Regional Oral History Office
The Bancroft Library
University of California, Berkeley

History of Science and Technology Program
The Bancroft Library
University of California, Berkeley

Medical Physics Oral History Series

John H. Lawrence, M.D.

NUCLEAR MEDICINE PIONEER AND DIRECTOR OF DONNER LABORATORY,
UNIVERSITY OF CALIFORNIA, BERKELEY

An interview conducted by
Sally Smith Hughes, Ph.D.
in 1979 and 1980

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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It is recommended that this oral history be cited as follows:

John H. Lawrence, M.D. "Nuclear Medicine Pioneer and Director of Donner Laboratory, University of California, Berkeley", an oral history conducted in 1979 and 1980 by Sally Smith Hughes, Regional Oral History Office/History of Science and Technology Program, The Bancroft Library, University of California, Berkeley, 2000.

Copy no. 1

John H. Lawrence, M.D., 1980.

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Cataloguing information

Lawrence, John H. (1903-1991) Physician and Laboratory Director

Nuclear Medicine Pioneer and Director of Donner Laboratory, University of California, Berkeley, 2000, v, 165 pp.

Norwegian/Midwestern family background; brother, E.O. Lawrence; undergraduate education at the University of South Dakota, medical training at Harvard; research with Harvey Cushing; nuclear medicine at UC Berkeley, Crocker Laboratory; financing research, the Macy Foundation and the Markle Foundation; tools of nuclear medicine, cyclotron, omnitron, bevelac, artificial radioisotopes, neutron radiation, radiophosphorus; treating acromegaly, Cushing's disease, Nelson's syndrome, lymph cancer; creation and purpose of Donner Laboratory and UC Berkeley's Division of Medical Physics and Biophysics; Atomic Energy Commission, Atomic Bomb Casualty Commission; work relationships with James Born, Hardin Jones, John Gofman, Melvin Calvin, Hal Anger, John Northrop, Cornelius Tobias; founding the Alpha Omega Foundation.

Interviewed in 1979 and 1980 by Sally Smith Hughes for the History of Science and Technology Program's Medical Physics Series. Produced by the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, in 2000.

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Acknowledgement

This interview with Dr. John H. Lawrence is one of several dealing with the development of Crocker and Donner laboratories, within the larger series of oral histories produced by the History of Science and Technology Program of The Bancroft Library.

Besides these interviews, the Program assembles other primary source materials, including the papers and personal memorabilia of scientists and engineers, and the papers of certain organizations with which they were associated. The information in the papers and interviews helps to demonstrate the development of science and technology not only in the western United States, but also in the nation as a whole.

The project was made possible initially by the generosity of William R. Hewlett and David Packard. Mrs. Calvin K. Townsend established the Doreen and Calvin K. Townsend Fund to provide ongoing support of the Program. The University Endowment Fund, National Science Foundation, and National Endowment for the Humanities have assisted diverse aspects of the Program with a series of grants. Further aid has come from the Marco Francis Hellman Fund, established to document science and technology and their relations to business in California. The John H. Lawrence oral history was also aided by a gift from the Chabot and Dieckmann Memorial Library Fund. Other donors to the project have included the Woodheath Foundation, the California Alumni Foundation, and the Watkins-Jones Company.

1980
University of California
Berkeley, California

James D. Hart
Director
The Bancroft Library

The Medical Physics Oral History Series

The series, conducted in 1978-1980 under the auspices of the History of Science and Technology Program [HSTP] at The Bancroft Library, was funded by the National Endowment for the Humanities to document medical physics and biophysics at the University of California, Berkeley. Sally Smith Hughes, advised by Roger Hahn and John Heilbron of the Office of the History of Science and Technology, conducted interviews with thirteen individuals associated with Donner and Crocker laboratories and the Division of Medical Physics. All of the interviews had been transcribed and edited when the grant terminated in 1980. Some of the transcripts were subsequently reviewed and approved by the interviewee, processed by various individuals associated with HSTP, and made available for research as bound and indexed volumes. They are: John Gofman, Alexander Grendon, William Myers, Kenneth Scott, and William Siri. Other transcripts have for years remained in various stages of completion, and only in 1999, under the aegis of David Farrell, the new HSTP curator, are being reconsidered for processing and release. Sally Hughes, now of the Regional Oral History Office, has been asked to finalize the remaining oral histories with James Born, Patricia Durbin, Donald van Dyke, Thomas Hayes, John Lawrence, Howard Mel, Alexander Nichols, & Cornelius Tobias.

The oral histories, in conjunction with archival holdings at The Bancroft Library and Lawrence Berkeley Laboratory, will be useful in constructing a picture of the growth and development of the fields of medical physics and biophysics, in which the Berkeley research and academic institutions played an early and significant role. The interviews are of particular historical interest for their depiction of the early use of cyclotron-produced radioisotopes and radiations in science and medicine. The series complements other oral histories, at Bancroft Library and at the American Institute of Physics, pertaining to the development of Lawrence Berkeley Laboratory and the subdisciplines of physics.

Sally Smith Hughes, Ph.D.
Research Historian

January 2000
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

HISTORY OF SCIENCE AND TECHNOLOGY PROGRAM
MEDICAL PHYSICS ORAL HISTORIES
August 2000

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Alexander V. Nichols
Cornelius Tobias

CURRICULUM VITAE

JOHN HUNDALE LAWRENCE

1904	Canton, South Dakota, born January 7 Father: Carl Gustavus Lawrence Mother: Gunda Jacobson Lawrence
1922-1924	Southern State Teacher's College
1924-1926	University of South Dakota, Vermillion (A.B.)
1926-1930	Harvard Medical School (M.D.)
1930-1931	Intern, Peter Bent Brigham Hospital, Boston
1931-1932	Assistant resident physician in medicine, Strong Memorial Hospital, Rochester, New York
1932-1933	Assistant resident physician, New Haven Hospital, and assistant in medicine, Yale School of Medicine
1933-1934	Resident physician in internal medicine, New Haven Hospital
1934-1937	Associate physician, New Haven Hospital, and instructor in internal medicine, Yale Medical School
1937-1945	Research Associate in Medical Physics, Crocker Radiation Laboratory, University of California, Berkeley
1937-1946	Assistant Professor of Internal Medicine, University of California, San Francisco
1941-1945	Director of Research Projects, World War II under the Office of Scientific Research and Development
1942	University of South Dakota, D.Sc. honorary
1945-1946	Assistant Professor of Medical Physics, University of California, Berkeley

1946	Member, Joint Task Force #1, Bikini Islands, atomic bomb test site
1946-1950	Associate Professor of Medical Physics, University of California, Berkeley
1947	Associate Professor of Experimental Medicine, University of California, San Francisco
1948-1970	Director of Donner Laboratory, University of California, Berkeley
1950-1970	Professor of Medical Physics, University of California, Berkeley
1954-	Physician-in-Chief, Donner Pavilion, Cowell Hospital, University of California, Berkeley
1958	University of Bordeaux, docteur honoris cause
1959-1970	Associate Director, Lawrence Berkeley Laboratory, University of California, Berkeley
1959	Catholic University of America, D.Sc. honorary
1970-1991	Emeritus Professor of Medical Physics, University of California, Berkeley
1970-1991	Emeritus Director of Donner Laboratory, University of California, Berkeley
1970-1991	Emeritus Associate Director of Lawrence Berkeley Laboratory, University of California, Berkeley
1991	Deceased

John Hundale Lawrence

1903–1991
PROFESSOR OF MEDICAL PHYSICS,
REGENT OF THE UNIVERSITY OF CALIFORNIA

Berkeley

In the summer of 1935, John Lawrence took leave from his post as Instructor in Medicine at Yale University to spend a few weeks at Berkeley to undertake studies with radioactive phosphorus, one of the first artificial isotopes produced by the beams of his brother Ernest's cyclotron. He injected soluble radioactive phosphorous into a group of leukemic mice and then left to go fishing on the Trinity River. When he returned a few weeks later, the mice were much improved, no doubt due to the beta radiation they had received. The excitement of this discovery was too much for John to resist. With the encouragement of his Harvard mentor, Harvey Cushing, he left Yale to become a pioneer of nuclear medicine at Berkeley. On Christmas Eve of 1936, he administered to a 28-year-old woman suffering from leukemia, a dose of radiophosphorus. This was the first time that a radioactive isotope produced by the cyclotron had been used to treat a human patient. In the years to come, the method became a standard treatment for the blood disease known as polycythemia vera, an uncontrolled proliferation of red blood cells. One of his later patients was Cardinal Aloysius Stepinac, who was given asylum by the American Consul in Budapest from Communist persecution. Five years later, in 1951, the Cardinal was released to the Tito government under restrictions. Summoned to Zagreb, Yugoslavia, John administered radiophosphorus to the cardinal, who was suffering from polycythemia. In recognition, Lawrence received a medal from Pope Pius XII.

The cyclotron was potentially capable of producing copious neutrons, discovered a short time earlier by Chadwick. Initially, these particles were considered to be relatively harmless as compared to X rays. For about two years, the physics staff moved freely about their

unshielded machine. The first meaningful scientific study of the biological effects of neutrons was carried out in 1935 by John and Paul Aebersold, who found that neutrons were much more harmful than X rays. Since lead shielding had proven ineffective, a large number of water filled cans were piled around the accelerator. These were among the first steps in the creation of a new field: health physics.

In 1937 John made another startling observation: He found that a rodent tumor, sarcoma 180, was more sensitive to neutron radiation than were normal tissues! No such difference was observed for X rays. John and Ernest Lawrence then proposed that neutrons be tested in cancer therapy. In response to their request, the Rockefeller Institute made a grant to finance the first "medical" accelerator, the 60-inch cyclotron. It was soon demonstrated that neutrons were effective in killing human tumors, but that they also produced late deleterious effects.

Lawrence's work with cancer patients attracted the interest of William Donner, a Philadelphia industrialist and philanthropist, whose son had died of cancer. Donner contributed funds for construction of the building at the Northeast corner of the Berkeley Campus that bears his name. Donner Laboratory was dedicated in 1942 to the "applications of physics, chemistry and the natural sciences to biology and medicine."

The availability of radioisotopes presented a tremendous challenge to investigate the dynamic turnover of chemical species in living systems. Experiments by Lawrence and his associates included work with radioactive sodium, potassium, iodine, iron, strontium and other elements.

John Lawrence elected to aid the war effort by applying the new science to the important field of aviation medicine. He was assisted by Hardin Jones, a physiologist, and Cornelius Tobias, a physicist. Working with nitrogen analogs, radioactive argon, krypton and xenon, they were able to prove that "preoxygenation" was a way to overcome "bends," a debilitating condition limiting the altitude ceiling of aviators. It was also discovered that breathing inert xenon gas produced anesthetic effects.

Initially, the Medical Physics Division of the Physics Department was the academic home of Lawrence and his associates. In 1947, an interdisciplinary faculty group was organized, doctor's degrees were offered in biophysics and medical physics, and master's degree in bioradiology. John encouraged and fostered innovation in many aspects of biophysics. The division eventually added B.A. degrees and became the Department of Biophysics. Macromolecular structure, lipoprotein dynamics, basic radiation biology and genetics, ir-

reversible thermodynamics and neurobiophysics were among the courses offered. In the ensuing decades, hundreds of individuals came to Berkeley to study with Lawrence and the staff, many from foreign countries.

In 1948, John became Associate Director of the Radiation Laboratory. Much of his time was occupied with planning new projects for Berkeley and the programs of the Atomic Energy Commission elsewhere. In 1955 he was one of the organizers of the "Atoms for Peace" conference at Geneva. He traveled extensively, gave many lectures, and accepted awards and honorary degrees from three universities, including his alma mater, the University of South Dakota. In 1970, he was elected to the presidency of the Society of Nuclear Medicine and received the "Nuclear Pioneer" award. John Lawrence's philosophy of leadership included creation and maintenance of an environment where leaders in medicine, science, and education could mingle with students and young investigators in the pursuit of new ideas in biological science, new technologies for medical science and methods for treatment of disease. This led to the discovery of cholesterol and lipoprotein relationships to heart disease as well as to major underpinning of nuclear medicine.

The years following the war also saw the beginnings of several important research programs. John and a group of young physicians used long-lived radioiron to label hemoglobin in red blood corpuscles and demonstrated that iron was transported to the bone marrow by a protein; they were also able to measure the life span of red blood cells. Later, with laboratory scientist Will Siri, John made expeditions to the Andes to study red cell production at high altitudes. This work led to pioneering studies of erythropoietin, the hormone that controls the production of red blood cells.

Robert Wilson, who eventually became the director of Fermilab, suggested at Berkeley in 1946 that high energy protons might be useful in cancer therapy because of the "Bragg effect," the property of delivering a good deal of the particle energy deep in tissue. Lawrence and his group became interested in this approach and began investigating it at a time when very little was known about the biological effects of these particles. The 184-inch cyclotron was completed in 1947, and investigations began with protons, deuterons and helium ion beams.

In 1951, Hal Gray of the Hammersmith hospital in London proposed that high LET radiations were more effective on tumor cells, which were often constrained to live in an oxygen-deficient milieu. The Swedish surgeon, Herbert Olivecrona demonstrated independently that surgical removal of the pituitary gland could produce

astonishing regressions of human mammary cancer. In 1954 the first patient with advanced mammary cancer received pituitary proton radiation. Eventually several hundred patients were treated, most of them suffering from acromegaly, a debilitating tumor of the pituitary gland. Medically, John was assisted by James Born and other physicians in his group. As a physician, Lawrence was compassionate, and dedicated to his patients. It was shown that nonsurgical pituitary particle radiation could permanently interfere with the production of somatotropic hormone and produce tumor regressions lasting for many years.

The avalanche of scientific and medical investigations that followed are still in progress today. Currently the largest Berkeley accelerator, the Bevalac, is being utilized. Trials are under way for the treatment of several types of cancer with heavy ion beams. The particles are also proving effective for the treatment of life threatening arterio—venous malformations in brain. A new medical proton accelerator has been built at Loma Linda University. Several countries, including Germany and Japan are building heavy particle accelerators for biomedical research and treatment.

In 1970, when Lawrence retired as director of Donner Laboratory, he was asked by then Governor Reagan to become a Regent of the University of California. During his 13-year-tenure, he was instrumental in promoting advanced education in the medical sciences. In 1983, he received the Enrico Fermi award for his "pioneering work and continuing leadership in nuclear medicine."

During the last few years of his life, he kept a table next to his bed, filled with scientific and medical books. When unable to sleep, he would get up to read at any time of day or night. John's interest in both cancer and atomic research never flagged. It was this indomitable spirit, and his ability to create an environment of freedom in research for scientists and students alike, that are attributes we most admired in John Lawrence. He had a great sensitivity to human suffering, and he believed that humans can solve many of their problems through scientific pursuits.

John lost his wife Amy in 1967. He is survived by their four children, Shelley de Rouvray of Paris, Mark of Los Angeles, James of Alamo and Steven of Alamo, and eight grandchildren.

THOMAS BUDINGER
HOWARD MEL
CORNELIUS A. TOBIAS

Interview 1: February 19, 1980

FAMILY BACKGROUND

Hughes: Dr. Lawrence, could you go back to your grandparents and tell me their names, on both sides of the family, their professions, and what they did with their lives?

Lawrence: Well, my father's father, Ole Hundale Lawrence, was a schoolteacher, and I have his license to teach school in Madison, Wisconsin hanging in the living room at home. He was born in Norway. My father and I once were in Norway together and we discovered that his name was on the list of about 50 or 60 people that left the church that he and his family went to, to go to the United States. So he went to Madison, Wisconsin and became a schoolteacher. He married rather late. He married, I think, at the age of about 50. I never knew him but I knew my grandmother, Bertha Marie Hull Lawrence, because she lived with my mother and father after her husband died. She was much younger. Apparently he was a good schoolteacher in an elementary and high school in Madison. Then the other side of the family was my mother's parents. I remember her father. He also came from Norway—a different part of Norway. The first grandfather came from Telemark and the other one came from farther north in Norway. He married a Norwegian girl. (Several years ago a Norwegian-American from the Livermore Lab called me. I did not know him. On his own he traced my father's side of the family back to 1300. His name is Chris Alland. My wife traced my mother's family when she and two of our children were in Norway. I guess she spent some time in northern Norway at Lom. I guess she wanted to check on my ancestry. My wife Amy and my mother were beautiful, intelligent, loyal and wonderful women.)

Hughes: Was he married in this country?

Lawrence: I think he married her in Norway, then came to this country and got one of those homesteads where they get 160 acres. He settled in South Dakota and became a very successful farmer. I remember him but I don't remember his wife. I can remember him when I was a young boy because he was living with one of my

mother's sisters.

Hughes: Had he been a farmer in Norway as well?

Lawrence: Yes, he'd been a farmer. He was raised on a farm over there. But he did very well in this country like lots of people did, of course.

Hughes: Can you tell me the names of all these people?

Lawrence: Well, the one on my father's side was Lawrence in Norwegian. When he got to Madison there was another man of the same name, so he changed it to Lawrence. That's what my father told me. My other grandfather was Erik Jacobsen. So both sides of the family were of Norwegian extraction.

Hughes: Did your father have brothers and sisters?

Lawrence: Yes, my father had one brother who was a minister, after whom I was named, who died as a young man from a ruptured appendix. I never knew him. Then he had two sisters, one of whom married a man who ran a store in Stockton, California. I met her and her husband (Beattie). Another sister married a man in Minnesota, but I can't remember what he did. (Haugen)

Hughes: And on your mother's side? Did she have brothers and sisters?

Lawrence: My mother had one brother who was a bachelor all his life. He became a very successful banker. I can remember him very well because he came out to visit my brother and me just before World War II. He was a very prominent citizen in a small town in South Dakota where he and another man owned three banks; it's Canton, South Dakota. When they had the Crash around '29 or '30, when the banks were all going broke, he and his partner paid everybody off. So nobody lost anything from his banks.

She had another brother who was a farmer in North Dakota. Then she had three sisters. Two of them lived in South Dakota. (Overseth) One sister married a farmer who was very successful. Another sister married a farmer in Minnesota (Clapp), and a third sister also married a farmer (Rise). I remember all of them.

Hughes: I know your mother graduated from college and had a considerable aptitude for mathematics.

Lawrence: Yes, she was a very good mathematician, and she taught school. That's where she met my father who was principal of the school when she was one of the teachers.

Hughes: Could you tell me something about your family when you were growing up? For example, who was responsible for disciplining the children?

Lawrence: I think that they did it mostly by setting an example. My father was a page in the capitol in Madison, Wisconsin. He nearly went to law school because he worked in the law offices of Robert Marion LaFollette who was a famous liberal. My father was basically a liberal although a different kind of liberal than some liberals are now. He nearly went with Robert LaFollette and nearly stayed with him and was going to go to law school. But after graduation from the University of Wisconsin he decided to get a job teaching if he could. He suddenly got this offer from a small college in South Dakota to become professor of Latin and history, which he accepted. He rode into the state on a bicycle and that's where he met my mother after he'd been there for three or four years.

Hughes: What did he study at the University of Wisconsin?

Lawrence: I think he majored in history and Latin. He knew Latin very well and he knew history very well. So he had a liberal arts education.

Hughes: You mentioned your father's interest in politics as a younger man. Did he keep on with that interest? Was he always politically oriented?

Lawrence: He was a great believer in Robert LaFollette, who founded the liberal party. He was a great patriotic American. He was a good citizen and was very much interested in politics, although he never went into it—although that was somewhat political, being a college president in a state university. The governor had more influence than he does here. I can remember that I met the various governors that he was under. He was an impressive fellow and he could get along with the politicians all right, but he wouldn't let them dictate to him. He was a pretty strong character, but he had enough stature mentally and physically so that they took him pretty seriously. He was a reasonable man, though, highly respected.

Hughes: Do you remember having political discussions at home?

Lawrence: Oh, yes. Then they came out here and retired here. He was basically a Republican, or perhaps a better definition would be an independent.

Hughes: Did you speak Norwegian at home?

Lawrence: My father and mother were very careful to avoid that, and I think they made a mistake. In that country where we were raised until we went away to college there were a lot of people that never learned to speak English well and my mother, particularly, wanted to be sure that we spoke English well. She spoke perfect English and so did he. But they could both speak Norwegian. But I think it was a mistake because I would appreciate... I can speak German and I can get by in French, but I think it's an asset to know languages. She just objected to these immigrants and their children who never learned to speak the American language.

Hughes: Who do you think had the most influence on you as growing up, aside from your immediate family?

Lawrence: The family doctor I liked very much, and that's what made me think I wanted to go into medicine when I was a young boy. My uncle, I had a lot of admiration for him. He was highly respected, my bachelor uncle. But I think most of the influence came from my father and mother, particularly my mother. She wanted us to both amount to something. She was the one that kept us aware that we had better do well in high school and in college. So I think she had the greatest influence, although my father was an excellent speaker and became a President of two different colleges and was highly respected.

He was very athletic. He was about six-foot-four. He was an athlete and he pitched on the baseball team and rowed on the crew. He was quite religious. He could substitute for a sermon; I've heard him do that. Yet he wasn't one of these people who is holier than thou. He was a real man. He'd smoke a cigar once in a while and later on in life he'd have a scotch with my brother and me when we came home. "John," he said, "if a man doesn't have some bad habits, at least one or two bad habits, there's something wrong with him." (laughter) So he was a very practical guy. But they didn't have any cocktails at home when we were growing up. But

later on in life, I think due to the influence of my brother and me... We said, "Well, as you get older, it's a good idea to have a little wine or a cocktail. It's probably more healthy than not."

Hughes: But not your mother.

Lawrence: No, not my mother. I can remember her even when he was older. She'd say, "Why Carl, you shouldn't drink that whiskey." So she never agreed to it. But she had a lot of respect for him, and he was a great leader in the state, but he had a lot of respect for her, too. It was a mutual thing.

Hughes: Would you say it was pretty much a traditional household—that your mother ran the household and he took care of the external affairs?

Lawrence: Yes. These two different colleges furnished him with a house, just like they do here, and a gardener to take care of the garden and so forth. She didn't have any regular help. She did all the cooking and all the housekeeping. I can't remember whether she had anybody come in to do cleaning or anything like that. I don't think she did. She had a very nice house and kept it very clean.

Hughes: Did the family have to do a fair amount of entertaining just because of your father's position?

Lawrence: Oh yes, she did a lot of the entertaining. She was a great, great hostess. She was a very handsome woman and had a nice personality and there was nothing put on about her. She wasn't social. She just entertained because she thought that was part of her job as the wife of the president.

Hughes: Did she ever help you with your homework? I'm thinking particularly of mathematics, since that was her particular field.

Lawrence: I don't think she did. I can't remember either one of them helping me in homework, either in the case of my brother or me.

Hughes: Can you remember them expressing any preference about what career either you or your brother might eventually decide upon?

Lawrence: Well, I remember my brother apparently wanted to go into medicine too. I was set on it when I was ten years old. But my brother changed when he went to the university. The first year he went to a Lutheran-backed college called Saint Olaf's, which is in Minnesota, and it's still going. He flunked out in electricity and magnetism, and he became one of the greatest living authorities on it. He didn't like it there. I think he thought it was too provincial, and so he transferred to the University of South Dakota.

There he came in contact with Lewis Akeley, who was a dean of engineering and professor of physics. He spotted Ernest as an unusual guy. After my brother died a fellow, Ferdinand Smith, who was in that same class with him, came out. He was a prominent businessman in Omaha and I remember he was on the baseball team. He told me how one day Dean Akeley, who spent a lot of time with my brother after class and gave him special things to do, one day in the class embarrassed my brother. He said, "There's a fellow in this class and I want you to all take a look at him. He's going to be famous someday." He said, "There he is right there. That's Ernest Lawrence. Here's a fellow that's going to amount to something."

Hughes: Do you remember ever having any doubts about medicine as a career?

Lawrence: No, no doubts at all.

EARLY EDUCATION

Hughes: Let's go into your educational background. Can you remember anything about your grammar school years? Where did you go?

Lawrence: I went to public school. I did very well up until I finished the eighth grade. I remember I was the best speller in the class. We had spelling bees in those days. I did very well in grade school and elementary school and got very good grades, and my brother did, too. Matter of fact, when I helped this man Herbert Childs go to the various towns that we lived in, why, we went to the school where my brother and I went when we were in the first grade. They pulled out the grades for us. They were very good grades.

I didn't do so well in the first two years in college. I played on the basketball team and I had a girl. This boy (Steven Lawrence) now that I've got, I'm going to take him to lunch at the Faculty Club; he's my youngest son and a sophomore in college. He hasn't settled down yet. I remind myself that I didn't either at that age.

Hughes: What settled you down eventually?

Lawrence: I think it was the realization that if I was going to get into medical school I'd better settle down. So at the end of my second year, I just quit everything but study, practically. Then I rose right to the top of my class and I was the first Phi Beta Kappa awarded at the University of South Dakota. I just worked like the dickens. I'd go to the library every night and about half the weekends. And I did that in medical school, too. I was at the top of my class at the end of the third year.

Hughes: Did you have Harvard in mind from early on?

Lawrence: Yes.

Hughes: Because it was the best.

Lawrence: Yes, that was known to be the best medical school. I mean, you could never say that sure but everybody thought that was the best. Just like Harvard now is supposed to be very good. My son Mark went there. Now he is an able chest surgeon.

Hughes: I believe you spent two years at...

Lawrence: Southern State Teacher's College, which is now a branch of the University of South Dakota. Then the last two years I spent at the University of South Dakota.

Hughes: Your father was at the Teacher's College at the same time. Was that why you went there to start with?

Lawrence: Yes, I think that that's one argument for going away, because I stayed at home and was having a lot of fun. I don't know what the answer is, but I didn't do very well those two years there.

Hughes: Is that why you transferred? Or is it only a two year college?

Lawrence: It's a four year college.

Hughes: Why did you transfer to the University of South Dakota?

Lawrence: The University of South Dakota was a bigger school and it was the number one school in the state.

Hughes: You were taking a pre-med course at that time?

Lawrence: Yes, I was taking the pre-medical courses.

Hughes: Were you writing back and forth to your brother during this period? Was he having any influence on what you were doing in school?

Lawrence: Yes, he had much influence on me. We were four years apart as far as school was concerned. So we never were in high school together and never in college together. I think we always wrote back and forth after I went away to the University of South Dakota. I think from that period of about two years before I graduated and from then on for many years we wrote once every two weeks or so to each other.

Hughes: From the very beginning was he telling you what he was doing as far as his research was concerned?

Lawrence: Yes, he used to, and I used to visit him, too, at various places. He went from Vermillion, the University of South Dakota, to Minnesota for one year with a professor there. That professor was going up the scale and so he took Ernest to Chicago where he was offered a better job. Then he was offered a job at Yale, so he took Ernest with him. This man's name was W.F.G. Swann, and I met him two or three times. He later became director of the Bartol Research Foundation in Philadelphia.

I remember when I enrolled at the University of South Dakota, Ernest was home at that time. He went down with me and I can remember the influence he had on me then. "You've really got to start hitting the ball now," he said, "because if you're going to get into a good medical school, you really better settle down."

Hughes: You paid attention to what he was saying?

Lawrence: Yes, I did, because he was doing well already. I don't think he'd gotten his Ph.D. quite yet. (Ernest Lawrence was awarded a Ph.D. from Yale in June 1925.) He graduated from South Dakota in '22. So in '24 he was just going to Chicago, I think. But he'd already done some experimental work and published a couple of papers. He was getting to be well-known already but he hadn't gotten his Ph.D. yet.

Hughes: You majored in chemistry. Why chemistry?

Lawrence: Well, my brother and I talked about science and medicine and both of us realized, even in those days, that there were too many people advising you to stay out of science if you were going into medicine. I think I've told you that the Department of Chemistry at the University of South Dakota named a seminar after me. It's called the John Lawrence Interdisciplinary Symposium, which they hold every year.

Hughes: And that's in chemistry?

Lawrence: Yes, it's in the Chemistry Department, but it's interdisciplinary and it's held in Sioux Falls, South Dakota, which is the biggest city. I've never been to this seminar, but I will go this year and speak. I majored in chemistry and did very well. I became a good friend of the chairman of the department, who was a very good organic chemist, Arthur Pardee.

Hughes: Did you do any research as an undergraduate?

Lawrence: I helped one of the professors a little bit, but I was only there for two years.

MEDICAL TRAINING

Hughes: Were you thinking about what sort of a doctor you wanted to be? Was there any thought at this time of perhaps going into academic medicine and doing research?

Lawrence: Well, there was a man in Sioux Falls who was the leading surgeon there, and I was thinking that what I'd do is to go back to South Dakota and get into this big clinic,

which is still going, called the Sioux Falls Clinic.

Hughes: When did you give that idea up?

Lawrence: Let's see if I can figure that out. Well, I think Harvey Cushing, whose picture is up there... I got to know him as a student. Just how it happened, I don't know. I remember once he called me down in front of the class of 135 and had me examine a patient and look at the eye grounds of a patient. He could see that I knew how to handle an ophthalmoscope. He was a brain surgeon, and he invited me out to his house, and so I saw a lot of him. I did my first research with him. He was an example of what I thought was fine with medicine—a man who's a clinician but also does research. So I think he had the influence on me.

Hughes: So before you had graduated from medical school then?

Lawrence: Yes. My brother being in research and being in a university, I became interested in getting attached to a university and combine clinical work with teaching and research. So I think Cushing had the most influence, plus my brother. Gradually I got away from the idea of going out to South Dakota and working in this fairly large clinic with a man whom I knew very well who'd more or less said, "Well, you'd better come back here and I'll give you a job working in the clinic.' (Dr. Stevens, a surgeon)

I didn't know whether I was going to do medicine or surgery, but then Cushing had a vacancy for an internship when I was a fourth year medical student, about the middle of the year. So Cushing called me up and wanted to see me. Later on I found out why the other man didn't appear—Cushing wouldn't take anybody who was married. So he just let this fellow go. I didn't know that until years later. I said, "Well, I'm not sure that I want to do surgery." But he said, "That don't make a difference." He said, "You want to really know how to take out an appendix, and you want to know how to set an arm and even if you are going to become an internist, you better take a year of surgery." So he talked me into it and picked me right out of the class. He knew that I was number one in the class. I said, "Well, what am I going to do about my M.D.?" He said, "I'll take care of that." He said, "You don't have to finish school." So I got my M.D.

Hughes: So you didn't really finish your fourth year?

Lawrence: No, I didn't finish it.

Hughes: Had you decided before that that you wanted internal medicine?

Lawrence: I was still open-minded about it. But I was tired of going to school anyway. I'd worked pretty hard for several years and I was anxious to get on with other things.

Hughes: During that internship year did you have time to continue your research with Dr. Cushing?

Lawrence: Yes, that's where I continued research and wrote a couple of papers in his laboratory. I worked with him off and on for several years, even after I went to Yale. He got me a job after I'd had my internship with him. Then I decided that I wasn't going to do surgery. He said, "I'll get you into a very good place, run by a man who did the same thing that you're doing. He had a surgical internship under me, decided he didn't want to do surgery. Now he's chairman of the Department of Medicine at the University of Rochester." So he called him up and said, "I've got a fellow that I want you to take on." So he took me on as assistant resident just as if I'd had my internship in medicine.

Hughes: Why did you decide against surgery?

Lawrence: I don't think I was particularly good with my hands and knot tying. I just didn't seem to be very expert with my hands. I just had a feeling that I wasn't naturally cut out to be a surgeon. Maybe that was a mistake; I mean, I could've become a surgeon, I think. But that was my feeling at the time.

Hughes: Had you decided upon internal medicine at that stage?

Lawrence: Yes, then I decided I'd go into internal medicine.

Hughes: I know you were at Rochester for a year. Why did you decide not to finish your residency there? You moved to New Haven.

Lawrence: I knew the chairman of the department of medicine down there. He was a Harvard medical graduate, a rather famous doctor, and it was a better medical school, I think.

I didn't like Rochester very much, anyway. It was very provincial. I got acquainted with New Haven when my brother was there. He was a young assistant professor there. Well, I went down to see him when I was a medical student, and he would come up to see me in Boston. We'd get together for Christmas and Thanksgiving, and so I saw him all through medical school. Let's see, when did he come to California?

Hughes: 1928.

Lawrence: Yes, that's right. He came at the end of my second year. But during my first and second year at Harvard I got out to Yale quite a bit and he'd come up to Boston.

Now that you mention this, I can remember when he left for California. Well, I can remember lots of things about that, but one thing I can remember is that one night after my final examination in pathology, we went to an Italian restaurant. In those days they'd serve you wine in coffee cups. It was illegal. So we went to this Italian restaurant and there was a famous and very attractive German by the name of Otto Stern who spoke broken English. He later retired and lived in Berkeley. He got the Nobel prize. Well, I think he was probably Jewish and so like Edward Teller, who's one of my closest friends now, he was sort of forced out of Hungary. We got a lot of people like that in this country.

We started talking about Ernest's idea about the cyclotron. I don't think he'd named it yet. He made a drawing of the cyclotron on the tablecloth with a pencil—the idea of whirling around the nuclei. He made a drawing of the dees with the magnet and everything. He'd done nothing on it. (pause) I remember Otto Stern said in German, "Sie mussen zurückgehen." This must've been during the year of '28-'29. That would be my third year in medical school. Otto Stern saw that this was a terrific idea and Ernest had done nothing about it. He got the idea in the library, reading an article by a Norwegian physicist, Rolf Wideroe, whom I met later on, who didn't suggest this exactly, but it made Ernest think of it. They'd been accelerating particles in a straight line and he said,"Why not do it in a circle and then keep on giving it a kick?" Stern became a great friend of my brother's over the years.

Hughes: Do you remember if your brother then quickly went back and made the first cyclotron?

Lawrence: Oh, then he went right back to work on it. Yes, then he jumped on the train the next day—he couldn't fly in those days—and he went to work on it right away.

I can remember later on when he got the first little unit put together... He wasn't a very good engineer but he could put things together and make them work. Sort of a haywire type of engineer. When he got the first glow in a little unit, he gave a paper back in Washington. By that time I was at Yale as a resident in medicine and I remember that he gave the paper at the National Academy meeting in Washington. A friend of his heard him give it. Ernest made the statement, "This method that we've developed, there's no reason now we can't go up to 100 million volts." No one had gotten above one million or two million volts in those days. This man who was a good friend of his said, "Your brother is losing his mind. He's crazy." He was really serious about this. He said, "The talk down there in Washington was that this bright young Lawrence is just off his rocker, the way he talks."

Hughes: Well, he seems to have done that throughout his career. The theoreticians would say, "Well, no, that can't be done. You can't get energies of that kind." And your brother would go ahead and do it anyway.

Lawrence: Yes, that's right. Rutherford said that you would never get energy out of the atom. I've got that newspaper clipping framed at home in my living room—a quotation from Rutherford and picture of Rutherford and a small paragraph about this young physicist who said, "Well, Lord Rutherford, you may be right but we're going to keep trying."

Hughes: What was it that made him so convinced that he could do it despite the theoretical background?

Lawrence: Well, he had a peculiar enthusiasm that I think was characteristic of the way he operated. He had the feeling that things could be done if they're sensible and reasonable. You can just keep working and with enthusiasm, why, it'll work out. I think that's the reason he had so much influence on a lot of younger men here and also at Yale, but mostly here. All these tremendous numbers of young men that became famous after working with Ernest—mostly because of his enthusiasm and the way he could come around and talk to them and influence them and encourage

them. It was part of his blood. He just was very enthusiastic.

When this fellow came to me and said that he's crazy, I said, "No, he's not crazy. There's nobody that I know that's got more horse sense than Ernest has." He had very good judgment—judgment of people, judgment of physics, judgment of the energy of the atom. He knew the energy was there, and he thought it could be gotten out.

I met Einstein once with Ernest. He never knew Einstein terribly well, but Einstein and Fermi—there are so many really great physicists, probably even greater than Ernest was. But Ernest was really great because of his enthusiasm and he was a great experimenter. Einstein was a great mind but not an experimenter. Fermi was maybe a theoretician too. I knew Fermi fairly well. I'd talked to him a few times. These are two of the great names in physics. And of course Teller is a great name in physics. They were all theoretical.

Hughes: Well, getting back to you and your move from Rochester to New Haven, I know that Harvey Cushing moved from Harvard to Yale at about that time. Was he there when you arrived from Rochester? That would've been 1932. He became Sterling Professor of the History of Medicine.

Lawrence: Yes, Cushing was there.

Hughes: Was that any influence on your decision to go to New Haven?

Lawrence: I can't specifically say that it was, although I was in touch with him, corresponded with him. He retired at 65, I think; you had to retire at the Brigham Hospital. So he went down there as professor of the history of medicine. He was a great historian, great reader, and a great writer, wonderful speaker. But he didn't speak very much.

My brother was awarded a degree at Stevens Institute of Technology when I was at New Haven. I was resident in medicine then. He had to give the commencement address. He didn't like to speak, my brother didn't. I knew Cushing could write so I took Ernest in to see Cushing. Ernest had known Cushing in Boston too, because I knew him and we'd visit with Cushing quite a bit. So Cushing practically wrote this commencement address. I heard Ernest give it. It was pretty good. And then he got an honorary degree. He was only about 30 years

old. He had a lot of honorary degrees when he was very young.

Hughes: Cushing never did any research at Yale?

PRE-BERKELEY RESEARCH

Lawrence: Yes, he did. I began working with Cushing at Yale on patients that had Cushing's disease, because when I was an interne with Cushing in Boston, I worked up the first patient that he'd made the diagnosis on. Nobody recognized this condition. I've just written a paper on it recently because I've seen a lot of patients with Cushing's disease and we've developed a method for treating it right here with one of the accelerators—very successful treatment. But in those days it couldn't be treated. So I began working with Cushing on patients with Cushing's disease and I wrote a couple of papers then, too. He had patients come from all over the world, even when he was down at Yale, with Cushing's disease particularly. When they came to the hospital I'd more or less take charge of them so I saw a lot of Cushing down there.

Hughes: I know you were working with the pituitary and one of the research projects was the effect of radiation on the pituitary.

Lawrence: Well, I first worked with the pituitary when I was an interne in Boston. I did a lot of experimental work on dogs on the pituitary, and the on patients too. I became interested in pituitary tumors because Cushing had a lot of them. So how I got interested in the pituitary is chiefly through the clinical work and then the animal work in Boston.

Then I got interested in hormones, so I started doing some experimental work with pituitary hormones—measuring them in the urine and the blood stream. There was a very famous endocrinologist at Yale by the name of Edgar Allen, and he helped me get started. Another fellow by the name of William Gardner, a great anatomist and biologist. So I'd go across the street to the basic science departments and work when I got a little time off from my teaching and clinical work. I had a laboratory of my own, too, with a technician and I had a colony of animals—a good sized colony of animals.

Hughes: Are you talking now about your residency or was this after you became an instructor?

Lawrence: My residency plus after I became an instructor. The lab started when I was a resident.

Hughes: Was Dr. Gardner an endocrinologist as well?

Lawrence: Yes, he became an endocrinologist, William Hugh Gardner.

Hughes: When did you begin to use irradiation?

Lawrence: Well, in the case of Cushing's disease, we became involved with the treatment of these patients and there I got acquainted with the head of the X-ray department, Hugh Wilson—well, I knew him. So with an experimental endocrinologist, Warren Nelson, I did most of the work. We did a series of experiments on littermate rats, radiating their pituitary with X-rays, which wasn't very successful. We wrote a paper on that. Wilson was the professor of radiology and the endocrinologist became a very famous man—he was an expert on endocrines of animals and on the histology of the pituitary gland. So I did the experimental work, but I had the help of the professor of radiology and also this professor of experimental endocrinology.

Hughes: At about this time your brother was helping to install the thousand kv-ray apparatus at the Medical School. That was 1933. Since you were now also interested in irradiation, do you remember any discussions of the possible adverse effects of X-rays on the technicians or the physicians working around the apparatus at the Medical School? That was before you had even come to Berkeley.

Lawrence: I can't remember anything that sort-of looms up.

Hughes: Do you remember when you were doing the irradiation work in connection with the pituitary, both in Boston and at New Haven, was there any discussion of protection and the need to be careful?

Lawrence: In those days people weren't conscious of the dangers of radiation. In '37, after I'd done my first experiments out here beginning in '35, I gave a paper with Paul Aebersold and Ernest. I gave it at the International Congress of Radiology. That

was in Chicago at the Palmer House, and I can remember meeting a lot of men from this country and from abroad—Sweden, particularly, and from England and Germany—who'd lost fingers or who had obvious scars on their hands where they'd had skin grafts. You would shake hands with some man, he'd only have two fingers or something like that, and he might've been a famous radiologist.

I went to the Mayo Clinic once because I was thinking of possibly going there eventually and I had an introduction to the chairman of the radiology department. He was a very famous radiologist. I think I must've been a medical student then, or maybe I'd finished my internship. I can remember he took me into the examining room where he was fluoroscoping patients, and I insisted on wearing a lead apron over my body, and I wouldn't palpate the patient with my bare hand. Well, here this famous radiologist was doing many patients in a row—six or eight of them—and he wouldn't wear any shielding. He'd just use his bare hands and give them a swallow of barium and he'd turn the fluoroscope on and then he'd fee' the stomach. Well, he died early. I'm sure he died of excess radiation because when I saw him the last time, he'd aged tremendously. He died as a young man. Maybe he was 62 or something like that. But he was really careless, and I was aware of that then. He was getting enormous doses of radiation. A lot of these radiologists did that. So there wasn't awareness of it. I was aware of it fairly early.

Hughes: Were you the one that warned Ernest, or was he already aware of the problem when the cyclotron started up operation and he began to collect people to work around him?

Lawrence: Oh, I think he was aware of... He wasn't so much worried about gamma rays or X-rays but he was aware of the potential danger of these new radiations—neutrons.

Hughes: Was that awareness mainly from the cloud chamber pictures?

Lawrence: Yes, those very thick, dense tracks in the Wilson cloud chamber from neutrons, compared to the very fine tracks produced by X-rays and electrons. So you couldn't help but think that this is a different kind of radiation, that it might be more damaging. So I think anybody that saw those tracks and knew a little bit about the physics of neutrons and X-rays would naturally get interested and say, "Well, I'd like to compare these with X-rays."

Interview 2: May 20, 1980

Hughes: You did your residence at Yale, and then you became an instructor, and were there from 1934 to 1937. What department was that?

Lawrence: That was in internal medicine. The chief of internal medicine was Francis Blake, who was a famous internist. There was another famous professor that I worked with, a Professor John R. Paul.

The most important influence on me then was the continuation of the influence of Harvey Cushing, whom I knew very well, as I've told you before, as a medical student, and then as an interne at the Brigham Hospital in Boston, which is still a very famous hospital, and still very much sought after by people who want to get training. Cushing had retired from Harvard. I think he was either 65 or 67. I think maybe it was 65, but the retirement age had come. So he then went to Yale, where he had graduated as an undergraduate, and became professor of the history of medicine. But he continued to have patients, and so I continued to see him very often all during my period at Yale. He had patients with Cushing's disease that I became interested in Boston. I just sent in a paper here, two or three months ago, on Cushing's disease, to Western Medicine, because I've had 30 to 40 years' experience with it. The first case of Cushing's disease I saw when I was an intern at Boston under Harvey Cushing. Cushing had a great influence on me and he was a very great man. He was a great teacher, a great surgeon, a great speaker, a great writer, a great historian. He worked all the time, and I guess that's probably the reason he was so great, because he worked so hard. Then when he got sick, I was senior resident in medicine, and the professor assigned me to take care of him. Instead of having an intern take care of the patients why. they assigned the chief resident, so I saw him, put him to bed every night and went in and talked to him a lot. He influenced me greatly, actually, in getting into this field of atomic energy.

Someone in Montreal where I had to go to a meeting last week, who is a neurosurgeon, asked me about Cushing's papers. Even before the discovery of radioactivity, Cushing began collecting the reprints of famous physicists like Rutherford and Niels Bohr, and many others, and his collection is in the Sterling Library at Yale, and it's called the Cushing collection. There's a special room for that, and they've got his desk and his chair there. So I told this neurosurgeon who was at the meeting at Montreal about this, and he's going up there to see the

collection. But Cushing had an intuition about the importance of this field. Artificial radioactivity was discovered... when? '32?

Hughes: '34.

Lawrence: Well, I was still in New Haven, and of course then Cushing really became interested. I'd take my brother in to meet Cushing. Cushing anticipated the terrific importance of artificial radioactivity and the new radiations to medicine. Here's a great surgeon who said, "This is going to be more important, if not as important, as Pasteur and bacteriology."

Hughes: Do you think that influenced your brother?

Lawrence: Oh, I think it did. My brother was already a famous guy at the age of, I guess, 28 or 29 or 30. He was well-known all over the country. Oh sure, I'm sure Cushing had an influence on him.

There he (Cushing) is right there (points to photograph on office wall). He was a very handsome man. He was a little man. He was about 5'8", and a remarkable person, and a hard worker. Now there you see (points to another photograph) he just came out of surgery, and he was making a note and a drawing of the operation. Every operation that he did, he would go out in the next room, he'd write a hand note, and he'd make a drawing of the operation, of the exposure. He was a pioneer in brain surgery.

Hughes: How was he treating Cushing's disease in those early days?

Lawrence: Well, we treated them with X-ray. The first one was a dentist from Chicago, and we couldn't get much radiation in—just a temporary thing. It didn't cure them like we can do now.

Hughes: Did you continue doing research on Cushing's disease?

Lawrence: Well, I began doing work on the pituitary in dogs in Boston when I was an interne with Cushing, and wrote my first paper. I continued working the pituitary in New Haven. Wrote a couple of papers on it there: The effects of X-radiation on the rat pituitary. And then I saw many patients with a clinical picture of pituitary disease,

and I wrote a couple of papers on that when I was there, too. So I continued that for all the time I was at Yale and I began it out here too, after we got to the point where we thought we could irradiate patients with acromegaly and Cushing's

Hughes: Do you remember being interested in what your brother was doing with the 1000 kV X-ray apparatus? I believe it was one of the two, or perhaps the, most powerful X-ray apparatus in the world at that stage.

Lawrence: Yes, there was a graduate student here.

Hughes: David Sloan.

Lawrence: I knew what EOL was doing, and we corresponded a lot for many years. I think the credit should go to the student. Sloan was Ernest's student. It sometimes is difficult to find out just how much influence the professor has over his student. My brother and Dave Sloan wrote an article on a million volt X-ray tube. Now this was either the first, or it was parallel to the million volt tube that Robert Van de Graaff made at M.I.T. I think that it was more or less parallel that those two tubes began being used in the therapy of cancer. That was the first time high voltage X-ray had been used. Of course, that's not high voltage now, but you get a better penetration and less skin effects.

Hughes: At Yale you had a mouse colony with certain tumors that you were able to grow.

Lawrence: With leukemia?

Hughes: What was the sequence from the pituitary to the cancer research? Was there a tie-up there?

Lawrence: No, I was feeling my way. I think the way I became interested in that was that I was irradiating... I did the first work, it's recognized as the first work, on radiation protection. And it was accidental. I was trying to induce leukemia in mice with radiation at Yale. Let's see now how that story goes. No, I think this is the way it was. A fellow by the name of William Gardner, who is about my age now, became professor of anatomy at Yale. There was another man by the name of L.C. Strong, who was also at Yale, and he had the first colony of mice that were inbred for many generations, hundreds of generations.

Hughes: Do you mean the first line anywhere of inbred mice?

Lawrence: First in the world, L.C. Strong, yes. He became a very good friend of mine. Bill Gardner was inducing leukemia by large doses of estrogen, and I thought, "Well, maybe I'll combine estrogen with X-ray." Then I found radiating mice with whole body X-ray and giving them estrogen would protect them somewhat against radiation. That's in the literature now. There's a fellow in San Francisco at the Medical School by the name of Harvey Patt who has written about it. He extended my work. Then when I came out here we repeated it and I got Anne Treadwell, who was my technician, to do most of the work. We repeated it and it was true, and we wrote a paper on that. So that was the first compound that protected against radiation.

But then we also induced leukemia by radiation. In other words, you give an animal a sublethal dose of radiation, and maybe do a hundred of them, and you'd get maybe 10 or 15 percent that would develop leukemia. Strong had a strain of mice, 100 percent of them would develop cancer spontaneously, cancer of the breast. So Bill Gardner was developing tumors: lymphosarcomas and sarcomas and leukemias. I'd get those strains from him. So I'd develop my own colony between Strong and Gardner and also from a wonderful medical scientist at Cornell in New York, Professor Jacob Furth. So the pituitary was not involved here.

There was another famous man there from whom I learned a lot about hormones. He later got the Nobel prize. His name was Edgar Allen. Allen and Doisy synthesized the female sex hormone. He's a professor of anatomy too. So I used to do some routine urine assays on patients, and learned his techniques. And that involved the pituitary hormone too.

Being under Blake, who was an infectious disease man, I had a colony of ferrets. See, those were the days when you couldn't produce leukemia or flu experimentally except by having a colony that you'd inbreed. I became interested in the flu, and I used to go around the small prep schools and high schools in the East on weekends and wash out the throats of children, bring the washings back to New Haven and inoculate my ferrets. There was a man in England who showed that ferrets would develop a disease like influenza if you injected into their throats the virus from kids who had the flu. So I was feeling my way in various directions.

Tumors and influenza in ferrets...

Hughes: Now this goes back awhile, and it came from a letter to your brother written in 1934 when you were talking about radioactive sodium chloride, which I think was just beginning to be produced in the cyclotron. You were talking about injecting the radiosodium.

Lawrence: I think I remember that. I think I wanted to get some radioactive sodium to do some studies at Yale.

Hughes: I don't know if it resulted in that, but you were discussing the possibility of injecting the radiosodium... (short blank in tape) It wasn't a good idea. Why?

Lawrence: Well, sodium would be rapidly diffused.

Hughes: Did your brother ever send you some radiosodium while you were still at Yale?

Lawrence: No, just too complicated a problem. They were very short of it, and I began coming out to Berkeley for short periods of time.

PRE-WAR ASSOCIATIONS AND RESEARCH AT BERKELEY

Hughes: Tell me how that was arranged. I know you had a few months off from Yale for a couple of years (1935-36).

Lawrence: Well, what I'd do is I'd go to this professor of mine (Francis Blake) and say, "I want to spend some time in California for a couple of months. My teaching schedule is all through for three months and I can keep my research going here with my two technicians. But now I've got this thing that I want to start in California."

I went down to New York and met the head of the Macy Foundation, Ludwig Kast. I later gave a lecture in his honor…[the Ludwig] Kast lecture at the New York Academy of Medicine, probably in about 1946. Ludwig Kast was the doctor of the Macy family. They'd just founded this big foundation and I'd heard about it. So I went down to see him about getting a grant to pay my expenses to come home to California. So he supported my trips out here; I made about three a

year. He gave me enough money so I could travel third class; take my mice with me on the train. I think I told you about that before.

Hughes: Yes, you talked a little bit about that.

Lawrence: I'd bring about a thousand mice, and in the wintertime I'd have to go up and feed them. I remember in Chicago once I had to transfer them in cold weather from one train to another. So I got my money from Ludwig Kast. Then I went to another foundation. Can't think of the name of that foundation

Hughes: Was it Markle?

Lawrence: Markle, yes. Archie Woods was head of Markle. So I got acquainted with Archie Woods. He made me a grant and I got other grants too. In those days the government wouldn't give any money. We had to get our money from private funds. That was true of my brother's work too.

Hughes: What was your statues as far as the University here was concerned? Were you a visitor?

Lawrence: I was just a worker, just like everybody else.

Hughes: Did you actually have a U.C. appointment as well?

Lawrence: No. Most of the people here now don't have any appointments either. Some of our best people don't have any University appointments. No, I had an appointment at Yale, and I was just a visiting scientist. We have visiting scientists here all the time.

The first work I did here, before I got the big colony out here, was on the sarcoma 180, which again was not a good tumor because I had a mouse that you could get very easily by shipping them from the East, called the Swiss mouse. But the Swiss mouse, although easy to buy, if you'd inoculate a hundred animals with these tumors, about 85 percent would grow and the other 15 percent wouldn't grow. Then the ones that did grow, some of them would regress. So here is the first example of immunity to a tumor. I could have followed that, too, and I got rid of those animals, because I wanted an animal that I could test for radiation in which... (background noise) I knew that the animal would grow the tumor if I didn't kill it

with radiation. (interruption)

I had a wonderful colony of animals and nobody else had them in California then. And a lot of special tumors that we used in all of our early experiments. I could have gone off in the direction of immunity, because here I had a natural animal that was resistant to this tumor. The tumor would grow and then regress. Well, that's what happened when you cure people from cancer. You treat them with radiation, but what really helps you is the body. But no one's been able to measure that. I don't know if I'd been able to do it either. I just wanted to study the effects of radiation and get ratios of neutrons and X-rays. I was sort of limited in my outlook at that time. I wanted something that was a real accurate measure. So I never followed that up.

Hughes: When you were getting rather striking results in mice with the total body exposure with X-rays were you extrapolating to the human? I realize that you knew that there was a danger there, if nothing else from the radium dial painters and all of that. But I don't think anybody in those days could put a numerical value on the danger of any form of radiation. When you were finding that it was not too difficult to induce leukemia, was it giving you concerns about the 1000 X-ray apparatus? Stronger and stronger machines were being built at that stage, and were safety precautions increasing with the strength of the radiation?

Lawrence: In the treatment of people like my mother and thousands of other people that were treated then and are being treated now, you're really treating a small volume of tissue. I don't think there's any evidence that you're inducing leukemia. If you irradiate the whole body, like at Hiroshima and Nagasaki, or people that got enormous doses to their whole body for arthritis of their spine, in England particularly, then you would get an increase in leukemia. But for a small volume of the body, you don't. Then of course, even if you did, if you have someone that's dying.... One thing that's not considered now in the newspapers is benefit versus risks. You have to take risks sometimes.

I think that's one reason that some people are so much against the atomic reactor; they don't realize the benefits from an atomic reactor in the form of electricity. All they think about is the risks.

You take a person who's got a goiter, for instance. The first person that was

treated in the world with radioactive iodine was treated by my colleague Joe Hamilton. I remember the first two patients that he treated with radioactive iodine. Now that's practically taken over the so-called exothalmic goiter, where people get a diffusely enlarged gland, in women mostly, young women very often. Then they get prominent eyes, some of them do, and it's called Grave's disease. I suppose millions of people have been treated with radioactive iodine, which was started here with an isotope that was discovered here.

Of course, as I have told you before, the minute those counters started going wild over there, and we read about Joliot and Curie's discovery of radioactivity, why then my brother and Ed McMillan and Martin Kamen and Luis Alvarez and Robert Wilson, a lot of them were making news. Most of these fellows became famous later. They were just young fellows. Seaborg soon joined the group, too. They started making all kinds of isotopes.

But to get back to the thyroid; I was thinking about it this morning. Wherever you go in the world, even in India, why, people are treated with radioactive iodine. If they got, say, ten millicuries, those patients would get ten R whole body radiation; I think that's true. But that's an enormous amount of radiation compared to fallout and compared to reactors. Reactors maybe give you a few milli-R or more. Maybe one reactor would give half a milli-R to the general population.

Hughes: Of course when Hamilton first did it, he must have used even heavier doses than that, didn't he?

Lawrence: No, I think those first two patients got about three millicuries.

Hughes: I thought because the instrumentation was much more primitive larger doses had to be given. Wasn't he using geiger counters?

Lawrence: Yes.

Hughes: I thought you needed a bigger dose of radioisotope just to be detected.

Lawrence: Oh, no, you could detect it. No, the doses were smaller than they use now. They were about three millicuries. Well now to get to my original statement: I think the

average patient was given about ten millicuries, and that gives you about one rem of whole body radiation, which is 1,000 millirem. So that would be 10,000 millirem for one patient's treatment. 6,000 R, the thyroid would get. And the body would get six R. So the thyroid would be treated successfully with the dose of say six millicuries. This would give 6,000 rads to the thyroid and six rads to the whole body. Six rads is 6,000 millirads.

Now several people, including people in this Laboratory, have written papers where they've followed patients who have received iodine-131 for Grave's disease, thousands of cases. One of them is an Englishman by the name of Sir Eric Pochen. Fellow about my age; he's still active. I saw him a couple of years ago in Europe. They've shown that in a control group of people that have gotten this treatment of radioactive iodine for Grave's disease, that there's no increase in cancer or leukemia in them compared to the controls. Matter of fact, the controls have a little bit more. Now that's a big dose of radiation. So you see, no one worries about that. Here you get people who get 6,000 millirads or 10,000 millirads. And here people get 2 or 3 millirads from reactors and they start worrying about it.

Three Mile Island, for instance, is a good example. There was nobody hurt at Three Mile Island. There were only three people at Three Mile Island that got over, I think, one rad, and they were working right around close. Most of them got way down less. I saw a list of the doses recently when I was at this meeting in Montreal. The one mistake they made was to shut off the water. Instead of leaving it on, they'd shut it off, and then that caused the leak to happen. The cure for that apparently is that they've got to train these operators a little better. If they'd called in some engineers, it never would have happened and we'd have never read about it.

Hughes: What were the main reasons for your decision to come permanently to Berkeley?

Lawrence: I think the simple answer is that my brother and I were four years apart in high school and college and yet we kept very close to each other regarding what we were doing. When I was at Harvard and when I was at Yale, why, even though he was never at Yale when I was at Yale, I'd see him at Yale and I would see him at Harvard when I was a student. So I knew everything that he was doing, and he knew what I was doing.

I never thought we'd ever get together, my brother and I, because we were in

different fields. Yet he and I talked about the lack of appreciation of basic science in the average medical school. Medical students were advised to stay away from mathematics and physics and chemistry, and take mostly biology. Cushing saw the tremendous potential of this field of artificial radioactivity. But I still didn't plan to come out here because I was working with leukemia and cancer—I had a large colony of mice and rats with tumors and I was working also with pituitary radiation because Cushing had patients with pituitary problems. So that's why I became interested in the pituitary. But I didn't plan to come out here to stay. I came out here just for a visit. That was in the summer of '35, and my brother had been here for several years already.

When I came in '35 I discovered that they were working around a radiation that they didn't know anything about, that is neutrons. So I borrowed a microscope from Herbert Evans, and some rats and mice. There was a young physicist here who was getting his Ph.D. with Ernest, Paul Aebersold. So we started to study the effects of neutrons on these animals. Then I went back to Yale after the first series of experiments and took a lot of the animals back with me. In the meantime we hired a technician to help us here.

EARLY USE OF ARTIFICIAL RADIOISOTOPES AND NEUTRON RADIATION

Lawrence: I kept coming back and forth over a period of two years. Then in the fall of '37 they offered me a job out here, between here and the Medical School in San Francisco. So my brother and I talked it over. There wasn't an awful lot of activity going on here. Here we had all these isotopes available, but very few people were using them. They were being made available to anybody that wanted to use them, but there wasn't the excitement that we saw and Cushing saw. Cushing urged me, too; he said, "I think you ought to do that if they offer you a job out there."

Hughes: Why was there so little enthusiasm?

Lawrence: I don't know. There were people in biochemistry and in physiology—fellows like D. M. Greenberg and I. L. Chaikoff. We began using radioactive iron. Dr. Chaikoff was professor of physiology and Greenberg was professor of biochemistry. Then we'd send isotopes to Dr. George Whipple at the University of Rochester in

New York, and a young fellow by the name of Joe Ross, who is now emeritus professor at UCLA, began using iron. We'd send George de Hevesy radioactive phosphorous and other isotopes. There were many people that appreciated it, but there didn't seem to be an awful lot of appreciation here. My first work was more or less parallel with Joe Hamilton's work. Joe was then a medical interne in that summer of '35 and he began using radioactive sodium in normal subjects.

Hughes: This was over at the Medical School?

Lawrence: I think he was doing most of the studies over here. On weekends he'd come over here and that's when I first met Joe Hamilton. I'd have to go back and teach at Yale and do ward rounds and take care of my experimental work back there. Three months later the Macy Foundation and later the Markle Foundation would put up my expenses; I'd come out third class on a train and do this about every three or four months. Then in the summers I'd come out too. I made many trips out here that way.

Hughes: What sort of arrangements did you make with Paul Aebersold while you were at Yale? When you would go back to Yale after a trip to Berkeley, would you leave instructions for him to carry on the experiments?

Lawrence: Well, he was a physicist, and I'm a physician, so it was a team. I couldn't have done a lot of this work without his help and he couldn't have done without mine. We would leave some of the animals here and he'd observe them, and he learned a lot of biology and I learned a lot of physics. But that's the history of the Laboratory, both on the Hill and at Donner. That is what you'd call interdisciplinary research—physics, chemistry, engineering. That's what we have here. We have M.D.s and Ph.D.s in various fields. So Aebersold was very important and he became an expert on the measurement of neutrons. I'd set up the experiment and study the animals after they were exposed. Paul would do the physics of the dosimetry and the measurements of the radiation to the animals

Hughes: How was he setting those standards in those very early days when so little was known about the biological effects of neutrons?

Lawrence: Well, we'd always compare the biological effects of neutrons with the biological effects of X-rays. And we still do that now in the work that's going on here. The

X-ray is the base and we found out that there is a variation in the effect per dose on the white count of the rat or on the tumor or on the life of a rat—how long the rat would live after a big dose. The ratio is all the way from two to maybe fifteen or twenty depending on what object you're studying. We set up safety standards for people working around the cyclotrons and limited the dose that they could receive per day or per week or per month to one-tenth of that allowable to the average X-ray worker on an X-ray machine.

Hughes: So you were more or less taking the average from all the biological experiments.

Lawrence: Yes. We'd take a rough average. I think the ratio of the lethal dose for a mouse or rat was about one-fifth of that of X-rays. So we set it at about a tenth. Everybody wore badges. We realized that neutrons were more dangerous than X-rays, but we didn't know an awful lot about neutrons and we've learned a lot since and we're still learning a lot about them.

Hughes: You and your brother were early warners of the dangers of neutrons. Was that warning heeded? Did other workers in the field take the necessary precautions?

Lawrence: I always felt badly about one thing. There were several places in the country that built cyclotrons in which people would get too close to the beam and look into it and then later have cataracts develop. We never had one here.

One of the first animals we exposed—I'm not sure that it wasn't the first one—we encased in a little brass cylinder with an air inlet and an air outlet and this was placed within the cyclotron between the two poles of the magnet near the beryllium target which was being struck by deuterons (alpha particles). So Paul and I told Ernest to turn off the cyclotron because we wanted to go back and see how the rat was. Well, the rat was dead. That scared everybody because it had only been exposed for about a minute and the dose was very low. We were very scared and we then recommended increasing the shielding around the cyclotron. Later on we found that the rat died of suffocation but not of radiation.

There were maybe twenty people in the United States that got cataracts elsewhere. It was found that neutrons were very dangerous so nobody ever got close to this beam after this. It was a beautiful beam to see—sort of like the rainbow. You see the beam coming out of the cyclotron and it's very tempting to

get in and look at it. Then you'd get a big dose of neutrons, so we avoided that thereafter.

Hughes: What was the attitude of physicists in the early days when you and Paul Aebersold were using the cyclotron for biological experiments?

Lawrence: I don't have any memories of people competing for time to the extent that they didn't like to have us around with the odor of rats and mice. But I can remember that there was some competition for time. There were a lot of young fellows here, like Luis Alvarez and Ed McMillan—they were a little older—then Bob Wilson who later became director of the Fermi Lab. Those men were doing research and theses; they wanted time, too. The cyclotron would break down a lot in those days, so that we were sort of a nuisance.

Hughes: Was your brother pretty supportive of the biological work going on?

Lawrence: Yes. He was always... Most physicists if they think that they can do something that would be helpful in medicine, being normal human beings, are glad to put themselves out to help. I think that's a natural thing. When the subject of cancer came up the physicists wanted to do all they could to help.

Hughes: I've read that one of the factors that supposedly influenced the biological application of the cyclotron was that Ernest Lawrence decided that funding for biological projects might be a bit easier than for some physical projects.

Lawrence: Yes and no. I don't think he was that practical. All of the early money for biology and medicine I got. I raised that money from these foundations. For instance, when they built that cyclotron on the Hill, the 184 inch, that had nothing to do with biology and medicine. However, when our first experiments with neutrons showed some selective effect on cancer cells, in getting Mr. William H. Crocker to build Crocker Lab the possibility of cancer therapy was certainly used as a selling point. Ernest wanted it for nuclear physics. I think that is true for the bevatron. What he was after was to get exceedingly high energy beams. However he certainly used the usefulness of medicine as a selling point. Radioactivity was obviously useful in medicine, biology and biochemistry and there was the possible use of neutrons in cancer therapy.

Hughes: Could you describe the laboratory accommodations you had when you first came permanently from Yale.

Lawrence: Well, I came out on a vacation from my work at Yale where I was on the medical faculty. I saw the Wilson cloud chamber over in the (Old Radiation) Laboratory showing these very dense tracts and the very fine tracts when the cyclotron was running. The Wilson cloud chamber was invented by a Scotsman who was a mountain climber. His name was C. T. R. Wilson, and he would notice in the hills or mountains of Scotland sudden formations of clouds. He said, those are really condensation of water droplets around particles. From that he had the idea of photographing ionization.

Luis Alvarez built a bubble chamber, which is based on a little different principle. The cloud chamber that (Wilson) developed was sort of a piston that would suddenly evacuate the chamber and cause it to be rarefied. It was full of hydrogen gas, so therefore it was full of protons from neutron bombardment and electrons. When it was being bombarded from the cyclotron, then the gamma rays from the target and the deuterons from the target would hit this rarefied atmosphere of hydrogen. There was something else in there too. The rarefaction would cause these ions to gather on the particles. The neutrons being neutral would strike the protons which are the same weight but have a positive charge. They would produce two kinds of tracks—one, the fine tracks of electron ionization and the other, the thick tracks of proton ionization. (The tracks) of the neutrons striking the protons were very thick and long and in the background there were the very fine tracks which were formed by gamma rays knocking off electrons from the hydrogen atoms. X-rays would do the same thing. So here you had the visual picture of two kinds of ionizations. One, a very dense ionization. That is, the ions are very close together so the track would be sort of like a pencil. The track would be loaded with little tracks. Then you'd have these long tracks—lots of those. Those tracks were formed by electrons—very thin and scattered out ionization. These were dense tracks formed by literally hundreds of ions per micron of track. They speak of the transfer of energy per micron. We call it KEV per micron. A hundred KEV is a hundred thousand electron volts transferred per micron of travel.

So I became interested in this. Here were all these young fellows like Ed McMillan and Robert Wilson who later became head of the Fermi Lab. Many other young fellows that were working here were being exposed to this kind of radiation.

No one had ever studied that kind of radiation before.

Hughes: Was there any shielding on the cyclotron?

Lawrence: There was shielding, sure. The total amount of radiation was very great around the cyclotron. But the quality was not particularly being attended to because no one knew about the quality. One roentgen or one red of radiation of the fine ionization was what you measured in cancer therapy with gamma rays or X-rays. One red of that is potentially different than one red of this dense ionization.

Paul Aebersold became an expert on the measurement of ionization, particularly dense ionization. Now we have a man here who is probably the greatest expert in the world. That's John Lyman. Right away I borrowed some rats from Herbert Evans, who was a famous biologist down in the Life Sciences Building and then I went over to Cowell Hospital and borrowed some blood counting pipettes and a microscope. I did all the work over there. I stayed here for about a month (in the summer of 1935) and we did the first experiments on the biological effects of neutron rays. Ninety-five percent of this radiation, Paul figured out, was proton ionization which is dense ionization. It's produced by neutrons hitting the protons, knocking them forward. We used a figure of about ten to one, the killing power of neutrons, the power of knocking down the white count.

Those heavy particles were found to produce cataracts. We never got a cataract here because after these experiments were carried out Ernest and his staff added a lot more shielding. No one ever got in close to the cyclotron and watched this beautiful beam come out because they were scared. We found that these (neutrons) were much more effective per unit dose (than X-rays).

Hughes: When you did come permanently from Yale in 1937 did Ernest give you space in the old Rad Lab?

Lawrence: (Even before I came to Berkeley on a permanent basis) I got a technician to work for me and she would stay here and Paul would work with her while I was gone. I'd come and spend about two months when I was not teaching (at Yale). I had my own laboratory at Yale. I had a colony of mice and rats, normal and with tumors, and a technician working for me and a couple of students working with me—wonderful facilities there. Also I had a colony of ferrets at Yale for several years.

So I kept the two places going: the laboratory at Yale and a small facility here in the Old Radiation Lab. The physicists didn't like that very well. I had a lot of good friends among physicists. One of them was here recently and called me but I didn't see him. His name was Bernard Kinsey. He just retired as professor of physics at the University of Texas. I used to keep the mice there too. The physicists didn't like the odor of mice. Bernard and I were very good friends. Once in a while he'd say with an Oxford accent, "God's bloody bladders! What an odor!" (Laughs)

Hughes: But the rats stayed.

Lawrence: Well, the rats stayed. I don't know how long we worked in that old lab. I think we were there for a couple of years and then Crocker Laboratory was built. I don't remember the year that Crocker was built. Ernest got the funds from Mr. Crocker in San Francisco who was a member of the Board of Regents. In those days there were no government funds.

Hughes: That was '37.

Lawrence: The first experiments I did were in '35 with the old cyclotron which was in the old building (the Old Radiation Laboratory).

Hughes: Yes. It would have been the 27 inch when you first came and then that was expanded to a 37 inch in 1936.

Lawrence: So we worked over there (in the Old Radiation Laboratory) and then pretty soon we moved over to Crocker. We had a laboratory of our own there and a technician and then a nice, small animal room.

Hughes: Who else was in Crocker in those very early days when it first opened?

Lawrence: Joe Hamilton was in Crocker too although he was in the Medical School some of the time. He was very active with sodium and radioactive iodine, those two particularly.

Hughes: You mentioned Herbert Evans. Did Dr. Evans have any other connection with the Radiation Lab?

Lawrence: No. Ernest and I considered him a really great man. I know he was a great asset to the University of California and one of the great leaders in endocrinology and physiology. We had great respect for him.

I had my brother painted by the artist Guido Greganti who was sent over here by the Pope (Cardinal Pacelli) because I went over to take care of Cardinal Stepinac who was sick, and I had an audience with the Pope. After I got back he sent over the Vatican's artist plus a Monsignor and they spent some time here to paint me. Well I said, "I don't want to be painted. But there are some other people that I'd like to have painted, like my wife and my brother and so forth."

Hughes: I was wondering when I was going over some of the early experimental work on the effect of neutrons why you often removed the tumor to expose it to the radiation?

Lawrence: One of the difficulties of neutrons is that you can't aim neutrons. That's the trouble with the neutron therapy which I'm going to see again in London when I go in September. Neutrons have no charge. You can't turn them around the corner. Pi mesons which they're using in Los Alamos or heavy particles we use here, we can turn them around the corner. But with neutrons, it's just a beam of radiation coming in all directions. You collimate them with shields. Neutrons can't be aimed. We couldn't take a small mouse with a little tumor, say as big as the end of your finger, and aim a neutron beam at it. So what we would do is remove the tumors and then place them in little cellophane packets and irradiate them. Then take pieces of them and inject them into another receptor mouse and then watch them grow.

I ran onto some charts the other day from the attic in which I'd made drawings of those early experiments. Every week I'd make a drawing of the size of the tumor or note whether the tumor disappeared. That's the way we determined the effects of neutrons on tumors. That technique is still used in the ascites tumors and I've used it too in the pi meson work that I did six, eight years ago. You irradiate the whole animal who has an ascites tumor. Then you just take and pull out some of that ascites fluid and inject that into another mouse. With pi mesons and with heavy particles like carbon and neon and protons, you can aim the radiation right at the tumor in the animal. But you couldn't do that with neutrons.

Hughes: Can you tell me who first had the idea of applying radioisotopes in medicine?

Lawrence: Oh, I suppose Hevesy in bacteria and in animals. This was obvious. I don't think that's very important. Harvey Cushing right away saw the tremendous potential of radioactive isotopes when I was working with him. Anybody could see that. You got two things: you got a tracer and you've got a source of radiation. You can follow things in the body, study metabolism in disease and metabolism in normal animals and normal people. You treat cancer by getting the isotope into the cancer. Or treat a thyroid tumor by getting it in there. It's very obvious. Hevesy happens to have been the first one that used an isotope, radium D. He was a great man, and visited us several times and worked here one summer.

Hughes: Do you remember anything about the scheduling of the 37 inch cyclotron for the production of radioisotopes? I'm trying to get at exactly how time was partitioned off for use of any of the cyclotrons.

Lawrence: I suppose Ernest had a committee. There were so many physicists and they were making discoveries all the time. They wanted time to find new isotopes.

Hughes: Do you remember having to fight for time?

Lawrence: No, we never fought. I think that Stone was sort of unhappy that, being in charge of an X-ray tube, he couldn't use the (cyclotron) anytime he wanted. I think there's pressure right now. That's always true if you've got an exciting program in physics, chemistry, medicine and biology. There's a competition between biology and the clinical use of isotopes.

Hughes: When the 60 inch cyclotron began to operate in 1939 did that mean that all production of radioisotopes for clinical purposes switched to the 60 inch?

Lawrence: Yes.

Hughes: So the 37 inch became solely for pure physics and the 60 inch became pure medicine and biology.

Lawrence: Not exactly. We transferred over to the 60 inch. It was for physics, chemistry, biology and medicine.

Hughes: Can you tell me something about how radioisotopes were distributed, particularly off campus? Would you give them to anybody that asked?

Lawrence: Yes, anybody. As a matter of fact, we tried to distribute them to people even if they didn't want them. We thought they should use them. For example, we had nobody in the Laboratory that knew anything about iron metabolism and we had isotopes of iron being made. So we shipped them to the greatest iron center in the country and that was Rochester, New York, where George Whipple was working. For many years we didn't use iron here at all until a young fellow by the name of Rex Huff and I began using it in patients. That was pioneering work in medicine. Then we sent to the Mayo Clinic radioactive phosphorous for the treatment of leukemia and polycythemia. We sent radioactive phosphorous to Dr. Cooper in Australia. We sent that all over when they'd request it. We sent iron and phosphorous and sodium to a lot of people on the campus, like Dr. D. M. Greenberg in biochemistry and Dr. Chaikoff. So anybody on the campus that wanted isotopes could get them.

Hughes: How much cyclotron time was devoted to the production of radioisotopes?

Lawrence: Not very much time. They could make so much so fast. They could make millicuries of sodium. Unbelievable the amounts that they could make. Martin Kamen was very helpful in making these isotopes, to me and to everybody around. He was in charge of arranging bombardments. He's retired as a professor of biochemistry at San Diego. He was wonderful. He did more things for more people. Some chemist had to arrange to do these bombardments for us. We'd help, but it had to be a chemist.

Hughes: He was primarily in charge of the bombardments?

Lawrence: Yes. He made the first carbon-11. Did I mention Sam...

Hughes: Ruben?

Lawrence: Martin made for Sam the first carbon-11 and Sam was the first one to show photosynthesis with radioactive isotopes, using carbon-11 before we had carbon-14.

Hughes: Before Calvin, then.

Lawrence: Oh, yes, before Calvin. He antedated Calvin. I think Calvin wasn't here then. I told you about Sam and the phosgene gas?

Hughes: Yes.

Lawrence: I was with him when he died. He would have been a famous man by now because he was a famous young assistant professor of chemistry—a very striking personality and a wonderful guy.

Hughes: That was tragic. What happened after the war when Oak Ridge took over so much of the production of radioisotopes? Did the 60 inch continue to make radioisotopes for worldwide distribution?

Lawrence: Well, less and less because Oak Ridge made most of them. We moved the 60 inch to Davis and the 37 inch to UCLA. Now there are lots of cyclotrons that Bill Brobeck has built all over the world. Now they're busier than ever making isotopes because the isotopes now that are exciting are the ones that Tom Budinger's using, the so-called positron emitters. The isotopes now that are exciting are fluorine-18, carbon-11, iron-52.

Tom is using a lot of other positron emitters where the radiation comes out directly opposite. Two gamma rays come out with a positron so that you can localize exactly where the isotope is in the body with an Anger camera. So they're using that now in localizing the beam. The beam strikes the target and then the two opposing gammas come out. With the double camera you can see exactly where the beam is so you can place it wherever you want. Well, you couldn't do that before. They're using it now in medicine. The other thing is that the positron emitters are very short lived.

I have a lantern slide that shows the dose of radiation that people were allowed to receive fifty years ago and forty years ago and thirty years ago and twenty years ago, and it's just gone down and down and down. Now I think it's gotten to be sort of a psychosis. I came in on a plane last night and sat next to a fellow from Los Angeles and he was asking me about atomic reactors. I said, it's the cleanest form of energy we've got right now. You're allowed to receive about twice the background radiation which is about a hundred milli-R a year, but if you go to Denver you get a hundred and twenty and if you fly to New York a few times

you get about a hundred and ten. They've gone too far in stopping reactor building in California. France is going ahead fast. West Germany's fast. Russia's fast. There's no evidence that these low levels do any harm. But I think X-ray departments are a little careless. I think that lots of times they give you maybe more radiation than necessary for a (gastrointestinal) series. I'm against that. But a hundred milli-R.... It's too bad that the people have been aroused against radiation to the extent that it's damaging the welfare of medicine and also energy. But we'll start building reactors again.

Hughes: May I read you a quote from your paper in Northwest Medicine and ask you to comment on it?

Lawrence: Yes.

Hughes: You said, referring to the early work with radioisotopes, that: "...one was usually pointed out as a 'half-quack' or long-haired dreamer, either doing these things that were unethical or dangerous to the patient or spending time carrying out studies and developing treatments that had no real application in medical research and practice."

Lawrence: Well, there was something to that. Hospitals and medical schools didn't have the isotopes, they didn't have the facilities. Radiation wasn't respectable when I was a medical student and it isn't now among a lot of doctors. X-ray therapy of cancer and diagnosis is all right, but we were treating people and....

That statement's a little bit strong but I was referring there to the work that I did on the treatment of polycythemia vera which is a standard treatment now. But for many years people said that it was dangerous to give a radioactive isotope in a dose that would produce biological effect. But as a result of my work, and I wrote a book on it at one time, thousands of people have been treated with radioactive phosphorous, for polycythemia and leukemia. For polycythemia it's the best treatment, but some of the textbooks for many years criticized it. Even my professors that I later saw criticized it. And I said, can you show me any results like the results that we get? I said, we follow these people for five, ten, fifteen, twenty, twenty-five, thirty years and there are no results like it in the literature. But they were against radiation.

I spent three weekends with Harrison Martland in New Jersey, who

discovered the people who had bone tumors and necrosis of the bones from (pointing their brushes while) painting dial watches with radium. I gave the Harrison Martland lecture many years ago—and I went down there and I became thoroughly knowledgeable on alpha particles. And I knew there was a difference before the experimental work was done. Alpha particles from plutonium and uranium, I wasn't dealing with those. Before I ever gave an isotope to a human being I became very familiar with all the work on radium poisoning. Then I started using radioisotopes in patients and the results were remarkable. That statement is a little over done but it was a new field and it still is new .

I gave a talk in Vancouver last fall on the treatment of acromegaly and Cushing's disease and Nelson's syndrome to a group of standard internists, professors of medicine up and down the coast. (Dr. Tobias, my long time physicist colleague, did the first work in animals with heavy particles, which led us to start using them in therapy.) They finally recognized us about twenty-five years ago but in the early days you were just out of the line. Even now a lot of people don't know much about isotopes and radiation.

I have two boys that are surgeons and they don't know much about (isotopes and radiation). One of them worked in the lab in the evenings with me one year when he was an eighth grader and did some experiments with ascites tumors and he worked on somatostatin with Dr. Joe Garcia one summer, but he's not research minded. He's a surgeon and the other one the same way. Radioisotopes and radiation were so new that you had to get a new generation of doctors. And now you've got them. They're all over the world now. But in those days we had a corner on the market. We'd furnish the isotopes to people who wanted them. We'd never turn anybody down. We wanted to get them out, both for therapy and for tracer research.

Hughes: Later on when your appointment here became permanent after 1937, were you also spending time at the Medical School? I'm talking now about the pre-war era.

Lawrence: Yes. The dean over there was a good friend of mine, Langley Porter, and he was naturally interested in this work, and he was a great supporter of mine. But I couldn't work in the Medical School and work over here, and so that.... It'd be only natural that the Medical School would be interested in this work. But the only one at the Medical School who was really actively interested in doing work was Joe

Hamilton. But a lot of people on this campus were interested.

I used to go over to the Medical School and work in Dr. Robert S. Stone's department a couple of days a week when we began treating patients with neutrons. But I soon got out of that because it didn't look hopeful to me. And I got more interested in radioactivity.

Hughes: Was Stone at all interested in radioisotopes as a treatment for cancer?

Lawrence: No, he wasn't interested in that. He was primarily what we'd call a teletherapist, and an excellent one. He treated my mother with the Sloan tube and cured her.

Hughes: Hamilton in those early days was going back and forth between Berkeley and the Medical School too. He was doing that independently of you?

Lawrence: Well, everybody does things more or less independently.

Hughes: Well, sometimes you collaborate on research.

Lawrence: I was working with P-32, and was interested in therapy. He was more or less interested in tracers, but he got into therapy and we didn't have anybody here that was experienced enough to handle thyroid disease. So he got a man by the name of Mayo Soley from the Medical School to help on that. That was a time consuming thing. So that was going on here all the time.

Hughes: I thought maybe because it was such a new field that perhaps you were communicating or...

Lawrence: Oh, we were communicating, yes, sure.

Hughes: But Hamilton had his own specific research interests and you had yours.

Lawrence: I was working in the Crocker Laboratory until I got the money for Donner. Then I moved over to Donner and turned over my office over there to him, and the animal colony space, everything. So he was in a different building. We'd see each other at meetings, and when we developed the Division of Biophysics and Medical Physics, why we'd see each other in connection with that.

Hughes: Can you tell me anything about the decision to treat a patient on Christmas Eve in 1937 with P-32 for the first time?

Lawrence: Well, it was a very obvious thing to do. Kenneth Scott was in on it, and Lowell Erf was in on it, as I remember it.

Hughes: Was Larry Tuttle in on it?

Lawrence: Larry Tuttle was in on it. We did a lot of work on mice, with and without leukemia. Hardin Jones was in on it. And I. W. Chaikoff, I think, on one paper. We did a lot of work on the distribution of P-32 in the normal mouse and the leukemic mouse. Then I had a colony of leukemic mice. We found out that the P-32 localized where you wanted it: bone marrow, liver and spleen. So it is a natural to use this to treat patients with chronic leukemia. It obviously was successful in the sense that there's nothing better right today. They usually treat them with chemicals now, but they get the same results that we got. For several years we wrote many papers on this, and the results were pretty good in some people. Some people lived five, ten, fifteen years. But most of them wouldn't. And it was tough work.

Hughes: What were Erf and Tuttle doing specifically in those experiments?

Lawrence: I can't remember. except we were all working together.

Hughes: Tuttle as I remember was not a physician.

Lawrence: No, he was a Ph.D., I think. And he later went to the A.E.C. We were a team.

Hughes: I was just wondering what everybody's role was. Scott I believe was in charge of determining things like dose.

Lawrence: Well, yes. I can't remember that specifically, but Scott was technically very good.

Hughes: Do you remember how Tuttle came into the picture?

Lawrence: Can't remember that, no. I think he came to me like Hardin Jones and Tobias; they just came to me and said they want to work here. A lot of people that came in those

early days just came and said, "I'd like to work here." We had a reputation of being anxious to get good people and giving them a lot of freedom.

A lot of the best people we've got, like Saul Winchell, came that way. One of the best men we've ever had, Tom Budinger, more or less came that way. I saw him the other night. But Saul Winchell was one of the best we've ever had. He just came over to see me when he was an intern over in San Francisco. He said, "I'd like to come over here and work." Rex Huff was a pioneer that called me on the phone and wanted to come out here and work after he got an internship. He became very well-known for his work with radioactive iron and many other isotopes. He and I worked together with iron.

Hughes: Before you even came to Berkeley on a permanent basis, did your brother try to find anybody that was interested in doing biological experiments with the effects of neutrons?

Lawrence: Yes, he did. Actually, the first summer that I came here was about the first time they became available. So in a sense I preempted anybody else. I didn't do it on purpose. I was here and I didn't come out for that purpose. I was on vacation. But I think also just before I came Ernest tried to get some people interested on the campus here. People weren't interested in radiation and a lot of people aren't interested in radiation now. Radiation was frowned upon by doctors in those days and now too. They say it's not the answer to cancer. They weren't worried so much about the safety of people either.

Hughes: But in those days they didn't <u>know</u> that it wasn't the answer to cancer—that is, some forms.

Lawrence: No, but it was just a new form of radiation and they were doing their own research and there was no money; I had to get the money to carry out the work in the first few years. A lot of people didn't really see the importance of radiation.

We have a fellow in this Laboratory right now who's famous, Hal Anger, who developed scanners. He with Val Mayneord in England and Benedict Cassen at U.C.L.A. were the pioneers in cameras and in scanning. People say, "Well, over in Donner Laboratory they develop instruments." Well, a lot of biologists and scientists don't care about instruments. That shouldn't be important in the

University. But things have changed. Well, his instruments are used all over the world. He got all kinds of prizes for them. I wasn't interested in instruments either but I supported him and helped him a lot and he's still working here. A lot of people didn't appreciate radiation or instruments of any kind. Everybody knows they're very important now.

Hughes: I understand that in 1936 you advised your brother not to make the proposed medical biological lab, which became the Crocker Laboratory, a national radiation cancer laboratory. Do you remember your reasons for giving him that advice?

Lawrence: That is not true. I said that I thought that the Laboratory should be very broad in biology and medicine, including cancer. I said that radiation is not going to be the answer to cancer and if you're going to build the Laboratory just around radiation treatment of cancer, I think that' d be too narrow. It should be for the application of physics, chemistry and the natural sciences to problems in biology and medicine.

Hughes: Was your brother more enthusiastic about the radiation treatment of cancer than you were?

Lawrence: He had the radiations. He wanted to see them tried and so did I. I saw that if we said that we were going to cure cancer and built the Laboratory around that, and not make it a general medical research laboratory including cancer, I thought that would be a mistake.

Hughes: Who first realized the potential of using high energy beams for medical uses?

Lawrence: Well, in the case of neutrons, Paul Aebersold and I did a series of experiments. Later Dr. Hardin Jones was in on it too and Dr. Cornelius A. Tobias soon was in on it. We showed that in these mice that I had that had various kinds of tumors that per unit dose neutron ionization was more damaging to the cancer cells than the normal cells. We didn't then understand that—we understand it now—because we were dealing with a dense form of ionization compared to X-ray ionization. Dense ionization is biologically destructive independent of the amount of oxygen that's present in the tissues. Well it just happened that in those days in our experimental set-up our tumor cells were relatively low in oxygen content, so we were fooled in a sense. We said, "We have a radiation here that picks out cancer and kills it and spares the normal cells." Well, that was because we were exposing cancer cells in a

situation where they didn't have as much oxygen as the normal cells did. Later, due to the work of Hal Gray in England—who used to come here—and others, the so-called oxygen effect was discovered. That's the exciting thing about what we've been doing the last twenty-five, thirty years with very heavy particles like carbon and neon. Dense ionization will kill tissues whether there's oxygen there or not, and that's the reason that patients with cancer today are being treated up on the Hill with a beam of carbon, which produces very dense ionization. The key man in the work with charged particles was and is Cornelius Tobias, a long time close colleague.

The mice experiments are what started us in the field of cancer therapy and the reason we got Dr. Stone to come over from the Medical School. I think Ernest used that argument in getting the building for the 60 inch cyclotron given by Mr. William H. Crocker. We already were using the 39 inch which had nothing to do with cancer. But the 60 inch I think was funded partly to use in the treatment of cancer. Then we got Dr. Stone, a very experienced radiotherapist, and he treated, I think, a couple of hundred patients with cancer. Then the war came along and that had to be stopped. Now it's being done at eight or ten or twelve centers in the world. It's too early to say how it's going to turn out but that's another long story.

Hughes: The first cancer patient was treated on the 37 inch cyclotron in September of '37. I was wondering how you came to the decision to treat a human.

Lawrence: That was due to the fact that Paul and I found that when we irradiated these packages of tumors, and then also irradiated animals we found that the tumors were killed with a lower dose than the animals were. I can remember Ernest saying, "Oh, it looks like neutron ionization has a special effect on cancer." That's when we started talking to Bob Stone who was the radiotherapist who later treated my mother for cancer. We thought that there was a favorable differential sensitivity of cancer cells. We tried various kinds of cancer, like breast cancer, lymph node cancer, and then the lymphoma type of cancer, all in mice. I had a wonderful set of tumors that I had at Yale that I was working with. Then we got Stone and that's the way neutron therapy started.

Hughes: Before you knew about this oxygen effect and when it was determined that there were bad effects from the neutron therapy, do you think that held off the therapeutic use of radiation for a number of years? In those postwar years, for example? Robert Stone wrote a paper in 1948 in which he said that all neutron therapy should be

stopped, that both the immediate and the late effects were too severe to warrant the small benefit.

Lawrence: Yes, he overdosed. I remember that I became somewhat disinterested in that work. I worked on it for the first year, then we got a young doctor from the Brigham Hospital, Dr. John C. Larkin, to come out. I saw the tremendous potential of radioactivity and began working with radioactive phosphorous, radioactive iron and other isotopes. But I could see that nothing really great was happening with the neutron therapy and the reason that some of the patients were overdosed.... In defense of the work, all those patients were going to be terminal. So I don't think it was an example of patients being hurt. There were some patients that did pretty well. I think there are a few patients living yet. Dr. Stone was an outstanding radiotherapist.

Hughes: But do you think that slowed down the whole field of radiation therapy? The Donner work on heavy particle therapy really doesn't come into the picture until the '50s. So there's at least a ten year gap.

Lawrence: Let's see now how I can discuss that. When the 184 inch cyclotron was operating, Robert Wilson wrote a paper on the Bragg peak. I think he got his Ph.D. here and he was a postdoctoral fellow. He wrote a paper which he published in Radiology, a journal, and he saw the potential of the Bragg peak in cancer therapy. If you have X-rays striking the skin, as you go down to the depths of the tissues, the dose will fall off so that the skin dose is much greater than the dose deep in the tissues. Of course that's the reason that you rotate the patients and use multiple fields. The Bragg peak from heavy particles like protons or alpha particles or neon or carbon, produces a much larger dose in the depths than on the skin surface. He was just thinking about the geometry and the cancer differential effect which we didn't understand.

I think the important thing is the dense ionization because cancers generally are anoxic or hypoxic. If you get the geometry, that's helpful but that can be handled by rotation and by multiple fields. The only hope, to my mind, for neutrons and for heavy particles is dense ionization, not the geometry. Neutrons have very poor geometry; they're like X-rays. But heavy particles, charged particles, have good geometry. But it's that dense ionization so that if you hit a tumor which is lacking in oxygen, it'll still kill it. I think that during the next ten years Dr. Castro

and his colleagues on the Hill are going to be able to cure some cancers that now are 100% fatal.

To get back to your question about slowing down. I think the neutron experience was so discouraging that I think that did have an effect. Alpha particles were all they had then, and alpha particles produced no increase in relative biological effect nor was there the so-called oxygen effect, as one gets with heavier particles like carbon, argon and neon.

Well, we were trying to decide whether to use the Bragg peak from the 184 inch for cancer therapy after Bob Wilson had written this article. So I got Merrill Sosman who was professor of radiology at Harvard, one of my teachers. Then we got Dr. Stafford Warren, who was a radiologist and was the new dean of the medical school at UCLA. My brother and I both knew him in the East; he came from Rochester, New York. And Dr. Stone, Dr. Tobias, my brother, myself, and several others. That's when we had this meeting: whether we should start using protons and alpha particles in the general treatment of cancer but we began using these particles in treating small lesions like the pituitary and small brain tumors. Also, they are being used now on the Hill, training our new team up there. And it's being used at Harvard with their cyclotron. But it's not going to do very much because with rotation you can do about the same thing with X-rays and gamma rays. That is, you can rotate your patient or you can rotate the tube. So you can spare the skin and get an adequate dose in, in the general treatment of cancer. However, early on we used the sharpness of the alpha particle beam and the Bragg Peak to treat small volumes such as the pituitary, or small brain tumors and this continues now.

During the war, the 184 inch cyclotron was used to make uranium 235. They built Oak Ridge on the basis of that first calutron unit up there on the Hill. They built multiple cyclotrons; they called them calutron.

I am excited now about the Bragg peak of really heavy particles. When the bevalac came along fifteen years ago, Dr. Tobias and I tried to get a machine that would produce heavy particles and we finally got it by tying up the Hilac and the bevatron suggested by Ghiorso on the Hill. Then we got excited again. I don't think there's been any lost time except in the case of neutrons. But that was picked up over 15 years ago. This was started the first time about 1938. It was picked up again probably in the early fifties in England.

Hughes: Yes, there was all that work at the Hammersmith Hospital.

Lawrence: Yes, I go there every year. They've been treating patients with neutrons there for fifteen years.

Hughes: Did Robert Stone do any neutron therapy after the war?

Lawrence: No.

Hughes: Going back to the early days again, could you say something about your official relationship to the Medical School and the Radiation Lab when you first arrived from Yale and were going to stay at Berkeley on a permanent basis?

Lawrence: I'd go over there to the Medical School about three half-days a week and work with patients with Dr. Stone, or work some in the medical clinic with students.

Hughes: Were people at the Medical Center other than Robert Stone supportive or interested in the work that you were doing?

Lawrence: Yes, they were interested. Of course, the Medical School in those days was not like the Medical School now. At that time it was not one of the great medical schools in the country. The work there was primarily done by people who were clinically oriented. The first two years of medicine were over here in Berkeley, and those are the people who were interested in isotopes as tracers.

Earl Miller came over here and took charge of our radiation safety program in the Laboratory during the war. He was a young assistant professor and now he's retired as a professor of radiology.

Hughes: How close did the standards used during the war follow the standards that you had set up in the early days for workers around the cyclotron?

Lawrence: I can't answer that because I've gotten out of that field now, but I think roughly the same.

Hughes: What about the response of funding agencies to the new field of nuclear medicine?

Did you find it was fairly easy to raise money for your projects in the early days?

Lawrence: Well, it wasn't easy. It took a lot of work. I worked like the devil on getting this first Donner Laboratory building by getting Mr. William Donner out here. I saw him in the East several times. This building here (the Donner Laboratory addition) I got through Mr. Donner's son Robert. The Donner Pavilion where patients are studied and treated and taken care of while they're being treated, was from private funds from patients that we'd treated or from friends and foundations who made donations and from the A.E.C.

Then we'd get private grants. I'm talking about nuclear medicine now. We'd get private grants from the Markle Foundation, which was a pretty big foundation, from the Macy Foundation, and the Rockefeller Foundation. There were no government funds then. So it took a lot of work. The same way with Ernest. He spent a lot of time raising money. For instance on the board of the Rockefeller Foundation was George Whipple, who later was dean of the medical school at Rochester. We sent him radioactive iron. He was very much interested in anemias and he got the Nobel Prize for discovering liver as a treatment for pernicious anemia. He appreciated the fact that cyclotrons here were being used to send him iron. Naturally he'd be sympathetic when Ernest went after the foundation to get a one-and-a-quarter million dollars to build that 184 inch cyclotron. One and a quarter million dollars, remember that. I can remember some people on the campus saying. "What a waste of money to build a machine for a million and a quarter dollars." Warren Weaver was the executive head of the Rockefeller Foundation and he's the one that put that over. That was an awful lot of money in those days. But it was very hard to get money in those days. You had to sell it yourself.

RELATIONSHIP WITH ERNEST LAWRENCE

Hughes: In the few letters that I've seen between your brother and yourself, there seems to have been a very intimate relationship. You seemed to be very close. Can you tell me anything about his influence on your career, if any? I know, for example, that he gave you a fair amount of advice when you were in medical school. Can you say something about your brother and his relationship with you?

Lawrence: We were far enough apart so that we didn't see each other very much. In other

words, when I was in eighth grade he was a senior in high school and when I was a senior in high school he was a senior in college. But we were pretty close brothers. I did have the ability of spotting people, like Harvey Cushing. Harvey Cushing I knew was a great man. And I knew my brother was a great man. He was a great guy personally. But I also knew that he really was somebody. I don't think he knew it so much himself. He was very easy to get along with, very intense. He was wonderful at seminars and very generous. He was very excited about research and could stimulate people really to do things. He appreciated Harvey Cushing. Cushing helped him write his first commencement address. I think it was about the only one he ever gave. I got him in with Dr. Cushing.

Well, to answer the question about his influence on me: sure, he had a lot of influence on me. When I was in the second year of college, I wasn't doing very well. I was on the basketball team and not getting very good grades. I was just having a good time and not studying very much. That was the first time I was aware that I'd better get going. By the time I graduated I was the first Phi Beta Kappa from that University. (South Dakota) Then I kept it right up so that when I was a third year medical student I was number one in my class at Harvard. And then Cushing took me out of school in the middle of my last year and said, "You don't have to go to school any more." I said, "Well, I've got to get my M.D." He said, "I'll take care of that." So I started interning and working with him.

But I do think that my brother being so successful and so enthusiastic did have a tremendous influence on me. He had a real influence on me and great empathy. Well, he was that way with a lot of people; a lot of these young fellows that came out of the Radiation Laboratory got that kind of influence from him. He'd just get you fired up; He was a great person to get you fired up.

That's what I've tried to do with my boys. We have three boys and a daughter. One of them is a very good heart surgeon. The other one is an assistant resident of surgery and the third one is going to UC Berkeley this year. Our daughter teaches, is married to an engineer and has three children.

But Ernest did have that influence on me, there's no question about it. We were very good friends although we didn't see each other in the Laboratories because he was so busy and I was so busy and the program that I developed was so big—not the size of his program but it was still big. We'd get al] kinds of help from

him too and from his staff. We really were the biological and medical section of the Radiation Laboratory. Soon they appointed me Director of Donner because we were brothers and in the University they thought we ought to be separated by separate appointments. I didn't ask for it.

I never intended to get together with Ernest; we thought that there were too many difficulties in doing that. But we saw each other a lot and then that accidental experiment that I did while on vacation out here got me more enthusiastic about it. We talked about it and we decided both of us were big enough to handle it (being brothers in the same institution). We never really had any major problems because he was famous at a very young age and I was a leader at my level. So he and I thought we could handle things.

To go back to the influence of my brother: I have a fantastic group of letters from him from before I came to California. We used to write each other about once every week or ten days. In his letters he'd cover his social life, his scientific life, his teaching, his graduate students and everything. A lot of his work that he later wrote papers on is in these letters that he wrote to me. He'd write me sometimes a two or three page letter, written on both sides longhand, from the Faculty Club.

Hughes: Would you consider giving copies of those letters to the University?

Lawrence: The University will eventually get copies of them. I've got double copies of them.

Hughes: Yes. Those would be most interesting.

Lawrence: He was so excited about his work that he'd take the time to write me about what he was doing scientifically and dinners that he'd go to or girls that he'd take out. We were very close brothers. We were both pretty independent but we never had any jealousies.

Hughes: Would you mind telling me a little bit about the decision to have your mother treated on the Sloan million volt X-ray apparatus? That was in 1937.

Lawrence: I ran onto some correspondence with the doctor there in Aberdeen, South Dakota. I guess that's in some correspondence that I promised eventually the University could take over. I've got that at home I think, and I've got double copies of it. She went to

the Mayo Clinic with my father. This was in '37, I think.

Hughes: I know she was treated on the Sloan X-ray tube in '37.

Lawrence: They found that she had an inoperable type of cancer of the uterus that couldn't be treated. My brother and I were on the phone with the doctors out there. We decided that I ought to go out there, so I went out to the Mayo Clinic. I was relatively unknown and right in front of my father one of the specialists said that she had about three months to live. I said, "Well, could I see the records?" I remember this very well because I was a fairly aggressive young guy then but not very well-known. He said, "No, they don't show the records to family doctors or relatives." I said, "I'm going to talk to Dr. Balfour." Dr. Balfour was the director of the Mayo Foundation and a famous surgeon. He'd been here two or three times. So I got him on the phone and he came right down and opened up all the records for me. He embarrassed these specialists. They had a big consultation.

So then I got on the phone with Ernest. I said, "They don't want to treat her here with radiation. How about my bringing her out and we'll talk to Dr. Stone?" We did talk to Dr. Stone and he said, "Sure, I'll take her." So I took her on the train, wheeled her across the station in Omaha. I had to change trains from Rochester. She was bleeding. She was pretty sick and I couldn't get a bedroom for her so I just got a lower berth and got her out here. She was about 67 or 68 years old then. Then I took her to San Francisco to be seen by a famous gynecologist, Dr. Ludwig Emge. he was professor of gynecology at Stanford. Then Dr. Stone saw her of course, and they started treating her through four fields (JHL demonstrates the four fields in the pelvic region.) To make a long story short, this massive tumor just started evaporating. At the end of ten years my mother finally agreed that she must be cured. It took me about ten years to convince her and she died at 83 and had the best years of her life. That's written up some place. It was really, really a fantastic result.

I used to drive her back and forth every day after the first week or two and I'd have to stop by the curb and let her lean out and have emesis, radiation sickness. I'd stand by and encourage Dr. Stone to give as big a dose as he could. I had to go through this business of seeing radiation sickness which you saw a lot of those days. But she was cured, no question about it.

Hughes: Did you ever consider cyclotron treatment?

Lawrence: No, we did not start that until 1938. Paul and I were right in the midst of those experiments with neutrons. But the standard radiation therapist even today with a tumor like that probably would lean towards X-ray. On the Hill we don't treat any cases like that now either.

NEUTRON THERAPY

Hughes: Could you describe exactly how neutron therapy was conducted in those early days—what the room looked like, who was responsible for doing what?

Lawrence: Well, that was entirely Dr. Stone.

Hughes: Would he accompany the patients from San Francisco?

Lawrence: Yes, he'd bring the patients over. Some of the treatments would be done when he wasn't here. But he would pick the patients. He was a standard radiotherapist and he'd pick patients that were hopeless. Some had received every form of surgery and every form of radiation. I mean, every form of surgery plus radiation. Some of them had not been treated at all, but he knew they were a hundred percent fatal like large head and neck tumors. So he'd pick them out and he'd bring them over and set up the schedule for dosage based on what we knew about dosage.

Hughes: I read that the 37 inch was only available one day a week for cancer therapy.

Lawrence: Yes, I think that's right but sometimes two days a week. Stone griped about that.

That's the big problem. No one understands just how to handle any of these radiations, X-rays or anything else—whether you should protract for a month or two weeks or six weeks. It's a great problem right now in using carbon or neon. I haven't talked to Joe Castro recently but I will before I go to Europe and then I'll find out down in Los Alamos how they're using pi meson now. I think it's anybody's guess over a quite big ball park on just how to use them.

The man who started protraction was a man that I knew, a famous French

radiologist by the name of Coutard. There was a man who had cancer in Colorado Springs by the name of Boise Penrose. Boise Penrose imported Coutard to treat him. He came over to Colorado Springs; he built a big cancer hospital for him and it's still running. Coutard was an older man then and he came to the United States as a private doctor who treated Boise Penrose. Coutard started protraction and that revolutionized cancer therapy because a lot of people were cured.

Hughes: When was that?

Lawrence: I suppose that was about maybe 1932. I heard him speak at Yale when I was on the faculty there in about '35. He was famous then, because everybody was going over to protraction, spreading radiation out. That's instead of giving it in one big dose or three big doses.

Hughes: That must have been somewhat of a worry with the 37 inch when the only way you could do it was in one fairly big dose if you only had access to the cyclotron once a week.

Lawrence: Sure it was. It must have been a great worry to Stone and he overdosed some patients, not knowing that he was.

Hughes: There were also problems with the operation of the 60 inch when it first started.

Lawrence: It wasn't reliable.

Hughes: What happened to patient therapy when the cyclotron would break down?

Lawrence: Well, you'd do like they do now if they break down, they transfer them over to X-ray. On the Hill now, some of the patients receive the first two-thirds of their treatment with gamma rays or X-rays and then the last third with heavy particles. This is planned this way. They're being very conservative now. This thing that I've had so much experience with, these new people have to learn from the ground up with their own hands. So they're going awfully slow as far as I'm concerned. I'm waiting for the day when they really get treating each patient with neon or carbon. They're not even doing that yet. It's too slow.

Hughes: Can you tell me something about the attitude of people at the Medical School to the

neutron therapy that was going on here?

Lawrence: No, I can't tell you about that because Stone was head of the X-ray department. I can't imagine them not being very hopeful.

EARLY THERAPEUTIC USE OF ARTIFICIAL RADIOISOTOPES

Hughes: In 1936 Hamilton and Stone administered radiosodium to three patients and they noted that there was no change in the blood picture after administration of radiosodium. Soon after they studied sodium absorption in normal patients. I have several questions. First of all, do you know anything about how the decision was made to use radiosodium on patients.

Lawrence: I think that Dr. Stone was going to give whole body radiation to these patients.

Hughes: I know two of them had leukemia.

Lawrence: I remember very well that the dose they gave was so small that it couldn't possibly help the leukemia. But it was justified in the sense that Stone was an able radiotherapist. He had done no experimental work in a laboratory with the radioactive isotopes but it was quite justified. But as I pointed out later, the isotope that should have been used—which I started using as soon as I could get ready for it—was P-32 which is still being used all over the world. It didn't make scientific sense because in the treatment of leukemia for whole body radiation you have to give something like ten to twenty reds or more per day up to, say, a hundred or two hundred reds. Sometimes you give much more.

The obvious isotope to use was the one that I had been doing a lot of work with on animals. I think on Christmas Eve of '37 I gave radioactive phosphorus to the first patient at the hospital in San Francisco. That made more scientific. sense because on the basis of our animal experiments that localizes in the bone marrow and in the leukemic cells and in the leukemic tissues.

Hughes: And the radiosodium does not localize?

Lawrence: The radiosodium is a way of giving whole body radiation; it doesn't localize at all.

Your body has got a lot of salt in it. So if you give salt it's distributed uniformly over your whole body; it just diffuses all over. So it's just like spraying the body with X-rays. The dose of radiosodium was so low that it wouldn't do anything and it didn't do anything.

Hughes: Did they realize in advance that the dose was too low to do anything?

Lawrence: They must have. I never did understand why they did it because the dose was so small. I told Joe, I said it won't do any good, it won't do any harm.

Hughes: I was reading a letter that you had written to Herbert Childs who was preparing the book about your brother. You were talking about this particular episode. You called Hamilton and Stone's administration of sodium to these patients a stunt.

Lawrence: Well, it wasn't a stunt. It was not a therapeutic trial and I can't imagine that Dr. Stone didn't know that. Sodium-24 is just like salt if you taste it. If you give a big enough dose you can get effects. You could kill an animal if you gave an enormous dose.

Hughes: Wasn't that a demonstration that used to be rather commonly made in the early days? I think I've heard stories about your brother having radiosodium cocktails.

Lawrence: No, he never used that. He did say that he could make enormous quantities of radiosodium as a substitute for radium, as an external source of radiation. He used the word cocktail as we did in the treatment of patients with thyroid tumors, Grave's disease and also thyroid cancer. You give them a drink of a good size dose and so we'd say, "Just give them a cocktail." I used to do that when I gave lectures in the early days. I remember one time in Columbus at Ohio State there was a doctor in the audience, a young Ph.D., who got into this field because I gave him a small cocktail of radioactive iodine and held a counter over his thyroid. He became very famous as a pioneer in this field, Dr. William G. Myers.

Hughes: Can you tell me more about the decision to administer radiophosphorus?

Lawrence: The decision was based on the fact that leukemia was being treated either by local X-ray to various parts of the body or by full body irradiation where you put the patient under an X-ray tube and spray the whole body. That was developed by Dr.

Heublein—he was a doctor at Memorial Hospital. I saw the set-up where they would have the patient lying out and spray the body with ten roentgens a day—something like that—until they got up to 200 or more roentgens. With all the work we'd done with normal animals, we found that the uptake of radioactive phosphorus was concentrated in the bone marrow and, in the case of leukemic animals, in bone too somewhat. In leukemic animals it would go to the bone marrow in greater concentration because that's where the leukemia cells were. Then it would go into the spleen and liver if they were infiltrated. So you had a partially selective way of radiation. It's a pretty simple story and it's based on what I've already said. You get some selective localization but it's not enough to limit the radiation just to the leukemic cells. You save a lot of the body from radiation. It prolongs life and it's used many places in the treatment of leukemia but no one's got the answer to leukemia. They've been getting fairly good results now for the last ten years with methyl trexate in acute lymphoblastic leukemia in children. Some of them live quite a long time. Leukemia is still a mystery and that's about the story.

I was just talking to Dr. Howard C. Mel about a friend of his who has polycythemia whom he wants me to see. I don't see patients very often now. P-32 is a great success with polycythemia. We first started treating that with P-32 in 1938 and now this is used throughout the world wherever you go and it's in the textbooks. It's a disease of the bone marrow and you can inhibit the production of red cells with small doses of P-32 and sometimes it'll stay inhibited for the rest of the patient's life. I remember a man that died —just recently. I think I gave him one dose about thirty years ago and he died at the age of 87. He had classic polycythemia. He was sent here from the Mayo Clinic by a friend of mine there, Dr. Byron Hall.

Hughes: I've read that for many years clinicians remained skeptical about the use of P-32 for the treatment of leukemia.

Lawrence: Yes, it's something like the skepticism that we face now about atomic reactors. It wasn't as bad I guess in those early days. But nobody had a way of treating polycythemia or leukemia that was successful. Then a young doctor who's worked with me for many years, Dr. Saul Winchell, and I wrote a paper on all of our experience on polycythemia and showed that there's no question about it. It extends life to nearly normal—it's a little better than insulin in diabetes —but it doesn't solve the problem completely. The skepticism I think is partly due to the fact that you're

using radiation.

A study group that's headed by one of my former students, Dr. Wasserman, who's now about ready to retire, has just completed its study of ten years of literally hundreds of cases. P-32 is the treatment of choice. Everyone on this international committee agrees with that. I used to go to their meetings.

Hughes: Is that the Polycythemia Vera Study Group?

Lawrence: Yes.

Hughes: When was that group founded?

Lawrence: Oh, about ten years ago.

Hughes: You were warning other M.D.s in 1945 about the therapeutic use of radioisotopes. Apparently you believed that the immediate effects of their usage could be dangerous and nobody knew much in those days about the long-term effects of the use of radioisotopes in therapy. I was wondering what the basis for this warning was. Did you have experimental evidence that pointed out the possible dangers of using radioisotopes therapeutically?

Lawrence: I remember when I went to the First International Radiological Meeting in Chicago at the Palmer House in 1937 I met many men from all over the world. I gave one of my first papers there on neutron radiation. Many of them had lost fingers; some had lost arms. It was very common to see these pioneer men, who at that time were 70, 75 years old, from Sweden and from England and from the United States. They were very careless in using X-ray. So I was always aware of risks versus benefits. I was warning people just like I did about working around the cyclotron after I wrote that first paper with Paul Aebersold. Radiation has hazards we don't know. We mustn't treat it carelessly.

Before I ever gave a radioactive isotope to anybody I took three trips to Harrison Martland's laboratory in New Jersey. He was the one that discovered the radium dial painters. I was convinced that we were dealing with something that was entirely different.

Hughes: Did he have any quantitative data about the possible harmful effects at that stage or was it still pretty theoretical?

Lawrence: I think the quantitative data is that you can have one microcurie of radium in your body without harm. There's been a lot more done on that by Gioacchino Failla, Maranelli and others at the University of Chicago.

At Davis they've got a dog colony now where they've been giving alpha particle emitters, plutonium and radium, and also strontium which is a beta emitter. And they're finding that the dogs are living longer and having fewer cancers and fewer leukemias than the controls. As a matter of fact, I think he told me when I talked to him recently, that up to a hundred times the so-called allowable dose the dogs are healthier and live longer and have less leukemia and less cancer. But you never read about that in the papers.

CROCKER LABORATORY, JOSEPH HAMILTON AND THE 60-INCH CYCLOTRON

Hughes: Could we turn to Crocker Lab? I know it was before you were officially here, but did you have any part in the planning for Crocker Lab which would have occurred in 1936? It opened in '37.

Lawrence: I was just enthusiastic about it. It had very little space in it. It was built to house a cyclotron that could produce adequate quantities of radioactive materials. But the cyclotron was used for physics all the time too so you had trouble getting time. That's true of all of these accelerators. Now everybody's after time on the bevalac.

Hughes: Do you know anything about how the scheduling was set up for the use of the 60 inch cyclotron? How was time apportioned amongst the various groups that needed to use it?

Lawrence: When I was in Los Alamos three weeks ago I gave a lecture on some of the early work and I pointed out that the average physicist is an average human being and so he is awfully interested in anything that might help in medicine. So there was no problem in getting time and I don't think there is now really if they think that the work you're doing is important. I suppose there was a committee. I don't know how

they schedule up on the Hill. I suppose they have a committee now on the accelerators. The 88 inch isn't being used very much now, I understand, but the rest of them are—the 184 inch and the bevalac.

Hughes: Do you think there was an official schedule in those early days?

Lawrence: Yes, there were schedules on the bulletin board. It was a small laboratory in those days and just give and take.

Hughes: Did you have space in the Crocker Lab?

Lawrence: Oh, yes. I was for many years in Crocker. After I got the money for Donner I moved over here and then Joe Hamilton took over my office in Crocker.

Hughes: Would you tell me something about Joseph Hamilton—what he was like as a person and a scientist?

Lawrence: He majored in chemistry at Berkeley. When I first came out here he was an interne in the Medical School. He was a little younger than I. He began using radiosodium about the same time that I was doing work on neutrons and P-32. He learned a lot of chemistry himself; he didn't get a Ph.D. in chemistry. He was a very intense worker and very enthusiastic. During the war he worked on plutonium. He became an unusually expert person to advise people on the dangers of plutonium and radium in the bone. Patricia Durbin-Heavey is still carrying that on. He was a very good looking, intense, hard working fellow who wasn't interested in therapy. He was interested in tracer research. Finally he got involved with the toxic aspects of the heavy particles. He, Dr. Tobias, Dr. Jones and I started the Division of Medical Physics. This man who just came to see me is a professor of medical physics. His name is Howard Mel. I got him to join the Division. In those days I was after people who were not trained in medicine but in other fields. Wendell Latimer was a very good friend of mine and I'd see him at the Faculty Club. One day he called me about this fellow Mel. He said, "I've got a very bright Ph.D. and he wants to work with you in the field of medicine and biology." So he sent him over and we got him. He's been a full professor here several years. That fitted in with the philosophy that I had and Ernest had in getting people like Tobias and Mel and Jones. You get plenty of M.D.s but what we wanted was the interdisciplinary approach.

Hughes: You wished to place more emphasis on basic research as opposed to clinical research?

Lawrence: Oh, yes. That's always been my philosophy and it still is. It was always Ernest's too. Louis Rosen, who's an old-time friend of mine who built the pi meson factory in Los Alamos, and I talked about the importance of interdisciplinary research. He's put the last fifteen years of his life into cancer therapy. He's a physicist. He worked on the atomic bomb during World War II down in Los Alamos. He's been there for many years. He's just about ready to retire now.

Hughes: Getting back to the early days at Crocker Lab, when you moved into Donner Lab did Dr. Hamilton take over the medical health and safety supervision of the staff?

Lawrence: No, we didn't have an M.D. All we had was a monitoring service. We had a group of physicists monitoring. Everybody wore badges. The general influence around the Laboratory was that you'd better not exceed the allowable dose of radiation. That's always been more or less handled by the physicists. That's true of Los Alamos too.

Now the doctor down there that corresponds to our doctor here is an old friend of mine. His name is George Voeltz. He's in charge of industrial medicine. He does physicals on everybody. But the real experts are the physicists who are monitoring people. They know how much radiation you're allowed to give. If you get more than that then they check you over pretty carefully and they're ready to take you out of the laboratory. But it's happened so little.

Hughes: So they would function only if there were an accident?

Lawrence: Right. We had two or three accidents here. I think I told you about the accident they had down at Los Alamos. Louis Slotin?

Hughes: No.

Lawrence: Well, Robert Oppenheimer came down to see me when I had my first office in Donner in the old building. Before they built Los Alamos he wanted me to go down there and head up the medical program. After an hour's talk I decided that I couldn't leave here because this is a sort of a fountainhead of nuclear medicine.

Oppenheimer had been selected as the director of Los Alamos. Los Alamos was the site of a boys' school and a classmate of mine's father ran the school. His name was Pond and they call it Ashley Pond. Well, this boy's name is Ashley Pond. It's still there. I stayed in an inn down there looking right out on Ashley Pond.

Anyway, Oppenheimer wanted me to go there and I said, "Well, there are two young fellows here that are working with me that have only been here about six months. They don't know much about radiation but they're the best you can get in the country. Why don't you talk to them?" One of them was Louis Hempelmann who's now retiring as a professor at Rochester. He interned at the Brigham Hospital. He went to Los Alamos and headed up their program. He had a couple of very serious accidents down there. It wasn't his fault; it was human error. One of them was Louis Slotin who put in a rod and made a reactor go critical by mistake. He got maybe 1500 rads of radiation.

George Voeltz had an accident up at Idaho. He was up at Arco and a reactor blew up there and killed two or three people. But the record otherwise in laboratories like this and in the commercial use of atomic energy is better than any other industry, no deaths.

Hughes: After Donner Lab opened in 1942 did you maintain your ties with Crocker Lab? Were you in and out of Crocker Lab or were your activities now in Donner Lab?

Lawrence: Well, we had periodic meetings. We saw each other around. Everybody had different interests. I think there are more meetings now than there used to be. But the philosophy that we had in those days was to give everybody a lot of freedom and a lot of support and have seminars and hear about what they were doing.

Hughes: Did the 60 inch cyclotron continue to make radioisotopes for general distribution during the war?

Lawrence: No, after the reactors were developed, they took over the production of radioisotopes.

Hughes: Completely?

Lawrence: Nearly completely. During the last ten years cyclotrons have been growing like

weeds all over the world because of the short-lived isotopes—positron emitters—like iron-52, fluorine-18, oxygen-15 that can be made in cyclotrons.

Hughes: In Herbert Childs' book about your brother, he mentions that your brother before the war discussed enlarging Crocker Lab to include some hospital space.

Lawrence: Well, I think we'd been talking about a hospital for many years. In later years we were talking about building a hospital up on the Hill. But I got the money to build this ten-bed hospital that we have now at Cowell.

Hughes: Do you remember specifically why that idea to add a small hospital to Crocker Lab was dropped?

Lawrence: I think it was probably financial.

I was telling somebody recently how I got the hospital here, the ten-bed hospital. I got the money from donations from patients that we had treated. I had about $250,000 of donations. At one time when I was at Yale I was thinking about going down to the Rockefeller Institute. What appealed to me about that was that you could do research there and have patients too in their ten-bed hospital.

THE FOUNDATION OF DONNER LABORATORY

Hughes: Who arranged the original funding for Donner Laboratory?

Lawrence: We've never had any original University funding. I got the money for the first laboratory from William H. Donner, who was in the audience when I gave the first paper on the biological effects of neutrons. The second laboratory I got from his daughter-in-law and his son. The Donner Pavilion, where the patients are, I got from donations from patients. This scanning Pavilion I got partly from Donner Foundation and partly from the Atomic Energy Commission and partly from private donations. But Donner Laboratory itself has never had any budget. I worked for years to get a University budget for it, but it's never had a budget. So we always had to get the money by going outside. It has approximately two million dollars endowment that I raised from private sources. But it's never been funded. Well, that's true of a lot of institutes and laboratories in the University. Very few of them

were funded. You had to go out and get the money.

Hughes: When Donner Lab opened its doors can you think back to how space was allotted and how equipment and staff were assembled?

Lawrence: I can't. Those things are just worked out. We had a space committee, I remember that. We had an instrumentations committee, I remember that. I just turned things like that over to this committee and it settled a lot of these questions. And if I didn't agree, which was very rare, why then I could say, "Well, look, I don't agree with this." I'd rotate the best people in the Laboratory onto the committee and I knew that what they did would be right.

Hughes: Was the research group system just a natural development or was there an effort to duplicate what was going on in the Radiation Laboratory?

Lawrence: No, there wasn't copying. This physicist that I saw down in Los Alamos—Dr. Raju—I picked him up in the halls of the Tata Hospital in Bombay 18 years ago when my youngest son Steve was six weeks old. He came to me with his thesis which was on the measurement of the electron. He got his Ph.D. in one of the universities in India. He looked after me when I was in Los Alamos two weeks ago and he's one of their leading scientists. I could see he was good and now he's an outstanding biologist. We picked up people like that.

Hughes: When you pick up a new person like that do they come to the Laboratory with their own project or do they join a research project that is already on the way?

Lawrence: Oh, that's variable.

Hughes: You're picking the man rather than his research?

Lawrence: Picking the man, oh yes. That's terribly important, I think. In some laboratories they'll have one director and he directs everything and when he's retired then they don't have anything left. But my philosophy and my brother's philosophy were to get outstanding people and so we've got a laboratory still going strong because we picked good people. Some of those people are retiring now too but I'm hoping that they're still doing that. Yes, it's the people. That's what counts.

Hughes: Can you tell me a little bit more about the philosophy behind the multi-disciplinary approach.

Lawrence: Well, the philosophy there is this: you go to medical school and then have contact with physics and the natural sciences like I had. I noticed that in medicine there was no close association with chemistry, physics, mathematics. As a matter of fact the advisors in college would keep you away from math, physics, and chemistry and get you to take comparative anatomy and biology. So early my brother and I felt very strongly that the great future of biology and medicine aside from radioactivity is tied up with basic science. But medical schools just don't have it yet. They have great heart surgeons and great clinicians and I think in our Medical School, the University of California, San Francisco, which is one of the best in the country, they have lots of basic scientists too.

They said, "Well, isn't it too bad you didn't have a hospital here?" I said, "No, I think it's too bad that the hospital doesn't have us over there." But you can't do that.

That means that you'd have to have Donner and the Lab on the Hill and this campus over there. And that's the reason that I told Chancellor Albert H. Bowker—and he agrees—that we ought to have a medical school right over here.

Hughes: A medical school but not a hospital?

Lawrence: No, you don't need a hospital. There are plenty of beds around here.

Hughes: How do you think this multidisciplinary business has worked?

Lawrence: Well, I think it has in this laboratory. If you talk to a fellow like Tom Budinger, if you want to see the best example of it, you ought to see his team. They're physicists, chemists, mathematicians and Budinger himself is a little bit of each. He's an M.D. but he also knows quite a lot of math, lots of physics, and some chemistry. Oh, I think it's worked. He has an endowed Professorship with money which I raised privately.

Hughes: After you moved into Donner Lab how much did you see your brother? Did that cut down on the amount of contact you were having with him?

Lawrence: I suppose a little bit because then there was a cyclotron in Crocker. Yes, I think it cut down some. But I used to see him quite a lot but he was so busy and I was so busy and we were sort of in different.... He was very intensely involved in biology and medicine. I had so many associations with my own group plus whenever I needed help from the physicists I had a lot of other physicists that were with him that I could see. Mostly because we were both so busy.

Hughes: When you were both in the same building though, in the Old Radiation Lab, did you see him more often?

Lawrence: In the Old Radiation Laboratory, he was never in that building. He kept his office over in the Physics Department.

Hughes: So even then you didn't see him all that often.

Lawrence: Oh, no. I was talking to a fellow in Las Vegas yesterday who is doing work like we're doing here on the pituitary gland. I attended a meeting on the pituitary gland because we have treated, oh for twenty-five years now, patients with pituitary tumors. We presented that material yesterday. This fellow in Boston is a neurosurgeon, but he's picked up our technique of treating the pituitary gland with heavy particles, first from the 184 inch cyclotron and now from the bevalac. At first he came out here and worked with us and we had our own physicist on that team. When he got back to Cambridge and the Massachusetts General Hospital, then he worked with a man who was chairman of the department of physics, Bill Preston. But that only lasted a short time until he got his team worked up and then it became sort of routine.

There was no reason that I should bother Ernest with a lot of the things that we were doing. Although we'd get together socially and dinners and parties and so forth and sometimes on vacations with his wife and my wife and so forth. He kept up on what I was doing and I kept up on what he was doing. As a matter of fact I've mentioned before that for a long time I knew exactly what he was doing from letters that he wrote to me before I came here. I still was pretty well aware of what was going on on the Hill, too. Actually, they hadn't moved to the Hill yet, you see.

Hughes: That was after the war, wasn't it?

Lawrence: Yes, that was after the war.

Hughes: But all through his life here he maintained an interest in biology and medicine through the biological application of radiations and radioisotopes?

Lawrence: Oh, sure, just like all the physicists.

Ernest at one time was going to become a doctor when he was in college the first couple of years but he got involved with Dean Akeley who was a great physicist, a great teacher, and he found out that physics really was what he was fitted for. Oh, sure, he always had a tremendous pride in anything that came out of physics or anything that came out of his group that would help in biology and medicine.

Hughes: When you did talk with him about what you were doing in the Lab did he ever say, well now why don't you try thus and so, or make suggestions of some kind?

Lawrence: I can't remember. I mean any scientist who is open is always talking to people and exchanging ideas. I can't remember anything specific.

Hughes: So he wasn't popping into Donner Lab?

Lawrence: He didn't have time. He was so deeply involved then with the building of the first atom bomb. By the time we got over here (Donner Lab) he was just terribly busy. He, like Edward Teller, Arthur Compton and Vannevar Bush, was just swamped during the war and right away didn't have time for anything else.

THE AEROMEDICAL UNIT AT DONNER LABORATORY

Hughes: The war work is what I think I'd like to talk about now. I was wondering first of all how the Donner Lab was chosen as the site of the Aeromedical Unit?

Lawrence: Well, there was a man that I knew that was head of the research program of the Air Force. He was a doctor whose name is W. Randy Lovelace. He knew that we had begun working with inert gases like argon, krypton, neon, nitrogen.

Hughes: Xenon?

Lawrence: I don't have a periodic table here, but anyway all these inert gases are in that one group in the periodic table and he knew we were working with those. At that time we didn't have pressure cabin aircraft so he wanted us to see what we could learn about the problem of high altitude bends. Without pressure cabin aircraft if you fly above twenty thousand feet, I think eighteen thousand is the maximum that you could fly, it would mean breathing a hundred percent oxygen at twenty—I've forgotten those figures now.

Hughes: Oh, we can check that.

Lawrence: Anyway I did a lot of flying at high altitudes with oxygen myself. Anyway we had radioactive gases. We were the first to use them and we were studying the circulation. Dr. Hamilton, the best chemist in our group, was always looking at the periodic table and new radioisotopes and pointed out the rare noble gases; Dr. Jones, Dr. Tobias and then many others joined in. As a matter of fact Dr. Tobias and a couple of other younger fellows and I did some experiments with ordinary xenon and found out that it was an anaesthetic.

I just wrote a letter to a Russian physicist that I hope to visit sometime this year in Dubna, that's one of the centers of physics in Russia. We wrote a paper on this, with two or three other fellows including Tobias and Jones, showing that you could use xenon as an anesthetic. At the same time, practically in the same issue of Nature, a Russian came out with the same results.

Anyway the bases that we were using were radioactive so you could follow them in the body. The problem was how to prevent the bends. If a flyer is flying at, say, an altitude above eighteen thousand feet he's liable to get what we call the chokes or the bends which are very painful; I've had them. It's due to bubbles of inert gases, nitrogen. See, the average person like you or me—I guess I have more than you do because I'm bigger than you are—at sea level has about 1,000 c.c. of nitrogen in his body. It's relatively inert; not exactly inert. It's chemically not completely inert. If you go to altitude or if you go down in the depths to say, thirty feet below the sea level that's two atmospheres. Every thirty feet you gain an atmosphere and that's the reason these scuba divers—two of my boys do that—if

they go down and stay there too long and come up too fast they'll get the bends, because the body saturates itself proportional to the atmospheric pressure. The situation in the case of people living at sea level and going to altitude is the same thing. If you go from, say, sea level to eighteen thousand feet, that's half an atmosphere, so that nitrogen has to get out of your body and if it goes too fast it'll bubble out and it'll get into your circulation, your heart, your lungs, or into your tendons and ligaments or joints. And it can kill you. So we developed a method. We finally got a plane assigned to us by Dr. Lovelace and we did a lot of high altitude flying. We classified people with the aid of radioactive gases as to those who could go to high altitude without the bends and those who couldn't go without the bends. That's about the story.

We did a lot of other things and I don't think it's important enough to spend much time on them. We were very busy for a period of at least three years on that and did many things with the products of atomic energy that were useful to the Air Force.

Hughes: How had you gotten interested in radioactive gases to begin with?

Lawrence: Well, I think that Joe Hamilton and Hardin Jones and I all talked about these gases. There were some very interesting isotopes that were positron emitters and they were very easy to measure and they were short-lived and they were very safe to use. We were looking at the whole periodic table. But Jones particularly showed—and I think it's piece of pioneering work—that the aging process and the rate of circulation are parallel. In other words a little baby when he sleeps at night can be without covers and he's warm. As you get older and you start aging that's a function of the circulation. You can measure the circulation very nicely with these radioactive inert gases. Although later a simpler method came out, which again Dr. Jones developed, measuring the recovery of the production of $C0_2$ after a standard step up exercise. We were looking at the whole periodic table and inert gases were obviously interesting to work with largely because they were short-lived and they were positron emitters. But that's such a long story. We did a lot of work in other areas of high altitude flight.

As I told you before Robert Oppenheimer wanted me to go to Los Alamos to head up the medical program there and I was not interested in the first place of spending my life in simple radiation protection. That's the reason I sent one of my

younger associates down there. I think I mentioned that to you before. That was Louis Hempelmann. He spent his life on radiation protection problems. Being a different type of person, interested in the positive applications of the products of atomic energy, I needed to stay here as a general medical influence in this whole program in connection with the very important war work of Ernest and the other people.

The second thing, I wanted to get involved—all of us did in some way—in war work that was not physics but that we were competent in. It came from the pressure from Lovelace who knew what we were doing in this Laboratory. I trained one of his people here for the war to go back to his clinic, the Lovelace Clinic in Albuquerque, New Mexico. So it was largely my contact with him that resulted in this. We did some very exciting things in several areas, instrumentation and selection. By the end of the war, the B-29s were available. They were pressurized. They were used in dropping the bombs on Nagasaki and Hiroshima.

In the case of the B-29s it's interesting that a lot of those planes would come back in the fog, out of gas, and a lot of them crashed until Luis Alvarez shortly before the end of the war thought of something: he's famous for thinking of things, Luis Alvarez is. I see him quite often now. He was one of the fellows that I used to talk with too. He gave out some ideas. He had a little more time than Ernest did. He thought of this thing called ground control approach where you talk an airplane in. Well, it isn't used very much by these pilots, like the 747 pilots that I flew in coming back from Paris or even on the Concorde which I flew over on, because they don't trust anybody but themselves. A lot of bomber pilots were saved coming back to, I guess it was, Iwo Jima where they landed after the bombings. This is not the atom bomb; this is just ordinary bombs when they were bombing Tokyo. They were talked down and saved. He got the Collier trophy for that and it was just an idea. He talked the first plane down at Andrews Air Force base in Washington just by having the man on the ground look at the radar screen and talk to the pilots say, "Come on a little lower down. Come this way."

Hughes: Did it become a fairly common practice for the rest of the war?

Lawrence: The equipment is all over the world. It's not used very much except in emergencies. Usually they have the ILA and other means of taking the planes in now.

Hughes: I meant specifically for the remainder of the war.

Lawrence: Oh, I don't know. This is the only example I know of.

Hughes: Do you remember anything else that you and Luis Alvarez talked about specifically?

Lawrence: I can't remember anything specifically, no.

Hughes: You mentioned Tobias and Hardin Jones. Was there anybody else working with you during that war period?

Lawrence: Oh, a lot of people. We have a lot of publications that were published. Some of them were secret during the war and some of them were then published later as declassified and open. They're in the reprint file.

Hughes: Did you hire people specifically to work on the war projects?

Lawrence: Oh, yes. We had a contract with the Air Force and the National Defense Research Council.

Hughes: Did they hire people with your approval?

Lawrence: No, we'd hire them. It's just like you do now. If you have a research grant or if you have a budget, if you need someone, why, you get them.

Hughes: There was a problem, though, during the war of just sheer manpower, was there not? So many physicists, for example, were already absorbed into projects in other parts of the country.

Lawrence: We had one physicist who was very important, William Siri. This was important work too and of course the physicists were doing all right in the various laboratories. Tobias was a physicist and we had others too.

Hughes: Did you have to fight to keep people, do you remember?

Lawrence: No.

Hughes: I've read about a low pressure chamber.

Lawrence: I think that was the first one in University Lab. That's in this building yet.

I just met a fellow in the hall, one of the brilliant young fellows that I really worked with in the early days. He's still young yet. That's Donald Van Dyke. I just talked to him as I came in. He left here because he got a bigger salary. It was a great loss to the Laboratory; he did a lot of brilliant things. He did an awful' lot of work in the low pressure chamber. I did a lot of work in it too during the war and afterwards because you could simulate high altitude flights. He did a lot of the work on experimental animals.

Then after the war we made three expeditions to the Andes and did a lot of studies on the nature of acclimatization in high altitude. These were medical and physiological and biological studies. Again we'd use isotopes. I remember on one trip I took to Peru we were the first to use tritium, tritiated water, and I had to sleep on it because it was classified material. In order to take it out of the country I had to guarantee that I'd have it on my person or sleep on it. So I had a small bottle of tritium in my bedroom wherever I was when I was on this one trip to Peru because it became a very simple way of measuring body water. Will Siri was the key man on that. We'd just take a drink of tritiated water and then fifteen minutes later take a blood sample, count the sample and you'd get the blood volume. It's the dilution.

Hughes: Did the war work, then determine to a certain extent the sort of research that you were doing after the war?

Lawrence: No. It was just a continuation of the applications of isotopes, although we had to become more practical when several things we did didn't involve isotopes. We were all interested in doing things that would help in the prosecution of the war. If we could use isotopes, why fine. That has been the story of this Laboratory ever since; we don't tie ourselves to radiation or isotopes.

Hughes: Did people have to have a security clearance to work at Donner Lab during the war?

Lawrence: Yes, they had to have a clearance. That was no problem. I don't remember anybody that we wanted that wasn't cleared.

Hughes: What about Tobias as an alien?

Lawrence: No different than Teller. Teller was an alien too. Teller is a Hungarian and Tobias is a Hungarian and Fermi was an Italian. We benefitted greatly by these aliens, who are extremely important American scientists and citizens.

Hughes: Yes, I would say so.

Lawrence: That clearance business was never any problem. I don't know of anybody that wasn't cleared.

Hughes: How long was clearance required?

Lawrence: Oh, I don't know. You'd have to get that from elsewhere.

Hughes: How much of the research that you were doing during the war was eventually published?

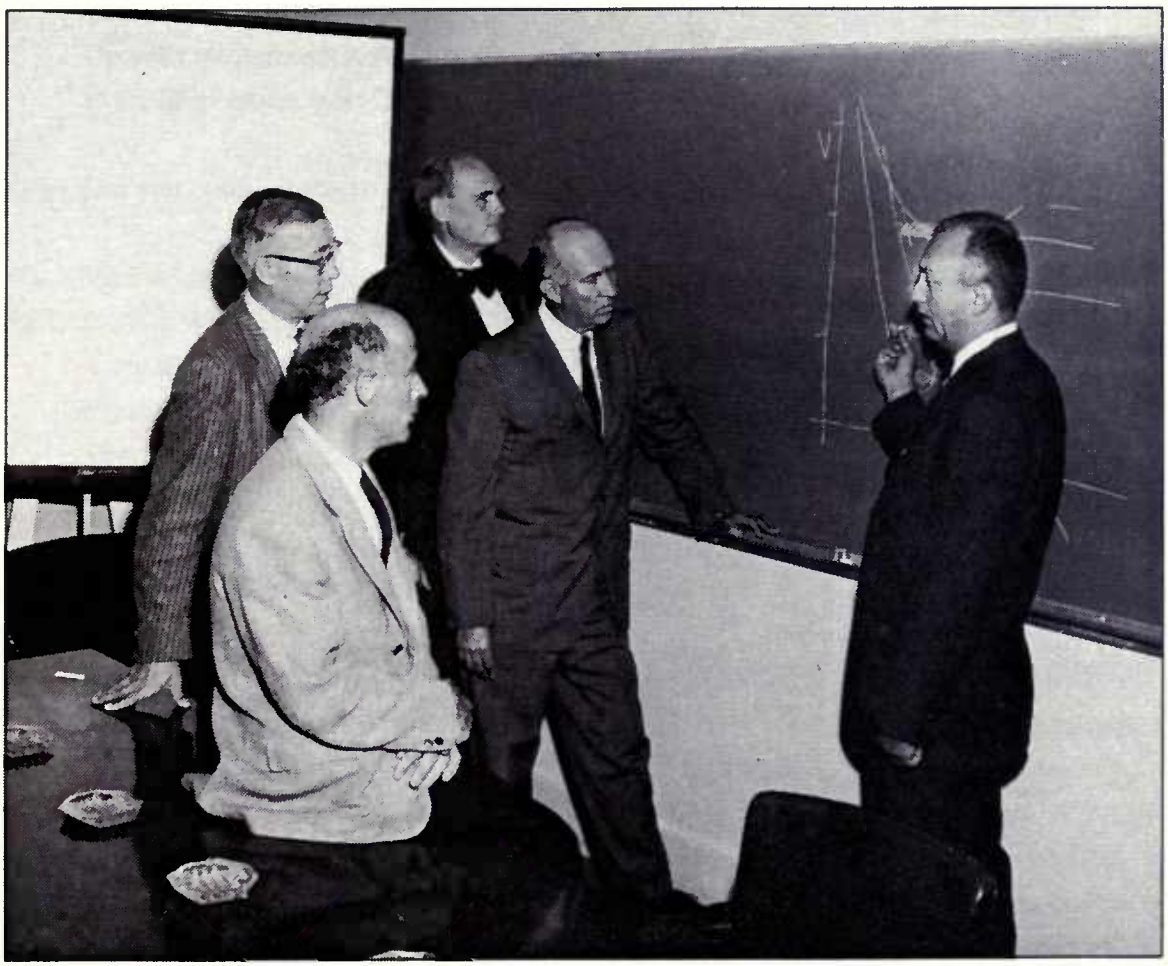
Lawrence: I think most of it.

Hughes: You couldn't publish during the war, could you? Did you have to wait?

Lawrence: No, we waited until after the war. We just didn't have time to publish. Like the xenon work, I think that came out in about '44. That was ordinary inert xenon, not radioactive xenon which was an anesthetic.

As a matter of fact there's a principle called the fat water solubility ratio. Any anesthetic has a high solubility in fat compared to water. You take ether and all of these anesthetics that they're using now. Xenon has a very high solubility in fat over water and radon does too. But of course you couldn't use radon because of the radiation. Then another one is oxygen. If you have enough oxygen in your body, that's an anesthetic too. For instance, if you go down very deep you can get oxygen narcosis because of the tremendous amount of oxygen in your body. There's more oxygen than there is nitrogen.

Hughes: I've read that investigators with OSRD (Office of Scientific Research and



Donner Laboratory senior scientific staff, 1962. From the left: Dr. James Born, Assistant Director; Dr. John Gofman, Professor of Medical Physics; Dr. Hardin B. Jones, Assistant Director and Professor of Medical Physics; Dr. John H. Lawrence, Director; and Dr. C.A. Tobias, Professor of Medical Physics.

Photo by LRL Graphic Arts.

Development) contracts sometimes had trouble getting the supplies they needed because the requirements for the physics work were given higher priority. Did you ever run into problems of that kind?

Lawrence: We never had that problem. The Laboratory, Donner and on the Hill, had a special way of getting away from red tape. Red tape now is pretty bad in the University. Up until quite recently we did get things through very fast. Most of that is just delay at the level of the University offices.

Hughes: But you could circumvent the University?

Lawrence: We had our own system of order. We didn't have to go through the University. It's too bad what's happened to the University now because it's become so bureaucratic. I see it as a Regent. It's slowed things down. It's mostly paper work and delay because you have to go from this to that and committee to committee and all that. There was a war on.

POSTWAR DONNER LABORATORY

Hughes: I have heard that before the war cyclotron time was pretty much determined by the project's intrinsic value. After the war it became much more how much money that particular group had put into the support of the cyclotron.

Lawrence: Oh sure, well you have to do that. But the amount of money that was put in had to be on important work. In other words, if I had an important project and you had an important project, somebody had to pay for the cyclotron. You can't weigh the importance of these various projects. It's not money. It's money in the sense you have to have money to run it. I mean, you've got to pay the bills, like you have to pay your bills at home. We're not living in an ideal world where you don't have to worry about money like you do in Russia where you have a Politburo that decides.

Hughes: Right after the war it seems to me as an outsider that there was a sudden influx of new people into the Lab. I was wondering what caused that?

Lawrence: What happened there was that fortunately we were really the first place, and I'm talking about biology and medicine, that was in that field of radiation and

radioisotopes. So I would get letters and calls from young men and women, mostly men in those days, that wanted to come and work here. Or if I'd go to a medical meeting, someone would single me out and say, "I'd like to come to your Laboratory." So nearly all of our outstanding people came on their own here. We didn't advertise for them

Hughes: That sudden influx after the war, did that have something to do with the fact that atomic energy was now in the forefront of people's minds because of the work on the bomb and that kind of thing?

Lawrence: Well, after the war of course a lot of the people that were at Los Alamos and Oak Ridge and up at Hanford stayed there, and a lot of them went into peacetime applications of atomic energy.

Hughes: Were you involved in any of the postwar planning for biology and medicine on a national level?

Lawrence: I tried to stay away from national committees although I naturally was on a lot of them. Not a lot of them but... I was, for instance associate director of LBL for biology and medicine. That was a national program which included LBL and Donner. Then during the war there was Brookhaven and Argonne and Los Alamos. Then after the war was Chicago. We'd meet every three months and we'd get papers from each laboratory and keep up there. Then I was on several committees, only one at a time, for fifteen years at the National Institutes of Health in the field of medical physics and medical research.

Hughes: What basically were you doing on the NIH committees?

Lawrence: Mostly going over applications for funds, whereas in the AEC committees we were just reviewing our programs. The people who would go over our programs would be the people in Washington. This is an inner circle of national laboratories supported by the AEC. Right after the war the AEC took over our work in biology and medicine.

Hughes: I was wondering what sort of impact becoming an AEC lab made on Donner Lab?

Lawrence: Marvelous. We had plenty of funds and we didn't have to worry getting them. We

had simply outstanding people that headed biology and medicine. That same thing was true of physics and chemistry. We had Shields Warren who was one of my professors in medical school. He headed the biology and medicine. Then finally Charlie Dunham who was a classmate of mine. These fellows were just outstanding people. They'd give you a lot of support, a lot of freedom. We didn't have the bureaucracy in those days that we have now in so many fields.

Hughes: Were you left free to choose the direction of your research?

Lawrence: Oh, we were given terrific freedom. We tried to do things that were important to the AEC in the field of radiation, on radiation protection and radiation dangers. We tried to do things where we could use isotopes but we wouldn't force ourselves to do that if we had some important problem like Gofman's work or... There are many other examples too.

Hughes: You were a member of the Task Force which conducted tests on people that had been exposed at the Bikini atom bomb tests.

Lawrence: Yes, that was '46 or '47.

Hughes: Yes, that was '46 I believe. Is the Atomic Bomb Commission the same thing as the Task Force?

Lawrence: No, the Task Force was the force that exploded the bomb. The Task Force was headed by an admiral in the Navy. Then they invited a group of scientists out there and I was one of them. They put me in charge of a gunboat. I was the commander of the gunboat. We were cruising around the bay when they shot this bomb off from underneath the water.

Hughes: What were you supposed to be doing on the gunboat?

Lawrence: Oh, we were supposed to be taking samples of water. Ken Scott was on that boat too. Ken was one of the key men as far as the technique of taking samples. Then the Atomic Bomb Casualty Commission was formed after that to study the effects of the Hiroshima and Nagasaki bombs on the people. Those bombs were dropped in August of '45, wasn't it?

Hughes: Yes.

Lawrence: That's 35 years ago. So they've got follow-up now for 35 years. They've got pretty good ideas about the dose from the epicenter at both of these cities. Recently they've made a study on these people and so far there's no evidence of any genetic effect in any of the survivors. But there is evidence of an increased incidence of leukemia and cancer. But it has to get up to a pretty good sized dose. I think it has to get up to somewhere between 10 and 20 R. See, this is where the bomb is, they measure the dose. And then, of course, neutrons come in too, so it's a complex thing. But at really low doses they don't have much evidence that it has done any harm.

Hughes: What was your particular role with the Atomic Bomb Casualty Commission?

Lawrence: I didn't have any role in that. I've always been more active in the use of isotopes and radiations in diagnosis and research and therapy. I've been interested in the low-level effects of radiation and did the first work here on that, but I never spent my life on the genetic effects of radiation like some of these people have. Bill Russell has spent the good part of his life on the genetic effects of radiation. I had too many other things I was interested in and I knew that there were a lot of people working on this. I wasn't officially involved in that.

Hughes: Were there people in the Lab connected with that work?

Lawrence: Well, that was handled by the AEC. They had their own group following that. I used to see those fellows and I heard one of the fellows talk last week on it up in Montreal, as a matter of fact. It's now run by a group in the HEW.

Hughes: Still following the same survivors?

Lawrence: Oh yes, still following those people. Oh, they'll be followed probably for the next 50 years.

ADMINISTRATIVE DUTIES

Hughes: I noticed on your curriculum vitae, Dr. Lawrence, that you officially became director of Donner Lab in 1948. I was wondering what that signified because you'd been essentially director of Donner Lab since the Lab was set up.

Lawrence: I was actually head of the Laboratory from the very beginning. I just got a letter that the University would like to have me officially appointed as director of the Donner Laboratory. Dean Alva R. Davis was a very good friend of mine and a very important man at the University. Over at the Faculty Club one day he said, "John, I'm going to recommend to the President that you be appointed director, formally, of Donner Laboratory. You are in fact." 'Sailor' Davis was very famous. That's his nickname. He was one of the most powerful men on campus and also a leader in his field.

R. E. Davis was another man that was a friend of mine. He was a man who was famous for making this cement that they use to build these big dams, like the Boulder Dam. Davis Hall over here is named after him. He died at about the age of 90.

'Sailor' Davis was Professor of Biology. So that's when I heard about it and I got a letter from the President officially appointing me Director.

Hughes: At the same time Hardin Jones became Assistant Director.

Lawrence: No, that was later. I wrote to the President recommending Hardin Jones to be Assistant Director.

Hughes: And what about James Born?

Lawrence: Then later on, to relieve Hardin Jones from administrative duties, I wrote the President recommending the appointment of James Born, and he appointed him.

Hughes: So Born was handling all the administrative matters.

Lawrence: Yes, he was primarily a very good administrator, whereas Hardin Jones was a scientist.

Hughes: Was he handling the scientific issues then?

Lawrence: He was doing a lot of the scientific research himself and advising others with me, whereas Born is primarily not a scientist. He's a terribly able personality in a doctor, but not a scientist. So he handled all the administrative things.

Hughes: Could you say something about your philosophy as Director of the Laboratory?

Lawrence: I've written that up in that article.

Hughes: You don't have anything to add?

Lawrence: No, the only way to have a laboratory like my brother and I had is to give independence. Get good people, then give them independence. This business of directing a laboratory in a certain direction is a very dangerous thing and I think it is happening more now. It certainly happened at NASA because they had a certain job to do and that was entirely directed; they had relatively no freedom. I think that's happened somewhat with the DOE now too in their relationship with the Laboratory on the Hill. (Lawrence Berkeley Laboratory) So the atmosphere has changed somewhat. But in a real research laboratory in a university and even in a company, you've got to give your scientists freedom because you don't know where they're going to run onto something very important even though it may not be practical. It's the idea that useless research very often comes up with something that's useful.

Hughes: How did you keep in touch? Did you pop into the lab or were there seminars where the different groups...?

Lawrence: Oh, we had seminars.

Hughes: Did you make a practice of going around to the different labs?

Lawrence: Oh, sure. I knew how everything was going.

Hughes: As this whole operation was expanding just after the war and through the '50s, did you find that you as a director had to do things differently just because more and more people were involved?

Lawrence: No. I should give you an article that I wrote. I ran onto it yesterday. It's about that subject. A director, in my mind, in my brother's mind, has to get outstanding people and support them, do all you can to support them. Also, at the same time, try to do something yourself too. But you have a responsibility, when you're getting the kind of funding that we were getting, to get top people. There's no problem there if you have an Anger or if you have a Calvin or Budinger or Tobias or Tom Hayes, who's one of the great electron microscopists in the country, and half a dozen others. Joe Garcia, who's been with me for 25 years, developed a new assay for erythropoietin, which can open up a whole new field of medicine. I've always helped him. But I've done enough things for myself so that not only do I get credit for my own things, but I get credit for these guys, too, and I'm not on their papers. People say, "Oh yes, he came from the Donner Laboratory, didn't he?"

So my philosophy and my brother's philosophy too... Look at the people he got. Look at Alvarez, Segrè, Seaborg, and McMillan. I talked to McMillan yesterday, by the way. Ernest used to tell me, "You know, Alvarez is so much brighter than I am." Well, I don't know that that's true or not, but he wasn't joking when he said that.

The other kind of director has got to have his finger on everything, and is worried about somebody doing something that's terribly important. Well, I'm not worried about that at all. That's what I want.

Hughes: Yet you did have assistant directors, Hardin Jones...

Lawrence: And Jim Born.

Hughes: What was the division of labor there?

Lawrence: Well, Hardin Jones was more or less the scientific director, and Jim Born was sort of a business manager. We had a committee that decided on funding. I'd hardly ever attend the meetings—how we'd split the money up that we had.

Hughes: What would Hardin Jones' functions have been?

Lawrence: Well, he was primarily a research man and really did a lot of teaching and a lot of

research rather than administration. But he did quite a lot of advising on it, getting new people and things like that.

Hughes: Would he have had direct contact with the different research groups if they wanted to change the direction of their research? Would they have had to consult with Dr. Jones?

Lawrence: No, they'd do that more or less by themselves. If you have good men, why...

When the AEC got into the picture supporting us, due to my contacts in Washington, we figured that 80 percent of our work should be of interest to the AEC. That is, you use isotopes and radiations and study problems that the AEC is interested in. But you've got to give us freedom for 20 percent of it to be unrelated (to AEC interests), and Gofman's work is a perfect example because for years he never used an isotope. I said, "You can't have outstanding people and restrict their research because you don't know where the path is going to lead. It might go this direction or that direction or this direction. If you tie them down to one technique, to one isotope...

Hughes: That was acceptable to the AEC?

Lawrence: Oh yes, that always was. Now it's not so easy because the boys that are running the Laboratory now tell me that DOE is demanding a lot more than the AEC ever did. There was much more freedom then.

Hughes: Was that 80 percent, 20 percent something that you...

Lawrence: That's what I figured.

Hughes: ...hit upon?

Lawrence: If you look over our reports, about 80 percent of them are obviously of interest to the AEC, like low-level radiation and big doses of radiation or the use of radiation therapy or in diagnosis or research. But you got to give some freedom so a fellow like Gofman can develop. Or a fellow like Hayes.

We were the first laboratory on this campus that had an electron microscope.

Well, I said, "Look, if you're studying function, like what a cell does, you've got to relate your function to the cell. And our methods of viewing cells here are not very good. The electron microscope has improved that so we know more about the morphology of the cell, because we're studying function of cells and organs." So we became a leader on this campus in electron microscopy and still are. Tom Hayes has gotten prizes for it now. I encouraged him, you see.

Hughes: In 1959 you became an associate director of Lawrence Berkeley Lab.

Lawrence: That was due to Ed McMillan. He became director of the Lawrence Berkeley Laboratory after Ernest, my brother, died. Then he appointed me an associate director himself. I think that's who did it. That was a natural thing to do.

Hughes: What are the duties of associate directors?

Lawrence: Well, the duties of the Director of Donner and the Associate Director of LBL in biology and medicine are obviously one in the same. We have always had a lot of our activities up on the Hill. We built an animal house up there many years ago. Now our clinic is in that animal house. Then we later built a very large research laboratory where I used to work, which is over on the other side of the Hill, over near the Botanical Museum.

Our whole biology and medicine program of the Lawrence Berkeley Laboratory used to be 90 percent in Donner. But now it's grown so much that I'd say maybe it's only 60 percent in Donner and then a lot of it's spread on the Hill. We've got our clinic up there now. I saw a patient there this morning. Then we've got Melvin Calvin's laboratory on the campus and he's also got a laboratory that's on the Hill too, a couple of laboratories up there. So he's spread between the Laboratory of Chemical Biodynamics and the laboratory on the Hill. That's all part of one large program. The whole idea of the Donner Laboratory, as I've told you before, is to get interdisciplinary research going, with close contact with the Radiation Laboratory. So that's the whole story with that. That's what we've done and it's still going on.

Hughes: I understand in the early '60s Strauss asked you to become an AEC commissioner.

Lawrence: Yes. Lewis Strauss was a very outstanding man who died just recently. I saw him

for the last time about three years ago. He was chairman of the Atomic Energy Commission. He kept working on me for about six months to become a commissioner of the Atomic Energy Commission. This was over twenty years ago. I got to know him through the Commission and through his visits here. He wanted someone who knew the medical aspects of atomic energy. I just couldn't leave the Lab. Then they got Seaborg later, I think a year or two later. It just didn't appeal to me. That kind of a job doesn't feel right—if I were to be a college president or a dean. The same way with my brother. They wanted him to be president of this university. They worked on him for a long time. He was offered presidencies of other universities and I was offered deanships. That kind of thing doesn't appeal to me. You're at luncheons and dinners and receiving lines and that's no fun. A little bit of that's okay. But if you're director and at the same time have time to do research, well; that's fun.

Lewis Strauss really worked on me. He was just waiting for me to give the okay and then I'd get a letter from President Eisenhower. He said, "All you've got to do is say 'yes' and you'll have a letter from the President appointing you commissioner." I'm sure glad I didn't do it. I mean it would have ruined me. I'd probably have been doing it for three years and I'd have lost out contact with research and teaching here. Then I'd have had to do something else in an administrative way. I've never regretted that.

Oh, what helped me decide, by the way, you'd be interested in that: I talked it over with a lot of people, including the Chairman of the Board of Regents, who at that time was John Francis Neylan, a great Regent and dedicated to the University. He urged me to take it. I said, "Well, why? I've got a great research program going here and that's all I'm interested in really. I don't want to be on a committee." Being on the Regents is bad enough. That's because I got older and I thought that it would be better to get out of the Laboratory and I could do the Regents' work. But he said, "Well, you know John, if you do that you'll become internationally known." I said, "Jack," (I knew him very well), "I don't want to become internationally known on that account. Whatever I get known for I want it to be on the basis of what I do in research and in teaching and in directing this Laboratory." So I turned it down right then. There are different kinds of people. I would have become very well-known in some ways, but I don't care about being known because I'm a member of the Atomic Energy Commission. There are other people that can do it better anyway. I think.

For instance, I was surprised when Glenn Seaborg did it. I think it's too bad that he did it, because he's a good scientist and it took him away from research. When Ken Pitzer became President of Rice, then he became President of Stanford, I told Ken, "That's the greatest mistake you ever made." When he went to Rice I said, "You're probably the leading theoretical physical chemist in the United States, if not in the world. You're a young man yet and that's a mistake." I see him now once in a while, but we never talk about it, but I believe now he probably would agree with me because he just wasn't fitted for that.

Hughes: Do you think it's hard for a man to get back into research once his term of office is up?

Lawrence: Yes, I think it is. There's a very great asset to being a university professor. I think it's very hard to get back after you've been away for more than a year.

Hughes: Well, another issue is that of Donner Lab itself and your efforts to keep it here on campus rather than have it moved up to the Hill. I was wondering what your reasons are for wishing it to remain on campus.

Lawrence: Oh, it's just like Calvin's laboratory. He got funds just like I did-to build a laboratory on the campus. So I feel very strongly, and I felt very strongly, that to have a laboratory on the campus close to the students was better than having it in San Francisco or up on the Hill. So I objected to building it on the Hill, then using money that the Regents came up with to replace the first part of this building, and turn it over to the Division of Medical Physics, which I really started, because I wanted to keep the Division of Medical Physics down here. But I didn't want to lose and separate the laboratory by moving more and more of it to the Hill. That's all. It's just one opinion, and I think I was right. But there are lots of factors.

At one time years ago, we were thinking of doing that very same thing. Then we were going to build a hospital up there, when we talked about the omnitron. Then we were thinking about moving the whole operation up there. But since then I've changed my mind and so I was against it. But I think it will work out all right. We'll still have this main laboratory here left. Part of it will be used for teaching and we have used part of the space for that. I have already seen the great opportunities for an Academic Division—which we started here and I was the first chairman. And it now continues to be a good Division, and benefits greatly from

being in the laboratory and from its associations with LBL.

Hughes: What are the arguments for having it on the Hill?

Lawrence: Oh, just a matter of more space for teaching down here. The Regents first voted to build it here with this money that we got from our Regents' fund which is overhead from our various contracts. We were going to extend it out this way (points towards Founders' Rock), and I had planned that for years. But the architects didn't like the idea of the expense and also getting too close to Founders' Rock. Also they didn't like to put it out this way close to Hearst Mining Building. So they just felt that there was no space close to here that was appropriate. Since we have a lot of our program up on the Hill anyway, why not just move some more of it up there?

Hughes: Do you think because now the academic department and the research department of biophysics on this campus are going to be physically separated that there will be less cooperation?

Lawrence: Well, there's a danger of that. I made a very strong speech on this before the Regents. I could give you a copy of that. Whether this can be remedied or not, I don't know.

The Division of Medical Physics grew out of our research program at Donner Laboratory. The men that are on the staff now, and have been on the staff, have their salaries paid three-fifths by the Lab and two-fifths by the University. We never had a budget. We've got a budget in medical physics now, but it's not big. The Department will lose contact with all our shops on the Hill and our shops here and so forth. We may have to move our library to the Hill too and consolidate it with the big library up there. So I think that's just exactly the wrong thing to do, to separate the teaching from the research. They don't have the facilities. But I'm hoping we can get around that so that a lot of these people can still have joint appointments and won't feel so strongly as they do about it. I considered it largely political because there's always been...

That's true down at Stanford, too, for the machine down there which they call, what do they call that? (PEP) Well, they have a big laboratory down there. The director was trained here, worked with my brother. His name is Pete Panofsky. Well, there's a lot of jealousy between the Stanford faculty and this enormous

laboratory. Just like there is on this campus between the campus here and this enormous Laboratory because we have wonderful research, lots of Nobel Prize winners, we're very famous for many contributions, lots of money. So down there at Stanford, Pete Panofsky was professor of physics and another fellow who's a friend of mine, who's head of Varian now, he was professor of physics. I don't see him very often. He was telling me, "Oh yes, we have the same problem down there. They don't like us down there at Stanford." It's that big linear accelerator that's about a mile or two long. They've done a lot of important work. So they've kicked them off of the faculty. Pete Panofsky's not a professor and this fellow that now is head of Varian, he's a distinguished physicist and he was kicked off too. It's too bad.

In the case of the Donner, I was trying to bring the Laboratory closer to the campus because we are on the campus and we get along pretty well with the campus people. But these things, I guess, are common in all levels of life.

HARDIN JONES

Hughes: Can you tell me about the circumstances of Hardin Jones's arrival in Donner Lab?

Lawrence: Yes, I remember the day when he came over to see me. He was getting some P-32 as a graduate student and he was working on fat metabolism with Prof. Chaikoff. He was one of the many professors for whom we produced isotopes and was a good friend of mine. He became a professor of Medical Physics and Physiology. They were studying fat metabolism in the normal mouse using P-32. Jones wanted to work with me on tumor metabolism, cancer metabolism. He knew I had a large colony of mice and rats with tumors that I brought here from Yale. So after he got his Ph.D. I got him a job here. He was one of our mainstays. He was a brilliant teacher and a brilliant research man, and a man of great talent and capacity for friendship.

Hughes: I know you published a fair amount together. Did you always have a close scientific relationship?

Lawrence: Oh, yes. He, like Edward Teller and of course my brother, and a few other people were my closest friends. Oh, yes, he was a great person, just like Edward Teller is.

Hughes: Can you tell me something about his theory of aging?

Lawrence: No, that's too long a story and also his work on drugs that he wrote a book on with his fine wife Helen. The theory of aging I think basically is that aging is a result of repeated insults to the body, particularly repeated infections over a period of years. Just how he related that to circulation I don't know, but he showed definitely that aging is related to the circulation rate so that you find some people at the age of seventy who have an age of, say, fifty physiologically. He went out and studied groups of people in companies. You find some people who are seventy or eighty still working and he found that they had a young circulation. I'm not competent to go into that. That's another story and it's too bad he's not here to talk to you because he'd be one of the most interesting people to interview. He was a wonderful fellow. If you wanted to you could talk to his wife, I suppose.

Hughes: What about Alex Grendon? I understand that they worked together for awhile.

Lawrence: Yes, Alex Grendon would be a good man to talk to. He's over eighty now and he's just as clear as a bell. I see him every month or so. He lives in Sacramento.

Hughes: Can you remember when he became associated with Donner Lab?

Lawrence: He came here after the war and he started working toward a Ph.D. in connection with an army program. They wanted to train some of their people in atomic energy. He primarily became attached to Hardin Jones. He's a brilliant man and is very active on radiation protection problems.

Hughes: Were they research associates?

Lawrence: Oh, yes. They wrote papers together. They got into the field of cancer too and they used mice.

JOHN GOFMAN

Hughes: Dr. Lawrence, I think you were going to say something about John Gofman.

Lawrence: John Gofman is an outstanding man. I was reminded of many things about John Gofman while going through some of my files. I saved the letters that he wrote to me because he's one of the most sensitive and intelligent and able scientists that I've ever known. He got his Ph.D. in chemistry. I don't know whether he got that under Glenn Seaborg or not. Did he?

Hughes: Yes.

Lawrence: He was one of the co-discoverers of an isotope which is fissionable. I think it's thorium; I can't remember the number. There are large amounts of thorium available, particularly in India, so that there are unlimited amounts of thorium for making atomic reactors. Then he went to medical school and got his M.D. degree, so he's both a Ph.D. and an M.D. But I think of all the people that I've been associated with in this Laboratory, and we've had some very outstanding ones, he's probably the most brilliant, and he was a very hard worker.

Hughes: How did he come to Donner Lab?

Lawrence: Well, I think that Ernest told me about him first. My brother had heard about John Gofman and that he was an unusual guy, and that's the way I got in touch with him. Pretty soon we had him working in the Donner Laboratory; and before long he initiated an outstanding research program on body fats, blood lipids.

Hughes: Did he start that right away? Is that what he came to do, or did that develop later?

Lawrence: No, he didn't start that right away. He started using isotopes in research, but the thing that you remember about him was the work that he did on fat metabolism, and that group is still going here. The fellows that he trained are men now that are no longer young men. Frank Lindgren is one of them. They have probably one of the leading groups for the study of fat metabolism in the world, and they're recognized all around the world. There are about four of them left here.

We could talk hours about Gofman because, first place, he had this

marvelous training. Secondly, he has this terrific imagination, terrific ability. Gofman did so many things that it'd take me an hour to tell you about them, but the most important thing was his contributions to the physical and biochemistry of body fats and blood fats. Of course, fats are so important in the problem of hardening of the arteries and strokes and heart attacks.

Then he did many other things. For instance, he did the first work on what we call X-ray fluorescence where you can take a sample of blood and shine a characteristic X-ray on it and then get secondary X-ray off of it, and that'll give you the amount of that element that's there, the Mosely numbers. So it's very sensitive for detecting nearly all of the elements of the periodic table. That is used widely now in biology and in chemistry and in physics and physical chemistry.

He was a very brilliant speaker and a good teacher. He trained a lot of young men who have jobs throughout the country now. Some of the best ones are here. I think about four of them, like Frank Lindgren and Tom Hayes and Alex Nichols, who are still here. So all I say is that my idea of building up a laboratory is to find people like that. So I am always looking for people like that, and my brother was too, both in the field of physics and chemistry, on the Hill. But anytime he'd hear about anybody, he'd tell me about it.

Hughes: Well, one area that I know he got into a little bit later was this low-level radiation business in the mid 'SOB. I know that was very controversial, and I imagine that it was also controversial here in the Laboratory, and I was just wondering how he was...

Lawrence: (interrupting) Well, it wasn't controversial in the Laboratory and I don't think it is now except on a very small minority scale. In those days I even debated Linus Pauling on this subject, must be 20, 25 years ago, in the Fairmont Hotel in San Francisco. No, it wasn't the Fairmont, it was the (pause)...

Hughes: Mark Hopkins?

Lawrence: Mark Hopkins, yes. There were about a thousand people there.

John Gofman would very often talk on the same subject, and he and I were both on the same side. We believed that the low levels of radiation that we received

naturally, like you receive now where you live or when you're here from cosmic rays and from radiation in the walls of this building, were harmless. We don't think there's any evidence that there's any harm. That's about 100 milliR per year that we all get, and if you add on your diagnostic X-rays for teeth and lungs and any other X-rays that you need, by then it grows out to maybe 125 to 150 milliR. But in order to demonstrate a biological effect in animals, you have to give about 10,000 milliR, which is ten R.

But Gofman changed. He (pause)... Well, one day Jonny Foster came down to see me. Jonny Foster was director of the Livermore Laboratory where I spent day before yesterday. And Jonny said, "I want John Gofman. I want to steal John Gofman from you, because we want to start a program in biology and medicine at Livermore." I said, "Well, Jonny, he's one of our best men, and I think he's probably our best man, but I'm interested in the future of science and if it's clear that he wants to go there, I think that that'd be a great place to have a program in biology and medicine just like we have here." I said, "I can't object to that. I mean, it's up to you and John." So he went out there and started a good strong group in biology and medicine, using the facilities of the Livermore Laboratory. It's still going, and it's a good group.

But he left that when he got interested in this lowlevel radiation. He switched completely; I mean, from where I stand. I've talked to him about it a couple of times, not during the last two or three years, and he and I don't agree on that at all. He claims that background radiation is bad, that the little radiation that we get from the atomic reactors is bad, that it causes cancer, and I think he's wrong.

Hughes: Does he have experimental evidence to support this?

Lawrence: No, no, no.

Hughes: Where does he get these ideas?

Lawrence: Well, he just, he just... What he does is like a lot of these people that are very vocal on this subject. (pause) Let's say 10,000 milliR, and as you go up, why, we'll say that the effect gets bigger and bigger and bigger. Well then, what he does, and a lot of these people do, is extrapolate right down to zero. So they say, "Well, we don't know what happens in this area here, but we assume that every little bit of radiation

that you had added on is bad and it produces cancer and leukemia, and so forth." If that's true, then you'd never want to live in Denver, or you'd never want to fly to New York in a plane. The pilots that fly the jets from here to New York get enormous doses of radiation because of the cosmic rays. The cosmic rays in Denver are about 20 milliR per year. If you live in La Paz in Bolivia, why, it goes up to maybe 40 milliR per year. So that's really the argument.

My philosophy is that you have to weigh risks versus benefits. Now, if you assume that they're right, that you extrapolate right down to zero and you don't want to add on any more radiation at all, then you've got to compare it with what other risks we have in our everyday life. It's like the Three Mile Island accident. Nobody was killed there, but all kinds of people have been killed in industry, every day and every year, and automobile accidents and all sorts of risks we take. Now particularly with the Russian situation, we've got to have energy and we've got to have electricity, and if there's a little risk, it's so small and the benefits are so great. And of course we have smog and we have all of these environmental things that we're breathing every day, and we have monitors. I think one reactor gives less than a milliR per year per person.

Now he and another young fellow (Arthur Tamplin), who worked at Donner, have been appearing before committees and have completely switched on this. Like this man Chauncey Starr. He spoke before the Regents here a year ago when they wanted us (UC) to get out of the Livermore and Los Alamos Laboratories. Charlie Schwartz was one who's a professor of physics here, but there's another one who gave away the Pentagon secrets. What's his name?

Hughes: Ellsberg?

Lawrence: Yes, I guess so. I've heard him speak before the Regents, and those fellows call us murderers that sponsor the Livermore and Los Alamos Laboratories. So I don't understand John that way, but I have the greatest admiration for him as a scientist and as a person, and he'll always be a friend of mine. I don't think he's right, but I wish he'd stayed in the laboratory out there doing research on cancer because I think he had a terrific future. He must be close to 60 years old now, or maybe older.

Hughes: I think he's retired now.

Lawrence: He became a full professor here before he went to Livermore, and he resigned from the University, and he wrote me a letter about that. He's very puzzling to me, because very few of my friends agree with him. I mean, I know a lot of people in atomic energy. He's one of a half dozen like that.

The majority of people that know atomic energy, both medical and biological and physical, are not worried about low-level radiation, and we are going to go ahead and build reactors in the United States. Jerry Brown, of course, has stopped us for the time being in California, but he won't win out. Jerry Brown has gone along with this to stop the building of atomic reactors.

Hughes: At the time that you and he were on the same side, when you were debating with people like Pauling, were you both arguing for the AEC safe radiation levels...

Lawrence: ...were okay.

Hughes: ...were all right?

Lawrence: Yes, yes, yes. He'd very often take my place when I couldn't give a talk, because I'd got more demands than he did because I was older than he and had been in the field longer. But when I couldn't give a talk, he'd take my place. I've got copies of some of his talks that 're much stronger than the ones I gave.

Hughes: Were you arguing against the current levels of safe radiation

Lawrence: Oh no, I...

Hughes: ...or were you saying it was all right?

Lawrence: I was arguing that you have to get up to a certain level before there's any danger, compared to the benefits achieved, as in the case of atomic reactors.

Hughes: Yet I remember in a previous interview you said that that threshold has been lowered over the years.

Lawrence: Yes, it has, but it's never been down to zero. Anyway, Gofman is what makes a

university great, and none of us understand what happens to some of us. Maybe he's right and I'm wrong. You see what I mean? So I just say that he's one of the great people we've had in this Laboratory, and I could talk about eight or ten others. But I think he's probably the most brilliant of them all.

MELVIN CALVIN

Hughes: What about Calvin? How did his association with Donner come about?

Lawrence: Well, there again, Calvin was an outstanding young chemist. (pause) In order to build up a laboratory, just like Gofman, we recognized that here's a man that is outstanding and we went after him and got him to join the Laboratory. I think he was a young instructor then. He became a very active member of the Laboratory.

I made some notes on the first man that showed, with carbon-11, the way the plants take carbon dioxide out of the air and produce oxygen. This is a note on the back of a letter about Sam Ruben. I knew Sam Ruben very well and I was with him when he died. I've got in his handwriting, in my files, the first paper he wrote. He hadn't even published it yet. He gave me the thing to read. But he apparently had it typed and gave me the handwritten article. He's the pioneer in photosynthesis. Well, Calvin is just as brilliant, I guess, as Sam Ruben. But Sam Ruben was a very young fellow like Calvin, and charming and extremely able. So that's the kind of people we were looking for. Ruben was working in the Laboratory too.

Hughes: Did Calvin build upon Sam Ruben's work, or was he starting out afresh?

Lawrence: Well, he knew from Sam Ruben's work that you could trace the path of carbon in photosynthesis. But he used carbon-14. We didn't have carbon-14 then (when Ruben did his work). Carbon-11 was the only isotope of carbon we had.

Hughes: Do you remember the arrangement when Calvin came to the Lab? What was he supposed to do for you?

Lawrence: Oh, he wasn't supposed to do anything for me.

Hughes: Well, I meant for the Lab.

Lawrence: We just set up an organization to build around Calvin to do... You see, chemistry and physics are very important. I was mostly in medicine. So he and his associates did some things for me, but he had his own group. He and some of his students... One of them is a professor at Boulder now, and he wrote a couple of papers on some studies we did on human beings with carbon-14 and CO_2. We built groups. Like Seaborg, he had a group, and Calvin had a group, I had a group.

Hughes: Where did that group system come from? I don't believe you had that formal system before the war, did you? The system of formal groups with a leader who was supposed to direct the research of the group.

Lawrence: No, that we didn't. But during the war, nearly all of them got involved in the war projects.

Hughes: But before the war, did you work in that system?

Lawrence: We started working with isotopes and radiations in 1935, so that we had quite a large group in the Old Radiation Lab and then in the old Crocker Lab. Then in '41, we built the Donner Lab, and so we had quite a few people working. Hardin Jones was one of the people that came to me as a graduate student and wanted to work with our group in biology and medicine.

Hughes: That group leader system, was that simply a development because of the sheer numbers within the organization?

Lawrence: No, that's the history of science. That's always the way it is. In Germany and in all countries, you have one man that's a leader, and he'll have a lot of people working with him. I don't think that's unique at all.

Hughes: I was wondering if the titles were something that developed because of the AEC sponsorship.

Lawrence: No, no, they just grew up naturally. I believed and my brother believed in so-called interdisciplinary research, so that the Laboratory was the first big

laboratory in the world, I guess... (pause) Yes, it's the first big laboratory in the world where they had people with various backgrounds working together. It's being done widely now because everything is getting so complicated that you've got to have mathematics and chemistry and physics and electronics, medicine, and biology and biochemistry and biophysics. So there's really interdisciplinary research.

My whole feeling, that has been neglected in medicine particularly, is to bring in people like Gofman or Saul Winchell, who got his Ph.D. with me, or Rex Huff, who got his Ph.D. here, or Ted Prentice, or Stephen Landau, or Nat Berlin. All these fellows were M.D.s who came here and got their Ph.D.s. Then we had a lot of people that came here just to get their Ph.D.s, without their M.D.s. So you'd have a few people like Gofman who had the ability to be a great chemist and also a good physician. If I were sick, a couple of times when I got the flu, I'd call John Gofman to come out and take care of me. He had terrific common sense. He was a good doctor.

Hughes: It's unusual to get both abilities in one man.

Lawrence: It's what we're trying to do now where I'm on this committee that's trying to find a new director for the Lawrence Berkeley Laboratory. We had a meeting last Friday, and we are interviewing candidates now, but the problem is to find a good scientist who's also a leader. Gofman was a good, great scientist. He's a good leader, great leader, but also he had the combination of training in basic science and medicine. There are very few people who have all those things.

HAL ANGER

Lawrence: I talked yesterday to Hal Anger, who's the first man to develop a camera for imaging parts of the body. I read last night the life history of another friend of mine, who is Benedict Cassen, who was the first man to develop a scanner at UCLA. Hal's been with us since he was a young fellow. And that's spread all over the world. You find Hal Anger's camera and Cassen's scanners all over the world, Russia or Hungary or Poland or community hospitals all over California.

Hughes: Did you bring Anger here specifically to develop instruments to detect...?

Lawrence: No. Just like Bill Brobeck came to my brother when he was building, I guess, the third cyclotron. Brobeck said, "I want to work here." And my brother said, "Well, I don't have any money." "Well, that don't make a difference," he said, "I still want to work here." He became the greatest engineer in the world for building cyclotrons. He resigned from the Laboratory after he had been with it for twenty-five years and founded a company called the Cyclotron Specialties Company, and they build cyclotrons all over the world now.

I was reading last night three books that Mrs. Hevesy sent me, who is the wife of George Hevesy. They tell about his early experiments with Ernest Rutherford, and his early experiments in which he used a form of radioactive lead, which is really an isotope of radium, in a tracer experiment in class. Well, nobody could help but be excited about a tracer experiment, to be able to follow a compound in the body. Any young fellow, in those days they flocked here.

Fellows like Bob Wilson, I used to go skiing with him. We were considering him as a possible director for the Laboratory on the Hill. I told David Saxon, "You know, Bob Wilson and I used to ski together." He was one of many people that came here to work with my brother in physics. But I said, "He's too old." And Dave Saxon said, "Well, how old do you think he is?" "I think he's about 65." Well, Saxon's assistant looked him up and he says, "You're right. He's exactly 65." Fellows like Bob Wilson came here. Luis Alvarez came here. Seaborg came here; he was in chemistry. G.N. Lewis was very close to my brother, and in a way he was close to me because I got to know him and I helped him on some experiments with heavy water. I think I told you about that.

Hughes: Yes, you did. Had Anger heard about Donner Lab's work and thought, "Well, this is the logical place to develop instrumentation for detection of these radioisotopes?" Also, he was attracted to Cornelius Tobias, the chief Biophysicist.

Lawrence: You'd have to have instrumentation.

Hughes: Do you think it was in his mind to come to Donner Lab specifically to build the instruments that you would need...?

Lawrence: No, I don't think so. I think he just came here because it was an exciting place and Cornelius Tobias attracted and helped him. I think I told you that he later got an offer to be a full professor in Chicago, didn't I?

Hughes: I don't think you did.

Lawrence: This fellow that's retired now from Chicago, an old friend of mine, Leon Jacobson, he did a lot of work with isotopes and is a little younger than I am, but not much. He became dean of the University of Chicago. Leon wrote me and he said, "Dear John, out of all fairness to you, I'm writing a letter to Hal Anger offering him a full professorship with tenure at the University of Chicago." Here's a fellow that only had a bachelor's degree in engineering. So I wrote back to Leon—I think I called him—and I said, "Leon, go ahead. We've always encouraged people to get as many offers as they can, particularly if they're interested. The more people that get offers, the better it is for our staff because then our salaries go up too." It's always worked that way. Well, he turned it down.

Hughes: What was his position here at that stage, do you remember?

Lawrence: He was a senior scientist. That's all. He's a senior scientist now. He called me yesterday and talked over something. He's a wonderful fellow. He's gotten all kinds of prizes and recognitions. Just a simple guy like he always was, like Ernest Lawrence was.

JOHN NORTHROP

Hughes: Well, another relationship that Donner Lab had for some time was with John Howard Northrop. I was wondering exactly how that came about? I know for some time he was a professor and research biophysicist here and I believe had a joint appointment in bacteriology.

Lawrence: Yes, I recommended to the President and he appointed him professor of biophysics. He was working down in the Life Sciences Building. We had two or three other people that had appointments with us but were working elsewhere. He is a man that is one of the great scientists, I think, of this century. He's still living

and I should go down and see him in Arizona soon and go clay pigeon shooting with him.

Hughes: He must be close to ninety.

Lawrence: Yes, he must be close to ninety. He's quite deaf, but he's still pretty well. He's a very strong man. I've known him for many years. My wife and Dr. Northrop and Mrs. Northrop and I, we hit it off when President Sproul asked me to show him around and get acquainted on this campus when they invited him out here. That must be over twenty years ago. So we were very close friends of Jack Northrop.

Hughes: Did Wendell Stanley have a part in bringing him out here?

Lawrence: No, I don't think so. I think that they both came at the same time, but they were not working together. They happened to both come from the Rockefeller Institute in Princeton. But I don't think they worked together there and I know they didn't here. They didn't work together and they worked on different subjects although both of them did chemistry and biology of viruses. For instance, Northrop got the Nobel Prize for crystallizing chymotrypsin, which is the enzyme in your stomach that digests meat, and Wendell Stanley got the Nobel Prize for crystallizing the tobacco mosaic virus. But that's typical of Sproul. He wanted to get new and outstanding people all the time in the University.

Hughes: Didn't Northrop come with Rockefeller money?

Lawrence: Yes, he came with Rockefeller money. He had a nice laboratory and one technician. He's a lone worker; he wouldn't have any graduate students. He said they were sort of a nuisance. That's the only tragedy about Jack Northrop. He wrote about one or two papers a year, and they were outstanding papers. But he told me, "Graduate students just take too much of my time." So he didn't do any teaching. But he did have a few people with him in the East. I don't think he had more than one man here, maybe one or two.

Hughes: He seems to have left rather suddenly in 1970. He retired from the University and went to Arizona.

Lawrence: Well, yes, he had to retire. I remember I had correspondence with the Rockefeller

people on that, because Jack didn't want to retire. But he had reached the age, I think, of seventy, and we stretched it out. (phone rings) I happen to know the head of the Rockefeller Institute (phone rings) and they kept him on for two or three more years. (pause to answer phone) He was a (Rockefeller Foundation) lifetime investigator, but after a certain age, I think it was sixty-seven, he had to retire. But they extended him, I think to seventy. I was in correspondence, fighting the battle for him to keep him on, because he was still very productive.

Hughes: Were you doing this largely because you were a friend of his?

Lawrence: Well, I was doing it largely because he was a great scientist, not on a friendship basis. You never do things like that, I never have, on the basis of friendship, for people in the Laboratory. I've had a lot of people in the Laboratory that I've supported that were not really good friends of mine. As a matter of fact, some of them have been problems. But I've recognized that they were so good that I backed them and helped them get support and everything. So I wouldn't do anything in the way of a public trust like I had, just on the basis of friendship. Oh, Northrop was recognized as a great scientist.

Hughes: I was just wondering why you were doing the battling rather than, say, the chairman of the Department of Bacteriology? Or were you both?

Lawrence: Well, I guess they were, but they were short of space down there. I just don't know. The Regents keep people on up to eighty. But why they didn't do that in the case of Northrop... What they should have done was taken him over and made him a professor, let him work until he was about seventy-five. We have quite a few people on all the campuses that are over sixty-seven and some are eighty and older.

Hughes: Why do you think the Regents didn't take over?

Lawrence: Well, they do that because they largely depend on faculty recommendations. So there are certain faculty members that have backing, the recommendations come to the Regents, and the Regents okay it. I can't think of any instance right now where the Regents independently have picked out a man and said, "Let's keep him on." It's come from the faculty.

Hughes: But that didn't happen in the case of Northrop.

Lawrence: No, he was one of the great scientists in the University. They should have kept him on, because...

Hughes: Did you talk to the Regents about keeping him on?

Lawrence: No, I didn't talk to the Regents about it. I was not on the Board of Regents then. No, no, you sort of have to work within the... No, I probably should have. But I thought that the Rockefeller people should support him. They were friends of mine. I guess I just never thought of going to the Regents; probably I should have.

THE DIVISION OF MEDICAL PHYSICS AND BIOPHYSICS

Hughes: Well, shall we talk a little about the Division of Medical Physics? I was wondering when the idea of having an academic unit associated with the medical physics group arose.

Lawrence: That was always my (pause) belief, that there's terrific opportunity on a great university campus like this, with a very fine laboratory like Donner, which is really a foot on the campus for the laboratory on the Hill, although we were more or less separate, geographically. It'd be a great place to have an academic division because we had all kinds of students coming here to get their Ph.D.s, but we had no way of awarding them at first. So from the very beginning I said, "We ought to have an academic division here." So we worked on that for years, and then finally it worked out that we have one. But it took a lot of work and a lot of politics.

Hughes: Why was the Department of Physics chosen as a location for the Division as opposed, for example, to the Medical School?

Lawrence: Well, there's no medical school here.

Hughes: Was it ever a consideration that you'd be affiliated with a medical school?

Lawrence: Oh, it was affiliated with it, but you can't affiliate with UCLA at that distance, or even at San Francisco. You have to work here. We have always had affiliations with the Medical School. But there was a lot of politics, a lot of discussion, and a lot of objections to it, because we were doing some medicine here. Those things are not interesting to talk about. It's more interesting to talk about the positive results.

Hughes: Was it ever a consideration to associate with one of the biological sciences? You were doing medical physics. You could swing either way.

Lawrence: No, the biological sciences were not what we were after. We were after the basic sciences of chemistry and physics, not the biological sciences like zoology and biology. We wanted the association with the physics, chemistry, mathematics and electronics. That's still true that a lot of the biological departments haven't come as far as we have. The Donner was one of the first laboratories in the country where they had real interdisciplinary research on problems in biology and medicine.

No, there was no consideration about affiliations of the Division with the Medical School at all. We weren't after that. We didn't need a large hospital; we didn't need a biology department. What we needed was close association with the basic sciences. That still hasn't taken hold all over the country.

Hughes: Was Dr. Birge always very supportive of the Division?

Lawrence: Oh, he was very supportive; he always was. He was one of the main supporters. Another man, who perhaps was our strongest supporter and advisor during the political times, was Leonard Loeb, a good friend of mine, whose father was Jacques Loeb. We needed older men because of... I see it now at the level of the Board of Regents; the politics that goes on at the University is always present. There are always jealousies and then there's always suspicion about new fields. So we needed the support of people like Birge and Leonard Loeb, and we got it. Finally now it's recognized as an outstanding department. It's got fifty graduate students in it now, and the first one was under me, is a doctor that must be about fifty-five years old now.

I think the simple story is that the Division grew out of the belief that I had

and my brother had that medicine didn't have what it needs. It didn't have the real basic sciences. Now they've changed in biology and biochemistry a lot since those days. A lot of them are trained much better. In molecular biology, they're trained much better. Fellows like Jones, Tobias, Tom Hayes, John Gofman, Saul Winchell, Stephen Landau, Tom Budinger, Nat Berlin. And then we had people from European countries. They all had good training in the basic sciences. It's not so simple now, of course. Molecular biology is getting close to physical chemistry.

Hughes: Was one of the leading reasons for founding the Division of Medical Physics so that people associated with Donner could have academic standing?

Lawrence: No, the reason for the Division of Medical Physics was very simple. It was a great opportunity for the University to benefit from research programs so that students could be working in the Laboratory toward their degrees. So it was simply a great gift to the University. We got the University to pay two-fifths of the salaries of assistant professors or instructors or associate professors. Even today I think all but two men are paid three-fifths by our laboratory budget most of which comes from the AEC or DOE or research grants from the NIH or from private foundations. So it's really a free ride for the University academically. Medical Physics has a small budget. Most of those people have to get their research funds from outside the University and from the Laboratory. The Laboratory supports most of the professors that we have.

THE INTERDEPARTMENTAL GRADUATE GROUP IN MEDICAL PHYSICS AND BIOPHYSICS

Hughes: This is about the Graduate Group in Biophysics which was founded in 1947. I was wondering how the idea got going and who was involved. I know it was an interdepartmental group.

Lawrence: Well, the idea was that here we had a fine laboratory in Donner and also we had the Hill. There was a great opportunity for the students with our Laboratory on campus to learn basic science, particularly those interested in biology. So very early, Joe Hamilton and Hardin Jones and Cornelius Tobias and later John Gofman--all of the early workers--wanted to organize a Division of Medical

Physics so that we could have an undergraduate and graduate program toward a Ph.D. in this field. It finally worked out, and we've got a pretty good department.

Hughes: Yes, but I'm speaking specifically of the Graduate Group in Biophysics which was a little bit different than the Division of Medical Physics.

Lawrence: The University has many interdepartmental groups that award the Ph.D. I can't remember all of them now, but there are quite a few. So the easiest way to organize this was to develop an undergraduate major for our group and then we join in with an interdepartmental group for the awarding of the Ph.D. So you can be a teacher in botany and have a student getting a Ph.D. in biophysics. But it's really run by this Division here; they control it. The chairman of it right now is Cornelius Tobias. Joe Hamilton and I were really the two key men at the beginning. It's really run by the Division of Medical Physics, or it's the Department of Biophysics and Medical Physics now. I'd say perhaps nearly 50 percent of the students are working in this Laboratory.

My brother and I always thought there was a terrific opportunity in biology and medicine to bring in physics, chemistry, mathematics, all the basic sciences which were neglected in medical schools.

Hughes: I'm still a bit confused because Alan Bearden is chairman of the Department of Medical Physics and Biophysics, and you say that Tobias is in charge of the interdepartmental group that you were talking about.

Lawrence: Yes. Tobias is in overall charge of the big group. They're having a meeting here in June. But others in other departments can have graduate students too, and have a strong influence.

Hughes: Then how does Bearden fit into all that?

Lawrence: Well, he's just head of the Department of Medical Physics and Biophysics. But he comes under that group too. He has to follow the rules of the group as far as Ph.D.s are concerned.

I've told the Regents that they've had a free ride for many years in a department which got very little money from the University. Three-fifths of the

salaries were paid by our research program until recently, and I still think about half are, of those fellows that are on the faculty. Bearden I think is fulltime at the University now. But most of the faculty get over half of their money from the Laboratory. This is an outcome of research at the Donner Laboratory.

RESEARCH AND THERAPY ON HUMANS

Hughes: One of the problems that did exist at this time with the Medical School was this business of human medical work—a battle that I think you waged for a long time.

Lawrence: We didn't have any battle there. We'd hear about it, but we didn't have any battle. And we weren't experimenting. We were doing things that we thought should be done, that you would do on yourself, given the conditions of the patients. We didn't have any battles.

I was talking with a man in the hall now who's in charge of this ten-bed research unit over at Cowell Hospital which was build with private funds. I raised the money for it from patients that I've taken care of, and it's probably the only place in the country now where you have a ten-bed investigative unit where you use isotopes and radiations, and associated with LBL and Donner and all the benefits and associations.

We just moved right along, but there were a lot of politicians and a lot of people who weren't politicians who objected to what we were doing. But we knew what we were doing and we didn't get into fields that we didn't know about. We always had competent M.D.s on our staff or associated with us.

Hughes: How did you set up the pavilion at Cowell Hospital?

Lawrence: Well, Dr. William G. Donald and I contacted President Sproul about that.

Hughes: You added a wing onto the existing hospital?

Lawrence: Yes, it's still very active. I don't get over there very often, but it's got a laboratory on one floor and beds on the other.

Hughes: Did that allow you to increase the volume of your research?

Lawrence: Oh, sure. We had patients in there all the time that we were treating or studying. We didn't consider that research on humans. We considered what we were doing was for the benefit of the patients.

There's so much talk now about research on humans. Last night my youngest son and I listened to the television, a program in which they were demonstrating how the army had given drugs to some of the recruits. They interviewed one recruit who got some drug, and I told Steve, "That's one patient. He might be a psychiatric patient, and I just don't believe that Army doctors, or any other doctors, are going to take a human being and treat him like a guinea pig." For instance, I wouldn't allow anybody connected with this Laboratory to give radioactive iodine to young people. We had it here. Other places were doing it, but we wouldn't do it. We wouldn't use students for experiments. We'd do our studies on patients who were sick, and most of them were very sick. Had serious diseases. Nearly all of our work has been on patients with serious diseases. Like that book I wrote twenty-five years ago on polycythemia. I don't know if I've a copy of it here now, but it's a disease that we treated with radioactive phosphorus, and it's used all over the world, including Russia. It's widely recognized and it's practically a cure for the disease.

Here's the last volume of Recent Advances in Nuclear Medicine that we edit. We also edit another book called Advances in Biological and Medical Physics. Now we're planning the next volume. Every two years we come out with a volume of each.

We were working with patients who had serious problems, like leukemia, polycythemia, aplastic anemia, and so the problem of experimentation didn't come up as much as it does now. Now you have to have a review committee to be sure that you're doing everything that should be done. We didn't take any chances.

We wouldn't use radioactive iron either. We'd send it away if people wanted it, hoping that they would use it on animals, but we wouldn't use radioactive iron in humans because there was not a short-lived isotope of iron.

A friend of mine, who retired about two years ago from UCLA as

professor of medicine down there, was a subject in radioactive iron experiments that we provided the iron for. Martin Kamen provided the iron for this fellow, who was working with George Whipple at the University of Rochester. He gave himself radioactive iron, labeled his red cells, but that iron is still in his body, so if you take blood from him, forty years later, his blood is still radioactive. We didn't use radioactive iron in patients until iron-59, which has a half-life of 49 days or 47 days, was discovered. So we were very careful about these things.

I am always afraid of radiation. For instance, my daughter happens to have a perfect set of teeth, and a French doctor wanted her to have full mouth X-rays. I said, "Shelley, you don't want to have any X-rays taken unless they're necessary." So I go along with this business of avoiding radiation, but I take the philosophy of risk versus benefit. I don't think she has more than one filling in her mouth. Perfect set of teeth which she inherited from my mother, who didn't have a single filling until she was about seventy-eight years old. So I still protect people from radiation and I don't believe in throwing it around.

Hughes: In 1950 I know you began a radioisotope unit at Highland Hospital.

Lawrence: Yes, that was the first hospital radioactive unit. It's still going, too. We ran it until about five years ago.

Hughes: Were you training people as you were doing this?

Lawrence: Oh yes, we had people in charge who worked out there, one or two people all the time, and they worked half-time here and half-time out there. Oh yes, they were all training interns and house officers.

Hughes: Were physicians in the community fairly...?

Lawrence: Physicians in the community were on the staff of Highland Hospital.

Hughes: Were they receptive to the use of radioisotopes?

Lawrence: Oh, of course. Oh, they never had any problem there. I see doctors of my age now all the time, who are retired now, who sent patients to us twenty, thirty, or forty years ago. Oh no, we didn't have any trouble with the medical profession.

Hughes: I was talking the other day to Kenneth Scott who used to be here at the Crocker Laboratory.

Lawrence: I used to see Ken a lot. He went over to the Medical School and founded a Nuclear Medicine Unit there. Now most of the staff were trained in Donner.

Hughes: Well, that's what I wanted to ask you about. Do you know anything about why that Radioactivity Research Center was set up there?

Lawrence: Well, how could you avoid it? They're all over the world now. They call this (Berkeley) the cradle of nuclear medicine.

Hughes: So it was just an outgrowth of what was happening...

Lawrence: Oh sure.

Hughes: ...on this campus.

Lawrence: It's happened all over the world. Oh yes, every place I go. The other night I was out at a dinner party and a man said, "I have polycythemia." I've never seen him as a patient. He says, "I'm treated by a doctor who trained with you." So that's spread all over the world.

THE OMNITRON AND THE BEVALAC

Hughes: Well, another issue that I was interested in, the story behind the bevalac. You mentioned at a much earlier interview that the AEC turned down the proposal...

Lawrence: I was talking about that when I was in Cincinnati last week before the wedding. I spent four days at the university there, in the Department of Nuclear Medicine. The head of that department, Eugene Saenger, is a very good friend of mine.

Since I began working with neutrons and Tobias began working with heavy particles, we got together many years ago and finally we realized that since

heavy particles produce very dense ionization in tissue, they have great potential in cancer therapy. So we wanted to get a machine that would produce very high energy particles that would penetrate deeply into tissue so that you could treat some of these hopeless cancer patients. Believe me, there's an awful lot of hopeless cancer, even though standard surgery and radiation and chemotherapy do much. This ionization that's produced by neutrons and heavy particles, such as carbon and neon and argon and krypton, is unique in its biological effect, because it'll kill cells in the absence of oxygen. X-rays won't. If you don't have adequate oxygen, X-rays are not very effective in killing cancer. In spite of that, a lot of people are cured from cancer by radiation. My mother was, my wife Amy wasn't, and that's another story.

This independence of the effect on the killing of cells of the oxygen content interested me from the first experiments I did way back in '35 on neutrons. At that time we found out that neutrons were more effective in killing cells than X-rays, per unit dose, but we didn't understand why. Then the oxygen effect information came along and then we understood it. So Toby and I wanted to get a machine that would be useful in treating patients with hopeless cancer. If you start looking for cancer patients, you don't have to look very far. You just go up on the Hill right now and you can see them. We are treating a lot of them right now. Pancreas, stomach, brain. those are three of the most important.

So we were practically urged to build this machine. Wish I could pull out a plan on it. We had a big, big brochure on it. It was going to be called the omnitron. The omnitron was going to produce heavy particles that would penetrate deeply and produce dense ionization much better than neutrons, because neutrons fall off in their dose to the tissues whereas heavy particles come up like that. (Draws Bragg peak.)

Toby and I got scared when we were being more or less told that we could have the money, thirty million dollars. But we couldn't get the physicists or the chemists interested. And we didn't want to build that machine for our use alone. Everybody was saying that we were going to cure all kinds of cancer with it. Well, it was probably going to do some good, but we had a terrible time and fortunately we hesitated. There were a lot of enthusiastic people from Washington and from NASA who wanted us to do this.

Hughes: And they were going to provide the money?

Oh yes! They were going to provide the money. I particularly was very hesitant about it. We hadn't turned it down completely yet; we were replanning it. Pretty soon a scientist on the Hill by the name of Albert Ghiorso, who's still working up there at the hilac--he's not a faculty member but he's one of the staff members of the Laboratory--got the idea of running a vacuum tube from the hilac, which is the machine that produced lots of new elements... That's one of the machines at least. It's fairly low energy; I think it's about 20 MV per nucleon. He got the idea of hooking the hilac up with the bevatron, which was at one time the biggest atom smasher in the world, and in that way using the hilac as the injector, then whirling those atoms around in the bevatron, and then pulling them out and using them. And that's what we got.

The AEC turned it down and I was very active on that. It was while I was a Regent. It's the first time I've entered into politics, in the sense that we (Toby and I) went to the highest man in the government that I could get in touch with.

I said, "Look, for two million dollars we are have a whole new program of ionization and a form of radiation that's got to be tried in cancer." Then I got Edward Teller to see him and I got Glen Campbell, one of the Regents, to see him. The AEC was overruled and the two million dollars was given to us. Not very many people know this, but I spent a lot of time on that. It's a cheap way of getting what we wanted, which would have cost thirty million dollars. Now it's going on and it'll be another five to ten years to know the results. It's not going to solve cancer, but I think it's going to cure some people that can't be cured now.

Hughes: Why did the AEC turn you down?

Lawrence: A lot of people don't think that irradiation is very important. But if you have cancer, or your wife has cancer, or your mother has cancer, or a patient has cancer, it's pretty. important. For the individual it's important. But a lot of people said there were better ways to spend the money, I suppose. I don't know why they turned it down.

Hughes: That's hard to understand when the figures show that cancer is the number two killer nowadays, just behind heart disease.

Lawrence: You go to these big hospitals and see a ward full of kids with cancer. And it's young people, too. We're not making any great progress in it. It's being done at Los Alamos with pi mesons, also dense ionization. We did the original work here with the 184-inch cyclotron, all the animal work. Then we shipped our men down there, and Raju was one of them. So they have a very big program going at Los Alamos and on the Hill now in the treatment of cancer. Excellent people running it.

Hughes: You mentioned that the chemists and the physicists weren't interested in the omnitron.

Lawrence: I don't understand that. We had committees several times come out, and I got Edward Teller to come out to my house one night. He talked to them and was very enthusiastic about the physics and chemistry that could be done with the bevalac. He was a very good friend of my wife and me too, and still is. He and I each received a medal in Montreal a couple weeks ago.

Hughes: What was it for?

Lawrence: His medal was for his contribution to the problems of energy, and mine was for work in medical research and diagnosis and therapy.

Hughes: What organization?

Lawrence: It was the American College of Nuclear Medicine, at their annual meeting. They held it in Montreal this time. So we had a great time. He's a wonderful man and a very hard worker, still very active.

I got him out to our house and he was the only physicist that I could get interested in the omnitron. Couldn't get any of the chemists interested. But now with the bevalac you have to cry for time. Scientists from all over the country come here and use it. They're very generous, and scientists from other universities too.

NUCLEAR MEDICINE AND THE MEDICAL CURRICULUM

Hughes: Can you think back to when instruction in nuclear medicine became a recognized part of medical education?

Lawrence: Well, I don't think it is yet. When I was in Cincinnati I attended a postgraduate course one Saturday. There were about two hundred young men and women there who had just completed their residency training in radiology. So this was sort of a review for these people. I think it was a four-day meeting put on by the Department of Radiology and Nuclear Medicine. So I heard a couple of talks there. One of them was by Gene Saenger on the diagnosis of bone lesions with radioactive pyrophosphate labeled with technetium. He gave a very good review. I stayed at their home with Gene and his wife. They have a large home and they like to have people stay there so they don't have to run back and forth downtown to pick them up. I said, "Gene, you know those people you talked to today, you know how much nuclear medicine they get in their training? They get practically none in medical school and they get three months of nuclear medicine training." And he said, "That's really true, John." He said, "That's really true." You can't learn nuclear medicine in three months.

Hughes: Why is that?

Lawrence: Well, again it's political in this sense, that the radiologists who are experts in imaging, that is in looking at X-ray films, realize that nuclear medicine is terribly important in the operation of a radiology department, and there's a lot of money in it. A third of all patients in this country now have some sort of a nuclear medicine procedure done when they're admitted to a hospital. That is a third of all hospital admissions have a nuclear medicine procedure, many of which have been started here.

So to get back to why this training is so poor. That's one of the reasons that we've trained so many excellent people here. I mean a lot of them like Tom Budinger, Saul Winchell, Rex Huff, and the fellow that's professor of medicine at Syracuse now. Well, there are dozens of them. They spent from one to five years here. A lot of them got their Ph.D.s. They really learned nuclear medicine. They learned the physics of it, the chemistry of it, and the patient care. You can't learn it in three months. But still these inadequately trained people will take this

nuclear medicine board exam and they'll be licensed to practice nuclear medicine.

Gene Saenger admits that it's wrong and we are training a lot of people in places like this that are the top people in the country. I think it's a matter of money. That's another problem in medicine nowadays: everything is so expensive. X-rays, nuclear medicine procedures are very expensive. An X-ray department doesn't want to lose the nuclear medicine part of it. I think that's largely political and largely money.

Well now, these radiation people, they learn radiation therapy with the cobalt and Varian tubes and so forth. They learn X-ray diagnosis and nuclear medicine all in about four years, but only about three months of it is nuclear medicine. That's not adequate. Most of the leaders in the country in nuclear medicine, the top people, are trained much longer, and many with a background in internal medicine.

Hughes: Is it the radiologists that are opposing longer training in nuclear medicine?

Lawrence: Yes, it's the radiologists.

Hughes: They just don't want to give up any space in the curriculum?

Lawrence: Yes, I was in on a lot of discussions on that in meetings with them. They just don't want to give up. So lots of departments of nuclear medicine are divisions of a department of radiology. But some of them are separate. A lot of private practitioners that are radiologists do some nuclear medicine too.

THE ALPHA OMEGA FOUNDATION

Hughes: In about 1960 you founded the Alpha Omega Foundation. Can you tell me about the purpose of the foundation?

Lawrence: Well, yes. I'm a great believer in foundations. We got started here from foundations. All my early work was supported by private foundations. I had some stocks that my uncle who's a banker had left to my brother and me. Then I'd made a few investments so I had this extra money and I thought I'd start a

foundation. It wasn't very much money, but it's big enough so that now I can have one of my boys on the board, and other persons on the board. I helped found another foundation in San Francisco, too, that's rather small. We give away $35,000 or more per year. It's a lot of fun. We give quite a bit to this Laboratory and to U.C. and to Stanford and elsewhere.

Hughes: It is all medically oriented?

Lawrence: It's for medical research and cancer research. But we could give it to a church or any tax-free institution that we wanted to.

Hughes: So it's really unspecified where the donations go?

Lawrence: Well, the smaller foundation is named the Cancer and Medical Research Foundation. We can give it to any legally classified philanthropic organization, like a university or a church. You can't give it to any individuals and you can't use any of the funds yourself. The only expenses that we have with it are the payment of the income tax thing and keeping track of our records.

Hughes: What is the foundation in San Francisco?

Lawrence: Well, that's called the Cancer and Medical Research Foundation. I'm on the board. Walter Sullivan, James Born and I founded it.

Hughes: Is there any difference between the two foundations?

Lawrence: No. Well, the bylaws of that one are somewhat similar to the bylaws of the one I founded. I've urged my children to support it, and the two doctors do. I get donations to it and I give to it a little bit every year. I hope it will amount to something.

The Donner Foundation started out with a million and a half dollars that Mr. Donner gave to found the International Cancer Research Foundation. I remember when it was founded. He lost his son from cancer of the lung at the age of about 35 or 36. Now that foundation is worth about $60 million with no further donations. It's grown because of growth of stocks and so forth. So I'm hoping that this foundation that I started, that I named after my wonderful wife,

will amount to something someday so you can do worthwhile things. We could give it to scholarships if we wanted to.

Hughes: I have seen reference to the Alpha Omega Foundation.

Lawrence: Yes, well we changed the name. I changed that a few years ago. I started out with the Alpha Omega Foundation.

Hughes: When you say that you like foundations, are you meaning in contrast to government support?

Lawrence: Oh yes. I think the government support of research is fine, but I think that we've got too much government and we've got too much taxes for people. Everybody's taxed so heavily now. If people would give money away, it's tax exempt, to a private foundation, I think they're managed a lot better than the government. I think we have too much government, I really do.

Hughes: I've heard it said that research nowadays is tailored to what is thought to be the current interest of the federal funding agencies. Consequently, it's not really free research.

Lawrence: That's a danger. It's not as free as it used to be. I simply believe that the American way is giving to others and helping important things. I think a lot of people that have a little extra money should start these little foundations and they grow. I think it's the American way. I think it's the only country in the world like it. There are hundreds of these foundations.

For instance, our board gave a couple thousand dollars to the director of Donner Laboratory just before I left for the East. One part of it I've been giving continuously for the Amy Lawrence Endowment Fund which I started about ten years ago. Then I gave the director $1,000, but I do this every so often, $2-3,000. I gave $1,000 free to whatever he wanted to do with it. Now that kind of money is hard to get. It has to be given to the Regents, but then the director of the Laboratory can use it anyway he wants. He can't buy theater tickets with it, but I mean in support of research. So I'm looking forward to getting back to a little more critical thrift in the operation of a government like you do in the operation of private enterprise. I think research in a place like this is pretty well done and

fairly thrifty. And I think that's probably generally true. But of course I'm in favor of government support for research, too.

Hughes: Well, Dr Lawrence, that's about all I have to ask you. Is there anything that you would like to add?

Lawrence: No, if you can think of anything else or if I think of anything else, I can call.

Hughes: Yes. Thank you very much.

Lawrence: I only wish we had talked more about research and its results, but your questions were so often concerned with people and situations, and so we neglected somewhat the research contributions.

APPENDIX

JOHN HUNDALE LAWRENCE

Biography

born: Canton, South Dakota, January 7, 1904

father: Carl Gustavus Lawrence
president, Northern State College, Aberdeen, South Dakota

mother: Gunda Jacobson Lawrence, housewife (formerly mathematics teacher)

brother: Ernest Orlando Lawrence, physicist
born: Canton, South Dakota, August 8, 1901
died: Palo Alto, California, August 27, 1958

education: University of South Dakota, Vermillion, South Dakota
(Major: Chemistry) AB 1926 cum laude,
Phi Beta Kappa
Harvard Medical School, Boston, Massachusetts, MD 1930
Top of class, member Alpha Omega Alpha (Phi Beta Kappa of medicine)
University of South Dakota, D.Sc. honorary, May 24, 1942
University of Bordeaux, docteur honoris cause, October 24, 1958
Catholic University of America, D.Sc. honorary, June 7, 1959

married: Amy McNear Bowles, June 20, 1942, San Francisco, California
born: San Francisco, December 1, 1920
granddaughter of Mr. Philip Bowles, former President of the Board of Regents of the University of California, donor of Bowles Hall, dormitory for men, UCB
attended University of California at Davis, major in soils and animal husbandry.
great-grandfather, California cattle pioneer, Henry Miller

children: John Mark, San Francisco, March 16, 1943
Amy Sheldon, San Francisco, May 1, 1946
James Bowles, Oakland, June 1, 1952
Steven Ernest, Oakland, February 28, 1961

hobbies: hunting, fishing, reading, etc.

religion: protestant (Lutheran, Episcopalian)

addresses: home: 220 Glorietta Blvd., Orinda, California 94563
office: Founder and Director Emeritus and Professor, Donner Laboratory, UCB, Berkeley 94720
home telephone: 415-254-2487
office telephone: 415-642-3591

Social Security #578-44-2136

Driver's license (California)Z1014590

License to practice medicine in California: G-125 (issued 1937)

Drug-narcotics license #3706

Description: 6'0" tall; weight 165#; eyes - blue; hair - gray-brown

Blue Cross - Blue Shield: Group 5-29616 G91-54-0769
CPIC Major Medical 99518

Clubs: Bohemian Club, San Francisco
Pacific Union Club, San Francisco
Faculty Club of the University of California, Berkeley (honora
Sierra Club

Principle field: Medical research; nuclear medicine.

CURRICULUM VITAE

John H. Lawrence, M.D.

Emeritus Professor of Medical Physics; Director of Donner Laboratory; Physician-in-Chief of Donner Pavilion; Associate Director of Lawrence Radiation Laboratory, University of California, Berkeley. Now, active member of the Research and Teaching Staff of the Donner Laboratory and the Lawrence Berkeley Laboratory, and of the University of California.

Graduate and Honorary Degrees:

A.B. cum laude	1926	University of South Dakota
M.D.	1930	Harvard Medical School
D.Sc. Honorary	1942	University of South Dakota
D.Sc. Honorary	1958	University of Bordeaux
D.Sc. Honorary	1959	Catholic University of America

Awards:

Caldwell Medal, American Roentgen Ray Society, Cincinnati, Ohio, 1941.
Mackenzie Davidson Medal, British Institute of Radiology, London, England, 1947.
Certificate of Appreciation, U.S. War and Navy Department, in recognition of outstanding services to the Armed Forces, Office of Scientific Research and Development, World War II.
Medal awarded by his Holiness Pope Piux XII in recognition of Dr. Lawrence's care of Aloysius Cardinal Stepinac in Zagreb, Yugoslavia, 1953.
Silver Medal of the University of Bordeaux, France, 1958.
Silver Cross of the Royal Order of the Phoenix, Greece, 1962.
Pasteur Medal, Pasteur Institute, Paris, France, 1963.
Nuclear Pioneer Award, Society of Nuclear Medicine, 1970.
Distinguished Scientist Award, Northern and Southern California Chapters, Society of Nuclear Medicine, 1971.
Marshall Brucer Award (Medal and Lecture), Arizona Center for Radiation Oncology Studies, Tucson, Arizona, 1972.
Elected Distinguished Fellow, American College Nuclear Medicine, May, 1974.
Judd Award, Memorial Sloan-Kettering Cancer Center, New York, June, 1975.
The First "John Lawrence Interdisciplinary Symposium," University of South Dakota, 1979.

Lectureships 1941-1977:

Edwin L. Kretschmer Memorial Lecturer, University of Chicago.
Harrison Stanford Martland Lecturer, New Jersey.
Mackenzie Davidson Memorial Lecturer, British Institute of Radiology, London, England.
Sigma Xi Annual Lecturer, Stanford University.
Ludwig Kast Memorial Lecturer, New York Academy of Medicine.
William Henry Welch Lecturer, Mt. Sinai Hospital, New York.

Lectureships 1941-1977 (continued):

Roger Morris Lecturer, Cincinnati Academy of Medicine.
Aaron Brown Memorial Lecturer, University of Washington, Seattle.
Steven Walter Ranson Memorial Lecturer, Northwestern University, Chicago.
University of Bordeaux Lecturer, Bordeaux, France.
Goodfriend Memorial Lecturer, New York.
Friedberg Memorial Lecturer and Visiting Professor of Medicine, University of Cincinnati.
Pasteur Lecturer, Pasteur Institute, Paris, France.
Richardson Memorial Lecturer, Massachusetts Genral Hospital.
Bruce K. Wiseman Memorial Lecturer and Visiting Professor of Medicine, Ohio State University.
Leo G. Rigler Lecturer, University of Minnesota, Minneapolis.
Marshall Brucer Lecture, University of Arizona School of Medicine, Tucson, Arizona, 1972.
George Von Hevesy Memorial Lecture, Int. Nuclear Medicine Society, Athens, Greece, 1973.
Sloan-Kettering Inst. Seminar, New York, June 1975.
American Physical Society, Washington, D.C., June 1975.
University of Georgia School of Medicine, May 1976.
University of Munich, November 1976.
Series of Lectures, University of Damascus and University of Alleppo, Syria, October 1976.
Lecture, Low Level Radiation Conference (dinner speaker), Georgia Tech., February 1977.
Panel Member, Course on the Pituitary Gland, Oshsmer Clinic, New Orleans, March 1977.
Two Invited Lectures, American College of Nuclear Medicine, Los Angeles, October 1977.
Lecture, Interurban Clinical Club, Vancouver, B.C., October 5-6, 1978.

Services to Outside Institutions and Government Agencies, 1942-1973:

Office of Scientific Research and Development, USAAF, US Navy, and Manhattan District, National Research Council.
United States Joint Task Force No. 1, Atom Bomb Tests, Bikini, Member of the Staff, 1946.
Leader of two expeditions to the Peruvian Andes for the study of high-altitude physiology, using radioisotopes to assess acclimatization, 1950-51.
State of California Radiological Safety Advisory Commission, 1953-56.
U.S. Delegation to Conferences on the Peaceful Uses of Atomic Energy, Geneva, 1955; Paris, 1957; and Buenos Aires, 1959.
U.S. Delegate to UNESCO, International Conference on Radioisotopes in Scientific Research, Paris, 1957.
U.S. Atomic Energy Commission Representative to the Atomic Energy Establishment, Trombay, India, 1961; also, Visiting Professor, University of Bombay for one month.
American University of Beirut, Lebanon, Visiting Professor of Medicine, Middle East Medical Assembly, 1963.

Services to Outside Institutions and Government Agencies, 1942-1973 (continued):

NASA-STAC, Science and Technology Advisory Committee for Manned Space Flight, Biomedical Sub-Committee Member, 1967.
Committee on Medical Radiation Physics, WHO/IAEA in Geneva, Dec. 1967.
HEW, FDA Radioactive Pharmaceutical Advisory Committee, Chairman, 1968-1971.
Member, Three-Man Medical Exchange Mission to U.S.S.R., 1974.

Memberships in Scientific Societies:

American Association of Neurological Surgeons (Harvey Cushing Society).
American Clinical and Climatological Association.
American College of Physicians.
American Diabetes Association.
American Medical Association.
American Nuclear Society (made Fellow of ANS)
American Physiological Society.
American Society of Clinical Investigation.
Endocrine Society.
Harvard Medical Alumni Association (President 1945-46; 1962-63).
Western Association of Physicians.
Distinguished Fellow, American College of Nuclear Medicine.

Honorary Memberships:

Phi Beta Kappa
Alpha Omega Alpha
Pacific Interurban Clinical Club
Sociedad Radiological Panemena
Societe Francais de Physique, Biologique et Medical
Society of Nuclear Medicine (Board of Trustees 1962-65; President 1966-67)

Books:

Lawrence, J.H. Polycythemia; Physiology, Diagnosis and Treatment Bases on 303 Cases. Grune & Stratton, New York, 1955.
Lawrence, J.H., Manowitz, B., and Loeb, B.S. Radioisotopes and Radiation: Recent Advances in Medicine, Agriculture and Industry. (1 of 4 volumes prepared under the auspices of the USAEC for the Geneva Conference on Peaceful Uses of Atomic Energy, 1964). McGraw-Hill Book Co., New York, 1964.
Editor. Progress in Nuclear Medicine, Grune & Stratton, New York, Vol. I, 1965; Vol. II, 1968; Vol. III, 1971; Vol. IV, 1974; Vol. V, 1978.
Co-editor with C.A. Tobias, J.W. Gofman, and T. Hayes. Advances in Biological and wedical Physics, Academic Press, Volumes 1-16, 1948-75.

Dates and Brief Descriptions of Some of Dr. Lawrence's Research:

1933-37 - Began using x rays and radioisotopes in biology and medicine; studies in normal and leukemic animals; tumors in animals; metabolic studies with P-32 in animals and man. Tracer studies in patients.

1937 - First used P-32 to treat patients with diseases of the blood.

1936-37 - With W.O. Nelson and H. Wilson, reported relative radioresistance of pituitary gland to roentgen radiation.

1935-38 - First biomedical studies with heavy particles with demonstration of the greater biological effect of the dense tissue ionization in normal and neoplastic tissues of animals.
Set up radiation protection program in the Radiation Laboratory.

1935-37 - Demonstration of compound (estrogen) that gave some protection against radiation injury to mice (confirmed 1943).

1940 - Founded Donner Laboratory with funds provided by the Donner Foundation and other private sources, dedicated to interdisciplinary research and to the application of the natural sciences to problems in Biology and Medicine; also, emphasizing the use of products of atomic energy (radioactive tracers and new radiations).

1944 - With C.A. Tobias, F.J.W. Roughton, W.S. Root, and M.I. Gregerson, first used positron-emitting isotope (C-11) in breath analysis studies of oxidation of CO and CO_2 in the human body.

1946 - With C.A. Tobias, W.F. Loomis, and F.H. Turpin, discovery of narcotic properties of gas xenon (since used as an anesthetic).

1949 - With C.A. Tobias, H.B. Jones, and J.G. Hamilton, report of first use of labeled inert gases in humans.

1952 - With N.I. Berlin, first description and diagnosis of human syndrome of relative polycythemia, polycythemia of stress.

1954 - With N.I. Berlin, better clarification of the nature of the anemia occurring with leukemia (by measuring red blood cell life span with glycine-2-C-14).

1947-present - Tracer studies in normal and disease states, using various radioisotopes.

1948-present - Further studies of the special advantages in therapy of the higher-energy, heavier-charged particles which have led, in association with his colleagues C.A. Tobias, J.L. Born *et al.*, to the successful use of heavy-particle beams in the treatment of several diseases.

Currently, studies continue, concerning the properties of heavy particles with experiments to determine the optimum particle for use in therapy, and in the investigational use in treatment of cancer.

BIOGRAPHY

John H. Lawrence,
private papers

JOHN HUNSDALE LAWRENCE

From: The Magnet
July, 1970

Dr. John H. Lawrence, pioneer of nuclear medicine and director of the Donner Laboratory since its establishment in 1936, was named to the University's Board of Regents by Governor Ronald Reagan, effective on May 15, 1970.

Dr. Lawrence, brother of the founder of the Lawrence Radiation Laboratory, Ernest O. Lawrence, began his career as an instructor in medicine at Yale University School of Medicine after graduating from Harvard Medical School. At Yale, he studied radiation and other effects on the pituitary gland with Dr. Harvey Cushing, and did experiments leading to the discovery of the first chemicals (estrogens) found to give protection against radiation.

He spent the summer of 1935 in Berkeley carrying out the first biomedical studies with the products of the newly developed cyclotron, and became interested in the possible use of artifically produced radioisotopes and nuclear radiation in medicine. In 1936 he came to the University of California with the encouragement of his teacher, Harvey Cushing, who told him, "You are pioneering in a very exciting new field, which will have a tremendous impact in medicine. Go to it." Dr. Lawrence founded the Donner Laboratory, within the University of California's Lawrence Radiation Laboratory, as the world's first nuclear medicine laboratory, and it became a major center for directing experimental medicine into new channels, as well as training physicians in atomic medicine by an extensive teaching and fellowship program. The academic Division of Medical Physics, housed within the Donner Laboratory, was founded by him and his associates, Professors Joseph Hamilton, Hardin Jones, and Cornelius Tobias.

Dr. Lawrence initiated the treatment of human disease with a radioisotope, phosphorus-32, in 1937. Polycythemia vera was the first disease to be successfully controlled with radioisotopes. With colleagues, he was the first to observe the large effects of neutrons on biological systems, and initiated the use of accelerator particle beams (neutrons) in the experimental treatment of cancer in 1939. Using iron-59, discovered by J.J. Livingood and G.T. Seaborg, in 1939, Dr. Lawrence and his colleagues have, in the intervening years, made a very significant contribution to the understanding of normal blood metabolism and blood disease. He, with Jones and Tobias, discovered the narcotic properties of the rare gas xenon, which has since been used as an anesthetic. For more than a decade, with his colleague Cornelius Tobias and others, high-energy particle

beams from the 184-inch cyclotron have been used successfully in the treatment of acromegaly, Cushing's disease, and several other conditions.

In 1955, Lawrence was a member of the U.S. Delegation to the International Conference on the Peaceful Uses of Atomic Energy. Later, he served on the State Department mission to Thailand and Pakistan on the development of atomic energy programs. In 1961, he represented the Atomic Energy Commission as a consultant to the Atomic Energy Establishment of India.

Dr. Lawrence's honors and awards have been world wide. He has received several honorary degrees, and his awards include the Caldwell Medal of the United States, the Davidson Medal of Great Britain, the Medal of the University of Bordeaux, the Silver Cross of Greece, and the Pasteur Medal.

Most recently, he has been selected as the Nuclear Pioneer for 1970 by the Society of Nuclear Medicine. This annual honor is accorded by the Society in its Nuclear Pioneer Lecture Series, in which the work of a distinguished nuclear scientist is presented by a Lecturer who is himself a leader in some field of nuclear science. At the Society's Annual Meeting in Washington, D.C., in July 1970, Dr. Lawrence's career was described by Dr. Joseph Ross, a leader in nuclear medical research at UCLA.

Dr. Lawrence and two of his four children live in Orinda. His wife, the former Amy Bowles, died in 1967.

In announcing Dr. Lawrence's appointment, Governor Reagan praised him as "an extremely distinguished scientist," and said he was grateful that he has agreed to devote some of his time to the solution of problems affecting the University. Lawrence hopes to have more free time for research now, and will continue to take part in the research and training program of the Laboratory.

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currents

LAWRENCE BERKELEY LABORATORY

Berkeley, California 94720 September 13, 1991 Vol. 18 No. 33

Inside Currents:

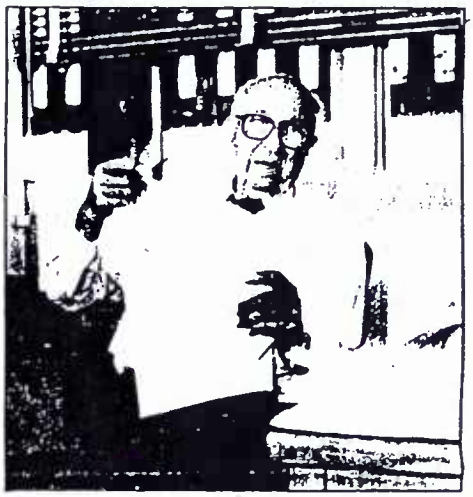
In Memoriam

Edwin Mattison McMillan

1907 - 1991

John Hundale Lawrence

1904 - 1991



By Lynn Yarris

This past weekend saw the passing of two scientific giants whose brilliance helped lead this laboratory to its storied fame. Edwin Mattison McMillan, Nobel laureate, former director of LBL, and professor emeritus of physics at UC Berkeley, died at 1:30 p.m., Saturday, Sept. 7, at his home in El Cerrito. He was 83 years old. Approximately 12 hours later, John Hundale Lawrence, a pioneer in nuclear medicine who established LBL's Donner Laboratory as the world's first nuclear medicine research laboratory, died at Alta Bates Hospital in Berkeley. He was 87.

McMillan, who suffered a stroke several years ago, died of complications from diabetes. Lawrence died from a stroke suffered two weeks ago.

McMillan shared the 1951 Nobel Prize in Chemistry with Glenn Seaborg as the co-discoverers of plutonium. He became LBL's director in 1958 after the death of founder Ernest Orlando Lawrence, John Lawrence's brother. McMillan headed the Lab for 15 years, until his retirement in 1973.

More than most contemporary scientists, McMillan fit the classical profile of a "natural scientist." Though he won the Nobel Prize in chemistry, he was trained as a physicist and later developed the concept of "phase stability," which overcame the energy limitations of cyclotrons and made possible today's giant particle accelerators. For this work, he shared the 1963 Atoms for Peace Prize with Soviet physicist Vladimir Veksler. In 1990, he was awarded the National Medal of Science, the nation's highest award for scientific achievement.

When told of McMillan's death, Seaborg said, "I have known Ed McMillan as a friend and colleague since 1934. His important and versatile scientific contributions spanning physics, chemistry, and engineering, and his great human qualities, form an important chapter in the history of science."

McMillan was born in Redondo Beach, Calif., and grew up in Pasadena. As a youngster, he was a builder of gadgets, such as high frequency coils. He also developed an interest in minerals and rocks that would last throughout his life.

McMillan earned his bachelor's and master's degrees at the California Institute of Technology, where his interest in chemistry led him to take more courses in this subject than is usual for a physicist. He received his doctorate from Princeton University doing research with molecular beams.

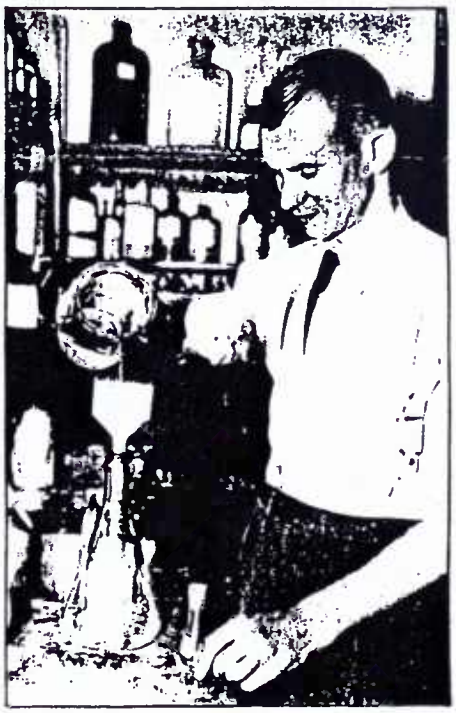
Drawn by Ernest Lawrence's invention of the cyclotron, McMillan came to Berkeley from Princeton, eventually joining the staff of Lawrence's laboratory and the faculty of the UCB Physics Department. His contributions were significant from the start — he made the first substantial verification of the important theory that gamma rays, upon striking a nucleus,

(Continued on page 2)

McMillan services

Services for Edwin McMillan will be held Saturday, Sept. 14, at 7 p.m., in the UC Berkeley Faculty Club.

An Edwin M. McMillan Scholarship fund has been established with the UC Berkeley Foundation. Persons wishing to contribute should contact the UC Berkeley Development Office, 2440 Bancroft Way, Berkeley, Calif., 94720. The phone number is 642-1212.



McMillan recreates the search for neptunium at the time of the announcement of the discovery, June 8, 1940.

In Memoriam . . . (Continued from page 1)

give rise to a pair of electrons, one positive and one negative.

In 1934, McMillan and M. Stanley Livingston discovered oxygen-15, and in 1940, he and Samuel Ruben discovered beryllium-10. Using the 60-inch cyclotron to study fission in uranium, McMillan and Philip Abelson discovered element 93, neptunium, the first substance beyond the 92 naturally occurring elements.

McMillan also found evidence suggesting the existence of still another element, but before he could complete his research, he was called away by World War II. A team lead by Seaborg completed the work with the discovery of element 94, plutonium.

During the war, McMillan did research on radar at the Massachusetts Institute of Technology, on sonar at the U.S. Navy Radio and Sound Laboratory in San Diego, and on the atomic bomb at Los Alamos with J. Robert Oppenheimer.

After the war, McMillan turned his attention back to accelerator research. He focused on the crucial problem that when particles are accelerated numerous times at very high energies, they fall out of step with the accelerating pulses.

In June of 1945, he came up with a solution — an accelerator in which particles are locked in step with the accelerating pulses, making it feasible to accelerate them virtually any number of times. McMillan coined the term "synchrotron" for this new type of accelerator. Synchrotrons are now the standard instrument of high-energy physics research.

McMillan was a capable and well-liked director, described by colleagues as a modest man of uncharacteristic calm and wit. He was an avid hiker, camper, and mountain climber — he scaled Mt. Whitney and the Matterhorn as a younger man. He also enjoyed gardening, and was especially fond of orchids.

Said McMillan's son, Stephen: "He was an explorer. He wasn't looking at what was known, but what wasn't."

McMillan is survived by his wife, the former Elsie W. Blumer, whom he married on June 7, 1941; his three children, Anne Chaikin of Bellingham, Wash., David McMillan, of Anacortes, Wash., and Stephen McMillan, of El Cerrito, Calif.; and by three grandchildren.

John Lawrence was born in Canton, S. D., four years after Ernest. The Lawrence brothers grew up in a family that valued academics. Their mother was a mathematics teacher, their father taught Latin. After graduating from Harvard Medical School, Dr. Lawrence taught medicine at Yale and studied the effects of radiation on the pituitary gland. With Dr. Harvey Cushing, he identified estrogen as the first chemical found to give protection against radiation.

In 1935, John Lawrence joined Ernest in Berkeley and carried out the first biomedical studies with the products of Ernest's newly developed cyclotron. At that time, he became interested in the possible use of artificially produced radioisotopes and nuclear radiation in medicine. With Dr. Paul Aebersold, he made the discovery that neutrons had a destructive effect on living tissue and that the damage was five times more than that caused by an equivalent dose of x-rays.

In 1936, Dr. Lawrence founded the Donner Laboratory within the Lawrence Berkeley Laboratory (then called the UC Radiation Laboratory). It quickly became a major center for directing experimental medicine into new channels, as well as for training physicians in atomic medicine. What is now LBL's Research Medicine and Radiation Biophysics Division got its start under Dr. Lawrence as well. In 1944, UCB established a division of medical physics within the Physics' Department. It was led by Dr. Lawrence and his associates, Professors Joseph Hamilton, Hardin Jones, and Cornelius Tobias.

Young John Lawrence contemplates a problem in 1938.

John (top) and his brother, Ernest Lawrence, consult at the controls of the 60-inch cyclotron in 1939.

Scientist-historian William G. Myers has called Dr. Lawrence the "true father of radiopharmaceuticals." The field began, Myers says, in 1937 when Dr. Lawrence used the radioisotope phosphorus- 32 to successfully treat polycythemia vera.

In 1939, Dr. Lawrence initiated the use of accelerator particle beams (neutrons) in the experimental treatment of cancer. The same year, using iron-59, he and his colleagues made significant contributions to the understanding of normal blood metabolism and blood disease. With Jones and Tobias, he discovered the narcotic properties of the rare gas xenon, which has since been used as an anesthetic. Later, he used beams from the 184-inch cyclotron to successfully treat acromegaly, Cushing's disease, and several other conditions.

Dr. Lawrence's many honors include the 1983 Enrico Fermi Award, presented by the U.S. Department of Energy for his "pioneering work and continuing leadership in nuclear medicine." In 1970, the Society of Nuclear Medicine recognized him for "a lifetime of accomplishment."

Dr. Lawrence retired from Donner in 1969, and the following year was appointed a Regent of the University of California by then-governor Ronald Reagan. He served until 1983. He was president of the Society of Nuclear Medicine in 1966-67.

When once asked which of all his achievements he was most proud, Dr. Lawrence answered: "I think that the opportunities we've made available for young doctors — Ph.D.s and M.D.s — to work in the field of atomic energy in biology and medicine, that's our most important contribution. We've trained a lot of good people and now they are training the next generation."

Dr. Lawrence's wife, the former Amy Bowles, died in 1967. He is survived by four children, Dr. John Mark Lawrence, of Long Beach, Calif.; Amy Sheldon De Rouvray, of Paris, France; Dr. James Lawrence, of Alamo, Calif., and Steven Lawrence, of the San Francisco Bay Area.

Funeral services for Dr. Lawrence were held Wednesday, Sept. 11, at St. Peter's Episcopal Church in Oakland.

Memorial contributions should be made to the John and Amy Bowles Lawrence Cancer and Medical Research Foundation in Berkeley.

January 20, 1964

correspondence, John H. Lawrence
private papers

Dean William B. Fretter
College of Letters and Science
224 Sproul
Campus

RE: Promotion of Dr. John Lawrence to
Professor Step V

Dear Dean Fretter:

I am submitting to you some data on the academic activities of Prof. John H. Lawrence in support of my request that his salary be increased to Step V.

John Lawrence as a medical research man has early recognized the impact of physical science on the future of medicine and he has worked in his entire career to foster medical research with the tools of physics, particularly nuclear physics. As Director of the Donner Laboratory, his influence was and is important since the best medical schools in the world have sent their representatives here for postgraduate training and the existence of the laboratory here is a factor not only in the acceptance of quantitative techniques in medicine but in the modernization of a number of medical schools and medical research units. The acceptance of Dr. John Lawrence's preeminence in the field is indicated by the lectures and public forums he is asked to participate in. His graduate course, Medical Physics 225 (which he delivers in cooperation with Drs. Rosenthal and Parker) is acknowledged as one of the finest such offerings in the world. Each year a number of physicians and other research men of high calibre seek to come here to Berkeley to work in collaboration with or under the direction of John Lawrence, many of them on national or international fellowships. I am enclosing a list of M.D.'s at Donner Laboratory in 1962-63 and 1963-64 for collaboration, postgraduate training and experience or for an advanced degree. Just mentioning two names from this year's visitors, they include Prof. Frederick Robbins, Nobel Laureate from Western Reserve University who is spending his sabbatical year here and Dr. John Loraine, Director of the Hormone Research Laboratory at the University of Edinburgh, an international expert on hormone physiology and biochemistry who will deliver a graduate course.

Lawrence's research activities include a study of the mechanisms in hemopoeises in normal individuals and in disease, clinical applications of high energy particle beams to various forms of disease and the mechanism of this action, and the study of radiation action on ascites tumors in laboratory animals. In the first field we should mention reference 2 and 7 concerning a humoral factor "erythropoietin". Working with Van Dyke, it was recognized for the first time that human patients with aplastic anemia carry a great deal of the substance erythropoietin in their urine. The first preparations were made here and the laboratory is a center for studies of the mode of action and nature of erythropoietin. In reference 7 the substance was

administered to humans. With Winchell (13), Lawrence applied an isotope technique, the use of radio Yttrium for selective irradiation of lymphatic tissues prior to bone marrow transfusion.

John Lawrence is heading the medical program which is concerned with the therapeutic assessment of high energy protons and alpha particles. So far the following disease classes have been tested: metastatic mammary carcinoma, malignant diabetes mellitus, malignant exophthalamus, acromegaly and Cushing's disease, (references 9,10,11,16,17 and 20). Malignant diabetes mellitus is an advanced form of this disease which results in blindness due to retinal hemorrhages and eventual death from nephrosis of the kidney. Results achieved with the high energy beams match favorably with those from any other technique. A relatively low dose irradiation results in reduced requirements of insulin and in some cases arrest of the retinal hemorrhages. This method opened up the field of hormonal control of insulin production to investigation. In acromegaly the method can be used even after surgery and other radiation methods fail and the results appear to be gratifying. In collaboration with Prof. Li the demonstration of abnormally high human growth hormone levels in acromegalic patients became possible. It was also demonstrated that pituitary irradiation causes a decrease in the circulating human growth hormone level (unpublished). In Cushing's disease and in malignant exopthalmus there are also good therapeutic results. In mammary cancer a number of dramatic regressions were found; however, it has been shown by Lawrence and others (reference 6) that testosterone is converted to estrogens even in hypophysectomized humans and metastatic lesions usually return even after prolonged periods with regression of the cancers. Reference 18 contains preliminary reports of a study of the effects of heavily ionizing radiations on neoplastic cells from mouse ascites leukemia. Lawrence is particularly interested in the so-called oxygen effect. Neoplastic tissues usually grow faster in the regions where the cells themselves are anoxic. Ordinary radiation is not as effective on anoxic cells as on oxygenated ones whereas in reference 18 Lawrence et. al. demonstrate that heavy ions affect the oxygenated and anerobic parts of ascites tumor cells to an approximately equal degree.

The recognition of the radiation methods developed here in Berkeley is apparent from the fact that during the last two years Harvard University and Cambridge have dedicated their cyclotron to similar work under a neurosurgeon, Prof. William Sweet and the University of Uppsala in Sweden under Prof. Svedberg is working on biomedical applications identical with our own.

Currently John Lawrence is president of the Harvard Medical Alumni Association, an example of the high regard in which his colleagues and alma mater hold him. In 1962 he received the Silver Cross of the Royal Order of the Phoenix in Greece and in 1963 the Medical of the Pasteur Institute. Both of these were given for his pioneering efforts to introduce the use of radioactive isotopes into basic medical research and for his studies on nuclear radiations. He is clearly one of the foremost internationally known representatives of the University.

I recommend this salary increase from Step IV to Step V.

Sincerely yours,

Cornelius A. Tobias
Vice-Chairman In Charge of
Medical Physics

RESIDENTS SERVING CALIFORNIA

Physician Serves On UC Board Of Regents

DR. JOHN LAWRENCE
...Helps education

By SUSAN SHOEMAKER
(Education Writer)

"It's probably the most powerful board you can imagine. It has complete responsibility for the governance of the university."

That's Dr. John Lawrence of Orinda speaking, and the board he's describing is the University of California's Board of Regents, of which he is a member.

One of the most recently appointed regents, Dr. Lawrence began his 16-year term in 1970, when he was named to the board by Governor Reagan. Although he had never met the governor at the time of his appointment, Dr. Lawrence has some theories as to why he was chosen.

"I'm a physician, and the governor thought it would be a good idea to have one on the board," he explains. "And he also knew I was outspoken and a person of integrity, with no axe to grind."

Dr. Lawrence says he accepted the appointment "not for the honor, but because I wanted to contribute something to the university. And I'm also interested in seeing the university strongly led by a great administration and faculty.

"It's also interesting being associated with this group of very able men," he added. "We have differences, but on important questions we come right up almost unanimously. We're commonly interested in the welfare of a great institution."

There has been some argument in recent years that the university may cease to be great if its funding does not increase, but according to Dr. Lawrence, the financial situation is not as desperate as some people think.

"With inflation, everyone's short of funds," he explains, "and there are so many things, like welfare and housing, that society needs to have done beside education. It's not whether you're for support, it's whether you can get it.

"As president, I suppose Charles Hitch has to take the view that we need more money, and I suppose we do, but belt tightening is not without some advantages," he says.

"I'm a realist, and I think sometimes Hitch is not. After all, the state has enormous financial problems. We can't do everything we want to do, but it is terribly important that the university get enough support that it doesn't go downhill.

"I think we're still quite competitive and still perhaps the leading state university," he added. "Even though we're tight, we're still viable, and we're hoping time will get better. We've still got a great university."

Much of the criticism on funding has been directed at the governor, but Dr. Lawrence says Reagan is "one of the most able, solid, respected and articulate regents.

"The press seems to give the idea that the governor is against the university, and this isn't true," Dr. Lawrence stresses. "He's a great believer in the university and a supporter of education, but he has the whole state to consider."

The Board of Regents most important job, in Dr. Lawrence's opinion, is selecting the university president and chancellors for the nine UC campuses. "The board should get advice from the faculty, but make the final decision themselves," he says. "After all, the actual running of the university is really in the hands of the president and chancellors."

But in many other areas, he says, the board has delegated a great deal of its authority. Speaking again of the regents' great potential power, he says, "It could be a very dictatorial board, but it's not. Over many years it has delegated more and more of its responsibility to the administration and faculty. It's only right that the faculty should have control over courses and the teaching of courses," he explains.

Although he has only been a regent for two years, Dr. Lawrence has been associated with the university for many more years. After graduating from Harvard Medical School and teaching for a while at Yale Medical School, he came to California and joined the faculty at Berkeley.

He was a prime mover in raising funds to build Cal's Donner Laboratory and develop there the new field of nuclear medicine, based on the use of atomic energy products in research, diagnosis and treatment of disease.

When his own department began to feel the effects of the tightened belt, Dr. Lawrence gave up his paid professorship so a new man could be hired, but he says with a grin that he's "still doing the same work I used to do," including research, advising students, and some teaching.

Dr. Lawrence admits that it takes a while to learn the job of regent, but adds that his long association with the university made this task considerably easier. He notes that the regents meet nine months a year for a day and a half per month, which he says is not much demand on his time. And in addition to his work at the university, he travels regularly around the world attending meetings and lecturing on nuclear medicine.

A longtime resident of Orinda, Dr. Lawrence says that when he had more free time he enjoyed horseback riding near his home, and was involved in some Orinda school district activities.

He is the father of four children, three grown and one still attending Orinda's Glorietta Elementary School.

Dr. John H. Lawrence graduated from the University of South Dakota and the Harvard University Medical School. After training for four years at the Peter Bent Bingham Hospital in Boston, the Strong Memorial Hospital in Rochester, New York and the New Haven Hospital, he joined the Faculty in Internal Medicine at Yale University. His early work after graduation from Harvard was in the laboratory of Dr. Harvey Cushing on the Pituitary Gland, and where he studied many patients with Cushings Disease and Acromegaly (Pituitary Tumor) At Yale he began working on the Biologic effects of Radiation and studied the induction of leukemia in mice receiving relatively large doses of radiation. He also discovered the first (drug or chemical) estrogen which gave animals some protection from radiation damage. In Boston and at Yale he began having much experience in the use of conventional radiation in treating various pituitary conditions with limited success.

After three years on the Faculty at Yale - in teaching and research - he worked half time at Yale and half time in Berkeley at the Radiation Laboratory doing pioneer work on the first use of radio-active isotopes and on the Biologic effect of heavy ions(neutrons), laying the basis for the first radiation protection program in the atomic age. During this period and also later he was in close association with his physicist brother, Ernest O. Lawrence.

He soon developed a group of associates in the Donner Laboratory (which he founded) working with isotopes and the new radiations and as an outgrowth of the work their developed an academic division of the University - the Division of Medical Physics.

Among his other contributions are the training of dozens of medical scientists now distributed throughout the world; the successful treatment of P.Vera, the first condidtn to be successfully treated with radioactivity; with his associates the discovery of the anesthetic properties of the noble gas Xenon; many contributions to the measurement of red cell production and destruction aided by isotopes; and the treatment successfully of two pituitary diseases, Cushings Disease and Acromegaly.

He is now active as Professor of Medical Physics Emeritus; Director Emeritus of the Donner Laboratory; in Reseach with Heavy Particles; participates in Medical and Scientific Meetings in and out of the U.S.A.; also is a member of the Board of Regents of the University of California.

JOHN H. LAWRENCE, M.D.

List of Published Papers 1976

32- 1 Lawrence, John H. and Dial, Donald E. (introduced by Harvey Cushing): Effects of intraventricular injections of pitutrin and pilocarpine in dogs.
Proc. Soc. Exptl. Biol. Med. 30: 49-54, 1932.

34- 1 Lawrence, Joh H. and Strauss, Maurice J.: Dermatitis due to potassium mercuric iodide.
Arch. Dermatology and Syphilology 30: 76-79, 1934.

35- 1 Lawrence, John H. and Zimmerman, H.M.: Pituitary basophilism report of a case.
Arch. Internal Med. 55: 745-759, 1935.

35- 2 Lawrence, John H. The clinical symptoms and signs of dissecting aneurysm of the aorta; with report of a case diagnosed during life.
International Clinics 2: 122-133, 1935.

36- 1 Lawrence, John H. and Lawrence, Ernest O.: The biological action of neutron rays.
Proc. Natl. Academy of Sciences 22: 124-133, 1936.

36- 2. Lawrence, John H., Aebersold, Paul C., and Lawrence, Ernest O.: Comparative effects of X rays and neutrons on normal and tumor tissue.
Proc. Natl. Academy of Sciences 22: 543-557, 1936.

37- 1 Lawrence, John H.: Artificial radioactivity and neutron rays in biology and medicine.
Yale J. Biol. and Med. 9: 429-435, 1937.

37- 2 Lawrence, John H., Aebersold, Paul C., and Lawrence, Ernest O. The comparative effects of neutrons and X rays on normal and neoplastic tissue.
Amer. Assoc. Advancement of Science, Occasional Publications No. 4, pp 215-219, June, 1937.

37- 3 Lawrence, John H., Nelson, Warren O., and Wilson, Hugh. Roentgen irradiation of the hypophysis.
Radiology 29: 446-454, 1937.

37- 4 Lawrence, John H. and Tennant, Robert: The comparative effects of neutrons and X rays on the whole body.
J. Exptl. Med. 66: 667-688, 1937.

37- 5 Lawrence, John H., Horn, Robert and Strong, L.C.: Radiation studies on a mammary carcinoma of mice.
Yale J. Biol. and Med. 10: 145-154, 1937.

38- 1 Lawrence, John H.: Artificial radioactivity and neutron rays in biology and medicine.
pp 1-18 in Handbook of Physical Therapy, A.M.A. Council on Physical Therapy, Chicago, Illinois, 1938.

38- 2 Lawrence, John H. and Gardner, William U. A transmissible leukemia in the "A" strain of mice.
Amer. J. Cancer 33: 112-119, 1938.

39- 1 Jones, Hardin B., Chaikoff, I.L., and Lawrence, J.H. Radioactive phosphorus as an indicator of phospholipid metabolism. VI. The phospholipid metabolism of neoplastic tissues (mammary carcinoma, lymphoma, lymphosarcoma, sarcoma 180).
J. Biol. Chem. 128: 631-644, 1939.

39- 2 Lawrence, John H. and Scott, Kenneth G.: Comparative metabolism of phosphorus in normal and lymphomatous animals.
Proc. Soc. Exptl. Biol. and Med. 40: 694-696, 1939.

39- 3 Tuttle, Lawrence W., Scott, K.G., and Lawrence, J.H.: Phosphorus metabolism in leukemic blood.
Proceedings Soc. Exptl. Biol. Med. 41: 20-25, 1939. notes

39- 4 Anderson, Evelyn, Lawrence, John H., Joseph, Michael, and Aebersold, Paul C.: Increased sensitivity of hypophysectomized rats to radiation.
Science 89(2321): 588-589, 1939.

39- 5 Lawrence, John H.: ARtificial radioactivity and neutron rays in medicine.
Pacific Coast Med. 6: 32-33, 1939. notes

39- 6 Lawrence, John H., Scott, K.G., and Tuttle, L.W.: STudies on leukemia with the aid of radioactive phosphorus. notes
New Interntl. Clinics 3 (series 2): 33-58, 1939 (Lippincott, New York).

40- 1 Lawrence, John H. Some biological applications of neutrons and artificial radioactivity.
Nature 145 (3665): 125-127, 1940.

40- 2 Lawrence, John H.: Communication to Chauncey D. Leake, January 8, 1940.
Annals Hist. Med., October 1940. re last visit to Washing

40- 3 Lawrence, John H., Tuttle, L.W., Scott, K.G., and Connor, C.L.: Studies on neoplasms with the aid of radioactive phosphorus. I. The total phosphorus metabolism of normal and leukemic mice.
J. Clin. Invest. 19: 267-271, 1940.

40- 4 Jones, H.B., Chaikoff, I.L., and Lawrence, J.H.: Radioactive phosphorus as an indicator of phospholipid metabolism. X. The phospholipid turnover of fraternal tumors. J. Biol. Chem. 133: 319-327, 1940.

40- 5 Lawrence, J.H. Nuclear physics and therapy; preliminary report on a new method for the treatment of leukemia and polycythemia. Radiology 35: 51-59, 1940.

40- 6 Stone, Robert S., Lawrence, John H., and Aebersold, Paul D.: A preliminary report on the use of fast neutrons in the treatment of malignant disease. Radiology 35: 322-327, 1940.

40- 7 Jones, H.B., Chaikoff, I.L., and Lawrence, J.H.: Phosphorus metabolism of the soft tissues of the normal mouse as indicated by radioactive phosphorus. Amer. J. Cancer 40: 235-242, 1940.

40- 8 Jones, H.B., Chaikoff, I.L., and Lawrence, J.H. Phosphorus metabolism of neoplastic tissues (mammary carcinoma, lymphoma, lymphosarcoma) as indicated by radioactive phosphorus. Amer. J. Cancer 40: 243-250, 1940.

41- 1 Tuttle, L.W., Erf, L.A. and Lawrence, J.H.: Studies on neoplasms with the aid of radioactive phosphorus. II. The phosphorus metabolism of nucleoprotein, phospholipid and acid soluble fractions of normal and leukemic mice. J. Clin. Invest. 20: 57-61, 1941.

41- 2 Erf, L.A. and Lawrence, J.H.: Phosphorus metabolism in neoplastic tissue. Proc. Soc. Exptl. Biol. Med. 46: 694-695, 1941.

41- 3 Scott, K.G. and Lawrence, J.H. Effect of radiophosphorus on blood of monkeys. Proc. Soc. Exptl. Biol. Med. 48: 155-158, 1941.

41- 4 Lawrence, J.H.: Medical applications of neutron rays and artificial radioactivity. pp 12-23 (Chapt. 2) in Practitioners Library of Medicine, George Blumer, Editor, Yale Univ. Press, New Haven, 1941.

A41- 5 Lawrence, J.H., Erf, L.A., and Tuttle, L.W.: Intracellular irradiation. Abstracts from the Conference on Applied Nuclear Physics, Cambridge, Mass., Oct. 28 to Nov. 2, 1940. J. Applied Physics 12: 333-334, 1941. (Abstract)

A41- 6 Lawrence, J.H., Hamilton, J.G., Erf, L.A., and Pecher, C.: Recent advances in clinical medicine with the aid of artificially prepared radioactive isotopes. Proceedings of the thirty-third annual meeting, Society Clin. Invest. J. Clin. Invest. 20: 436, 1941. (Abstract)

41- 7 Axelrod, Dorothy, Aebersold, Paul C., and Lawrence, John H.: Comparative effects of neutrons and X rays on three tumors irradiated in vitro.
Proc. Soc. Exptl. Biol. Med. 48: 251-256, 1941.

41- 8 Erf, L.A. and Lawrence, J.H.: Clinical studies with the aid of radiophosphorus. III. The absorption and distribution of radiophosphorus in the blood of, its excretion by, and its therapeutic effect on, patients with polycythemia.
Ann. Internal Med. 15: 276-290, 1941.

41- 9 Erf, L.A., Tuttle, L.W. and Lawrence, J.H.: Clinical studies with the aid of radiophosphorus. IV. The retention in blood, the excretion and the therapeutic effect of radiophosphorus on patients with leukemia.
Ann. Internal Med. 15: 487-543, 1941.

41-10 Erf, L.A. and Lawrence, J.H. Clinical studies with the aid of radiophosphorus. I. The absorption and distribution of radiophosphorus in the blood and its excretion by normal individuals and patients with leukemia.
J. Clin. Invest. 20: 567-575, 1941.

41-11 Tuttle, L.A., Erf, L.A., and Lawrence, J.H.: Studies on neoplasms with the aid of radioactive phosphorus. III. The phosphorus metabolism of the phospholipid, acid soluble and nucleoprotein fractions of various tissues of normal and leukemic mice following the administration of "tracer" and "therapeutic" doses of radiophosphorus.
J. Clin. Invest. 20: 577-581, 1941.

42- 1 Lawrence, John H.: Observations on the Nature and treatment of leukemia and allied diseases.
Edwin R. Kretschmer Memorial Lecture, Institute of Medicine, Chicago, November 11, 1941.
Institute of Med. of Chicago, Proceedings 14: 39-49, 1942.

42- 2 Aebersold, Paul C. and Lawrence, John H.: The physiological effects of neutron rays.
pp 25-48 in Annual Review of Physiology IV, Annual Reviews, Inc., Stanford, California, 1942.

42- 3 Lawrence, John H.: The new nuclear physics and medicine.
The Caldwell Lecture, American Roentgen Ray Society, Cincinnati, September 1941.
Amer. J. Roentgenol., RAd.Ther. 48: 283-301, 1942.

42- 4 Newell, R.R., Falconer, Ernest H., Hill, H.P., Lawrence, J.H. Wood, David A., and Wyckoff, Harry: Panel discussion on the leukemias and lymphoblastomas.
Radiology 39: 298-305, 1942.

42- 5 Treadwell, Anne G., Low-Beer, B.V.A., Friedell, H.L., and Lawrence, J.H.: Metabolic studies on neoplasm of bone with the aid of radioactive strontium.
Amer. J. Med. Sciences 204: 521-530, 1942.

A42- 6 Hamilton, Joseph G. and Lawrence, John H.: Recent clinical developments in the therapeutic application of radiophosphorus and radioiodine. Proceedings of the thirty-fourth annual meeting, Society of Clinical Investigation.
J. Clin. Invest. 21: 624, 1942. (Abstract)

42- 7 Low-Beer, Bertram V.A., Lawrence, John H., and Stone, Robert S.: The therapeutic use of artificially produced radioactive substances; radiophosphorus, radiostrontium, radioiodine, with special reference to leukemia and allied diseases.
Radiology 39: 573-579, 1942.

R42- 8 Jones, H.B., Smith, R., Sears, N., Wu, C., Larkin, J., French, R., Hamilton, J.G., and Lawrence, J.H. The uptake of inspired radioactive argon: a method of determining the efficiency of inert gas exchange in man.
Committee on Aviation Medicine (CAM) Report 51, May 29, 1942.

43- 1 Treadwell, Anne G., Gardner, W.U., and Lawrence, John H.: Effect of combining estrogen with lethal doses of Roentgen-ray in Swiss mice.
Endocrinology 32: 161-164, 1943.

44- 1 Lawrence, J.H.: Aviation medical problems, with special reference to altitude pain.
Naffizer Number of the J. Nervous and Mental Disease 99: 703-711, 1944.

44- 2 Cook, Sherburne F., Williams, O.L., Lyons, W.R., and Lawrence, John H.: A comparison of altitude and exercise with respect to decompression sickness.
War Medicine 6: 182-187, 1944.

44- 3 Bridge, Ezra V., Henry, Franklin M., Cook, Sherburne F., Williams, Owen L., Lyons, William R., and Lawrence, John H. Decompression sickness: nature and incidence of symptoms during and after artificial decompression to 38,000 feet for ninety minutes with exercise during exposure.
J. Aviation Med. 15: 316-327, 1944.

44- 4 Tobias, Cornelius A., Lyons, William R., Williams, Owen L., Bridge, Ezra V., Lawrence, John H., Helmholz, H.F., Jr., and Sweeney, A.R., Jr.: Study of bends; a comparison in low pressure chamber and actual flight.
Air Surgeon's Bull. 1: 9-10, 1944.

44- 5 Henry, F.M., Lawrence, J.H., Bridge, E.V., and Williams, O.L.: Protective effects of pre-oxygenation on abdominal gas pain; results of a study of preflight breathing of oxygen on pain resulting from decompression to 38,000 feet.
War Medicine 6: 395-397, 1944.

45- 1 Bridge, E.V., Henry, F.M., Williams, O.L., and Lawrence, J.H. "Chokes"; a respiratory manifestation of aeroembolism in high altitude flying.
Ann. Internal Med. 22: 398-407, 1945.

45- 2 Lawrence, John H. and Lund, Douglas W.; Two severe altitude flight reactions.
United States Navy Department BUMed News Letter, Aviation Supplement 4(7): 3-5, 1945.

45- 3 Bridge, E.V., Henry, F.M., and Lawrence, J.H.: Observations of multiple joint pains in the extremities at 38,000 feet.
J. Nervous and Mental Disease 101: 583-588, 1945.

45- 4 Lawrence, John H.: Communication to the Editor, August 17,1945.
J. Amer. Med. Assoc. 129: 91, 1945.

45- 5 Lawrence, J.H., Tobias, C.A., Lyons, W.R., Helmholz, H.F.,Jr., and Sweeney, A.R.,Jr., (with the flight assistance of Bridge, E.V. and Williams, O.L.): A study of aeromedical problems in a Liberatof bomber at high altitude.
J. Aviation Med. 16: 286-310, 1945.

45- 6 Lawrence, J.H. and Tobias, C.A. Is CO oxidized to CO_2 in the human body?
Air Surgeons' Bull.2: 452, 1945.

45- 7 Tobias, C.A., Lawrence, J.H., Roughton, F.J.W., Root, W.S., and Gregersen, M.I. The elimination of carbon monoxide from the human body with reference to the possible conversion of CO to CO_2.
Amer. J. Physiol. 145: 253-263, 1945.

46- 1 Graff, W.S., Scott, K.G., and Lawrence, John H. The histologic effects of radiophosphorus on normal and lymphomatous mice.
Amer. J. Roentgenol. Radium Ther. 55: 44-54, 1946.

46- 2 Lund, D.W., Lawrence, J.H., and Lawrence, L.B.: Latent neurologic manifestations following decompression; report of a case of severe reaction following ascent to 38,000 feet.
Occupational Med. 1: 75-80, 1946.

46- 3 Lawrence, J.H., Loomis, W.F., Tobias, C.A., and Turpin, F.H. Preliminary observations on the narcotic effect of xenon with a review of values for solubilities of gases in water and oils.
J. of Physiol. (British) 105: 197-204, 1946.

47- 1 Lawrence, John H.: Radioactive isotopes in medicine.
Reprint of one of a series of broadcasts, "The Doctors Talk it Over", sponsored by Lederle Laboratories Division, American Cyanimid Company, February 17, 1947.

47- 2 Lawrence, John H.: The use of isotopes in medical research.
J. Amer. Med. Assoc. 134: 219-225, 1947.

47- 3 Tobias, C.A., Loomis, W.F., and Lawrence, J.H. Studies on skin temperature and circulation in decompression sickness.
Amer. J. Physiol. 149: 626-633, 1947.

47- 4 Lawrence, J.H. Tracer and therapeutic studies with isotopes.
Chicago Medical Society Bull. 50: 93-98, 1947.

48- 1 Lawrence, J.H. Atomic energy in medicine.
J. Med. and Pharm., South Dakota, January 11-18, 1948.

48- 2 Dobson, R.Lowry and Lawrence, J.H.: Physiological effects of radiant energy.
pp 479-500 in Annual Review of Physiology, Vol. X. Annual Reviews, Inc., Stanford, California, 1948.

48- 3 Dougherty, Ellsworth C. and Lawrence, John H. Heavy and radioactive isotopes in clinical and experimental medicine.
pp 1-43 (Chapter 1) in Advances in Biological and Medical Physics, Vol. 1, J.H. Lawrence and J.G. Hamilton, editors, Academic Press, New York, 1948.

48- 4 Lawrence, John H., Dobson, R. Lowry, Low-Beer, Bertram V.A., and Brown, Bruce R.: Chronic myelogenous leukemia; a study of 129 cases in which treatment was with radioactive phosphorus.
J. Amer. Med. Assoc. 136: 672-677, 1948.

48- 5 Lawrence, John H.: Constructive and destructive aspects of atomic energy.
The Harrison Martland Lecture, Essex County Anatomical and Pathological Society, New Jersey, 1947.
J. Med. Soc. New Jersey 45, 337-340, 1948.

48- 6 Dougherty, E.C. and Lawrence, J.H.: Isotopes in clinical and experimental medicine.
California Med. 69: 58-73; 148-153, 1948.

48- 7 Lawrence, John H.: Isotopes in neoplastic disease.
Rocky Mountain Medical Journal 45: 980-984, 1948.

48- 8 Lawrence, John H.: Some tracer and therapeutic studies with artificial radioactivity.
Sir James Mackenzie Davidson Memorial Lecture, British Institute of Radiology, London, July 17, 1947.
Brit. J. Radiology 21: 531-543, 1948.

49- 1 Wasserman, L.R., Dobson, R.L., and Lawrence, J.H.: Blood oxygen studies in patients with polycythemia and in normal subjects.
J. Clin. Invest. 28: 60-65, 1949.

49- 2 Moffitt, Herbert C.,Jr., and Lawrence, John H.: Chronic leukemia of long duration, with a report of 31 cases with a duration of over five years.
Ann. Internal Med. 30: 778-790, 1949.

49- 3 Lawrence, J.H., Low-Beer, B.V.A., and Carpender, James W.J.: Chronic lymphatic leukemia; a study of 100 patients treated with radioactive phosphorus.
J. Amer. Med. Assoc. 140: 585-588, 1949.

49- 4 Lawrence, J.H. and Rosenthal, R.L.: Multiple myeloma associated with polycythemia report of four cases.
Amer. J. Med. Sciences 218: 149-154, 1949.

49- 5 Lawrence, J.H.: The control of polycythemia by marrow inhibition a ten-year study on 172 patients.
J. Amer. Med. Assoc. 141: 13-18, 1949.

49- 6 Lawrence, John H. Foreword.
in Isotopic Tracers and Nuclear Radiations with Applications to Biology and Medicine, McGraw-Hill, New York, 1949.

49- 7 Tobias, C.A., Jones, H.B., Lawrence, J.H., and Hamilton, J.G. The uptake and elimination of krypton and other inert gases by the human body.
J. Clin. Invest. 28: 1375-1385, 1949.

A49- 8 Huff, Rex L., Hennessy, Thomas G., and Lawrence, John H.: Iron metabolism studies in normal subjects and in patients having blood dyscrasias. Proceedings for Forty-first Annual Meeting American Society for Clinical Investigation, May 1949.
J. Clin. Invest. 28: 790, 1949. (Abstract)

50- 1 Lawrence, John H. Isotopos en medicina.
Anales de la Facultad de Medicina, Lima, Peru, 33: 1-24, 1950.

50- 2 Lawrence, J.H. and Wasserman, L.R.: Multiple myeloma; a study of 24 patients treated with radioactive isotopes (P^{32} and Sr^{89}).
Ann. Internal Med. 33: 41-55, 1950.

50- 3 Huff, Rex L., Bethard, W.F., Garcia, J.F., Roberts, B.M., Jacobson, L.O., and Lawrence, J.H.: Tracer iron distribution studies in irradiated rats with lead-shielded spleens.
J. Clin. Invest. 36: 40-51, 1950.

50- 4 Huff, R.L., Hennessy, T.G., Austin, R.E., Garcia, J.F., Roberts, B.M., and Lawrence, J.H.: Plasma and red cell iron turnover in patients having various hematopoietic disorders.
J. Clin. Invest. 29: 1041-1052, 1950.

50- 5 Berlin, Nathaniel I., Lawrence, John H., and Gartland, Jean : The blood volume in chronic leukemia as determined by P^{32}-labeled red blood cells.
J. Lab. and Clin. Med. 36: 435-439, 1950.

50- 6 Lawrence, John H.: The clinical use of radioactive isotopes.
The Ludwig Kast Memorial Lecture, New York City, October 10,1949.
New York Academy of Medicine 20: 639-669, 1950.

50- 7 Lawrence, John H. and Goetsch, Anne T.: Familial occurrence of polycythemia and leukemia.
California Medicine 73: 361-363, 1950.

50- 8 Berlin, N.I., Lawrence, J.H., and Gartland, J.: Blood volume in polycythemia as determined by P^{32}-labeled red blood cells.
Amer. J. Med. 9: 747-751, 1950.

51- 1 Berlin, N.I., Tolbert, B.M., and Lawrence, J.H. STudies in glycine-2-C^{14} metabolism in man. I. The pulmonary excretion of $C^{14}O_2$.
J. Clin. Invest. 30: 73-76, 1951.

51- 2 Berlin, N.I., Hyde, Grace M., Parsons, Robert J., Lawrence, John H. and Port, Shirley: Blood volume of the normal female as determined with P^{32}-labeled red blood cells.
Proc. Soc. Exptl. Biol. Med. 76: 831-832, 1951.

51- 3 DeBacker, Jean and Lawrence, John H.: L'Association de la leucemie à la polycythemia vraie.
La Press Medicale 59: 461-463, 1951.

51- 4 Moffit, Herbert C., Jr., Lawrence, J.H., and Berlin, N.I.: Polycythemie vraie aspects cliniques et experimentaux diagnostic differentiel.
Revue d'Hematologie 6: 463-469, 1951.

51- 5 Lawrence, John H.: The medical use of radioactive isotopes. Amer. J. Roentgenol. Radium Ther. 65: 953-954, 1951.

51- 6 Berlin, N.I., Rowles, Donald F., Hyde, Grace M., Parsons, Robert J., Lawrence, John H., and Port, Shirley: Blood volume in pulmonary tuberculosis. A.M.A. Archives Internal Med. 88: 17-19, 1951.

51- 7 Berlin, N.I. and Lawrence, J.H.: The changes in the bone marrow differential in chronic leukemia treated with P^{32} and Y^{90}. Acta Medica Scandinavica 140: 99-118, 1951.

51- 8 Lawrence, John H.: Therapeutic uses of isotopes. General Practice (GP) 4: 65-71, 1951.

51- 9 Huff, Rex L., Lawrence, John H., Siri, William E., Wasserman, Louis R., and Hennessy, Thomas G.: Effects of changes in altitude on hematopoietic activity. Medicine 30: 197-217, 1951.

51-10 Lawrence, John H.: Therapy with radioactive isotopes. J. Kansas Med. Soc., Cancer Supplement 52(9): 7a-9a, 1951.

51-11 Lawrence, John H.: Tracer research and cancer. J. Kansas Med. Soc., Cancer Supplement 52(9):31a-34a, 1951.

51-12 Berlin, N.I., Lawrence, J.H., and Lee, Helen C.: The life span of the red blood cell in chronic leukemia and polycythemia. Science 114: 385-387, 1951.

51-13 Huff, Rex L., Elmlinger, Paul J., Garcia, Joseph F., Oda, J.M., Cockrell, M.C., and Lawrence, J.H. Ferrokinetics in normal persons and in patients having various erythropoietic disorders. J. Clin. Invest. 30: 1512-1526, 1951.

51-14 Prentice, Theodore C., Berlin, N.I., Hyde, Grace M., Parsons, Robert J., Lawrence, John H., and Port, Shirley: Total red cell volume, plasma volume, and sodium space in congestive heart failure. J. Clin. Invest. 30: 1471-1482, 1951.

52- 1 Lawrence, John H. Polycythemia vera; method of John H. Lawrence, M.D.
pp 228-230 in Current Therapy; H.F. Conn, editor; W.B. Saunders & Co., Philadelphia, 1952.

52- 2 Rosahn, Paul D., Tobias, Cornelius A., and Lawrence, John H.: Effects on the white mouse of a single whole-body exposure of 190-MeV deuterons.
Amer. J. Path. 28: 37-49, 1952.

52- 3 Tobias, C.A., Anger, H.O., and Lawrence, J.H.: Radiological use of high energy deuterons and alpha particles.
Amer. J. Roentgenol.,Radium Ther, Nucl. Med. 67: 1-25, 1952.

52- 4 Berlin, N.I., Hyde, G.M., Lawrence, J.H., Parsons, R.J., and Port, S.: The blood volume in pre-eclampsia as determined with P^{32}-labeled red blood cells.
Surgery, Gynecology and Obstetrics 94: 21-22, 1952.

52- 5 Lawrence, J.H., Huff, R.L., Siri, W.E., Wasserman, L.R., and Hennessy, T.G.: A physiological study in the Peruvian Andes.
Acta Medica Scandinavica 142: 117-131, 1952.

52- 6 Huff, R.L., Tobias, C.A., and Lawrence, J.H. A test for red cell production.
Acta Haematologica 7: 129-142, 1952.

52- 7 Hyde, G.M., Berlin, N.I., Parsons, R.J., Lawrence, J.H., and Port, S.: Blood volume in portal cirrhosis as determined by P^{32}-labeled red blood cells.
J. Lab. Clin. Med. 39: 347-353, 1952.

52- 8 Prentice, T.C., Berlin, N.I. and Lawrence, J.H.: Effect of therapy on blood volume, blood pressure and spleen size in polycythemia vera.
A.M.A. Arch. Internal Med. 89: 584-590, 1952.

52- 9 Prentice, T.C., Siri, W.E., Berlin, N.I., Hyde, G.M., Parsons, R.J., Joiner, E.E., and Lawrence, J.H. Studies of total body water with tritium.
J. Clin. Invest. 31:412-418, 1952.

52-10 Lawrence, J.H. and Berlin, N.I.: Relative polycythemia -- the polycythemia of stress.
Yale J. Biol. Med. 24: 498-505, 1952.

52-11 Wasserman, L.R., Lawrence, J.H., Berlin, N.I., Dobson, R.L., and Estren, S.: The bone marrow picture in polycythemia vera before and after treatment with radioactive phosphorus.
Acta Medica Scandinavica 143: 442-449, 1952.

52-12 Berlin, N.I., Hyde, G.M., Parsons, R.J., and Lawrence, J.H.: The blood volume in various medical and surgical conditions. New Engl. J. Med. 247: 675-684, 1952.

52-13 Lawrence, J.H., Elmlinger, P.J., and Fulton, G.: Oxygen and the control of red cell production in primary and secondary polycythemia; effects on the iron turnover patterns with Fe^{59} as tracer.
Cardiologia 21: 337-346, 1952.

53- 1 Lawrence, John H. Polycythemia vera; method of John H. Lawrence, M.D. pp 235-238, in Current Therapy; H.F. Conn, editor; W.B. Saunders & Co., Philadelphia, 1953.

53- 2 Picon-Reategui, E., Fryers, G.R., Berlin, N.I., and Lawrence, J.H.: Effect of reducing the atmospheric pressure on body water content of rats.
Amer. J. Physiol. 172: 33-36, 1953.

53- 3 Elmlinger, Paul J., Huff, Rex L., Tobias, C.A., and Lawrence, J.H.: Iron turnover abnormalities in patients having anemia; serial blood and in vivo tissue studies with Fe^{59}.
Acta Haematologica 9: 73-96, 1953.

53- 4 Lawrence, J.H., Berlin, N.I., and Huff, R.L. The nature and treatment of polycythemia: studies on 263 patients.
Medicine 32: 323-388, 1953.

53- 5 Lawrence, John H.: Life in the Andes, where the air is thin.
California Monthly 44: 9-10; 38-41, October 1953.

54- 1 Lawrence, John H.: Polycythemia vera; method of John H. Lawrence, M.D.
pp 260-263 in Current Therapy; H.F. Conn, Editor; W.B. Saunders & Co., Philadelphia, 1954.

54- 2 Lawrence, John H.: The treatment of chronic leukemia.
Medical Clinics of North America 38: 525-540, 1954.

54- 3 Lawrence, John H.: Statement of Dr. John Hundale Lawrence, Director, Donner Laboratory, University of California at Berkeley.
Hearings before the Sub-committee on Research and Development of the Joint Committee on Atomic Energy, Congress of the United States.
The 83rd Congress, 2nd Session on the Contribution of Atomic Energy to Medicine, June 2,3,4, 1954. (pp 8-26).

54- 4 Contopoulos, Alexander N., Van Dyke, Donald C., Ellis, S., Simpson, Miriam E., Lawrence, John H. and Evans, H.M.: Evidence for a pituitary erythropoietic factor. Gazeta Medica Portuguesa 7: 99-104, 1954.

54- 5 Van Dyke, Donald C., Contopoulos, Alexander N., Williams, B.S., Simpson,M.E., Lawrence, John H., and Evans, H.M.: Hormonal factors influencing erythropoiesis. Acta Haematologica 11: 203-222, 1954.

54- 6 Contopoulos, A.N., Van Dyke, D.C., Simpson, M.E., Lawrence, J.H., and Evans, H.M.: Failure of newborn rat to respond to hypoxia with increased erythropoiesis. Proc. Soc. Exptl. Biol. Med. 86: 713-715, 1954.

54- 7 Lawrence, John H.: My visit to Aloysius Cardinal Stepinac. Talk delivered at Baccalaureate Ceremonies, Newman Hall, Berkeley, California, June 17, 1954.

54- 8 Contopoulos, A.N., Simpson, M.E., Van Dyke, D.C., Lawrence, J.H., and Evans, H.M.: A comparison of the anemia produced by hypophysectomy with that resulting from combined thyroidectomy, adrenalectomy and gonadectomy. Endocrinology 55: 509-515, 1954.

54- 9 Reynafarje, C., Berlin, N.I., and Lawrence, J.H.: Red cell life span in acclimatization to altitude. Proc. Soc. Exptl. Biol. Med. 87: 101-102, 1954.

54-10 Berlin, N.I., Reynafarje, C., and Lawrence, J.H.: Red cell life span in the polycythemia of high altitude. J. Applied Physiol. 7: 271-272, 1954.

54-11 Siri, W.E., Reynafarje, C., Berlin, N.I., and Lawrence, J.H. Body water at sea level and at altitude. J. Applied Physiol. 7: 333-334, 1954.

54-12 Contopoulos, A.N., Ellis, S., Simpson, M.E., Lawrence, J.H., and Evans, H.M.: Production of polycythemia in hypophysectomized rats by the pituitary erythropoietic factor. Endocrinology 55: 808-812, 1954.

54-13 Berlin, N.I., Lawrence, J.H., and Lee, Helen C.: The pathogenesis of the anemia of chronic leukemia; measurement of the life span of the red blood cell with glycine-2-C^{14}. J. Lab. Clin. Med. 44: 860-874, 1954.

55- 1 Contopoulos, A.N., Van Dyke, D.C., Ellis, S., Simpson, M.E., Lawrence, J.H., and Evans, H.M.: Prevention of neonatal anemia in the rat by the pituitary erythropoietic factor. Blood 10: 115-119, 1955.

55- 2 Berlin, N.I., Hyde, G.M., Parsons, R.J., and Lawrence, J.H. The blood volume in cancer. Cancer 8: 796-802, 1955.

55- 3 Lawrence, John H., Rosenthal, N., Stickney, M., Wasserman, L.R., and Dameshek, W.: Panel in Therapy. iii. The treatment of polycythemia vera. Blood 10: 655-661, 1955.

55- 4 Lawrence, John H. Les isotopes radio-actifs en therapeutique hematologique. Medecine et Hygiene (Geneve) 3: 403-405, 1955.

56- 1 Lawrence, John H. and Tobias, Cornelius A.: Radioactive isotopes and nuclear radiations in the treatment of cancer. Cancer Research 16: 185-193, 1956.

56- 2 Lawrence, John H.: Polycythemia vera; method of John H. Lawrence, M.D. pp 170-173 in Current Therapy; H.F. Conn, editor; W.B. Saunders & Co., Philadelphia, 1956.

56- 3 Lawrence, John H. Early experiences in nuclear medicine. Northwest Medicine 55: 527-533, 1956.

56- 4 Julian, Logan M., Lawrence, John H., Berlin, Nathaniel I., and Hyde, Grace M.: Blood volume, body water, and body fat of the horse. J. Applied Physiol. 8: 651-653, 1956.

56- 5 Lawrence, John H.: Isotopes and nuclear radiations in experimental medicine. A.M.A. Archives Internal Med. 97: 680-693, 1956.

56- 6 Dobson, R.L. and Lawrence, J.H. Radioisotopes in hematology. pp 27-36 in V. Kongress der Europaischen Gesellschaft fur Hematologie in Freiburg i.B., September 20-24, 1955, Springer Verlag, Berlin, 1956.

56- 7 Rosenthal, Donald J., and Lawrence, John H.: The use of radioactive isotopes in medicine. Medical Clinics of North America 40: 1515-1543, 1956.

56- 8 Tolbert, Bertram M., Lawrence, John H., and Calvin, Melvin: Respiratory carbon^{14} patterns and physiologic state. pp 281-285 in Proceedings of the First International Conference on the Peaceful Uses of Atomic Energy, 1955. (Z/Conf.8/12) Vol. XII, United Nations, New York, 1956.

56- 9 Lawrence, John H.: Radioactive isotopes in hematologic therapy. pp 142-151 in Proceedings of the First International Conference on the Peaceful Uses of Atomic Energy, 1955. United Nations (Z/Conf.8/12) Vol. XII; United Nations, New York, 1956.

56-10 Berlin, N.I. and Lawrence, J.H.: Recent advances in methods for the study of red cell mass and red cell production and destruction.
pp 365-372, in Proceedings of the First International Conference on the Peaceful Uses of Atomic Energy, 1955; United Nations (A/Conf.8/10), Vol. X (Session 8C, P/183); United Nations, New York, 1956.

56-11 Tobias, C.A., Roberts, J.E., Lawrence, J.H., Low-Beer, B.V.A., Anger, H.O., Born, J.L., McCombs, R.K., and Huggins, Charles.: Irradiation hypophysectomy and related studies using 340-MeV protons and 190-MeV deuterons.
pp 95-109 in Proceedings of the First International Conference on the Peaceful Uses of Atomic Energy, 1955; United Nations (A/Conf.8/10), Vol. X (Session 8C,P/201); United Nations, New York, 1956.

56-12 Lawrence, John H.: Some studies of function and structure in medicine.
Stephen Walter Ranson Memorial Lecture, Phi Beta Pi Fraternity, Northwestern University Medical School, October 27, 1955. Quarterly Bull., Northwestern Univ. Med. Sch. 30:215-227, 1956.

57- 1 Lawrence, J.H. and Dal Santo, G.: Therapy of polycythemia vera with radioactive phosphorus.
Acta Med. Patavina 17: 460-470, 1957.

57- 2 Berlin, N.I., Lawrence, J.H., and Elmlinger, P.J.: Recent advances in the knowledge of total red cell volume, production and destruction.
Blood 12: 147-164, 1957.

57- 3 Masouredis, S.P. and Lawrence, J.H.: The problem of leukemia in polycythemia vera.
Amer. J. Med. Science 233: 268-274, 1957.

57- 4 Rosenthal, D.J. and Lawrence, J.H.: Radioactive isotopes in medicine.
pp 361-388 in Annual Review of Medicine, Vol. 8; D.A. Rytand & W. Creger, editors; Annual Reviews, Inc., Palo Alto, Calif. 1957.

57- 5 Contopoulos, A.N., McCombs, R.K., Lawrence, J.H., and Simpson, M.E.: Erythropoietic activity in the plasma of patients with polycythemia vera and secondary polycythemia.
Blood 12: 614-619, 1957.

57- 6 Lawrence, John H.: Proton irradiation of the pituitary.
Cancer 10: 795-798, 1957.

57- 7 Berlin, N.I., Beeckmans, M., Elmlinger, P.J., and Lawrence, J.H.: A comparative study of the Ashby differential agglutination, carbon^{14} and iron^{59} methods for the determination of red cell life span.
J. Lab. Clin. Med. 50: 558-569, 1957.

57- 8 VanDyke, D.C., Garcia, J.F., and Lawrence, J.H.: Concentration of highly potent erythropoietic activity from urine of anemic patients.
Proc. Soc. Exptl. Biol. Med. 96: 541-544, 1957.

57- 9 Lawrence, John H.: ATomic Medicine.
California Monthly 68: 16-21, December 1957.

58- 1 Lawrence, John H.: Polycythemia Vera; Method of John H. Lawrence, M.D.
pp 223-226 in Current Therapy; H.F. Conn, editor; W.B. Saunders & Co., 1958.

58- 2 Tobias, C.A., Lawrence, J.H., Born, J.L., McCombs, R.K., Roberts, J.E., Anger, H.O., Low-Beer, B.V.A., and Huggins, Charles B.: Pituitary irradiation with high-energy proton beams; a preliminary report.
Cancer Research 18: 121-134, 1958.

58- 3 Lawrence, John H. and Dal Santo, G.: Factors regulating red cell production (studies with radioisotopes).
Minerva Nucleare 2: 51-56, 1958.

58- 4 Lawrence, John H.: Low level irradiation.
Panel discussion, "Whould the United States Continue Nuclear Testing?, World AFfairs Council of Northern California, San Francisco, June 25, 1958.

58- 5 Lawrence, John H. and Donald, William G., Jr.: Giant follicular lymphoblastoma; its treatment with radioisotopes.
Ann. Internal Med. 49: 1-16, 1958.

58- 6 Rosenthal, D.J. and Lawrence, J.H. Radioisotopes in medicine.
pp 471-521, in Radiation Biology and Medicine; W. Claus, editor; Addison-Wesley, REading, Massachusetts, 1958.

58- 7 Lawrence, J.H., Van Dyke, D.C., Garcia, J.F., and Contopoulos, A.N.: Studies on the regulation of red cell production aided by isotopes.
pp 192-211, in proceedings of First UNESCO International Conference on Radioisotopes in Scientific Research, held in Paris Sept. 1957, Vol III, Pergamon Press, New York & Paris, 1958.

58- 8 Lawrence, J.H., Tobias, C.A., and Born, J.L.: Hypophysectomy for advanced breast cancer using high energy particle beams -- proton and alpha particles.
Third International Symposium on Radioactive Isotopes in Clinical Medicine and Research, Bad GAstein, Austria, 1958.
Sonderband Strahlentherapie 38: 245-261, 1958.
Radioaktive Isotope in Klinik und Forschung. III; Feilinger, K. und Vetter, H., editors; Urban und Schwarzenberg, Muenchen, 195

58- 9 Van Dyke, D.C., Garcia, J.F., and Lawrence, J.H.: Biological and chemical characteristics of urinary erythropoietin.
pp=79-84 in Proceedings of the Second United Nations International Conference on the Peaceful uses of Atomic Energy, P/1899, Session D-14, Geneva, Vol. 25, Pergamon Press, London, 1958.

59- 1 Parker, Howard G. and Lawrence, John H.: Iron metabolism and polycythemia.
pp 226-233 in Eisenstoffwechsel; Beiträge zur Forschung und Klinik; W. Keiderling, editor: George Thieme Verlag, Stuttgart, Germany, 1959.

59- 2 Lawrence, John H. and Donald, William G., Jr.: Polycythemia and hydronephrosis or renal tumors.
Ann. Internal Med. 50: 959-969, 1959.

59- 3 Lawrence, John H. and Donald, William G., Jr.: The incidence of cancer in chronic leukemia and in polycythemia vera.
Amer. J. Med. Sciences 237: 488-498, 1959.

59- 4 Lawrence, JohnH., Naets, Jean P., and Van Dyke, Donald C.: Physiological and clinical significance of erythropoietin as studied with isotopes.
Proceedings of the Fifteenth General Assembly of the Japan Medical Congress, Tokyo, April 1959.
Medicine of Japan in 1959 4: 177-179, 1959.

59- 5 Lawrence, J.H., Born, J.L., Tobias, C.A., Carlson, R.A., Sangalli, F., and Welch, Graeme: Clinical and metabolic studies in patients after alpha particle subtotal or total hypophysectomy.
Proceedings of the Fifteenth General Assembly of the Japan Medical Congress, Tokyo, April, 1959.
Medicine of Japan in 1959, 5: 859-862, 1959.

59- 6 Born, J.L., Anderson, A.O., Anger, H.O., Birge, A.C., Blanquet, P., Brustad, T., Carlson, R.A., VanDyke, D.C., Fluke, D.J., Garcia, J Henry, J.P., Knisely, R.M., Lawrence, J.H., Riggs, C.W., Thorell B., Tobias, C.A., Toch, P., and Welch, G.: Biological and medical studies with high-energy particle accelerators. (Geneva Conference Paper 1958, P/2377). pp 189-206 in Progress in Nuclear Energy, Series VII, Volume 2, Medical Sciences; J.C. Bugher, J.Coursaget, AND J.F. Loutit, Editors; Pergamon Press, Great Britain, 1959.

60- 1 Born, J.L., Toch, P., Tobias, C.A., Lawrence, J.H., Sangalli, F., and Carlson, R.A.: Proton or alpha particle hypophysectomy for breast cancer, acromegaly, and malignant diabetes. Proceedings of Ninth International Congress of Radiology, July 23-30, 1959, Muenchen, Germany. Transactions of Conference, pp 833-844; George Thieme Verlag, Stuttgart, Germany, 1960.

60- 2 McCombs, Rollin K., Carlson, Richard A., Steinkamp, Ruth C., and Lawrence, John H.: Ueber die Verwendung radioaktiver Isotope in der Haematologie. pp 47-111 in Handbuch der gesamten Haematologie, 2 Aufl., Bd. 3; L. Heilmeyer and A. Hittmair, editors; Urban and Schwarzenberg, Verlag, Muenchen and Berlin Germany, 1960.

60- 3 Pollycove, M. and Lawrence, J.H.: Agranulocytosis and leukopenia. Method of Myron Pollycove and JOhn H. Lawrence. pp 171-172 in Current Therapy; H.F. Conn, editor W.B. Saunders and Co., Philadelphia, Pennsylvania, 1960.

60- 4 Lawrence, J.H. and Parker, H.G.: Radiation safety and hazards in nuclear energy. pp 101-109 in proceedings of Inter-American Symposium on the Peaceful Application of Nuclear Energy, Second; Buenos Aires, June 1-5, 1959, Third Session, Radioisotopes and Radiation in the Life Sciences; 1960.

60- 5 Lawrence, J.H.: Atomic ray in medicine. Atomic Energy Guide Letter, Washington D.C. 6(33): 2-3, August 15, 1960.

60- 6 Dobson, R.L., Lawrence, J.H., Pollycove, M.: Isotopes in hematology including treatment. pp 530-537 in Il Pensiero Scientifico (Proceedings of the VIIth International Congress of the International Society of Hematology, Rome, September 7-13, 1958), 1960.

60- 7 Constable, J.D., Lawrence, J.H., Born, J.L., Tobias, C.A., Ariotti, P.E., Sangalli, F., Carlson, R.C., and Toch, P. Effect of alpha particle hypophysectomy on disseminated cancer of male breast. J. Amer. Med. Assoc. 174: 1720-1723, 1960.

60- 8 Lawrence, John H.: Medical and biological applications of atomic energy. Testimony before House of Representatives Committee on Appropriations.
pp 137-144 in proceedings of eighty-sixth Congress, 2nd session: Part 3. Atomic Energy Commission, Nuclear Propelled Aircraft program, 1960. United States Govt. Printing Office, Washington, D.C., 1960.

61- 1 Aggeler, Paul C., Pollycove, M., Hoag, Syllvja, Donald, W.G., Jr., and Lawrence, John H.: Polycythemia vera in childhood, STudies of iron kinetics with Fe^{59} and blood clotting factors.
Blood 17: 345-350, 1961.

61- 2 Van Dyke, D.C. and Lawrence, J.H.: The regulation of erythropoiesis as studied with isotopes: erythropoietin.
pp 697-705 in Kunstliche Radioaktive Isotope in Physiologie, Diagnostik und Therapie, (Zweite Auflage, Zweiter Band), 1960; H. Schwiegk and F. Turba, editors; Springer-Verlag, Heidelberg, Germany, 1961.

61- 3 Born, J.L., Lawrence, J.H., Tobias, C.A., Carlson, R.A., Sangalli, F., and Welch, G. Hypophysectomie with nuclear radiations.
pp 828-832 in Kunstliche Radioaktive Isotope in Physiologie, Diagnostik, und Therapie (Zweite Auflage, Zweiter Band), 1960; H.Schwiegk and F. Turba, editors; Springer-Verlag, Heidelberg, 1961.

61- 4 Cleveland, Anne, Braun-Cantilo, Jorge, LaRoche, Gilles, Tobias, C.A., Constable, J., Born, J.L., Sangalli, F., Carlson, R.A., and Lawrence, J.H. Alpha particle pituitary irradiation in metastatic carcinoma of the breast: metabolic effects.
pp 190-198 in proceedings of Conference on Research on Radiotherapy of Cancer, American Cancer Society, 1961.

61- 5 Van Dyke, D., Lawrence, J.H., and Siri, W.E. Erythropoietin as an etiologic factor in blood dyscrasias.
Acta Isotopica 3: 217-226, 1961.

61- 6 Van Dyke, D.C., Layrisse, Miguel, Lawrence, J.H., Garcia, J. and Pollycove, M.: Relation between severity of anemia and erythropoietin titer in human beings.
Blood 18: 187-201, 1961.

A61- 8 Lawrence, J.H., Tobias, C.A., and Born, J.L.: Pituitary alpha particle irradiation. Abstract for 8th Annual Meeting of Society of Nuclear Medicine, Pittsburgh, Pennsylvania, June 1961.
J. Nuclear Med. 2: 133, 1961. (Abstract)

61- 7 Lawrence, J.H.: Récents Développements de la Médecine Nucléaire. University of Bordeaux Lecturer, October 1958
Colloque de Biophysique; Journées Médicales de Bordeaux (October 24-25, 1958); Société d'Édition d'Enseignement Supérieur, Paris, 1961.

62- 1 Lawrence, J.H., Tobias, C.A., and Born, J.L. Acromegaly. 7th Annual Meeting, American Clinical andClimatological Association, Wiliamsburg, Virginia, Nov. 2-4, 1961. Trans. Amer. Clin. and Climatol. Assoc. 73: 176-185, 1962.

62- 2 Braun-Cantilo, Jorge A., LaRoche, Gilles, Novitsky, Mary, and Lawrence, John H. Conversion of testosterone to estrogens in humans. Acta Isotopica 1: 351-377, 1962.

62- 3 Lawrence, J.H.: Next time I return to Japan: Impressions and experiences of the Fifteenth General Assembly of the Japan Medical Congress, Tokyo, April 1-5, 1959. Tomio Ogato, Editor, 1962.

62- 4 Lawrence, J.H., Tobias, C.A., Born, J.L., Sangalli, F., Carlson, R.A., and Linfoot, J.A. Heavy particle therapy in acromegaly. Acta Radiologica 58: 337-347, 1962.

62- 5 Lawrence, John H.: Acromegaly. Method of John H. Lawrence. p 297 in Current Therapy; H.F. Conn, editor; W.B. Saunders and Co., Pniladelphia, Pennsylvania, 1962.

62- 6 Lawrence, John H., Tobias, C.A., Born, J.L., Wang, C.C., and Linfoot, J.H.: Heavy particle irradiation in neoplastic and neurologic disease. J. Neurosurgery 19: 717-722, 1962.

62- 7 Lawrence, John H. Isotopes and nuclear radiations in medicine; a quarter century of nuclear medicine. pp 1-22 in proceedings of the International Conference on the Use of Radioisotopes in Animal Biology and the Medical Sciences, Mexico City, November 21-December 1, 1961, Volume I; The International Atomic Energy Agency, the Food and Agriculture Organization of the United Nations, and theWorld Health Organization; Academic Press, New York and London, 1962.

62- 8 Parker, H.G., Sargent, T., Pollycove, M., Carpe, Lestra, Linfoot, J.A., Saito, H., and Lawrence, J.H. Biomedical tracer studies with the whole body counter. pp 101-108 in proceedings of Inter-American Symposium on the Peaceful Application of Nuclear Energy (Fourth), Mexico City, April 9-13, 1962, Volume II; Pan-American Union, Washington D.C., 1962.

62- 9 Pollycove, M. and Lawrence, J.H.: Ferrokinetics of refractory anemia. pp 42-51 in Proceedings of VIII International Congress of Hematology, Tokyo, Japan, September 4-10, 1960, No. 257; Pan Pacific Press, Tokyo, Japan, 1962.

62-10 STeinkamp, R.C., Lawrence, J.H., and Born, J.L.: Comparative study of long and short term survivals in chronic lymphatic leukemia.
Pp 574-578 in Proceedings of VIII International Congress of Hematology, Tokyo, Japan, Septmmber 4-10, 1960, No. 257; Pan Pacific Press, Tokyo, Japan 1962.

62-11 Van Kyke, D.C., Lawrence, J.H., and Pollycove, M.:Erythropoietin. pp1004-1009 in Proceedings of VIII International Congress of Hematology, Tokyo, Japan, September 4-10, 1960, No. 257. Pan Pacific Press, Tokyo, Japan, 1962.

A62-12 Lawrence, John H.: The future of therapy beams of heavy particles.
Abstract for Tenth International Congress of Radiology, August 26-September 1, 1962, Montreal, Canada. (Abstract)

63- 1 Lawrence, J.H.: Heavy particles in neoplastic, metabolic, and neurological disease.
Chapter 10, pp , in Proceedings of the 12th Annusl Meeting of the Cpngress of Neurological Surgeons, November 1, 1962. Williams and Wilkins, Co., Baltimore, Maryland, 1963. (?published 1964)

62- 2 VanDyke, D.C., Lawrence, J.H., Pollycove, M., and Lowy, Peter: Preliminary results from the use of erythropoien in human volunteers.
pp 221-230 in Proceedings of the Endocrine Society Meeting, Hormones and the Kidney, Aberdeen, Scotland, 1962; The Aberdeen University Press, Ltd., Aberdeen, Scotland, 1963.

63- 3 Lawrence, J.H. and Parker, H.G. Use of radioisotopes in hematology.
J. Amer. Med. Assoc. 184: 136-138, 1963.

63- 4 Winchell, Harry S., Pollycove, M., Richards, V., and Lawrence, J.H. Selective irradiation of lymphatic tissues in preparation for tissue homografting.
Amer. J. Surgery 105: 177-183, 1963.

63- 5 Steinkamp, R.C., Lawrence, J.H., and Born, J.L. Long-term experiences with the use of P^{32} in the treatment of chronic lymphocytic leukemia.
J. Nucl. Med. 4: 92-105, 1963.

63- 6 Lawrence, John H. Twenty Years of Nuclear Medicine.
pp A1-A2, in Frontiers of Medicine, Roche Report, May 17,1963.

63- 7 Lawrence, J.H., Tobias, C.A., Born, J.L., Gottschalk, A., Linfoot, J.A., and Kling, R.P.: Alpha particles and proton beams in therapy.
J. Amer. Med. Assoc. 186: 236-245, 1963.
(See also, Editorial accompanying article, pp 254-255)

63- 8 Linfoot, J.A., Lawrence, J.H., Born, J.L., and Tobias, C.A. The alpha particle or proton beam in radiosurgery of the pituitary gland for Cushing's disease.
New Engl. J. Med. 269: 597-601, 1963.

63- 9 D'Angio, G.J., and Lawrence, J.H.: Medical research with high-energy heavy particles.
Nucleonics 21: 56-61, 1963.

63-10 Lawrence, J.H., Tobias, C.A., Linfoot, J.A., Born, J.L., Gottschalk, A., and Kling, R.P.: Heavy particles, the Bragg curve, and suppression of pituitary function in diabetic retinopathy.
Diabetes 12: 490-501, 1963.

63-11 Sillesen, K., Lawrence, J.H., and Lyman, J.T.: Heavy-particle ionization (He,Li,B,Ne) and the proliferative capacity of neoplastic cells *in vivo*.
Acta Isotopica 3: 107-126, 1963.

64- 1 Winchell, H.S., Pollycove, M., Loughman, W.D., Lawrence, J.H.: Autologous bone marrow transplantation studies in dogs irradiated by Y^{90}-DTPA Urine recycling technic.
Blood 23: 44-52, 1964.

64- 2 Lawrence, J.H.: Heavy particle suppression of pituitary function for severe diabetic retinopathy. Postgraduate course, University of California School of Medicine, San Francisco, Department of Ophthalmology, 1963.
in Ocular Pharmacology and Therapeutics and the Problems of Medical Management; S.J. Kimura and E.K. Goodner, editors; F.A. Davis Co., Philadelphia, Pennsylvania, 1964.

64- 3 Winchell, H.S., Pollycove, M., Loughman, W.D., and Lawrence, J.H. A method for maintenance of Curie quantities of Y^{90} DTPA in the human body for defined time periods: technique and dosimetry.
J. Nucl. Med. 5: 16-26, 1964.

64- 4 Winchell, H.S., Pollycove, M., Andersen, A.C., and Lawrence, J.H.: Relatively selective Beta irradiation of lymphatic structures in the dog using Y^{90}-DTPA.
Blood 23: 321-336, 1964.

64- 5 Lawrence, J.H.: Particules Lourdes en Thérapie.
The Pasteur Lecture, Pasteur Institute, Paris, France, May 9, 1963.
Presse Medicale 72: 1349-1352, 1964.

64- 6 Sargent, T., Linfoot, J.A., Stauffer, Henry, and Lawrence, J.H. Use of a whole body counter in turnover studies with Ca^{47}.
J. Nucl. Med. 5: 407-416, 1964.

64- 7 Lawrence, J.H., Tobias, C.A., Born, J.L., Linfoot, J.A., and Gottschalk, A. Alpha and proton heavy particles and the Bragg peak in therapy. Proceedings of Annual Meeting of American Clinical and Climatological Association, Hot Springs, Virginia, November 5, 1963.
Transactions, Amer. Clin. and Climatol. Assoc. 75:111-116, 1964.

64- 8. Saito, Hiroshi, Sargent, Thornton, Parker, Howard G., and Lawrence, John H. Normal iron absorption in man.
pp 511-522 in Proceedings of the IXth congress of the International Society of Hematology, Volume III, 1964.

64- 9 Tobias, C.A., Lawrence, J.H., Lyman, J., Born, J.L., Gottschalk, A., Linfoot, J.A., McDonald, L., : progress report on pituitary irradiation.
pp 19-35 in Proceedings of 2nd International Symposium on Response of the Nervous System to Ionizing Radiation, Los Angeles 1963; T.J. Haley and R.S. Snider, Editors, Little,Brown, Boston, 1964.

64-10 Saito, H., Sargent, T., Parker, H.G., and Lawrence, J.H.: Whole-body iron loss in normal man, measured with a gamma spectrometer.
J. Nucl. Med. 5: 571-580, 1964.

64-11 Manougian , Edward, Pollycove, Myron, Linfoot, John A., and Lawrence, John H.: C^{14}-glucose kinetic studies in normal, diabetic, and acromegalic subjects.
J. Nucl. Med. 5: 763-795, 1964.

64-12 D'Angio, G.J., Lawrence, J.H., Gottschalk, A., Lyman, J.: Relative efficiency of high-LET radiation (Bragg-peak lithium ions) on normal rabbit skin, using integral dose as a basis for comparison.
Nature 204: 1267-1268, Deeember 26, 1964.

64-13 Lawrence, John H. Atomic Energy, Science, and Education (from the Albert S. Harding Lecture given at South Dakota State College, Brookings, South Dakota, April 21, 1964).
Congressional Record, United States Senate, pp 21787-21791, September 22, 1964.

64-14 Lawrence, John H.: The use of heavy particles and the Bragg peak in therapy.
Paper given at International symposium "Radio-biological destruction of the Hypophysis in Endocrinology, Milan, Nov. 30, 1963.
Acta Isotopica 4: 327-345, 1964.

65- 1 Lawrence, John H.: Nuclear techniques in biomedical research.
Nucleonics 23(1): 48-49, 1965.

65- 2 Lawrence, John H., Tobias, C.A., Linfoot, J.A., Born, J.L., Manougian, E., and Lyman, J.T.: Heavy particles and the Bragg peak in therapy.
Annals Internal Med. 62: 400-407, 1965.

65- 3 Lawrence, John H. and Tobias, Cornelius A.: Heavy particles in medicine.
Chapter 6 (pp 127-246) in Progress in ATomic Medicine, Vol. I; J.H. Lawrence, Editor; Grune & Stratton, New York, 1965.

65- 4 Born, J.L., Lawrence, J.H., Linfoot, J.A., Tobias, C.A., Manougian, E., and Snyder, N.J.: Hypophyseal suppression with heavy particles in diabetic retinopathy.
Chapter 33 (pp 475-482) in Proceedings of the Fifth Congress of the International Diabetes Federation, Toronto, Canada, July 1964 On the Nature and Treatment of Diabetes; Excerpta Medica International Congress Series No. 84; Amsterdam, Holland, 1965.

65- 5 Shkurkin, Carol J. and Lawrence, John H.: Treatment of blood diseases and malignancy with radioactive phosphorus in relation to other therapeutic agents.
Chapter 27 (pp 675-704) in Nuclear Medicine; W.H. Blahd, Editor; Blakiston Division, McGraw-Hill Book Co., New York, 1965.

65- 6 D'Angio, Giulio J., Gottschalk, Alexander, and Lawrence, John H.: Medical applications of high-energy nuclear particles.
Chapter 30 (pp 779-798) in Nuclear Medicine; W.H. Blahd, Editor; Blakiston Division, McGraw-Hill Book Co., New York, 1965.

65- 7 Lawrence, John H. : George de Hevesy -- a genius.
Letter to the Editor (DeHevesy's 80th Birthday), International Journal Applied Radiation and Isotopes 16: 511, 1965.

66- 1 Fish, V.J., Winchell, H.S., and Lawrence, J.H. Thymic and splenic irradiation in the treatmentof acute lymphatic leukemia.
Amer. J. Roentgenol., Radium Ther., Nucl.Med. 97: 989-990, 1966.

66- 2 Winchell, H.S., Pollycove, M., Loughman, W.D., Richard, V., Kim, L., and Lawrence, J.H.: Homotransplantation studies in dogs following selective radioisotopic lymphatic ablation.
J. Nucl. Med. 7: 416-423, 1966.

66- 3 Fawwaz, R.A., Winchell, H.S., Pollycove, M., Sargent, T., Anger, H.O., and Lawrence, J.H.: Intestinal iron absorption studies using iron^{52} and Anger positron camera.
J. Nucl. Med. 7: 569-574, 1966.

66- 4 Hazel, J.J., Lawrence, J.H., and Tobias, C.A. Heavy particle radiations -- some therapeutic considerations.
J. de l'Assoc. Canadienne des Radiologists XVII; 158-161, 1966.

66- 5 Lawrence, John H.: Some early studies with the products of nuclear physics.
Isotopes and Radiation Technology 4: 21-24, 1966.

66- 6 VanDyke, D.C., Nohr, Mary Lou, and Lawrence, J.H.: Erythropoietin in the urine of normal and erythropoietically abnormal human beings.
Blood 28: 535-543, 1966.

66- 7 Pollycove, M., Winchel, H.S., and Lawrence, J.H. Classification and evolution of patterns of erythropoiesis in polycythemia vera as studied by iron kinetics.
Blood 28: 807-829, 1966.

66- 8 Lawrence, J.H., Tobias, C.A., Born, J.L., Linfoot, J.A., and D'Angio, D.: Heavy particles in experimental medicine and therapy. Scientific Exhibit, Annual Meeting of American Medical Association, New York City, June 1965.
J. Amer. Medical Assoc. 196: 166-170, 1966.

66- 9 Winchell, H.S., and Lawrence, J.H. Polycythemia vera: method of H.S. Winchell and John H.Lawrence.
pp 245-247 in Current Therapy, H.F. Conn, Editor; W.B. Saunders and Co., Philadelphia, Pennsylvania, 1966.

67- 1 Lawrence, John H. and Tobias, C.A. Heavy particles in therapy.
Chapter 15,(pp 260-276) in Modern Trends in Radiotherapy, Vol. I, T.J. Deeley and C.A.P. Wood, editors; Butterworths, London,1967.

67- 2 Manougian, E., Pollycove, M., Linfoot, J.A., and Lawrence, J.H.: C^{14}-glucose kinetic studies in normal, diabetic and acromegalic subjects following glucose infusion.
pp 260-270, in Radioaktive Isotope in Klinik and Forschung VII International Symposium in Bad Gastein, Austria, January 10-13, 1966; K. Fellinger and R. Hofer, Editors; Urban, and Schwarzenberg, Vienna, 1967.

67- 3 Lawrence, J.H.: Heavy particle irradiation to the pituitary in metastatic breast cancer.
pp 173-174, Proceedings of Colloque International de Lyon on Major Endocrine Surgery for the Treatment of Cancer of the Breast in Advances Stages, in Lyon, France, May 5-7, 1966; M. Dargent & Cl.Romieu, Editors; SIMEP Editions, Lyon, 1967.

67- 4 Garcia, J.F., Linfoot, J.A., Manougian, E., Born, J.L., and Lawrence, J.H.: Plasma growth hormone studies in normal individuals and acromegalic patients.
J. Clin. Endocrinol. & Metab. 27: 1395-1402, 1967.

67- 5 Lawrence, John H.: Radiobiological studies with heavy particles as related to therapy.
Radiation Research, Suppl. 7: 360-368, Academic Press, 1967.

67- 6 Lawrence, John H.: President's Annual Report (presented at the Annual Meeting, Society of Nuclear Medicine, Seattle, June 1967).
J. Nucl. Med. 8: 699, 1967.

68- 1 Feola, J.M., Richman, C., Raju, M.R., Curtis, S.B., and Lawrence, J.H. Effect of negative pions on the proliferative capacity of ascites tumor cells (lymphoma) in vivo.
Radiation Research 34: 70-78, 1968.

68- 2 Bresson, Y., Bunnag, T.W., Chomicki, O., Lagunova, I.G., Lawrence, J.H., Mayneord, W.V., Parker, H.M., Reboul, J., Strajman, E., Walstam, R., Webster, E.W., Windeyer, B. MEDICAL RADIATION PHYSIC
Report of a Joint IAEA/WHO Expert Committee (Meeting held in Geneva, Dec. 12-18, 1967). Wld. Hlth. Org. Tech. Rept. Series No. 390, World Health Organization, Geneva, 1968.

69- 1 Lawrence, J.H., Winchell, H.S., and Donald, W.G. Leukemia in polycythemia vera: relationship to splenic myeloid metaplasia and therapeutic radiation dose.
Annals Internal Med. 70: 763-771, 1969.

69- 2 Weber, P.M., Pollycove, M., Bacaner, M., and Lawrence, J.H. The cardiac output in polycythemia vera.
J. Lab. and Clin. Med. 73: 753-762, 1969.

69- 3 Ronai, P., Winchell, H.S., Anger, H.O., and Lawrence, J.H. Whole-body scanning of ^{59}Fe for evaluating body distribution of erythropoietic marrow, splenic dequestration of red cells, and hepatic deposition of iron.
J. Nucl. Med. 10: 469-474, 1969.

69- 4 Lawrence, J.H. Reducing effects of acromegaly.
Medical World News 10(23): 35-36, 1969.

69- 5 Chong, C.Y., Linfoot, J.A., and Lawrence, J.H. High-energy heavy charged particles in medicine.
Radiologic Clinics North America 7: 319-343, 1969.

69- 6 Fawwaz, R.A., Winchell, H.S., Frye, F., Hemphill, W., and Lawrence, J.H. Localization of ^{58}Co and ^{65}Zn-hematoporphyrin complexes in canine lymph nodes.
J. Nucl. Med. 10: 581-585, 1969.

69- 7 Ngo, T.M., Winchell, H.S., Williams, M.A., and Lawrence, J.H. Decreased $^{14}CO_2$ production in thiamine-deficient rats given pyruvat 1-^{14}C and acetate-1-^{14}C: a possible means for early diagnosis of beri-beri?
J. Nucl. Med. 10: 676-682, 1969.

69- 8 Feola, J.M., Lawrence, J.H., and Welch, G.P. Oxygen enhancement ratio and RBE of helium ions on mouse lymphoma cells. Radiation REsearch 40: 400-413, 1969.

69- 9 Linfoot, J.A,, Born, J.L., Garcia, J.F., Manougian, E., Kling, R., Chong, C.Y., Tobias, C.A., Carlson, R.A., and Lawrence, J.H. Metabolic and ophthalmological observations following heavy-particle pituitary suppressive therapy in diabetic retinopathy. Chapter 24, (pp 277-289) in proceedings of Symposium on the Treatment of Diabetic REtinopathy, Warranton, Virginia, Sept. 29 to October 1, 1968; M.F. Goldberg and S.L. Fine, editors; U.S. Public Health Service Publication No. 1890, Government Printing Office, Washington, D.C., 1969.

70- 1 Linfoot, J.A., Garcia, J.F., Hoye, S.A., Schmitt, J., and Lawrence, J.H. Heavy particle therapy in acromegaly. Proc. Roy. Soc. Med. 63: 219-221, 1970.

70- 2 Lawrence, J.H., Tobias, C.A., Linfoot, J.A., and Fink, R.A. Heavy particles in neurology and neurosurgery: a review. Chapter 12, (pp 313-326) in Radionuclide Applications in Neurology and Neurosurgery; Y. Wang and P. Paoletti, Editors; C.C. Thomas, Springfield, Illinois, 1970.

70- 3 Lawrence, J.H., Tobias, C.A., Linfoot, J.A.; Born, J.L., Lyman, J.T., Chong, C.Y., Manlugian, E., and Wei, W.C. Successful treatment of acromegaly: metabolic and clinical studies in 145 patients.
J. Clin. Endocrinol. & Metabolism 31: 180-198, 1970.

70- 4 Linfoot, J.A., Garcia, J.F., Wei, W., Fink, R., Sarin, R., Born, J.L., and Lawrence, J.H. Human growth hormone levels in cerebrospinal fluid.
J. Clin. Endocrinol. & Metabolism 31: 230-232, 1970.

70- 5 Linfoot, J.A., Lawrence, J.H., Tobias, C.A., Born, J.L., Chong, C.Y, Lyman, J.T., Manougian, E.: Progress report on the treatment of Cushing's disease. Proc. meeting American Clinical and Climatological Association, Hilton Head Island, S.Carolina, Oct.22, 1969 Trans. Amer. Clinical & Climatolog. Assoc. 81: 196-212, 1970.

70- 6 Lawrence, John H. Radioisotopes and Nuclear Radiations in Therapy: Historical Perspectives and Current Investigations. pp 12-16 in Souvenir, published for the II Annual Conference of the Society of Nuclear Medicine, India, October 15-16, 1970, Delhi, India. 1970.

70- 7 Černelč, Milan, Winchell, H.S., Sargent, T., Pollycove, M., Shkurki C., and Lawrence, J.H. Iron absorption after porto-caval anastomosis.
Medical Journal of Slovenia 39: 83-85, 1970.

70- 8 Lawrence, J.H., Born, J.L., Linfoot, J.A., and Chong, C.Y. Heavy particle radiation treatment of pituitary tumors. Letter to the Editor; J. Amer. Med. Assoc. 214: 2061, 1970.

70- 9 Winchell, H.S., Stahelin, H., Kusobov, N., Slanger, M., Fish, M., Pollycove, M., and Lawrence, J.H. Kinetics of CO_2-HCO_3^- in normal adult males. J. Nucl. Med. 11: 711-715, 1970.

70-10 Feola, J.M., Raju, M.R., Richman, C., and Lawrence, J.H. The RBE of negative pions in 2-day-old ascites tumors. Radiation Research 44: 637-648, 1970.

71- 1 Lawrence, J.H. Radioisotopes and nuclear radiations in medicine. Northwest Medicine 70: 245-250, 1971.

71- 2 Lawrence, J.H. Treatment of acromegaly (Cont). Letter to the Editor, New Engl. J. Med. 285: 56-57, 1971.

71- 3 Lawrence, J.H., Tobias, C.A., Lyman, J.T., and Linfoot, J.A. Heavy particles in the treatment of acromegaly and Cushing's disease and their potential in value in other neoplastic diseases. Chapter 32, (pp 806-820) in Nuclear Medicine, 2nd Edition; W.H. Blahd, Editor; McGraw-Hill, New York, 1971.

71- 4 Lawrence, J.H., Okerlund, M.D., Linfoot, J.A., and Born, J.L. Heavy particle treatment of Cushing's disease. Letter to the Editor, New Engl. J. Med. 285: 1263, 1971.

71- 5 Tobias, C.A., Lyman, J.T., and Lawrence, J.H. Some considerations of physical and biological factors in radiotherapy with high-LET radiations including heavy particles, Pi mesons, and fast neutrons. Chapter 6 (pp 167-218) in Progress in ATomic Medicine: Recent Advances in Nuclear Medicine, Volume 3; J.H. Lawrence, Editor; Grune & STratton, New York, 1971.

71- 6 Linfoot, J.A., Chong, C.Y., Garcia, J.F., Cleveland, A.S., Connell, G.M., Manougian, E., Okerlund, M.D., Born, J.L., and Lawrence, J.H. Heavy-particle therapy for acromegaly, Cushing's disease, Nelson's syndrome, and non-functioning pituitary adenomas. Chapter 7 (pp 219-238) in Progress in ATomic Medicine: Recent Advances in Nuclear Medicine, Volume 3; J.H. Lawrence, Editor; Grune & STratton, New York, 1971.

71- 7 Lawrence, John H. Heavy particles: comments on historical research and medical applications.
Yale Scientific Magazine 45(5): 2-4, February 1971.

71- 8 Lawrence, J.H., Tobias, C.A., Born, J.L., Linfoot, J.A., Chong, C.Y., Lyman, J.T., and Manougian, E. Radioisotopes and nuclear radiations in therapy: historical perspectives and current investigations.
pp 262-269 in Oncology 1970, Proceedings of the Tenth International Cancer Congress, Volume III. Diagnosis and Management of Cancer: General Considerations; R.L. Clark, R.W. Cumley, J.E. McCay, and M.M. Copeland, Editors; Year Book Medical Publishers, Inc., Chicago, 1971. (congress held in Houston, Texas, May 1970).

72- 1 Lawrence, John H. Radioisotope therapy in hematology.
Chapter 17 (pp 155-167) in proceedings of Third Annual Nuclear Medicine Seminar, "Hematopoietic and Gastrointestinal Investigations with Radionuclides", Miami Beach, Florida, March 1971; A.J. Gilson, W.M. Smoak III, and M.B. weinstein, Editors; C.C. Thomas, Springfield, Illinois, 1972.

72- 2 Lawrence, John H. Positron emitting isotopes: investigative and diagnostic studies.
Chapter 27 (pp 247-263) in proceedings of Third Annual Nuclear Medicine Seminar, "Hematopoietic and Gastrointestinal Investigations with Radionuclides", Miami Beach, Florida, March 1971; A.J. Gilson, W.M. Smoak III, and M.B. Weinstein, Editors; C.C. Thomas, Springfield, Illinois, 1972.

72- 3 Price, J., Stauffer, H.H., Hogan, W.D., and Lawrence, J.H. Correlation coefficients between retinal lesions and visual acuity in diabetic retinopathy.
Brit. J. Ophthalmology 56: 21-24, 1972.

72- 4 Lawrence, John H. Heavy particle therapy held effective in endocrinopathies.
Internist Observer 10(7): 2; 4; November 1972.

73- 1 Lawrence, John H. Heavy-particle radiation therapy found beneficial.
Diabetic Outlook 8(1): 2, January 1973.

73- 2 Lawrence, John H. Acromegaly: method of John H. Lawrence, M.D. pp 444-445, in Current Therapy, H.F. Conn, Editor; W.B. Saunders & Co., Philadelphia, Pennsylvania, 1973.

73- 3 Lawrence, John H. Acromegaly.
IRCS (Journal International Research Communications)
Vol. 1, No. 4 : 19, June 1973.

73- 4 Lawrence, J.H., Chong, C.Y., Born, J.L., Lyman, J.T., Okerlund, M.D., Garcia, J.F., Linfoot, J.A., Tobias, C.A., and Manougian, E. Heavy particles in acromegaly and Cushing's disease. pp 39-61 in proceedings of 16th Annual Clinical Conference on Cancer, "Endocrine and Nonendocrine Hormone-Producing Tumors", The University of Texas at Houston M.D.Anderson Hospital and Tumor Institute, November 10-12, 1971. Year Book Medical Publishers, Inc.; Chicago, Ill., 1973.

73- 5 Lawrence, J.H., Chong, C.Y., Lyman, J.T., Tobias, C.A., Born, J.L., Garcia, J.F., Manougian, E., Linfoot, J.A., and Connell G.M. Treatment of pituitary tumors with heavy particles. pp 253-262 in "Diagnosis and Treatment of Pituitary Tumors" proceedings of a conference sponsored jointly by the National Institute of Child Health and Human Development and the National Cancer Institute in Bethesda, Maryland, January 15-17, 1973. P.O. Kohler and G.T. Ross, Editors. Excerpta Medica Amsterdam, American Elsevier Publishing Co., Inc., New York, 1973.

73- 3 Lawrence, John H. Acromegaly.
IRCS (Journal International Research Communications)
Vol. 1, No. 4 : 19, June 1973.

73- 4 Lawrence, J.H., Chong, C.Y., Born, J.L., Lyman, J.T., Okerlund, M.D., Garcia, J.F., Linfoot, J.A., Tobias, C.A., and Manougian, E. Heavy particles in acromegaly and Cushing's disease. pp 39-61 in proceedings of 16th Annual Clinical Conference on Cancer, "Endocrine and Nonendocrine Hormone-Producing Tumors", The University of Texas at Houston M.D.Anderson Hospital and Tumor Institute, November 10-12, 1971. Year Book Medical Publishers, Inc.; Chicago, Ill., 1973.

73- 5 Lawrence, J.H., Chong, C.Y., Lyman, J.T., Tobias, C.A., Born, J.L., Garcia, J.F., Manougian, E., Linfoot, J.A., and Connell. G.M. Treatment of pituitary tumors with heavy particles. pp 253-262 in "Diagnosis and Treatment of Pituitary Tumors" proceedings of a conference sponsored jointly by the National Institute of Child Health and Human Development and the National Cancer Institute in Bethesda, Maryland, January 15-17, 1973. P.O. Kohler and G.T. Ross, Editors. Excerpta Medica Amsterdam, American Elsevier Publishing Co., Inc., New York, 1973.

75- 1 Lawrence, J.H. Some Early and Recent Experiences in Nuclear Medicine. George Von Hevesy Memorial Lecture. Pp 23-51, Proceedings 11th International Meeting of Society of Nuclear Medicine, Athens, Greece, September 24-29, 1973. F.K. Schattauer Verlag, Stuttgart, 1975.

75- 2 Linfoot, J.A., Chong, C.Y., Lawrence, J.H., Born, J.L., Tobias, C.A. Lyman, J.T. Acromegaly. Chapter 4, pp 191-246, in Hormonal Proteins and Peptides, Volume III. C.H. Li, Editor. Academic Press, New York, 1975.

75- 3 Lawrence, J.H., Linfoot, J.A., Born, J.L., Tobias, C.A., Chong, C.Y., Okerlund, M.D., Manougian, E., Garcia, J.F., and Connell, G.M. Heavy particle irradiation of the pituitary. pp 272-294, in Progress in Neurological Surgery, Volume 6. H. Krayenbuhl, P.E. Maspes, and W.S. Sweet, Editors. S. Karger, Basel, Switzerland, 1975.

76- 1 Lawrence, John H. Epilogue. Seminars in Hematology 13: 85-86, 1976.

76- 2 Lawrence, J.H., Tobias, C.A., Linfoot, J.A., Born, J.L., and Chong, C.Y. Heavy-Particle Therapy in Acromegaly and Cushing Disease. J. Amer. Med. Assoc. 235: 2307-2310, 1976.

77- 1 Lawrence, J.H., Tobias, C.A., Chong, C., Lyman, J., Born, J.L., Linfoot, J.A., and Manougian, E. "Heavy Particle Irradiation of the Pituitary". In: The Pituitary - A Current Review, 1977, Chapter 26, Academic Press, New York, pp. 443-451.

77-2 Pituitary Symposium S.F. June 1978
1st paper (submitted to Dr. J. Linfoot for publication)

77-3 Recent advances in nuclear medicine
Lawrence + Bennett
Book published by Grune + Stratton - 1978

also Book - by Lawrence, et al about 1963

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