Dazzling sight!

One day, Bob Moseley, then Chairman of the Radiology Department of the University of Chicago, came aboard for a day of sailing. He was very tired from a long stretch of hard work. The boarding was celebrated by a Bloody Mary, followed by undiluted Beefeaters for the rest of the day. I have a picture of Bob with the drink blowing out of his glass as he manned the helm. One of the advantages of a trimaran is that, while your drink may blow over the side, it will never tip over. Preparing to come into berth, I started to put down the sails and start the kicker. Bob asked me what I was doing... "Every good sailor sails into the berth." he said. I said, "Go ahead." Without a minute's hesitation, he took the helm and brought the boat into a perfect landing. About three o'clock, the next morning, he woke up in a cold sweat thinking about this and never forgave me for letting him do it.

I was interested in the mind of Russ Morgan at work. He was a graduate Electrical Engineer and Professor and Chairman of the Department of Radiology at Johns Hopkins. He came aboard one day, having never been on a sail boat in his life. After we had started, I told him to take the wheel. He did, quietly as always. He looked up at the sails, turned the boat both into and off the wind while studying the action. I warned him only about gibing and explained that maneuver and its consequences, again only once. He sailed the whole day like a pro.

Enough about sailing.

There was another way in which Eddie Krumbiegel enriched my life. He played the piano. Both he and I had had the usual lessons. The results were nothing spectacular. We attended dances at the Wisconsin Roof where there was a piano player who became the soloist. Some time each night the rest of the big band pretended to get into a squabble and one by one they left the stage. The last one off put a huge megaphone over the piano player's head and he started to play. All the dancing stopped and the assembled mob stood close to watch and listen. We were entertained by a unforgettable jazz concert.

Eddie decided to take lessons from this man. He charged $10 for a half hour session, a huge sum in those days. He taught the fundamentals of harmony and a solid left hand using 10ths in the base. I listened to the change in Eddie's playing and decided that I too would go to the man for lessons. Using the money that I was earning from giving lessons on the very popular ukulele, I took six piano lessons. This experience changed my life. I now understood how music was made and from that time on I could make music. During College days, I played piano in dance bands at Fraternity parties and for awhile I held a job accompanying Bruce Will, a singer buddy of mine, on the local radio station in Madison. That paid $20 each for a half hour's work.

I also learned an economic lesson while teaching the uke. With time I gathered more and more students. I decided to double my fee from $3 per hour to $6 per hour in order to reduce the number of students and have more time for myself. Instead the number of students doubled. The lesson was that if I charged double I must be worth double. But I didn't change.

Our families couldn't send us away to College so Eddie went to a local normal school and I went to the University of Wisconsin Extension Division in a dreary office building in Downtown Milwaukee. Three nights a week I worked in a bowling alley setting up pins for five cents a game, and no tips. I developed a strong back and an acquaintance with Acey Deucy, a fast betting card game. I had a chance to watch some of the pros at work in this game and have never gambled with people I didn't know since. Dinner was usually a one pound bowl of "Chili" which sold for fifteen cents. I can't look a bean in the face since, and the feeling is mutual. I nearly flunked out that first year at the college level. The only thing I remember clearly was a statement made by the professor of Chemistry. He came from Waukesha, Wisconsin, the home of the "World's Finest Spring Water." "The best drink in the world," he said, "is one drop of Waukesha Spring Water in a barrel of Whiskey." So much for higher education.

My family took a long motor trip during one summer and Eddie and I played loud music at my house with pick-up bands. Since the windows were all open, I'm sure the neighbors wanted to kill us.

Summer ended and I decided to go to the same school that Eddie attended. At least there was a campus there with playing fields and girls. I learned that Eddie was a straight A student. I remember the day I said to myself that if he could get all A's, so could I. So I did.

Eddie went on to Medical School at Marquette where he was considered to be the best student they ever had there. For example, one day an examination was given. Eddie turned in his paper at the end of the first hour. He was informed that it was to be a three hour exam. He received the highest mark in the class. He went on to be elected and reelected to the office of the Chief of Public Health in Milwaukee where he served with distinction for the rest of his active life. I went on to Madison and we lost track of one another.

College days.

Madison, beautiful Madison, became my home for a number of years after that. I studied physics and mathematics there. Being poor, I lived in the dorms that were provided by the University, lovely, large stone buildings on the shores of Lake Mendota. We poor people lived like kings. Being poor during summer school in Madison is like being rich on the Riviera.... in spades and bikinis! The thirties were depression days, but there were some compensations. Filet Mignons sold for ten cents a steak! When barbecued over an open fire on Picnic Point, they were delicious indeed. When lighted by the spectacular Mid-west sunsets, all was right with the world!

Pocket money was made by playing in dance bands at fraternity and sorority parties. It was interesting to see how the other half lived. I remember getting a prize in a French class: the booby prize for having the worst pronunciation the professor had ever encountered.

One day, the great Dr. Warren Weaver, later Head of the Rockefeller Institute, teaching statistics to about 25 post graduate students in physics and mathematics in Wisconsin in 1930, said to us, "Think of a number." Pause. "I'll give you a hundred to one odds that the number is less than 10 to the 18th." He was a man who thought "BIG." I never learned the art, but I've met some since who thought the same way.

Professor Wahlin, during my post graduate study in physics, gave me a task of finding forbidden transitions in the infrared spectrum of copper and suggested that I use an interrupted copper arc to bring them about. It was fun making up the apparatus, but a little disconcerting when the lab burned up one night. Nothing much was hurt because the lab was in a concrete hole in a basement room which housed a Bowen 21' grating where the spectroscopic work was done. I tried again, and after monitoring the arc for 365 hours (I remember that because it was exactly the number of days in a year), and getting no data on the photographic plate, I decided that if I had found all the forbidden transitions in all of the spectra of all of the elements, it would not have made any real difference to PEOPLE.

After a short talk with my professor who seemed relieved that I was leaving, he suggested that I might go over to the Medical school and talk to Dr. Pohle, head of the Department of Radiology, to see if work in that area might be more to my liking. I was ushered into "The Presence" by a red-headed secretary. She wasn't shaking in her boots, but I was. After what seemed like hours, THE MAN looked up from his papers, listened to my story, and assigned four papers to be read and reported on in a week. Having nothing else to do, I read the assigned ones and then read all of those listed in those bibliographies. this sounded like fun. I reported in to THE MAN through the red head, and had the same wait. He asked me about the assignment and then suggested that I read about ten more papers. After each, I said, "I read it." One was Cade's book on Radioactivity. That stopped him for an eternity which by actual time was about four seconds.

He rose, strode out of the door, and said, "Follow me." That I did, like a little dachshund. Down the corridor, down the steps, to the left, and then to the left again. No words. He took a key from his key ring, unlocked a door, turned to me, gave me the key, and said, "This is your lab, " and walked off

There I was in a basement again. I didn't see him for about a week. During that time, I stole all the equipment that I could get from the basement of the hospital preparing to do work that I wasn't sure about, but it seemed like the lab needed some more goodies. The reading helped too, but it was unaimed but fun. Then came a tour of the department where I met Dr. Lester Paul. From that day on, work became a real joy. Dr. Paul let me see him at work and we finally rigged up a means of doing serial films on barium enemas to study the physiology of the colon. That was the start of my life-long interest in presenting anatomical and physiological data simultaneously.

The boss had finally assigned me the job of calibration of therapy equipment. That gave some track to the day. I was also given the task of looking into the ultra-violet therapy machines and calibrating them. Physical therapy was a part of Radiology then Dr. Pohle also became interested in therapy with positive and negative ions in air. This was an exciting idea but it didn't take long for me to realize that I needed to know much more about a lot of things before I could make any sense out of such a complicated process. The investigation soon died an ionized death.

Dr. Pohle told me about a job at the Rockefeller Institute in New York working in the Department of Biophysics. I was off to the Big Apple. What a year! New York in the depression days was a friendly place in which one could walk anywhere anytime, and walk I did. I had an actual salary which, at that time, seemed magnificent. That let me try the exciting foods and see the exciting places. The third Avenue El takes one to the Battery and the Staten Island Ferry. The subway whisks one from the Rockefeller to Columbia in minutes. Columbia at that time ran night classes, and there I got my first taste of Biology and Zoology. At Cooper's Union, there were night classes in advanced mathematics. The worst time of the year was having Christmas dinner alone in a automat. The work at the Lab consisted of raising fruit flies and irradiating the eggs with a copper anode pumped out tube for the purpose of creating mutations (copper, again!). Flies and me don't get along too well right at this moment. One of the pieces of equipment on my desk is a fly swatter.

In the spring, Dr. Pohle invited me back to Wisconsin to run the radon plant and to be the physicist for the department. I drove from New York to Madison in an open, three seated, 1917 Willy's Knight, car for which I paid $75. The only accident I had was when a fire occurred in the engine outside of Akron, Ohio. A farmer and roadside sand put me on my way again after the farmer rewired the car. Good Samaritans were plentiful in those days. The trip was made in three days at 35 miles per hour, except in Chicago where I had to speed up. The car died in front of the dorm in Madison, was towed off, and never moved again. I think Chicago dealt is a mortal wound, but it waited to die in a beautiful place. That car had a real personality.

The job was a half-time job that paid $73.08 a month but did include a $50 a month research budget for materials. The money came from the Steenback patent for making Vitamin D. This money was given to research people in the University and was the greatest idea yet. If one ever wants to leave money to a University for young researchers, this is the way to do it... Not to pay salaries, but to give to the workers, the materials and tools to do the work.

It didn't take long to get the radon pumping job down to a system. I did the work at night with a total investment of twenty minutes of time spread out over about one and a half hours, three times a week. This left plenty of time for study and classes. The other part of the work was calibration of the therapy equipment on a daily basis, and changing the air-cooled tubes when they blew out.

The BIG MAN never made a mistake! But he did once. Harry Hook was a technician in the diagnostic department. He was small enough to get his arms into the tank that held the burned out tube and he could get it out of the tank. Harry could get into more trouble in a minute than an army could in a year. The Boss, grounded the big capacitors with a single stroke of the grounding rod. Harry reached in for the tube. Out the door he flew when the big arc hit him. He swore, picked himself up, and went in

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and grounded the capacitors in the same way that the Boss had done. He reached into the tank and out the door he flew! That was Harry all over.

I build a copper transmission anode pumped-out tube like that we had used at Rockefeller but it was to be used for trying to get Laue patterns from various mammalian tissues. The anode was water-cooled. It consisted of a thin copper (again) sheet. The water was piped to the tube from a sink on the other end of the lab, through glass tubes held together by rubber sleeves. Dr. Pohle was waiting for me one morning with a scowl on his face. He hadn't liked the idea of having to have forty-seven pails of water mopped up in the lab when the water line broke during the night. It must have been the copper.

Dr. Pohle used to invite the whole staff for dinner on frequent occasions at his beautiful house out in Shorewood. His supply of drinks found willing users because we couldn't afford liquor in those days (since it was prohibition). Target shooting in the basement was a usual after dinner sport. Your target from the night before was always on your desk in the morning.

One day in the late Spring after a couple years on the job, I went to the office to talk to the chief about what he thought about my going to Medical School. I was tempted in this direction because,while I knew more than the doctors about the radioactivity that they were using, they were the ones that applied it to people and made a difference to them. They signed their names, and I could only assure them that what they got from me was the right stuff (the red-head was still there). This time, the chief rose almost immediately after hearing my opening gambit and said, "Follow me!" I'd heard those words before, and I landed in the basement! Without a word, we went through the halls out into the sunshine, and over to the little house where the Dean of the Medical School held sway. I didn't know that that was where we were heading. In fact I didn't know that the Medical School had a Dean.

The boss lead me into an office, introduced me to the great Dean Bardeen, saying, "Here's a candidate for your school," and left without another word. After he got my records, the Dean accepted me. Things were simpler in those days. I think I got in because the boss and the Dean were friends. A whole new life opened up. In those days, they used to take anyone with a "C" average into medical school. On our first day in class, there were 180 students. At the end of three days, after going into the anatomy lab, there were about 160. After the first six week's exams, there were about 120. After the Pathology exam in the second year, there were about 90. Only 50 were inducted into the third year. It is said that this is a bad system and that it leads to killing competition between the students. Exactly the opposite was true. We helped each other as much as we could. Those who made it into the third year really wanted to practice medicine, and I think that, in general, it made for a fine medical profession. They were tested in the fire.

At the present time, a medical student is practically given his MD at the time he enters medical school. By the time he graduates, that person is not necessarily the same one who applied to that school. Just because he's a bright guy doesn't mean that he will become a good doctor. It is said that the Wisconsin system is too expensive. I think that the cost of one poor doctor exceeds the cost of starting the training of many and weeding out those that do not cut the mustard and who are unwilling to do what is necessary to become a part of this great profession. During our four years in school, our teachers found out more than how much we knew. They found out what kind of persons we were. The class was small enough so that our major teachers know us all by name. A class of 50 students seems to be about the right number and when the classes get too large, this important aspect of teaching is lost. The students also learned something about the teachers as people. They became our heroes rather than our faceless antagonists.

I remember one time almost asking Dr. William Middleton, Professor of Medicine, why his patients sometimes died. I thought that any physician as great as he would never let them die. I bit my tongue just in time...and it hurts yet. Dr. Middleton, "The Big Train," was a short man who came from Pennsylvania. He gave the whole morning each day to teaching medicine during the third year. One day before I got into school, he made the crack to some poor erring student that he got the brown derby for his luckless answer to a question. A brown derby is Pennsylvania slang for a dunce cap. The boys got together and presented him with a real brown derby. Brown derbies were hard to get in those days, but with such a project, persistence paid off. The hat was about a size 9. That made it drape over one's ears in a silly fashion.Each year thereafter, a brown derby was found and presented to the Big Train by the third year class. Dr. Middleton wore it the first day of class as a symbol of humility. It was a touching experience.

Then came the second day! He entered class on the minute as usual, picked up the hat, and asked the first question. The day's assignment was on gout. Victor Neu was a PhD in Pharmacy. He had worked in research on uric acid in chickens during his post graduate days. "What's the normal level of uric acid?" The answer was given in chicken units...and the hat sailed across the room to be signed and worn by a man forever after named "Hyperuricacidemia Neu". He was never addressed any other way...and the Big Train never forgot. The hat was worn until the next Boo-boo when it was passed gratefully to the next red-faced victim. I was lucky enough never to have had to wear the hat. I'll never knew why...but I do remember that when it was announced that I had made AOA, The Medical School Honor Society, the Big Train snorted. He knew I wouldn't amount to anything.

The Big Train had a nickname for almost all the students. One of the students in the class had been straight A from "day one" in school. He was a homely Irishman who was too bright to exist. We called him Dog Face, the Big Train called him Big Shot. We were partners in Physiology laboratory, four of us. When Dog Face got within ten him to recording data and all was well. He became a successful psychiatrist.

The Big Train ran all of this classes by means of quiz sessions on assigned material. Woe unto anyone who was not prepared. I can't think of a better way to teach. At the end of a class, the MAN would summarize the whole thing and bring it all together for us. Marvelous!

He also ran the weekly Pathology conference. The protocol was handed out on the preceding week. He stood before us on conference day and quizzed the class on all of the findings and demanded that we knew what they meant. Then he would put it all together and make his diagnosis. The pathologist, Dr. Bunting, would rise and, if possible, tear the diagnosis to shreds. It didn't happen to often, but when it did, there was great glee among the peons. It was part of his teaching of humility, and we all respected him for his demonstration of it. He put himself on the line.

Half of our last year in school, during 1936, was spent away from the campus in various activities. One of these included living in an insane asylum for two weeks. I remember my first real meeting with a paranoid schizophrenic. He was a well-trained engineer and very bright. His trigger point was that he knew the world was flat. When asked about this, he would embark upon a long tirade about the fact that all the measuring instruments were affected by the position on the flat earth dish, and this could account for all of the observations. Scary...and so reasonable.

We also spent several weeks in Chicago on the home delivery service. I saw houses still lighted by Kerosene lamps and water drawn from the corner hydrant. I remember sitting up all night with a patient waiting to deliver what ultimately turned out to be nine month-sized fibroid, the fetal head and extremities were so clearly outlined. That started my interest in error of interpretation.

When waiting for deliveries, we used to put cups of kerosene under the legs of the chairs in which we attempted to sleep to keep the lice and other bugs off us. It worked. We sat yogi fashion on the chairs. I've been bowlegged ever since.

The system of delivery depended upon using newspapers to make a bed. They are sterile and they were the only things that were except those that we brought with us in our little black bag. That black bag and the white coat was a free pass through purgatory. There never was a "doc" assaulted or robbed, even though we passed through the toughest part of Chicago on the way to our cases during its toughest days. We were the "angels of mercy" and nobody bothered us. We successfully delivered a baby on the second floor of a house one night. At the same time, a murder was committed on the front steps. The world came out even.

We spent two weeks in a pest house (hospital for infectious diseases) and making rounds with the city health officer, Dr. Foxx, who quarantined the houses in which contagious disease occurred. I'll never forget the requirement that we join Dr. Foxx in smoking his many cigars throughout the day. It nearly killed me, but the trouble was that I go to like them just when I couldn't afford them. It was during this time that I saw a hospital full of patients with small pox in every stage described in the texts. This was during a special visit to an outlying hospital that the boss arranged. That was one of the most harrowing sights I ever encountered.

Part of the time I spent as an extern in a private Catholic hospital. One of my first acts was to stop all medication. The patients had pages full of prescriptions, and I thought that was too many. Most of the patients did very well, but I was called on the carpet when the incidence of fecal impactions became noticeably higher than in the past. My cure for dull needles sent from central supply was to break them and send them back. I never knew why they weren't happy with that. The sisters put in an organ for the entertainment of the interns and patients. I used to play jazz on it. One day it disappeared. Not a word was spoken, but I got the message. I only now really understand why so many people were happy to get me out of their sight.

Graduation came at the end of a lovely June. The Big Train handed out the diplomas. He addressed us all as Mister or Miss, emphasizing the fact that we still had to pass our State Boards before we were really licensed doctors.

I headed for Kansas City, Missouri, for my internship in the Research Hospital there. I drove down with a friend in a box on spools known as a Chevy coupe, purchased for $35. I'll never forget the taste of Mississippi cat fish served by a woman who could have served as a model for Aunt Jemima. It was in a town along the River which consisted of a bar-restaurant and a filling station. If she had moved to New York, she would have put the 21 out of business.

Internship was year of years. We were given rooms in the hospital. I got there early, so I had a room to myself. There were five of us in a two hundred bed acute care hospital. Each of us had 40 beds to care for. During that year, a full night's sleep was a thing we read about. I slept through the night only two times. I remember napping in a chair while waiting for a delivery. The phone rang. I picked up a coke bottle on the table beside me, stuck it in my ear, and kept shouting "hello" till the laughter woke me up.

We lived off the fat of the land. We had a room, 6 intern togs, 4 big meals a day, and $25 a month. On that, we drank Vat 69. I had wanted to live in a boss-run town, and I got my wish. What an experience! The car thieves used to put a piece of yarn about their steering wheels to indicate to other car thieves that this was a car-thief's car. The docs around the hospital all had yarn around their steering wheels once they heard about it. It worked. If you found a corner of your license plate bent up, you immediately bent it down again. That was the mark that indicated that you had a particular brand of radio for which your car was to be stolen when the order went out for that particular brand. One of the staff of the hospital was an insider in the "party." He knew all the ropes. One day, all of his tires were stolen. He mentioned it to the policeman on his beat. The new ones were delivered and even put on his car for him by the police.

One man was repairing a section of his sidewalk by himself. He was approached by a passerby who asked what kind of cement he was using. He said, "Portland." Within a few minutes, the water company truck drove up and the driver said that, unfortunately, there was a leak in the pipes in the neighborhood, and he would have to shut off the water to his house. The man went nuts waiting for the water to be turned on again. He talked to some of his neighbors and finally got the message that the bosses owned Readimix. He bought Readimix, displayed it prominently, and the water went on. No connection I'm sure.

Cherry Street was the red light district and a place for bars, gambling, and marvelous music. John Leach, one of my classmates, went out on the town one night and landed in a joint. There was a bubble dancer there. Oh course, Johnie could not help hitting the balloon with his cigarette whereupon the balloon burst. Since the balloon was all she wore, she was naked as a jay bird. She landed in his lap. He took a good look at her and realized that her skin was covered with the sores of secondary syphilis. He dropped her on the floor, went to the bar, ordered two gins and washed with them.

One of the attending physicians tended to write whole pages of orders and all were marked "stat." One patient came in one night for some trivial complaint (she needed rest). The usual orders were written. The interns had to do all of the lab work at night. Johnie Leach drew the task. He realized that none of the orders were necessary as an emergency. He let them go until 3am, when he called the doctor and engaged him in a long discussion about the case. He repeated the call at 4am for further discussion. It was my first experience with an intern curing a doctor of an almost incurable disease .called: graphic diarrhea.

I had heard about the "old" Docs making a diagnosis with their noses, but I had never seen it in action until one day when I called in the top internist on the staff to help out with a case that was puzzling to me. The patient was in a L-shaped room out of sight of the door. The Doc opened the door, sniffed, closed the door, and said, "Typhus" and walked away. His diagnosis proved to be right.

Dr. Ira Lockwood was the Radiologist for the hospital. He was a busy and wonderful man. I saw mammography being practiced there for the first time. I remember a particular case where the surgeon diagnosed a benign lesion on one side and a malignant one on the other. Dr. Lockwood disagreed with both diagnoses. Dr. Lockwood was right. He used an ordinary Par speed films and screens. I think it might be interesting to see how many of the lesions could be diagnosed correctly using these techniques, at a great saving of patient exposure. I have the feeling that any lump over about 2 cm in diameter could be handled as well with these techniques as they can be the present methods.

I applied to Dr. Robert Newell at Stanford for a residency in Radiology to start on July 1, 1937 and he accepted me.. Only two year residencies were offered there. Part of the time was spent in therapy and part in diagnosis. Six months were spent in Pathology. I spent seven months at the San Francisco County Hospital under Harry Garland.

Dr. Newell was a most kind and gracious gentleman. I remember being invited to his house to a welcoming party for me. Chiefs did that in those days. He make great martinis. I was deep in conversation and sipping away with joy in my heart. Dr. Newell kept coming around with the martini pitcher refilling my glass. I said, "You are very persistent, Doctor Newell. He countered with, "You are very persistent, Dr. Miller."

During the second year of the residency, there were three Miller's as residents: Edith, Ivan, and Earl. Each of us got tired of our own names.

My time in Pathology was spent under Dave Wood in Oncology and the young Bill Dock who was then the head of the Pathology department. Dr. Dock had a photographic memory. One day, a resident asked him a question. He answered it and told the resident to go to the Journal "X", look on the right side of page "x" for the reference. The resident did, and there it was!

Besides handling the routine surgical specimens and autopsies, I started to study, under Dr. Dock's direction, the coronary arteries by injection of a lead oxide gel mixture, making stereo radiographs of the specimens and reradiographing them after dissection. Among other things, we noted that most of the coronary artery disease was wide-spread, and only a rare case showed a singe area of obstruction. Either the disease has changed, the diagnosis is made earlier, or we had a selected group, because the single lesions seem to be more common now.

Under Dr. Wood, I took on the study and classification of brain tumors. This was not only rewarding but frightening as well. I grew to have great respect for pathologists through this experience.

I wrote my first paper at that time. It concerned Carcinoma of the thymus in a child with widespread osteoarthropathy. The exercise led me through both carcinoma of the thymus and osteoarthropathy as well. It taught me the pain of writing. The pain has endured to this day, and it gets worse rather than better with continued efforts to write. I guess the reason that one continues to write is that it feels so good when one stops. If I had a wish, it would be to write easily.

My time at the County Hospital was spent under Dr. L. Henry Garland... and when I say, "under," I mean "under." He seemed to be on top of me all the time, but in that, I was not alone. He taught by scarification, but teach, he did. I kept studying him for a long time in the attempt to figure out what made him the great man he was. I came to the conclusion that one of his outstanding characteristics was his immeasurably high intelligence. He was the fastest man with a word that I know. It was strange to me how many times his name comes up in conversation long after he was dead. He made a lasting mark. This fantastic man was an artist, a charming host, a skier of note, a wit with a sharp edge, a good poker player, a patron of the arts, a connoisseur of wines, and, above all, a gifted radiologist. One could fill a page with adjectives. Let's say he is just a phenomenon.

Tomography had just come into being, and there was no device for it at the County. I went down to the shop and with the help of some of the "guys" there, I build the gadget, and attached it to one of the x-ray machines. There was a great deal of tuberculosis in the hospital, and a whole ward was set aside for the treatment of this terrible disease. One of the great problems that occurred in these patients was a chronically positive sputum in patients in whom no cavities could be identified and treated by collapse. In order to test the technique, I asked the physicians on the TB service to send me six patients in whom no cavities had been demonstrated by serious and thorough radiographic study and who still had a positive sputum. In all six, the tomograms showed previously unidentified cavities. This was a banner day for the x-ray department. It's funny how looking for holes can be so rewarding. Must be what a golfer feels like.

My two years at Stanford was coming to an end, and I had to find a place to complete my third year of residency. I wrote a glowing description of myself (biased, of course) and sent it off to five places all on ocean shores. I had suffered from rag weed allergy so severely that I found out that I could only live on the coast where the stuff didn't grow. Out of the five, I chose Yale because I wanted to live in the East again to see if it still was as exciting as it had been.

Yale was a different kettle of fish. I had been offered an instructorship, and I jumped at it. To get paid $3,000 a year to study at a great school was heaven indeed. I was started in therapy because, presumably, I knew something about it. It was there that I did the first radiography with a therapy machine for localization of the beam relative to the organ being radiated. At first, this was a scary experience since, even with a larynx, it was possible to miss it by a mile in spite of very careful attention to beam localization. After awhile, it became routine, and I could sleep nights.

Hugh Wilson ran the department. He was a gracious host and a fine person. In the department, he was a tyrant. Like Harry Garland, he taught by scarification. He saw every case done in the department every day and every case was seen by every staff member and resident in the program. He was uncanny in his ability to detect lesions. I remember the time when he was called to the phone from a morning conference. He was stopped on the way by a physician who wanted him to "just look at" a series of about 40 films that were strung around on the illuminators in the staff viewing room. Hugh was in a hurry, but he glanced over them at an angle and called out the lesions he could see. We studied the case later at leisure.. and very thoroughly. He had called every one.

We used wax cylinders for reporting cases. Hugh Wilson was having the usual trouble with the administration trying to get typing help. "Jake," the secretary (Mrs. Jacobs actually) did it all. She'd work till midnight getting out the reports. The boss finally told her he'd fire her if she worked overtime. The cylinders piled up until they covered a whole wall of filing cases. He kept ordering new cylinders. Finally, the head of the purchasing department came in to see what was going on, and Hugh just pointed to the mass of untyped cylinders...That got action. They finally got a new typist.

"Jake" was one of those unbelievable people with a photographic memory. I remember one time when Hugh Wilson asked her to bring the films of a red-headed minister whom we had studied several weeks before. Jake came in with the films in about a minute (God's truth).

It did not take long for me to realize that I really knew very little about diagnostic radiology in terms of the way that it was practiced at Yale. This was a humbling and eye-opening experience. The second year residents knew so much more than I did that I was ashamed. My Board examinations were coming up at the end of the year, so that meant "get busy." I examined the situation and decided that the only way I could possible learn enough was to read the whole literature published in Radiology and the American Journal of Radiology for the last ten years. I divided the number of volumes by the number of days I had to study and realized that I would have to master one half of a bound volume per night...no matter how long it took.I relearned how to burn the 3am oil, but it seemed to have paid off. For some reason, they let me pass the boards

I actually enjoyed taking them. I wouldn't want to repeat the experience now. How these young people master all they have to know is beyond comprehension. It's obvious that they are more intelligent and talented than we were or better trained. The teaching by specialists in each branch of the field may account for some of the changes, but also the material with which the teaching starts is at a higher level now than it ever has been. Now, the top people in their classes are choosing radiology, and it is exciting to be around them. They're so smart as to be unreal.

Near the end of my time n New Haven, I was invited to Boston to consider a job in private practice there. Of course, the red carpet was laid out to impress the youngster, and I was taken to the Harvard Club for dinner. For those of you who might never have seen this magnificent structure, I must tell you that it was imposing indeed. As one enters the wood lined and quiet main hall, one is faced with a magnificent wide stair case. It was an awesome sight when, from the top of the stairs, an imposing white-haired gentleman of the old school started down the stairs, drunk as a skunk and weaving from one bannister of the other. I maintained an outward demur demeanor, but I nearly busted a gut in the process. I still like Boston, but there are some things about it that impress me less than they did earlier.

Dr. Robert Stone invited me to come to UC as an instructor at $3,500 a year, starting July 1940. He was a Scotsman to the core with a tight fist on the buck and a really great person. He was the first to have administered radioactive material to a human being for the purpose of therapy. He had the first continuously operating million volt therapy machine. He had a three phase radiographic machine for chest radiography before 1940. He initiated a neutron therapy program in Berkeley using at the cyclotrons as source. The retroactive study of his work is a monument to his thoroughness and care.

The x-ray department at UC in 1940 consisted of three diagnostic rooms (chest and two "general rooms," one of which had a sinus machine) and one fluoro room. There was a separate therapy area. Bob Stone had divided therapy from diagnosis, something new at the time when most of the big shots in the field felt that all radiologists had to be generalists and trained in both diagnoses and therapy, and to be expert in every phase of the field. There was a small waiting room and one manual darkroom. The hallway was used as the inpatient waiting room. There was a visiting-staff viewing room just outside the file room. The radiologists read the films on the "poop deck" up three steps from the normal floor level over the high ceilinged surgeries. There were five reading spaces, the chief had an office, and I finally got one down the hall in an old radon pumping plant (back to the old days). All the rest of the staff and residents worked totally at their reading desks.

We still had a mechanical rectifier for the x-ray machine for use in the bone room and open bowl tubes. Shock hazard was a real peril in these days.. and shocks did happen. But no law suits. No spot films were take in fluoro because Bob Stone used old therapy tubes for rectifiers, and there wasn't enough power to take a spot. Dr. Jim Irwin and I changed that real soon since we took spots, overloaded the tubes and kept burning them out. That led to action. We finally got a generator with enough power to take spot films

. Most of the spot films were terrible...either under or over exposed. The mortality rate for films was about 50%, and the other 50% were awful. I thought that there ought to be a way to more accurately time the films, so I assigned the job of coming up with an idea to a clever house officer for correcting the problem.. About 11 o'clock one Saturday morning, he came into the Poop Deck and admitted that it couldn't be done. By one o'clock that afternoon, I had devised and calibrated a simple system for lowering the spot film tower to contact with the patient, a time scale mounted beside the tower indicated the proper time for the exposure. We never lost a spot film after that because of improper exposure if the device was used and there was not too much motion.

In doing the calibration, I was surprised to learn that, to produce the same background density on a film, a patient 20 cm thick took twenty times a much exposure as one ten cm. thick. Few people guess anywhere near that ratio when asked the question. Dr. Lee Lusted ultimately made a device for automatically setting the time, using the same basic idea. I still think that this method is the best one for timing fluoroscopic film exposures...even better than the photo timer because it saves patient exposure. If the field is covered by a barium filled gut, the exposure of the spot film is much higher than it need be because the timer doesn't recognize the fact that there is barium in the way. The background only needs to be monitored, and the thickness measuring system accomplishes that.

When I first came to UC, the day consisted of finding a foot high stack of films at each reading place in the morning (constantly replenished), reading films, consulting with the visiting doctors, running to surgery and autopsies, going to medical surgical and pediatric rounds, doing fluoro (after dark adaptation for 30 minutes) at 1pm after a film of the abdomen was taken. The barium had been given at 8am on the ward so we saw the patient first with a 5 hour distribution of barium in the gut. Myelograms were done after GI fluoro. I held "clip rounds" from 5-7pm. The name "clip" came from the fact that we furnished each reader with bull dog clips. If a case was interesting from any point of view, the reader put a clip on it. At 5 pm, we just went down the files and pulled out those cases with clips on them and that furnished an interested rounds. The fluoro man read his own fluoros, but everybody read everything else. All private cases were checked with a radiologist before the patient was dismissed from the department. The place was buzzing and getting a myelogram or an encephalogram into the schedule was a real hassle. House officers did emergency cases after regular hours. They spent the first six weeks of their training as technicians. They all hated the chore but thanked us for the experience once they got into private practice.

Anything "special" was done after 5pm. For example, Dr. Edwin Boldrey, (a neurosurgeon) brought a sheep to the bone room one evening for a cerebral angiogram. One has to be careful with sheep because when you try to do an angiogram on them, they tend to ram the needle right back at ewe.

A few days later, we did the first cerebral angiogram on a human patient. Sally M. had a buzzing in her head, and she was losing the sight of one eye. Five serial films were taken on hand thrown cassettes after surgical exposure of the common carotid artery and injection of Diadrast. The result was a beautiful lateral angiogram of the carotid showing an aneurysm 1x2cm lying over the sella. The films were shown the next morning at Neurosurgical conference. They were put up without a word. The professor, Dr. Howard Naffziger, the BIG MAN at UC looked at them and quiet reigned. He had not been consulted before the action was taken.

Cerebral angiography was off to the races. The cases were always done after 5pm in the chest room. We introduced stereo filming shortly after the start. The examinations were made in a space about 6 feet wide between a fixed table and the wall. An anesthetist, Dr. Boldrey, a person the "throw" the films, a person to catch the films, one to move the stick for the stereo shift and one to expose the films in the anteroom, comprised the team. Films were exposed on the word "shoot." It sounded like the home office of "Murder, Inc."

Dr. Mary Olney, a pediatrician, was interested in patients with congenital heart disease. 1941 was in the days before any kind of cardiac surgery. Cases were selected that had no chance of survival and they were studied by intravenous angiocardiograms using Diadrast as the opaque medium with the parent's informed consent. At that time, I read the cases three times at various intervals before a final report was made. There was a lot to learn.

About that time, surgery for coarctation of the aorta was being started. The cases were selected by Dr. Brodie Stevens and Dr. Mary Olney. We used retrograde arteriography for this work. The opaque medium was injected up-stream in the brachial artery. This flooded the aorta and all of its branches. The injection via a catheter on the same route was introduced shortly thereafter, with the tip of the catheter at the aorta. These produced creditable aortic angiograms. Lower extremity venograms became common about this time for the study of varicies.

Dr. Hideo Minagi reminded me of a patient who came into the hospital in the early 1940's. She was a sweet child with "Popeye's" forearms. They were HUGE. The underlying cause was bilateral lymphagiomata. The first lymph angiogram was performed on her.

At the start of World War II, Dr Stone was recruited to run the Radiation Safety Division of the Metallurgical Laboratory at the University of Chicago. I was recruited to run the Radiation Safety and Health Physics section of the Manhattan District in Berkeley on a "half-time" basis, by Colonel Stafford Warren, M.D, Chairman of the Department of Radiology at Rochester, New York.. This amounted to two "afternoons" a week which ran from 1pm to midnight. I also became acting and then later chief of the department of Radiology at UC. When Dr. Stone came back, I resigned as chairman of the department with a sigh of relief and never had the urge to become a department chairman again. I had got it out of my system!

The Manhattan District Days were exciting indeed. Becoming acquainted with people who became future Nobel Prize winners like Ernest Lawrence, Ed. MacMillan, Bob Oppenheimer, Glen Seaborg, Emile Segre, Leo Szilard, Bob Thronton, and Luis Alverez was the most exciting part.

I almost went to work for Dr. Oppenheimer. I received a call one night, asking whether I would have dinner at this house. Gas rationing was on so I took the train to Berkeley and was to meet him at the triangular drug store on University Avenue. This store had three doors. Finding each other looked like a Keystone Cops comedy, each of us going around and around before finally meeting. A friend of Dr. Openheimer's finally tracked me down, and as we were going to the car, he asked me whether I liked hot food. I said I did. I hate hot food! We drove up to the beautiful home at 1 Eagle's nest, had a drink and then "dinner." This consisted of a special macaroni dish laced with special hot hot peppers that a friend had sent to Bob from Mexico. French bread and an excellent red wine. I nearly died.... There was some guarded talk after dinner about a place called Los Alamos and my willingness to go there. Even if he had been wiling to take me, after that dinner the fate was decided. If I had to live through that kind of food, the deal was off.

Our monthly trips to Chicago to attend a meeting of all the heads of sections of the project provided an opportunity to see people like Fermi, Wiegner, and others at work. Being in a room full of Nobel prize winners is a exciting experience. I soon learned my place in the world...as a mouse in the corner.

On one of the trips, as the train was going through the desert, Ernest Lawrence unsheathed a portable radio, turned it on, and placed it in the corner of the window. The reception was excellent. Ed MacMillan, later also a Nobelist in physics, said that he would have put the radio in the middle of the window. Lawrence pointed out that the window frame acted as an excellent aerial...and then, with a sly smile commented, "If it hadn't worked, I could have given you an equally logical explanation."

I also remember a six o'clock meeting in Lawrence's office. The secretaries had left and the phone rang. It had about six lines coming into it with lights on the wall to indicate the line. He picked up the phone, began punching buttons, and getting nowhere! He slammed down the phone and said "I can't answer the darned thing." Nobelists in physics are human too.

After the War, Dr. Howard Bierman came into view. He worked at the Oncology Lab in the Laguna Honda "Old People's Home" in San Francisco where a laboratory had been set up. The x-ray equipment was antiquated, but it was on this fluoro equipment that he catheterized all the major arteries of the body and I made the angiograms. The most exciting time spent at Laguna Honda was when we started the work on the use of adrenaline in the hepatic artery in patients with metastases in the liver.

An angiogram was made, adrenalin was injected, and a repeat angiogram was taken. The normal arteries contracted and the abnormal tumor vessels did not. Nitrogen mustard was injected, and Howard calculated that the dose to the tumor was about 20,000 times that which the tumor would have received if the material had been injected by vein. This still seems to me to be a useful form of therapy for people with metastatic tumors in the liver and perhaps in other tumors.. With the new drugs, it should be successful. At that time, the method was tried on only those with terminal cancer as a last ditch effort. This work was published in the early 1950's.

During the War, Dr. Mayo Soley from UC and Dr. Joe Hamilton from the Radiation Lab in Berkeley had been working on Radioiodine and its uptake in the thyroid. Ken Scott said that he could provide me with some radioactive iodine (I131) if I wanted to work on the thyroid problem. I jumped at the chance. Over a period of about ten years, I worked with Dr. Soley and Dr. Morris Dailey on the problem of the diagnostic and therapeutic value of I-131 in both benign and malignant disease of the thyroid. The uptake rate proved to be a tremendous importance in the diagnosis of thyroid activity. And the material was of importance in the treatment of hyperthyroidism, and it still is used successfully.

In the early days of this work, I spent a great deal of my time with my head in the bowels of the electronic scalers trying to keep them working. Geiger counters and later sodium iodide crystals were used to measure the radioactivity. It was difficult to get reliable multiplier photo tubes in those days. We put several in a deep freeze and after having determined their background counting rates, I checked the background after the freezing. I finally picked one that was the most stable and it worked for years. I still think this is a good test for the best tube.

After the War, there was a lot of interest in radiation exposure of technicians and patients. Film badges were introduced and checked. Certain of our technicians were heavily exposed. Review of the situation showed that only those who had radiographed babies were in jeopardy. They held the babies during the filming. I put up a prize of a big doughnut (BIG spender) for the design of a useful baby holder that would work in all positions.

Nobody collected the prize even though a number of designs and models were made by the technicians. I got to thinking about papoose carriers and went to the campus shop and had them cut out some pieces of 1/4 inch plywood forms to which the baby could be immobilized by the use of Ace elastic bandages. A wooden vice to hold the boards upright was included. This worked like a charm, and I even got a prize from the RSNA as "gadgeteer" for it. It still didn't have a name. My wife, Blossom, suggested "Brattbored" and won the doughnut. It's still the best baby immobilizer around. "Bratt" because you would never call a baby a "brat," "bored" because when immobilized and kept warm with a blanket, the babies always went to sleep.

I'll never forget the day that a bright pediatrician came in to the department toting a 50 pound, lead encrusted gadget for scanning legs. He had labored mightily on this. He was interested in determining leg length accurately for his growth studies, in the treatment of dwarfism. I hated to do this to him, but I put a child on the fluoro table, made a horizontal slit of the shutters, pulled the fluoro tower smoothly along its track during exposure of a hand held film, and handed him a scanogram. Later, a motor driven device which moved the tube stand as in tomography was made that employed a slit in the filter holder of an ordinary x-ray tube and that settled that problem.

Dr. Stone was given a transverse tomographic device which employed a stationary tube, a rotating patient and a synchronously rotating film. He was interested in having it used for localization of malignant lesions in the mediastinum in preparation for their treatment. The patient was strapped in the chair which rotated at a fast rate during the exposure of the film...a dizzying sight and experience. As the machine stood, it could not be used successfully because there was no way to tell where the "cut" would be. Normally, I found that if we worked for about two to three hours on a patient, we could

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get some usable results. I rigged a rod from the tube to the film holder gadget. At the intersection of the rod and the midline of the patient at a given level, the level of the cut could be determined. With this help, I remember having a patient in and out of the department in 40 minutes after having made a series of successful transverse tomograms of the whole mediastinum at 1 cm intervals. The dose to the patient was high and even the best films left much to be desired so the worked stopped.

In July of 1949, I became the youngest full professor on the San Francisco Campus. I never will forget the feeling of having "arrived" and also the feeling of security that resulted from that promotion. Promotions were easier in those days. I couldn't be fired if I never did another lick of work. That idea lasted for about a day when I wondered what I would do with myself if I did that. I got back to work with a vengeance and have enjoyed it ever since. It must have been something like the time that I read about a professor of chemistry who had just won the Nobel prize. A picture of him showed that he was the classical caricature of the ragged, absent-minded professor. I commented at dinner that I wondered what he would do with the $47,000 prize. My wife said immediately, "Oh, he'll buy the shotgun he always wanted, caress it for an evening, and then get back to work." Some of us are like that.

I've often wondered why one works so hard in his profession and in other activities, e.g. sports. The obvious answer is to get ahead in one's work, to gain promotion, to gain applause, to gain the esteem of one's colleagues and friends, to increase one's income, but there seems to be much more. I've learned this since I've retired, since no matter how hard I work, I can't increase my income since it is fixed, and I expect no applause for it, yet I've never worked harder in my life on the most difficult problem I have ever attacked. Somehow, it must be primarily for self-satisfaction.

Where did this drive for self-satisfaction come from? It seems to be one of the most important things that can be instilled into a child, since, if it is instilled, one need never worry about this child afterward, since he or she will take care of himself or herself and, will set high personal standards of productivity. This seems to be a form of "long-term" selfishness because it is clear that the payoff comes late in life, not early. I felt that I should pay for the opportunity to work, rather than being paid for it. Fun is what it's called. An academic works hard so he can have a glowing obituary!

In the middle 50's, I was offered the chairmanships of a number of university departments around the country. Most radiologists my age were offered similar jobs. I had had my belly full of chairmanship from my earlier experience with administrative jobs. Finally, in 1957, a big one came up at Columbia, New York, that included a three story building for research. I wanted the research room, but I didn't want what went with it. I kept quiet about that.

With the permission of the Dean and Bob Stone, I was allowed to have dinner with President Sproul in the Faculty Club in Berkeley to discuss the situation. I ended up telling him that if I could have some research space on the San Francisco campus, I telling him that if I could have some research space on the San Francisco campus, I would be satisfied to continue where I was. He promised that I could have it, and that was the happiest day of my life.

Bob Sproul was a truly great man with a voice to match. He knew everything that went on in the whole University as big as it was, and his word was his bond. I came up with the design of a laboratory of 3,000 square feet that included a shop, an electronics lab, an x-ray room, a dark room, and a conference room. True to his word, he came up *with $80,000 to produce and to equip the lab just as I had designed it. The University* dug out the space from an old Pathology storage room under the old Medical School building, and there was my shiny lab (back in the basement again!). Over the years, I had to support the lab with outside funds and for the most part, this was accomplished with some help over the rough spots between grants. The lab became my reason for being, and I am grateful to Bob Stone and to Alex Margulis for the half of my time that I could spend in it. This lab earned its keep over the years by supplying to the department and to my physiology lab in the Cancer Research Institute on the 13th floor of Moffatt Hospital the equipment that was needed and could be supplied in no other way.

In about 1953, the funds for the new Moffitt Hospital came through, and I became involved in the planning of the new diagnostic x-ray department. One of the biggest problems was looking into the future and attempting to predict the rate of growth of Radiology in a University department. I studied the twenty years of records of the number of examinations, number of staff, published papers, income and any other data I could get. It was clear that the growth was exponential from every point of view

It is dangerous to extrapolate log curves but it seemed to be the only thing to do. I predicted that the department would be obsolete in ten years. It was a proud day when I reexamined the data in 1965, ten years after the new hospital had opened to find that the prediction was right on the extrapolated line of numbers of patient examined and number of examinations performed. The prediction of obsolescence also proved true. The work load exceeded the capacity of the department in the space provided by that time.

The department was designed as a box within a box with the dark rooms in the center. Outpatients were handled on one side and Inpatients on the other. The technical work flowed from the outside toward the center and the completed films flowed forward to the reading and consultation rooms. The design was good for its time, but with the growth of "special" procedures the whole face of Radiology has changed. One of my many losing battles was to attempt to influence the design of the new department. It has become clear that what is now called the central x-ray department is really a special procedure suite. Plain film examinations are mixed in with them and complicate the working. I define a special procedure as one that demands the presence of a radiologist during the performance of a study. Plain film examinations can be carried out by technicians alone.

It is my considered opinion that all of the plain film examinations should be carried out on the patient wards. Each of these wards is about a 50 bed "hospital." Each of these would have a part-time radiologist who would make rounds with the attending physicians on the ward and design the radiological procedures. The plain films would be read there. The referring physicians would have immediate access to these films and it is to their advantage to see that these films are not stolen or lost. No patient would have to be transported off the floor for any plain film examination. The ward department would consist of one x-ray filming room, a dark room and a reading room. All files would be kept in the reading room for the duration of the patient's stay on the ward.

It turns out that 80% of the examinations done on in-patients consist of plain films. The problem of lost films, referring physician transport, patient transport, elevator usage, and referring physician's disgust because he has to wait for films would all disappear. The radiological service to the patients and to their physicians would be immeasurably improved. Reports from special procedures done in the special procedures suite would be transmitted to the radiologist on the ward for immediate access by the referring physician and integration into the complete radiological examination of the patient. The radiologists who work part-time on the ward would be doing special procedures for the rest of the day. They would become doctors again and general radiologists with special expertise in some special areas. The idea gets turned down by all radiologists who have heard about it, immediately, and without due consideration.

When the money for the new hospital came through, Bob Stone got a million dollars from the NIH for a cancer research center and ward which was put on the thirteenth floor. In it was an x-ray room to be dedicated to the work on arteriography on cancer patients and to other special procedures that would develop. However, by this time, Howard Bierman had left the University and the place fell to me because of Dave Wood. He was the Director of the Cancer Institute and thought I could do some effective work there.

This became a physiological laboratory where combined anatomical and physiological studies were carried out on speech and swallowing and on the physiology of the lower urinary tract. The Radiological Research Lab fed equipment and ideas into the lab on the 13th floor. We did our first movies using image intensifiers in this lab and introduced the idea of taking movies off the monitors. A Vidicon camera read physiologic data from paper recorders in the case of the urinary tract and from CRT's presenting data from sound spectrographs during speech. The video signals from the two TV cameras were mixed and presented on on monitor from which the cinescopic movies were made. The data were also supplied to video tapes

Thus, we had the original data from the paper recorders, the tapes for immediate play back, and the movie films for leisurely study and filing. The importance of this approach is that it provided physiologic and anatomic data simultaneously. It is my

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considered opinion that the interpretation of either one in the absence of the other can lead to gross error of interpretation in any organ that changes quickly as a function of time. The idea applies naturally to the gastrointestinal tract, the lung, the cardiovascular system and to the urinary tract.

I met Lucie Lawson at a cocktail party one night, and she got me started on the study of abnormal, mostly hypernasal, speech. This lasted for about 20 years.

Dr. Frank Hinman first got me interested in the lower urinary tract. Dr. Goran Enhorning, a Gynecologist from Sweden who worked with Frank and me for several weeks, introduced the whole idea of the study of the urethral pressures in combination with the bladder pressures and this allowed us to really begin the study of the tract. These were productive years and fun. Enhorning was an inventive guy. Among the many things that he did was to mold a swimming pool out of plastic and float it in the sea in front of his house in Stockholm, and impressive sight in an impressive site.

The device that made the urinary tract studies really come alive was developed by Ed McCurry, an engineer in the lab. He put a chopper in the lines carrying data from the transducers in the bladder, urethra, rectum, and anus and was thus able to display on each channel of the recorders, two lines of data, as a function of time. These showed the absolute values of the data as well as the differences between two interdependent variables. For example, the bladder and rectal (intraabdominal) pressures were shown on one channel, and the bladder and various parts of the urethra were shown on another. 8 channels were used in all, in addition to sound. Voiding rates were determined as a function of time. The mechanisms of continence, incontinence, and normal voiding were elucidated. Emile Tanagho, the new head of the urology department at UC did great work in the anatomical and physiologic aspects of the lower urinary tract and has become a world expert in the field through these studies.

Perhaps the most important thing that was learned from these studies is that water runs downhill. The head of pressure in the bladder must exceed the pressure in every part of the urethra for water to flow from the bladder. The bladder pressure can rise due to detrussor contraction or by passive response to a rise in intraabdominal pressure. The intraurethral pressure can fall with strain and thus reduce the pressure gradient between the bladder and urethra and this too will make water flow. Intraurethral pressure can drop with no other change and water flows. A similar situation occurs on defecation unless the stool is liquid. The gradient between the colon and anal channel also must become negative before defecation can occur. It all seems so simple now, but it was a great struggle to see our way clear. The study of the lower urinary tract by means of fluoroscopically taken movies is not without its dangers. One patient managed to hit the fluoro unit with a strong stream of urine and the sparks flew. This almost led to total incontinence for all the people in the room.

In the early 1960's, I became interested in the matter of eye strain while reading x-rays. It seemed to me that when one first turned on the lights or pulled a big film off the illuminator that one was hit with a bright blast of light to which the pupil had to respond. This happened many times a day and caused fatigue. Also, glare from unused lighted illuminators reduced the contrast that one could see due to the small pupil size and cone pigment bleaching. Dr. William Saunders who was at that time working at the Veteran's Hospital, and I began to work on the problem of making an illuminator that would shut itself off when no film was on the box and turn itself on when a film was put up on the box. The device operated on a micro switch which was actuated by the thickness of the film and worked well.

General Electric came up with a system which operated even better than ours. It used the movement of the edge of the film when it was put up on the illuminator to actuate the switch and this became an item in their line. When using the device, one should keep only the central light on at all times, and all the rest off. In addition to reducing the glare from one's own light boxes, it reduces the back lighting from other workers in the same room. It turned out that it was better to have the light go from "high" to "medium" rather than having it go from "off" to medium. The change and the fatigue is less in the latter case, and one has at least some light to work by at all times.

During the time that I was reading emergency films we would attempt to telephone reports to the Emergency Room. The phones were so busy that I could almost never get them. I thought it might be a good idea to have a direct connection between the two places. An engineer from the Dictaphone company said it couldn't be done. Ed McCurry designed a system for connecting two dictating machines, one on which the original report was dictated and one in the Emergency Room that repeated the report as it was being dictated. A light indicated that a new report was on the magnetic belt. The people in the Emergency Room simply backed up the belt to the previously indicated space made at the beginning of the report and listened to the report at the time that was convenient for them. This worked great for a number of years.

The system was designed so that it could have been put on all the wards and each of the wards and the clinics could have had immediate reception of the reports as they were dictated. The total cost for such a system was about $25,000 for nine floors in the hospital and for nine clinics. In spite of the success of the Emergency Room system, the idea was never adopted by the main department. I still think it is the best way to get information to the patient's areas, unless one went to the idea of the department on each floor.

We had a video disc on loan to us in the Lab for examining its usefulness and quality. This led to the idea that perhaps we could transmit the x-ray images to the various floors and clinics. This also worked well, but it too never got off the ground. The device employed an inexpensive Vidicon TV camera and a zoom lens so that the ultimate resolution of the system was determined by the size of the grain on the film. As the films emerged from the darkroom, a technician would record the requisition on the line of the disc and then each of the films taken for that examination. A radiologist would circle any positive finding and that part of the film would later be enlarged and recorded. The name of the patient was always easily visible on the constant position on the display so that the people on the ward could "page" through the various disc images at a rapid rate, find the ones they wanted and then examine those. This system would cost about $250,000.

We decided not to use a random access system because of its high expense and thus came up with the idea of having parts of dedicated discs for each of the wards. This reduced the expense considerably and would have worked well. The discs held enough images so that several days films of patients on the ward were always available. We even came up with the idea of having images side by side on a split screen . Such systems, much reduced in size and concept, have been used elsewhere with success. Perhaps the biggest objection to such a system is that the referring physicians can by-pass the Radiologist for his consultation. I think it would not occur because in the difficult cases the referring physicians would come down to talk about the case. In the "normals" and easy cases, they would be saved the trip. These considerations led me to the concept of the individual x-ray department on the floors which would, I think, solve all the problems and would pay for itself very quickly if one considered all of the costs of patient and doctor transport, and wasted time.

During my residency at Stanford, chests, sinuses, and even wrists were taken with stereoscopic films. I learned the power of the technique. I also became aware of serioscopy, the technique by which one superimposes two stereo films and slides one with respect to the other in the direction of the shift. The images that are at the same level come into sharp relief, and the level of the lesion and other images can be determined, since all things at the same level come into "focus' at the same time. This idea stuck in my head for years. I thought that there ought to be some way in which those "matched" images could be made to stand out from the rest of the images on the film.

We started out to work on this, and again Ed McCurry came up with a really classy idea that ultimately showed that you could not get the desired images out from only two films, in a useful form. One TV camera looked at one of the films of the stereo pair and other other looked at the second one. The video signals from the two cameras were fed to a subtraction unit. Where the signals were identical, the output of the subtraction unit was zero. Where they were different, the output was either positive or negative. A positive or negative signal triggered a circuit which blanked the video signal. The area that was blanked could be shown on the monitor as a uniform area of brightness which could be adjusted from black to white. Where the output of the subtraction unit was zero, the video signal was transmitted without alteration. What we saw on the monitor were the "matched" images interspersed with black, gray, or white patterns. The patterns created by the non-matched images so overwhelmed the picture that they were nearly all one saw, although the matched images could be distinguished with effort from the mishmash.

This troubled me for quite awhile since when one looked at the films sterioscopically, the eye had no difficulty seeing the matched images; the others were suppressed. The unmatched images were patternless blurs. (This became an important part of later work on "seeing" and pattern recognition). I finally got it through my thick head that what I really was trying to do was tomography with two films, and that simply does not work.

In the middle of the night some weeks later, it occurred to me that, if one was using a single phase radiographic machine with an exposure time of one second and one made a tomogram with it that one was taking 120 radiographs of the object, each from a slightly different angle and each with 1/120 of the exposure of the finished film. Why wouldn't it work with few films?

We used a dried skull for a model, took seven films of it from equally spaced positions, each with 1/7 of the exposure time required to produce "good" film, superimposed the films in register, and had an infinite number of tomograms from 7 films. Eureka! We even devised a very simple system for sliding the films relative to one another by means of a lever, and it works. No one uses the system. Ah well!

I still had the problem of understanding why this system of tomography itself really worked. What was it that allowed an observer to see a nodule in the lung so well on a tomogram? There are facile explanations for this, but none really satisfied me.

I had carried out a simple experiment some time before. I had taken a radiograph of an aluminum step wedge using a cardboard film holder that gave a "good" picture of 12 steps. I then put ten films in the same holder and exposed the lot with one tenth of the previous exposure. On each of the ten films taken when the group was exposed, one could recognize only about 6 steps. When the ten films were superimposed, all 12 steps could be recognized and measurements of the transmissions through each of the steps were identical to those on the "good" film when allowance was made for the absorption by the film base.

I checked this by making the measurements with nine "clear" films superimposed upon the "good" film. The superimposition of the ten films created contrast enhancement and edge enhancement. Densities did indeed add (something that was known long before), and there were images invisible to the eye in the underexposed films in the parts that appeared to be clear. The edges of the steps could not be seen. It became clear that it was the underexposure of each of the individuals films that was the important aspect. Unmatched sections of the film did not give the contrast enhancement that allowed one to see edges and structures below a threshold. This too became an important part of the later work on pattern recognition and seeing.

When the matched portions of the underexposed films are superimposed they do create increase contrast and edge enhancement and structures can be identified that enhancement does not occur and the structures remain invisible. It is the underexposure of the unregistered structures that make them invisible on tomograms when they do not lie in the tomographic plane.

I turned the situation around for use in mammography. If one gets increased contrast by the superimposition of films in register, why not put two films in the cardboard holder when a radiograph of the breast was taken? A "good" film of the breast was taken. On a second exposure, two films were stapled together in the holder and half the previous exposure was given. When the two underexposed films were viewed in register, the "good" film with twice the patient exposure could not be distinguished from that which had been taken with half the patient exposure. A positive photographic *contact print of a radiograph can be superimposed on the original to create the same* result. This is useful also for showing small gall stones that are hard to see. This isn't being used either. Ah, well!

The techniques employing subtraction have been used with great success. For example, in cerebral arteriography the image of the skill nearly disappears by the superimposition of a positive and negative radiograph of the skull before and after the introduction of a contrast medium. The result comes about because of the elapse of time between exposures and the introduction of the opaque medium. In proper register, all of the edges of the skull disappear when properly exposed positive and negative images are superimposed.

It occurred to me that with the use of the TV subtraction unit, we might selectively remove unwanted edges from a single films. The television camera looks at a radiograph. The video signal is delayed and inverted, the video signal from the delayed image is subtracted from the original and the result is displayed on a monitor. Where the signals are equal, no image appears on the monitor. Unequal signals are amplified and fed to the monitor. This results in the disappearance of all of the edges parallel to the horizontal raster lines, and the enhancement of all the lines not parallel to the raster lines. By rotation of the film, successive edges can be made to disappear. This is useful, for example, for making the images of ribs disappear and removing the images of the horizontal plates of vertebral bodies for the study of the posterior parts of the spine.

It occurred to me that this might also be accomplished with photographic and radiographic images, employing the original and a slightly shifted negative of the original. This also works and is available to those who do not have TV capability. The use of the technique provides a sort of cameo effect, a pseudo, three dimensional shadowing that is startling. But it is the subtraction of the unwanted images that is the important thing.

It was my crying need to understand how one saw edges, their length, orientation, radius of curvature, and rates of change of luminance in the z-axis and rates of change of the various parameters in the x-y plane of an image required for the recognition of of the various parameters in the x-y plane of an image required for the recognition of patterns that lead to my early retirement in order to spend full time on the study. I'm still at it and struggling. Some work like this is going on in Philadelphia, but I remember Philadelphia for many other reasons.

Philadelphia was a Mecca for Radiology. Dr. Eugene Pendergrass has been the Big Wheel for decades. He, perhaps more than anyone else, influenced the Board exams and the thinking of radiologists toward the concept that everyone should be a generalist in both therapy and diagnosis. I no longer know whether he still maintains the stance. But there were other younger people there who became my friends. among them was Dick Chamberlain. What a guy! He had a his and hers kitchen in "Old Rottenstone," his house in Chestnut Hill. There he prepared gourmet meals and lovely martinis.

I remember once when Herb and Joanie Stauffer were there for cocktails. Dick always put his martinis in the freezer and they developed a fuzz of ice on top. Herb took one look at his glass and said, "Hah! a slurry with a binge on top." That led shortly to the crack that "Absinthe makes the frond grow hearter."

Dick collected gems and gadgets. I remember seeing television in his house for the first time. I sat for three hours watching Bert Parks answer a telephone during a drive for funds for cancer research. That was the only program on the tube. I thought about this later and concluded that anyone who spent his time in that way was ready for the loony bin. Unfortunately that applied to me.

Phil Hodes was a part of the Philadelphia contingent. I remember meeting him first at the end of an AMA meeting in Atlantic City. He was always hungry late at night. As we walked the Boardwalk toward our hotel, he spotted a man on a bench and politely inquired whether there was any place near where we could get a small snack of steamed clams and a big lobster. The man indicated that there was the best place in town two blocks west and two blocks north. As we turned away, the man said, "Tell them the chef at the Hadden House sent you, I always eat there." (Hadden House was a luxury hotel) It turned out to be lovely meal with me stuffed to the eyebrows but happy. We referred everyone at the hotel to the place thereafter. They must have made a million. The character that played the organ was a joy.

Phil and I also met again in New Orleans where we spent the evenings "doing" the French Quarter. At the last place we hit about 3am, the drinks were expensive because of the strippers on stage, and I finally allowed as how I was out of cash. Phil swears that later I was staggering down the street with a slide rule in my hand calculating that it had cost us $1.08 per minute to be in the joint! The next morning he went into the bathroom to prepare for the day. He came running out saying, "There's a bum in there." When I went in and looked in the same mirror, I had to agree.

We left New Orleans for the Inter American Congress in Mexico City. I was first on the around for something to happen. Finally, two men appeared on the platform and started talking into the microphone. I was informed by someone who could speak English that they were calling for me to begin. As I came up on to the platform, I was faced by a whole flock of TV and news cameramen who had come to record the conference for the news. One of the news men said, "Say something!" That phrase is guaranteed to make a person tongue-tied, and it had that effect on me. My hemming and hawing did not make the news that night. There are several other conversation stoppers that I've learned since: "Say something good about something." Have a beautiful woman pass by. Serve good food to hungry people.

I might just add that when my paper on angiography was published, all the illustrations were upside down.

I remember another time in Mexico when I was a visiting professor at the University of Mexico Medical School. Among the several talks I gave was one on the x-ray analysis of speech. I started out the talk with a detailed analysis of mechanisms of communication. I had got as far as explaining the need to develop the thought, turn it into words, inspire, speak the words, have the listener hear the words.... and on and on. I suddenly realized that to my audience I was speaking in a foreign language on the art of communications, and I burst out laughing. There was a stunned silence in the auditorium. I explained to the translator who in turn explained to the audience. An appreciative roar rose from the assembled multitude, and I went on speaking very slowly about the x-ray analysis of the movement of the mouth and pharynx during speech, the spectral analysis of the sounds, and other simpler things, to a much relaxed audience.

I retired on April Fool's Day 1974 in order to travel on cruise ships. In addition I wanted time to think about the problem of the two definitions of the word "seeing" in relation to error in radiologic interpretation. To see is (a) to perceive with the eyes and (b) to understand the meaning of the retinal image.

The subjects below are indexed to the document numbers in the chronological bibliography that follows.

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April 22, 1995

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## CHRONOLOGICAL BIBLIOGRAPHY

Earl R. Miller, M.D.

1. Miller ER: Carcinoma of the thymus with marked pulmonary osteoarthropathy. Radiology 32:651-660, June 1939.

2. Boldrey E and Miller ER: Calcified ependymoblastoma of the fourth ventricle in a four-year old girl: Roentgen demonstration. Radiology 38: 495-497, April 1942.

3. Benninghoven CD and Miller ER: Coccidiodal infection in bone. Radiology 38:663-666, June 1942.

4. Miller ER and Herrmann WW: Argentaffin tumors of the small bowel: a roentgen sign of malignant change. Radiology 39:214-220, August 1942.

5. Miller ER: A simple foreign body localization device applicable to standard fluoroscopes. Radiology 39:464-466, October 1942.

6. Miller ER: Foreign body localization by X-ray. California and Western Medicine 57:349-351, December 1942.

7. Smyth FS, Soley MH, Lisser H, Goldman L, Miller ER and Lindsay S: Medical Staff conference on parathyroid gland disease. Radiology 39:715-730, December 1942.

8. Miller ER, et al: Reexamination of men rejected for general military service because of the diagnosis of cardiovascular defects: Report of the San Francisco board. American Heart Journal 27:493-501, April 1944.

9. Miller ER: Porencephaly. J Nerv Ment Dis 99:783-785, May 1944.

10. Olney ME and Miller ER: Use of intravenous cardiography in the study of congenital heart disease with cyanosis. Clinics 3:235-244, 1944.

11. Dailey ME, Miller ER: A search for symptomless gastric cancer in 500 apparently healthy men of 45 and over. Gastroenterology 5:1-4, July 1945.

12. Naffziger EC, Boldrey E, Miller ER: Cerebral tumors without pneumographic manifestations. Trans American Neurological Association, pp 70-72, June 1946.

13. Miller ER: Technique and interpretation of lower extremity venograms. California Medicine 65:1-7, July 1946.

14. Soley MH, Miller ER: Treatment of Graves' disease with radioactive iodine. Med Clin N Amer pp 3-17. Jan 1948

15. Low-Beer BVA, Miller ER: Biological effects of radiations from external and internal sources. UCRL-98, Jan-Mar 1948.

16. Low-Beer BVA, Miller. ER: Biological effects of radiations from external and internal sources. UCRL-157 39-43, Apr-June 1948.

17 Low-Beer BVA, Stone RS, Miller ER: Medical and health divisions quarterly report. Part . UCRL-193:32, July-Sept 1958.

18. Miller ER, Soley MH, Dailey ME: Preliminary report on the clinical use of radioactive iodine ($I^{131}$). American J Roentgen 60:45-50, July 1948.

19 Miller ER: Medical and health divisions quarterly report. UCRL-270:29, Oct-Dec 1948.

19A. Soley, MH, Miller ER, and Foreman M: Graves' disease: treatment with radioiodine. Brookhaven Conference Report BNL-C5 Radioiodine July 28. 1948. p 63.

20 Soley MH, Miller ER, Foreman N: Graves' disease: treatment with radioiodine ($I^{131}$) J Clin Endocr 9:29-35, Jan 1949

21 Boldrey E. Miller ER: Unilateral paralysis of eye muscles associated with intracranial saccular aneurysms. California Med 70:96-98, February 1949.

22. Freeman NE, Miller ER: Retrograde arteriography in the diagnosis of cardiovascular lesions. I. Visualization of aneurysms and peripheral arteries. Ann Int. Med 30:330-342, Feb 1949.

23. Low-Beer BVA, Miller ER: Medical and health divisions quarterly report. UCRL 332:65-69. Jan-Mar 1949.

24. Miller ER: Medical and health divisions quarterly report. UCRL 414:35, Apr-June 1949.

25. Miller ER: Roentgen findings in carcinoma of the lung. Kansas City Med J, May-June 1949.

26. Soley MH, Miller ER, Foreman N: Graves' disease: Treatment with Radioiodine ($I^{131}$). Mississippi Valley Med J 71:131-134, July 1949.

27 Boldrey E. Miller ER: Arteriovenous fistula (Aneurysm) of the great cerebral vein (of Galen) and the Circle of Willis: Arch Neurol Psychiat. 62:778-783. December 1949.

28. Bowman KM. Miller ER, Dailey ME. Simon A, Frankel R. Lowe GW: Thyroid function in mental disease measured with radioactive iodine ($I^{131}$). American J Psychiat 106:561-572. February 1950

29 Kreutzer FL. Miller ER. Soley MH, Lindsay S: Histologic localization of absorbed radioactive iodine in some human thyroid diseases. Arch Surg 60:707-720. April 1950.

30 Freeman NE. Miller ER. Stephens HB. Olney MB: Retrograde arteriography in the diagnosis of cardiovascular lesions. II. Coarctation of the aorta. Ann Int Med 32:827-841. May 1950.

31 Miller ER: Experience at the University of California with the treatment of patients with hyperthyroidism by $I^{131}$. UCRL 948:3-36, October 1950.

32. Miller ER: Correlation of the initial rate of $I^{131}$ uptake in the thyroid with the clinical evaluation of thyroid function. UCRL 949:3-21, October 1950.

33 Bowman KM. Miller ER, Dailey ME. Simon A, Mayer BF: Thyroid function in mental disease: a multiple test survey. J Nerv Ment Dis 112:404-424. November 1950.

34. Miller ER: Isotopes: Radioactive: Radioiodine. Med Physics 2:437-443, 1950. Year Book Publishers.

35. Miller ER, Dailey ME. Holmes AV. Alexander GL, Sheline GE: Studies with radioiodine. I. Function and rate of $I^{131}$ uptake of thyroid. Radiology 57:37-46. July 1951.

36. Miller ER, Dailey ME. Soley MH, Foreman N, Holmes AV, Alexander GL. Sheline GE: Studies with radioiodine. II. Treatment of patients with hyperthyroidism by $I^{131}$. Radiology 57:227-233. August 1951.

37 Yerushalmy J, Garland LH, Harkness JT. Hinshaw HC, Miller ER, Shipman JJ, Zwerling HB: An evaluation of the role of serial chest roentgenograms in estimating the process of disease in patients with pulmonary tuberculosis. American Review of Tuberculosis 64:225-248, September 1951.

38. Zwerling HB, Miller ET, Harkness JT, Yerushalmy J: The clinical importance of lesions undetected in a mass radiographic survey f the chest. American Review of Tuberculosis 64:249-255, September 1951.

39. Johansen R, Gardner RE, Galante M, Marchi FF Ledwich TW, Soley MH, Scott KG, Miller ER, McCorkle HJ: An experimental study of thyroid regeneration following subtotal thyroidectomy. Surg Gynec Obstst 93:303-309, September 1951.

40. Steinbach HL, Lyon RP, Miller ER, Smith DR: Extraperitoneal pneumography: a preliminary report. California Medicine 75:202-206, September 1951.

41. Bierman HR, Miller ER, Byron RL, Jr., Dod KS, Kelly KH, Black DH: Intra-arterial catheterization of viscera in man. American Journal of Roentgenology 66: 555-568, October 1951.

42. Miller ER, Sheline GE: Studies with radioiodine. III. Problem of dosage in the treatment of hyperthyroidism. Radiology 57:720-728, November 1951.

43. Saunders JBdeCM, Davis C, Miller ER: the mechanism of deglutition (2nd stage) as revealed by cineradiography.

44. Miller ER: Radioactive iodine in medicine. Cyclo of Med (Radiology) 11:971-988, 1951.

45. Garland LH, Miller ER, Zwerling HB, Harkness JT, Hinshaw HC, Shipman SJ, Yerushalmy J: Studies on the value of serial films in estimating the progress of pulmonary disease. Radiology 58:161-175, February 1952.

46. Bierman ER, Lanman JT, Dod KS, Kelly KH, Miller ER, Shimkin MB: The ameliorative effect of antibiotics on nonlipoid reticuloendotheliosis (Letterer-Siwa disease) in identical twins. J Pediat 40:269-284, Mar 1952.

47. Miller, ER: A device for immobilizing children during radiographic examinations. Radiology 58:421-423, Mar 1952.

48. Miller, ER, Dailey ME, McCorkle HJ: Evaluation of treatment of hyperthyroidism with radioiodine. Arch Surg 65:12-16, July 1952.

49. Newell RR, Saunders W. Miller ER: Multichannel collimators for gamma-ray scanning with scintillation counters. Nucleonics 10:36-40, July 1952.

50. Steinbach, HL, Lyon RP, Smith DR, Miller ER: Extraperitoneal pneumography. Radiology 59:167-176, August 1952.

51. Miller, ER: Use of intravascular opaque material in the diagnosis of intra-abdominal tumors. Rev Mex Radiol 8:65-69, Jan-Feb 1953.

52. Pickering DE, Miller ER: Thyrotropic hormone in infants and chldren. Amer J. Dis Child 85:135-140, Feb. 1953.

53. Steinbach HL, Bierman HR, Miller ER, Wass WA: Percutaneous transhepatic portal venograph, Radiology 60:368-373, Mar 1953.

54. Dailey ME, Lindsay S., Miller ER: Histologic lesions in the thyroid glands of patients receiving radioiodine for hyperthyroidism. J Clin Endocr 13: 1513-1524, Dec 1953.

55. Miller, ER: Hypertyroidism-Definitive therapy with radioactive iodine. In Endocrinology in Clinical Practice (ed. Gordan and Lisser). Chicago. Year book Publishers, 1953, pp.47-50

56. Hinman, F Jr. Miller GM, Nickel E, Miller ER: Vesical physiology demonstrated by cineradiography and serial roentgenography. Radiology 62: 713-719, May 1954.

57. Miller ER: X-ray movies: Radiology 63:571-572, October 1954.

58. Hinman F Jr., Miller GM, Nickel E. Steinbach HL, Miller ER: Normal micturition: Certain details as shown by serial cystograms. Calif Med 82:6-7, Jan 1955.

59. Miller, ER: How much radiology should we teach our medical students? Proc Amer Coll Radiology, pp 7-9, Feb 1955.

60. Lusted LB, Miller ER: A pneumocolon bottle. Radiology 64:424-425, Mar 1955.

61. Miller, ER, Scofield NE: Studies with radioiodine. IV. Collimating cones for crystal counters. Radiology 65:96-107, July 1955.

62. Miller, ER, Lindsay S, Dailey ME: Studies with radioiodine. V. Validity of histologic determination of $I^{131}$ radiation changes in the thyroid gland. Radiology 65:384-393.

63. Miller ER, Lusted LB, Nickel ED: Cineradiography. IRE Convention Record 9:119-123, 195.

64. Lusted LB, Miller ER: Progress in indirect cineroentgenography. Amer J. Roentgen 75:56-62, Jan 1956.

65. Miller ER: the first stages of swallowing. Clin Sympos 8:37-41, Feb 1956.

66. Boldrey E, Miller ER, Maass L: Role of atlantoid compression in etiology of internal carotid thrombosis. J Neurosurg 13:127-139, Mar 1956.

67. Miller, ER: Why do we join the College? Amer Coll Radiol Newsletter 12:1, May 1956.

68. Silen W, Mawdsley DL, Miller ER, McCorkle HJ: The experimental production of a competent aortic valve. Surg 40:78-85, July 1956.

69. Lusted LB, Miller ER: An electronic position timer for the fluoroscope. Radiology 67: 259-262, Aug 1956.

70. Sheline GE, Miller ER: Studies with radioiodine. VI. Evaluation of radioiodine treatment of carcinoma of the thyroid based on the experience at the University of California from 1938 to 1954. Radiology 69:527-545, Oct 1957.

71. Kerner JA, Miller ER, Palmer A: Still and motion picture hysterosalpingography. Surg Forum 7:510-513, 1957.

72. Rider JA, Moeller HC, Miller ER: The pathophysiology of achalasia. Clin Res 6:57, Jan 1958.

73. Whitelaw MJ, Miller ER: New water-soluble medium (Sinografin) for hysterosalpingography. Fertil Steril 10:227-239, May-June 1959.

74. Sheline GE, Miller ER: Radioiodine Therapy of hyperthyroidism. Arch Int Med 103:924-9??, June 1959.

75. McCorkle HJ, Jones MD, Harper HA, Miller ER: The results of an experimental approach to the clinical problems of patients requiring gastrectomy. West J Surg Obstet Gynec 67:205-207, July-Aug 1959.

76. Miller ER: Cinefluorography in practice. Radiology 73:560-565, Oct 1959.

77. Miller ER: Roentgenopraphy: Cinematography. Med Physics 3:609-611, Year Book Publisher, Chicago, 1960. (ed. O. Glasser)

78. Miller ER: Evaluation of programs of conferences of teachers of radiology. Proc Amer Coll Radiol, Feb 6, 1960, pp 35-43.

79. Blackfield HM, Miller ER, Owsley JQ Jr. Lawson LI: Comparative evaluation of diagnostic techniques in patients with cleft palate speech. Plast Reconstr Surg 29:153-158, Feb 1962.

80. Hutch JA, Hinman F Jr. Miller ER: Reflux as a cause of hydronephrosis and chronic pyelonephritis. J Urol 88:169-175, Aug 1962.

81. Blackfield HM, Miller ER, Owsley JQ Jr., Lawson LI: Cinefluorographic evaluation of patients with velopharyngeal dysfunction in the absence of overt cleft palate. Plast Reconstr Surg, 30:441-451, Oct 1962.

82. Hinman F, Jr. Miller ER, Hutch JA, Gainey MD, Cox CE, Goodfriend RB, Marshall S: Low pressure reflux: Relation of vesicoureteral reflux to intravesical pressure. J Urol 88:758-765, Dec 1962.

83: Miller ER: Significance of speed and resolution characteristics of certain cassette-screen-film combinations. Radiology 80:103-113, Jan 1963.

84. Miller ER: Editorial; Ivan J. Miller. Radiology 80:312-314, Feb 1963.

85. Miller ER Kafafian J, Maddison FE: Patient exposure during fluoroscopy. (This contains a definition of a new unit of patient exposure). Radiology 80:477-485, Mar 1963.

86. Hutch JA, Miller ER, Hinman F Jr: Vesicoureteral reflux. Amer J Med 34:338-349, Mar 1963.

87. Miller ER, Nickel ED, Scofield N, Mott C: Radiographic density and contrast versus quantity and quality of radiation. Radiology 80:668-685, Apr 1963.

88. Blackfield HM, Owsley JQ Jr. , Miller ER, Lawson LI; Cinefluorographic analysis of the surgical treatment of cleft palate speech. Plast Reconstr Surg 31:542-553. June 1963.

89. Hutch, JA, Miller ER, Hinman F Jr. Perpetuation of infection in unobstructed urinary tracts by vesicoureteral reflux. J Urol 90:88:91, July 1963.

90. Hinman F Jr, Miller ER: Mural tension in vesical disorders and ureteral reflux. Trans Amer Asso GU Surg, 1963.

91. Miller ER: Kinescopic recording of fluoroscopic information: Usefulness and limitations. In: The Reduction of Patient Dose by Diagnostic Radiologic Instrumentation. (Ed. Moseley RD Rust JH) CC Thomas, 1964, Springfield ILL.

92. Samilson RL, Miller ER: Posterior dislocations of the shoulder. Clin Orthop 32:69-86, Jan-Feb 1964.

93. Enhorning G, Miller ER, Hinman F Jr: Urethral closure studied with cineroentgenography and simultaneous bladder-urethra pressure recording. Surg Gynec Obstet 118:507-516, Mar 1964.

94. Lusted LB, Miller ER: A pneumocolon bottle. In: Special Procedures in Roentgen Diagnosis (Ed. Berenbaum SL, Meyers PH). CC Thomas, 1964, pp 414-415, Springfield, Ill. (Also Radiology 64: 424-435, Mar 1955).

95. Steinbach HL, Bierman HR, Miller ER, Wass WA: Percutaneous transhepatic portal venography. In: Special Procedures in Roentgen Diagnosis (Ed. Berenbaum SL, Meyers PH) CC Thomas, 1964, pp 485-486. (Also, Radiology 60:368-373, Mar 1953.

96. Hinman F Jr. Hutch JA, Miller ER: Role of reflux in pathogenesis of chronic pyelonephritis. Excerta Med Internatl Congr Series 78:305-306, 1963.

97. Maddison FE, Miller ER: The efficiency of radiographic tubes. Radiology 84: 347-350, Feb 1965.

98. Lawson LI, Miller ER, Owsley JQ Jr. Blackfield HM: Evaluation of velopharyngeal dysfunction in individuals with no overt cleft of the palate. J Calif Speech Hear Assn 14:8-13, Nov 1965.

99. Hutch JA, Miller ER: Vesicoureteral reflux: role in pyelonephritis. In: Pyelonephritis (ef. Kass EH). FA Davis, Philadelphia, pp 613-630, 1965.

100. Tanagho EA, Miller ER, Meyers FH, Corbett RK: Observations on the dynamics of the bladder neck. Brit J Radiol 38:72-84, Feb 1966.

101. Owsley JQ Jr, Miller ER, Lawson LI, Blackfield HM: Cleft palate speech in the absence of cleft palate. GP 33:93-98, Feb 1966.

102. Miller, ER: The future educational needs of radiology - The Eleventh Ross Golden Lecture. Bull NY Acad Med 42:241-247, Mar 1966.

103. Tanagho EA, Hutch JA, Miller ER: Diagnostic procedures and cinefluoroscopy in vesicoureteral reflux. Brit J Urol 38:435-444, Aug 1966.

104. Owsley JQ Jr, Lawson LI, Miller ER, et al: Experience with the high attached pharyngeal flap. Plast Reconstr Surg 38:232-242, Sept 1966.

105. Miller ER: Robert Spencer Stone, M.D., L.L.D., Bull Calif Radiol Soc 5:1, Mar 10, 1967.

106. Owsley JQ Jr. Chierici G, Miller ER, Lawson LI, Blackfield HM: Cephalometric evaluation of palatal dysfunction in patients without cleft palate. Plast Reconstr Surg 39:562-568, June 1967.

107. Miller ER: Techniques for simultaneous display of X-ray and physiologic data. In: Neurogenic Bladder (ed. Boyarsky S), Williams and Wilkins Co, Baltimore, 1967, pp 79-85.

108. Miller ER: Equipment for the roentgenologic examination of the gastrointestinal tract. In: Alimentary Tract Roentgenology (ed. Margulis AR, Burhenne HJ). CV Mosby Co., St. Louis, 1:37-52, 1967.

109. Miller, ER (ed.): Status of research in diagnostic radiology. Report, Radiology Training Committee, National Institute of General Medicine, May 1968.

110. Miller, ER: The digestive tract. In: Status of research in Diagnostic Radiology. Radiology Training Committee, National Institute of General Medicine, May 1968, pp 22-32.

111. Miller ER: Teaching and learning in diagnostic radiology. In: Status of research in Diagnostic Radiology. Report, Radiology Training Committee, National Institute of General Medicine, May 1968, pp 106-111.

112. Miller ER, McCurry EM: Immediate hospital-wide access to x-ray film images. Radiology 92:225-230, Feb 1969.

113. Miller ER, McCurry EM, Hruska BB: Immediate remote access to radiologists' reports. Radiology 93:13-16, July 1969.

114. Owsley JQ Jr, Lawson LI, Miller ER, Harvold EP, Chierici G, Blackfield HM: Speech results from the high attached pharyngeal flap operation. Cleft Palate J 7:306-317, Jan 1970.

115. Holbrooke DR, Shibata HR, Hruska BB, McCurry EM, Miller ER: Diagnostic Holography -- A feasibility study. In: Acoustical Holography, 2:251-263, 1970.

116. Miller ER: Color coding of x-ray films for efficient retrieval. Radiology 94:685-686, Mar 1970.

117. Tanagho EA, Miller ER: Initiation of Voiding. Brit J Urol 42:175-183, Apr 1970.

118. Miller ER: Combined monitoring for the study of continence and voiding. In: Hinman F Jr: Hydrodynamics of Micturition. CC Thomas, 1970, Springfield, Ill.

119. Miller, ER, McCurry EM, Hruska BB: Anisotropic subtraction and edge-enhancement of roentgenographic images. Radiology 97:27-32, Oct. 1970.

120. Miller, ER: Immediate aims of acoustical imaging in medical practice. In: Acoustical Holography, 3:19-22, Plenum Press, New York, 1971 (ed. Metherell AF).

121. Miller ER, McCurry EM, Hrusha BB: An infinite number of laminagrams from a finite number of radiographs. Radiology 98:249-256, Feb 1971.

122. Tanagho, Miller, Lyon, Fisher: Spastic Striated external sphincter and urinary tract infection in girls. British Journal of Urology, Vol XLII, No. 1, Feb 1971.

123a. The Fate of Two Deans: A Fable, The Pharos of Alpha Omega Alpha, July 1971, Vol 34, pp 104-105; copyright 1971 by Alpha Omega Alpha Honor Medical Society

123. Miller: Physiology of the Lower Urinary Tract: Proc of Post Grad Course, UC Radiology, 1971.

124. Miller: Physiology of the Lower Urinary Tract: Proc of Post Grad Course, UC Radiology, 1971.

125. Tanagho, Miller: Abnormal Voiding and Urinary Tract Infection. International Urology and Nephrology 4 (2), 1972.

126. Miller: Continence and voiding in females. Proc of UC Post Graduate Course in Radiology, Feb 28-March 3, 1972.

127. Miller: Bladder and Urethra: an Oveview. Investigative Radiology, Vol 7, No. 4, July-Aug 1972.

128. Lawson, Chierici, Castro, Harvold, Miller, Owsley: Effects of Adenoidectomy of the speech of children with potential velo-pharyngeal dysfunction. Journal of speech and hearing disorders, Aug 1972, Vol 37, No. 3.

129. Miller: Photographic anisotropic subtraction and edge enhancement. Radiology Vol 106, No. 1, Jan 1973.

130. Tanagho, Miller: Functional Considerations of urethral sphincteric dynamics. Journal of Urology, Vol 109, Feb 1973.

131. Miller: Studies of mechanisms of incontinence, incontinence, and voiding: urodynamics, Upper and lower urinary tract. Editors: Lutzeyer, Melchior, Springer-Verlag, 1973.

132. Miller: A multiple-film technique for contrast enhancement and/or reduction of patient exposure. Radiology, Vol 110, No. 2, Feb 1973.

133. Miller, McCurry, Hruska: A simplified procedure for viewing multiple films to create an infinite number of laminograms. Radiology, Vol 110, No. 2, Feb 1973.

134. Miller: Image Manipulation. Proc of UC Post Grad course in Radiology, Mar 1974.

135. Miller ER: The Beginnings. Clinical Urodynamics: Urodynamics Bases of Clinical Urology. Edited by Graham Whiteside and Richard Turner-Worwick. Publisher: W.R. Saunders, Philadelphia, Feb 1979.