DOE/EH-0479

HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of
Donner Lab Administrator
Baird G. Whaley

Conducted August 15, 1994

United States Department of Energy
Office of Human Radiation Experiments
September 1995

FOREWORD

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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

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

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

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

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DISCLAIMER

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

DOE/EH-0479
September 1995

Interview with Baird G. Whaley
Setting: August 15, 1994, Berkeley, California
Interviewer: Anna Berge, Lawrence Berkeley Laboratory
(Archives and Records Office)

ORAL HISTORY OF DONNER LAB ADMINISTRATOR BAIRD G. WHALEY

On August 15, 1994, Ms. Anna Berge of the Lawrence Berkeley Laboratory Archives and Records Office interviewed Mr. Whaley at his residence in Berkeley, California.

Baird Whaley was selected for the oral history project because of the high administrative position he held at Lawrence Berkeley Laboratory's (LBL's) Donner Laboratory (Division of Bio-Medical Science). The interview covers his career as an administrator for Donner Lab from 1954 to 1986. He discusses how Donner Lab operated as an administrative unit within LBL; how Donner changed as a result of changing directorships; and how Dr. John Lawrence charted a somewhat administratively independent course for Donner Lab. Mr. Whaley also covers Donner Lab's problems in the early 1970s, the ousting of Dr. Born, and Dr. Alpen's reorganization. He talks about the personalities of the people at Donner and what they were working on. He also discusses the Donner family and their relationship to the lab.

Short Biography

George Baird Whaley, Jr. was born April 20, 1929, in Berkeley, California. Married since 1954, he has two children. Mr. Whaley graduated in 1951 with a B.S. in Math with honors from the University of California, Berkeley [(UC Berkeley)]. From 1951 to 1953, he served as an infantry lieutenant during the Korean War. In August 1954, he took an administrative position at Donner Laboratory. From 1954 to 1977, he served as administrative assistant to the director of Donner Lab, and later as codivision administrator for personnel, non-DOE funds, and general administration. From 1977 to 1985, Mr. Whaley was the division administrator for Donner Lab. He served for a short time as a special assistant to the director of Donner Lab, until retiring in August of 1986.

During his 32-year career at the lab, Mr. Whaley served on many LBL committees:

- 1965—Administrative Service Salary Committee
- 1973 to 1976—Member and Chairperson (1974–76), Employee Relations Committee
- 1979—Prepared a special report to the Director of LBL on split verse joint faculty appointments
- 1981—Special fact-finder for the Director of LBL
- 1983 to 1985—Technical Salary Committee.

Mr. Whaley has been a member of Phi Beta Kappa and Pi Mu Epsilon since 1950 and a member of the Society of Research Administrators since 1974. From 1988 to 1991, Mr. Whaley came out of retirement to work on a special assignment for LBL. Mr. Whaley still lives with in Berkeley, California, with his wife, Mary Ann.

Hiring On at the Donner Laboratory

BERGE: [My name is Anna Berge. I work at the Lawrence Berkeley Laboratory (LBL) Archives and Records Office. I am conducting an oral history of Baird Whaley] on August 15, 1994, at his residence in Berkeley.

I was wondering if you could start us off with a little description of how you got to be what you got to be, and how you ended up at the lab?

WHALEY: I went to the University of California at Berkeley and majored in Math, and graduated in 1951. I was in ROTC [(Reserve Officer Training Corps)] when the Korean War had broken out; so I was called up into active duty. I was in for about 19 months, wounded, got out. I didn't have anything in particular to do. My uncle was an entrepreneur. He had the Oldsmobile agency in Berkeley, and a number of other small businesses. He liked to buy sick businesses and cure them, or try to cure them. He had a stationery store that he was trying to sell and needed a manager, and I needed a job.

So I went to work for him for about a year. Then he found a buyer. I was about to get married at that point [(1954)]. I got married with no job and decided that since the University was the biggest employer in town, and since I wanted to stay in Berkeley, I would try there.

I went to the Bureau of Occupations and talked to Nancy Corson, who I think was the assistant there at that time. She wasn't very helpful, and I left without anything particular to follow up on. I got home, and that afternoon she called and said there was a position at what was then known as the Rad Lab.[1] They were looking for someone to do administrative work at the Donner Laboratory[2]; but who also had a scientific background. And math counted as a scientific background.

I went in and talked to Bill Bigelow. And he then arranged an interview for me with [the Donner Laboratory director], Jim Born. Of course, I had one with [Berkeley Radiation Laboratory Director] John Lawrence.[3] That's how I got there.[4] I was hired as an administrative assistant. He [(Lawrence)] wasn't very big on titles during those days. I started on August 5, 1954. I stayed there until I retired, on August 31, 1986.

While Dr. Lawrence was the director,[5] I had essentially the same job. At one point, I did take over the administration of what we called the "odd

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1 UC Radiation Laboratory; renamed Lawrence Radiation Laboratory; now Lawrence Berkeley Laboratory, a national laboratory of the U.S. Department of Energy; founded by Ernest Lawrence in 1936

2 a laboratory set up at the UC Radiation Laboratory in Berkeley during the 1930s specifically to conduct experiments in medical physics

3 Dr. John Lawrence, brother of Ernest O. Lawrence, was Director of the Division of Medical Physics at the University of California, Berkeley. He operated a clinic at Donner Laboratory, where he treated leukemia and polycythemia vera patients with radioactive phosphorus.

4 Mr. Whaley added later: "We used to joke a bit that one of Dr. Lawrence's principal ways of concluding an interview was to hire people. He had a little difficulty sometimes in saying 'no'."

5 Mr. Whaley added later: "He was the director until 1970, and then went on the Regents."

funds" (the non-AEC[6] research grants, like the U.S. Public Health Service and various others, that at that time couldn't come through the Laboratory). They came through the campus. Those were a much smaller fraction of our support than the AEC. But I looked after those, and also the personnel side of the operation. There was a kind-of dual management.

A man named Bob San Souci (he has since passed away) was the business manager. Again, that wasn't an official title. He looked after the AEC side of it and the "things" side: purchasing, the plant maintenance, etc. He moved to Livermore,[7] when Jack Gofman[8] left Donner and went out to head up the new biomed program at Livermore.[9] Igor Blake was hired to replace him. Igor and I also had kind-of dual status, which amounted to kind-of coadministrators, until Ed Alpen came in in 1976. Within a year, Igor went into central administration and wasn't replaced. I became the administrator of the whole division, from 1977 until I retired in 1986.

Where would you like me to go from that?

BERGE: Maybe we could start in some kind of chronological order; starting with your first years in the 1950s. For example, you mentioned that you worked primarily with the non-AEC funding. What other institutions did Donner have dealings with?

Non–AEC-Funded Projects; the "Odd Funds"

WHALEY: There was a large amount of donations from patients and families of patients, and friends of Dr. Lawrence. He and his wife were very well-connected socially, in San Francisco and on the Peninsula. These were wealthy people.[10] And they became interested in his work, and contributed to the Laboratory. John Gofman had Air Force contracts, and he had support from Eli Lilly [in Indianapolis]. I don't know if they still exist. They were a big pharmaceutical firm,[11] and they may have been merged into something else.[12] Merck—they were also a pharmaceutical

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

7 Lawrence Livermore National Laboratory, Livermore, California

8 John Gofman was a physician and biophysicist. For the interview with Gofman, see DOE/EH-0457, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. John W. Gofman, M.D.* (June 1995).

9 The Biomedical Division at Lawrence Livermore was set up in 1962 by the chairman of AEC's Division of Biology and Medicine, Charles Dunham. The establishment of this laboratory is discussed under "Livermore Biomedical Division; Conflicts With John Gofman (1962–72)" in the John R. Totter transcript (DOE/EH-0481, September 1995). For insight into discussions leading to establishing this laboratory from Dr. Gofman's perspective, see "Establishing Livermore Laboratory's Division of Biology and Medicine" and "Jack, all we want is the truth" in the Gofman transcript (DOE/EH-0457, June 1995).

10 Lawrence belonged to the Bohemian Club, an exclusive social club in the San Francisco area known for back-to-nature retreats to Yosemite and the power and influence of its members.

11 a firm that develops and manufactures drugs approved for human use

12 Lilly has not merged with or been acquired by another firm.

house.[13] Also, the Albert and Mary Lasker Foundation. Those were mostly John Gofman's support. I think that Dr. Lawrence's [support] was principally in the National Institutes of Health [(NIH)] area.[14] I've learned that your memory gets a little fuzzy. Later, there was a shift in the handling of these funds, and they came through LBL.

The reason that these funds originally came in through the campus, as I mentioned before, was that they couldn't come in through the Laboratory, because the Laboratory was strictly an AEC (later ERDA[15] and DOE[16]) operation. It wasn't permitted to have other kinds of financial support. That's been a source of problems all along. I think one of the things that the AEC looked at was the fact that *they* paid for everything. They paid for the buildings and the roads, and the whole works. Why should, then, some other agency come in and get a free ride? Which was a point of view you could certainly argue either way. Donner was on the campus. That's an important point.

When I first went there in 1954, the original wing of the Donner Laboratory (which had been built in 1941 and '42) was there, and construction was underway on what I still call the new building, although it's 40 years old now; the wing that comes out to the north from the original building. Our office[17] and the Technical Illustrations section were in Room 100, Building T-1, which was the easternmost of a string of former Navy barracks that were dismantled and brought in from Camp Parks, and located on campus in Botany Glage. T-1 was located roughly where the front porch of Evans Hall is now. The animal colony was then in Building 55, which now is, or was when I was last there, Research Medicine (the building that is just below Building 90).

We had one small piece of space, probably no more than a room, in Building 50. At that time, Crocker Laboratory and the sixty-inch cyclotron[18] were still in existence on the campus. Although Crocker was under the same contract, we had very little connection with Dr. Hamilton's[19] group. Crocker was, de facto, almost separate administra-

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13 Merck & Co., Inc., based in White House Station, New Jersey, remains one of the largest pharmaceutical firms in the United States.

14 Mr. Whaley added later: "I went back and helped with a lawsuit in 1990. And using my recollection of things that had gone on in the late seventies and early eighties, I was surprised at the number of things that I had forgotten or I hadn't remembered quite correctly."

15 The U.S. Energy Research and Development Administration succeeded the AEC in the early '70s, and in turn was replaced by the DOE in 1977.

16 U.S. Department of Energy, created in 1977

17 Mr. Whaley added later: "When I speak of 'our office,' that included Jim Born, who was the assistant director. He was also heavily involved in the medical research end of it; and Janice DeMoor was the secretary that we shared. She is certainly somebody you should talk to, and was there longer than I was. I think she's still alive and functioning and lives here in Berkeley."

18 an accelerator in which particles move in spiral paths in a constant magnetic field

19 Joseph Hamilton, an M.D., worked at Crocker Laboratory, then the site of a 60-inch cyclotron that he operated to produce radioisotopes in support of research and some medical diagnosis and treatment. Crocker was part of the Lawrence Radiation Laboratory.

tively. I don't know really, why, other than perhaps history and possibly personality. He and Dr. Lawrence were both very strong personalities.[20] So was Dr. Gofman.[21]

At that point, when I came in, the Donner Pavilion had just been finished (the little hospital unit located at Cowell Hospital). I think it was probably within a month or so after I came, that they irradiated the first patient at the 184-inch cyclotron. I remember that very well, although I didn't have anything to do with the science end of it. Jim Born, who was really a wonderful person to work for, sensed that it was quite a historic moment. And he told me to come on up with him, and to be there and observe the first treatment.

I remember E.O. [(Ernest)] Lawrence[22] coming in. Besides Dr. Lawrence, there was Dr. [Hardin] Jones, who was the other assistant director[23]. He was essentially more for science, while Dr. Born was assistant director for administration. Dr. Born was really the administrator. He had a legal background as well as medical.

And there was Dr. Gofman and Dr. [Cornelius] Tobias.[24] They were working in a field that had originally been called, in the earlier days, "medical physics." They were beginning to call it "nuclear medicine"[25] now. I'm probably not the best person to talk about it. I think a lot of it had to do with politics and power. Medical physics was coming to be seen as a very narrow part of the field. They preferred it to be called "biophysics,"[26] because that was a more encompassing field. For the strictly medical end, they liked "nuclear medicine" better, as terminology. And that was the name of their professional society. It was the "Society of Nuclear Medicine," not the "Society of Medical Physics."

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20 The rivalry between Crocker and Donner, two branches of the Lawrence Radiation Laboratory, is discussed by Gofman in the transcript of his interview (DOE/EH-0457).

21 Gofman's public views and outspoken style brought him into frequent conflict with the AEC. For Gofman's account of these conflicts, see "The Controversy Over Nuclear-Armed Antiballistic Missiles (1969)" in the Gofman transcript. A contrasting view is presented by the AEC's John Totter in his transcript (DOE/EH-0481, September 1995).

22 U.S. physicist, 1901–58; a pioneer in nuclear physics who built and operated (with M. Stanley Livingston and Milton White) the first cyclotron in 1930 on the Berkeley campus of the University of California; established the University of California Radiation Laboratory in 1936 and served as its director until his death.

23 Hardin B. Jones, M.D., studied isotope applications in nuclear medicine and showed uptake of iodine-131 by human and bovine thyroids. He was a member of the National Advisory Committee on Radiation of the Federal Radiation Council. Politically conservative, Jones took a stand against the counter-culture movement at Berkeley during the '60s. He died of a heart attack in the late '70s. See "Reflections on Hardin Jones" in DOE/EH-0476, *Human Radiation Studies: Remembering the Early Years; Oral History of Physiologist Nello Pace, Ph.D.* (June 1995).

24 Tobias was a professor of medical physics and radiology at the Donner Laboratory and the University of California at Berkeley. Dr. Tobias's main research focused on the biological effects of radiation; cancer research; and space medicine. For the transcript of the interview with Tobias, see *DOE/EH-0480, Human Radiation Studies: Remembering the Early Years; Oral History of Biophysicist Cornelius A. Tobias, Ph.D.* (July 1995).

25 diagnostic and therapeutic medical techniques using radionuclides or radioisotopes

26 the branch of biology that applies the methods of physics to the study of biological structures and processes

I remember Toby [(Tobias)] had a colleague—I think his name was Franklin Hutchinson—at Yale, where they had a Department of Biophysics. People were using that terminology here. And they had formed this little academic department, which was called the Division of Medical Physics, back in 1944. They were trying periodically to get that to be called " the Department of Biophysics." That was very political on the campus, biophysics being a more encompassing field. There were faculty members in other departments that said, "*We* really are the Department of Biophysics." They solved that for a long time by creating the Graduate Group in Biophysics, which was an interdepartmental organization.

Eventually, the Division of Medical Physics (I think it was on the verge of being dismantled) was reviewed by an academic committee, which was so impressed with what they had done, that instead of dismantling it and parceling out the faculty members to other departments on campus, they gave it departmental status; which is what the Division of Medical Physics faculty wanted. I think they called it "the Department of Biophysics and Medical Physics."

Over the years, we had lots of involved conflicts with them over space in the Donner Laboratory building. That was an issue that reoccurred time and again. It generated some acrimony. Someplace, certainly in George Pappas's files, and someplace in our files, (if the young folks, who came in and who are great for throwing things away, because they couldn't see the point in saving them, have kept them) I have an enormous file on the space controversy.

In addition to the work I mentioned (the first patient being treated at the cyclotron), there was the Donner Clinic, which was located for many years in the Donner building; and then moved to Building 55, when Building 55 was no longer the animal house but was the home base of what, at that time, was called the Research Medicine Group. Ed Alpen reorganized the division very extensively when he came in 1976. The clinic, to the best of my knowledge, goes back to almost the very beginnings; in the late '30s, John Lawrence. I don't know exactly when they started staffing it with laboratory technologists, but certainly they did that as time went on. It goes way back in the history. In that end of it there were various people.

Jim Born had a lot to do with the clinic—in other words, the blood work. I guess, when I first went there, that was under Nat Berlin. Not long after I arrived, Nat left and went to National Institutes of Health (I think he was with the [National] Cancer Institute). There was Rex Huff, who I think ended up in Seattle. Paul Elmlinger—I think I remember. After that group, which was basically phasing out when I arrived, Myron Pollycove[27] came and was there for quite a time. Then he moved across the bay to San Fran-

[27] a hematologist who conducted biophysics research at the UC Radiation Laboratory; as of this publication, Dr. Pollycove was serving as a consultant to the Nuclear Regulatory Commission, in Rockville, Maryland.

cisco General [Hospital] for the UCSF[28] isotope service there. Jim McRae came from Australia. Bring[ing] McRae here was Dr. Jim Born's doing.

BERGE: Can you spell McRae?

WHALEY: Capital M-C, capital R-A-E—James. Then, of course, Tom Budinger. That's much later on. I think that Tom would have come in the late '60s. He was there while Lawrence was still here. He'd come from Livermore, I think. I'm not clear what he was doing; whether he was with the Medical Department out there, or whether he was a researcher. After Ed Alpen came, Tom was the head of that whole medical group: the Research Medicine Group, which for a time included the cyclotron irradiation program.

At this point, Joseph Castro had come on-board. When I say, "At this point," I'm not sure exactly when Castro came. I think that was in the early, mid-'70s; not long before Ed Alpen came to the Lab. Castro eventually used the Bevatron,[29] using the cyclotron for certain kinds of cancer. Whereas, the other program had been [focusing on pituitary gland irradiation for] acromegaly[30] and Cushing's disease.[31] They started out early trying to treat breast cancer,[32] but it wasn't very successful. That's something that Jan[ice] DeMoor could tell you an awful lot more about than I could.

I think I'm slightly out of gas at this point. Is this the direction that you want to go in?

BERGE: Sure. Do you want me to just keep asking questions?

WHALEY: Yes.

BERGE: I'm interested in—you mentioned quite a number of people who were very active at that time. I was wondering if you could tell me a little bit more about them, such as Hardin Jones or Cornelius Tobias and Rex Huff.

WHALEY: I don't know very much about Berlin, Huff, Elmlinger. I know that, when I arrived, Paul Elmlinger had had some serious psychological problems. He left the laboratory very shortly after I arrived. Other than by reading things, most of which I'm afraid I've forgotten at this point, I don't know very much about them.

Hardin Jones was a very early associate of John Lawrence's. It goes back I think to the late '30s, probably. He was a physiologist.[33] I remember Dr. Lawrence telling me one time that Jones was the finest experi-

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28 University of California at San Francisco

29 an accelerator in which protons are raised to very high energy levels, measured in billion electron-volts (BeV), by modulating the frequency of accelerating voltage. Derives its name from BeV + a (connective) + tron.

30 a disorder of the pituitary gland in which too much growth hormone is produced, resulting in enlargement of the head, hands, and feet

31 a disorder of metabolism caused by overproduction of the hormone ACTH in the pituitary gland, resulting in hypertension, striated skin, accumulations of fat on the face and other areas, and various other disturbances

32 by irradiating only the pituitary gland, believing that breast cancer was a hormonally driven disease

33 a biologist who studies the functions and activities of living organisms and their parts

mentalist that he had ever run across. He became, at some point, the scientific assistant director under Dr. Lawrence. He had a group. Ernest Dobson[34] worked under him—Lola Kelly;[35] Margaret White. In later years: Colonel Lawrence Milch, Colonel Alex Grendon.

A little diversion there: There had been a group called "the RZ Group." I don't know where that title came from, but they were military people who came in to study what was called "bioradiology." [The University] offered a master's degree in Bioradiology. I think you perhaps better ask somebody else before we put this into print, but it was more focused on the industrial safety end of things. Colonel Grendon became, I guess it was under [California Governor] Pat Brown, the Coordinator of Radiation Safety for the State of California. Thomas Jukes was another who briefly, before he went to the Space Science Laboratory, came under the aegis of Hardin Jones.

BERGE: What years were these?

WHALEY: Oh, boy—the RZ Group—my impression is, late '40s, or early '50s. Alex Grendon would have been there during—certainly late '60s to early '70s. It might have been longer than that. I can't remember when Larry Milch was there. It was not very long. Tom Jukes was there for a long time, but not at Donner. He was with the biological end of the Space Sciences Laboratory. He may still be. He still writes letters to the editor. He's pretty well-along now; I bet he's pushing 90 at this point. He was not on the tenured faculty, but a step down, in the Division of Medical Physics.

Hardin became very (both he and Dr. Lawrence were politically very conservative) much upset by, and then involved against, the counter-culture movement of the '60s—very much involved and concerned about, "the marijuana question." He had a course about marijuana, which was very popular. He was strongly against marijuana use. He devoted perhaps more effort on that than what was appropriate, considering what his other responsibilities were. He died of a heart attack. It's been a long time ago, I would say it was late '70s, after Ed Alpen came on.

BERGE: Why do you suppose he and Dr. Lawrence were so conservative?

WHALEY: I don't think they became conservative because of anything that went on at the Laboratory. I think they just lived conservative. Dr. Lawrence's background—he grew up and sometimes referred to himself as "a South Dakota farm boy." He was a product of his times. He was appointed to the Regents by [then-Governor] Ronald Reagan, not by Pat Brown. The two of them were apparently quite conservative. Dr. Tobias, I hadn't ever had the slightest idea what his politics were. Jack Gofman was very

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[34] Ernest L. Dobson, Ph.D., was a biophysicist who was [redacted] and became a U.S. citizen. He worked as a physiologist at the Lawrence Radiation Laboratory from 1946 until his death, conducting research on the physiology of the circulatory system.

[35] Lola Szanto Kelly, Ph.D., a biologis[redacted] who became a naturalized U.S. citizen. Dr. Kelly worked at the Donner Laboratory under Hardin Jones. She worked with Ernest Dobson on phosphorus-32 colloids in the liver and conducted research on nucleic acid metabolism and the biological effects of radiation.

much on the other side of the fence: "Any radiation was damaging. There is no threshold there to stop at."

BERGE: You mentioned before, also [Joseph] Hamilton's group. What did you know about Hamilton?

WHALEY: Very little. Pat Durbin[36] is the person who is still at the laboratory, that could tell you everything that you wanted to know, and more, about Hamilton. Crocker Lab was physically separate (it was only a block away), and it was very much administratively separate. They did all of their own salary, administration, etc. They dealt directly with the main administration at the Laboratory, not through Donner.

I think one of the really interesting things is: "What *was* the Donner Laboratory?" In my opinion, there is no definitive agreement or document anywhere that describes specifically what the Donner Laboratory was. I'm not talking about the Donner Laboratory *building.* But as an administrative unit, "What was the Donner Laboratory?"

BERGE: Were there any specifications that Mr. Donner had for what the building was for?

Creation of the Donner Laboratory

WHALEY: Yes. You do know about the plaque on the building? I just refreshed my memory a touch this morning, by looking in the Fiftieth Anniversary booklet. That's another question: "When is the fiftieth anniversary?" It's fifty years after the place started. Well, when did it start? Anytime you care to name, from 1936 up through 1941. For reasons that I don't know, they picked 1937. So that made the fiftieth anniversary, 1987. It's a booklet that you ought to get ahold of. It's not very well-written, but it has a lot of interesting information in it. It has a picture of the plaque, which as far as I know is still down on the front of the building. The plaque says, "The Donner Laboratory. A gift to the Donner Foundation, March 14, 1941, in memory of Joseph William Donner," who was the son of William Henry Donner (who was the head of the foundation). Joseph died at age 37 from cancer. "For the application of physics, chemistry and the natural sciences, to biology and medicine." William Henry Donner's portrait (there's a picture of it in here) hangs in the lobby, or it used to hang in the lobby, of the Donner Laboratory building.

He had become acquainted with the work of Ernest Lawrence, and, through him, the work of John Lawrence. And, because of the untimely death of his son, he was very much interested in Dr. John Lawrence's work. He was very wealthy and had founded the International Cancer Research Foundation, which then mutated into the Donner Foundation. They gave, if my memory serves me, the money that built the original

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[36] From 1951 to 1977, Durbin worked as a chemist and radiobiologist at Crocker Laboratory. For the transcript of the November, 11, 1994 interview with Durbin, see DOE/EH-0458, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Patricia Wallace Durbin, Ph.D.* (June 1995).

wing of the building, and they gave half the money for the addition. Because at that time, there was a lot of competition for state funding. You could get to the top of the [state's funding] list if you could get private support for half the cost of the building.

John Lawrence went to the Donners, and they put up half the money. I think the building cost, it seems unbelievable now, was somewhere in the neighborhood of five-hundred thousand dollars—that's the addition. The original building was under two-hundred thousand. You couldn't do that now. They also gave money, but not the whole thing, for the Donner Pavilion. They gave enough so they got their name on it. And they made various gifts for remodeling the old wing of the building. The clinic got remodeled, and various other things were renovated, after the new wing had been built.

They gave money for the publication of a brochure, which we decided to do in two versions: the long brochure and the short brochure. It was very difficult to get everybody to agree on the text. I spent a lot of time trotting manuscripts around to various people. They were changing other people's changes. As a consequence, to the best of my knowledge, the long brochure never was completed; and the money for it was still kicking around somewhere on campus. When I left, they did do the short brochure, which was the sort of thing you'd slip in an envelope to respond to inquiries about what goes on at the Donner Laboratory.

BERGE: You mentioned that it's not really clear what the purpose of the Donner Laboratory was?

WHALEY: Well, no, I didn't make that clear. I don't think there's any question as to what the purpose of the Donner Laboratory was. But I think there was considerable question as to: "What was the essential nature?," not in terms of its scientific program, but in terms of it as an administrative entity. "What exactly was the Donner Laboratory? Was it just another name for the Biology and Medicine Division of the Lawrence Berkeley Laboratory? Was it, in fact, an organized research unit under the Berkeley campus? Was it an organized research unit, with references statewide?" You can find all of those things supported at one time or another; in various publications that may or may not still be available.

John Lawrence liked to steer an independent course. He, I think it's fair to say, held that the Donner Laboratory was all of those things at different times; whichever particular one suited whatever the goal was he was pursuing at the moment. Great men very seldom, great people I should say, very seldom accomplish great things by following all the rules and procedures, and bowing and scraping to all of the people in line above them. They find ways of getting things done. John Lawrence was a person that did that. People who don't find those ways, generally don't leave any monuments behind them.

I don't think that anybody knows what Donner was, as an administrative entity. I don't know that I understood what it was. It was a chameleon. It changed its nature according to what was most expeditious at the time.

That did not always sit well with the faculty, who were a small fraction of the professional staff of the Bio Med Division. As far as the Donner Laboratory being an organized research unit: there were all kinds of conditions and requirements that there be a faculty oversight committee, although there never was anything like that.

BERGE: When were the requirements put in place?

WHALEY: That, I don't know.

BERGE: By "faculty oversight committee," what does that mean?

WHALEY: That would be a group of faculty. There would be an institute or laboratory, or something or other, and it has a faculty director. It may have a small staff, and then faculty members from various departments (appropriate to whatever field of study the institute has) conduct research there. It's different from an academic department. There are rules and requirements, which I'm pretty fuzzy on at this late juncture. Every five years or every ten years, it may have changed somewhere along the line, a faculty oversight committee would look at the program and the functioning of this institute or laboratory or bureau or whatever it was; really for the purpose of seeing whether or not it ought to be continued, or whether it had outlived its usefulness. That put these things under the control of the faculty. The Donner Laboratory never had that kind of oversight. The oversight committee was John Lawrence.

BERGE: He just went out—

WHALEY: He did his thing.

BERGE: So when you needed to do something that he couldn't do, by being a university entity, how did you go about doing it?

Reflections on John Lawrence as Laboratory Director

WHALEY: If the campus was about to bear down on him, on some administrative bureaucratic requirement that he should do thus-and-such, he would say, "We're part of the Radiation Laboratory. We're the Biology and Medicine Division of UCRL [(University of California Radiation Laboratory)], or LRL [(Lawrence Radiation Laboratory)], or LBL, whatever it was at the time. We don't respond to you people." Likewise, if something were coming down [on him] from the Laboratory (this was less true in the latter part of his regime, when he tended to be more LBL-oriented), he would assume a campus position.

I think that there where probably a flock of factors. I think his desire to be independent (amateur psychology being just as valuable as amateur brain surgery)[37] probably had something to do with a wish not to be totally overshadowed by his brother. Because, in his own right, he accomplished a great deal. He might have accomplished different things

[37] Whaley is apologizing for the armchair psychology that is to follow.

in a different place if his brother hadn't invented the cyclotron. But he did, and John Lawrence came to Berkeley. And he took that situation, and he parlayed it into quite a considerable scientific achievement. But, certainly, very many people, when they hear Lawrence and Berkeley, they think it's Ernest.[38]

BERGE: How does John Lawrence, then, end up directing the Donner Lab?

WHALEY: He had either the skill or the good fortune to associate with him some very capable people, and the good sense to give them their head. He wasn't an easy man to work for. I don't know how the scientists felt. Administratively, he wasn't easy to work for. I remember Jack Schooley often said, "That was the one great thing about Donner—is that John Lawrence let you do your thing scientifically." He hired you because he had an idea of where it was that you wanted to go, and so, he let you go there. If he wasn't interested in where you were going, then he probably wouldn't hire you. I think that was one of his strengths. He didn't always pick top-notch people.

Everybody who works in that end of things makes some boo-boos. I certainly did. The other thing is: he was very good at raising money. He certainly cultivated the Donners. They put, over those early years, a lot of money into the Laboratory. Later, the foundation changed. There again, Robert Donner, who was William Henry's son, lived in Colorado Springs. He was, politically, exceptionally conservative. Whether there was any rapport because of that, I don't know. William Henry Donner was certainly a conservative man. But the foundation, and I don't remember at what point, split. The older members of the family formed (I think it was called), the Liberty Foundation. It remained in Philadelphia, which is where the headquarters for the Donner Foundation was.

The money came out of Donner Steel Company, which was absorbed by one of the big steel companies; either Bethlehem or Republic. It's a wonderful story. I don't know whether it's apocryphal or not. Supposedly, William Henry Donner had been badgered, by whichever of the large steel companies it was, to sell out his little company; which was in Donota, Pennsylvania (named after the Donners). He is reported to have sold out for cash, at the very height of the boom before the 1929 crash. I think it was $20 million. Which, according to the story, he took in currency and put in a safety deposit box.

The crash came. And when things got down to the very bottom, he took his money out of the box and he started buying stock right left and center, with considerable acumen. And that's how the family got very wealthy. That's a good story. I don't really know if it's true or not, but certainly they had a lot of money. The older half of the family, the Liberty Foundation, they were involved in conservative political causes. The other half of the foundation were the younger members of the fam-

[38] Ernest Lawrence's ingenuity and drive made the Berkeley-based Radiation Laboratory a center of nuclear physics in the United States.

ily. There was one of the Roosevelt grandchildren. I met some of the young people way back in the '60s. I think it was when the board of directors came out here. They kept up the medical end of it for a bit.

They moved their headquarters to New York, and then it went to Roanoke, Virginia. I don't know why. I think maybe because the guy that was president lived in Roanoke. I don't know that for a fact. They became interested in health care delivery, the health care for native Americans in the southwest, and gradually pulled away from us. However, before the end, they made a pledge of $20,000 a year for 20 years, to fund a Donner professorship of Nuclear Medicine. That's a whole other long story: the difficulties, the impossibilities. Budinger was the only one that ever got, [a salary] even *close to that*. Berkeley campus is not set up for medical professors on its salary scale. The way these things were set up, called for "research professorships." And there is no such thing as a research professor.

You can't endow a professorship. You can endow it in the sense that you can provide money to support the professor's research, while the salary comes off of the regular budget. But if you endow salary, then they take the equivalent amount of state money away. This was the source of incredible conflict. Because the university, although full-well knowing that, for some reason accepted some of these gifts that required them to do things, their rules prohibited them from doing. They weren't thinking. All of those professors of Nuclear Medicine, with the exception of Budinger, who had a different title, never got appointed.

I'm diverting here from Donner.

I should mention, before it goes out of my head, that Robert Donner's daughter (Peggy Donner Spencer), when I last knew, was still alive—lives in Marin County[, California] someplace. If you're interested in getting into the history of the Donner Foundation, she might be somebody to contact. She often came out here with her dad. There was periodic contact with the laboratory.

I think I diverted about three or four times from what I was talking about originally, which has sort of gone out of my head at this point.

BERGE: You made a couple of interesting [points] about John Lawrence. I was wondering. You mentioned that he would hire anyone who was going to do research that he was interested in. What types of things?

WHALEY: That's something you should ask the scientists. But I think he always kept his interest in blood research, anything to do with that. He had developed the ^{32}P [(phosphorus-32)] treatment for polycythemia.[39] Some say the thyroid,[40] the iodine-131, was the first treatment with radioactive isotopes. I don't know what was the first one. But certainly, ^{32}P was an

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39 polycythemia vera, a disease characterized by overproduction of red blood cells

40 an endocrine gland located at the base of the neck and secreting two hormones that regulate the rates of metabolism, growth, and development

early one, and a very successful one. We did the ^{131}I also—and also, things like Jack Schooley's basic research. There were the people who treated patients and who were interested in the use of isotopes for diagnosing hematologic[41] disorders, and treating them. Then there were the people who liked to do basic research. Schooley would be an example.

BERGE: I heard from someone else that the person responsible for discovering ^{32}P's usefulness for polycythemia was someone else—Hollaender—something like that.

WHALEY: There was an Alex Hollaender.[42] It certainly wouldn't be the Hollander I'm thinking of, who was a hematologist at Kaiser Oakland [Hospital]—but that was decades later. There was a Hollaender who was one of that very early group of researchers. I think his name was Alex, and I think he worked at Oak Ridge,[43] way back when. I don't know. A scientist might know.

Certainly, whether or not Dr. John Lawrence was the very first to do that [form of systemic radiotherapy], he certainly was the one that exploited the heck out of it. He treated a lot of people. They were still treating polycythemics when I joined the laboratory, and continued to do so for quite a good number of years. Have you run across the story where he traveled to Yugoslavia to treat Cardinal Stepinac? That was newsworthy, and the kind of publicity that doesn't hurt fundraising, either.

BERGE: You also mentioned that he was difficult to work for as an administrator.

WHALEY: I think maybe I prefer not to go into that. It's not an unknown phenomenon that people who are outgoing, and aggressively pursuing the building of great institutions, are perhaps not going to get the Nobel Prize for being marvelous supervisors. It's a privilege to have been a part of all of that. But it wasn't always a pleasure.

John Gofman: Research and Controversy

BERGE: We talked a little bit about Hardin Jones. Can you tell me a little bit about people like Gofman and Tobias?

WHALEY: I don't know an awful lot about Jack Gofman. I guess he was just a certified genius. He had his Ph.D. [in Nuclear/Physical Chemistry]. He worked with Glenn Seaborg[44] on the plutonium work.[45] Then at some point—I

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41 relating to the nature, function, and diseases of the blood and of blood-forming organs

42 Dr. Alexander Hollaender became the director of the Biology Division at Oak Ridge National Laboratory.

43 During World War II, the Manhattan Project had built a vast complex of highly classified facilities in and near Oak Ridge, Tennessee, to process and enrich uranium for use in atomic bombs. The Atomic Energy Commission assumed control of these facilities upon its creation and, today, they belong to the Department of Energy.

44 U.S. chemist, born 1912, professor of Chemistry at the University of California, Berkeley, discoverer of several heavy elements, and Nobel Prize recipient in 1952. Seaborg later served as Director of the AEC.

45 From 1941 to 1943, Gofman developed two processes for separating plutonium from the uranium and fission products of irradiated fuel. This work, conducted with Dr. Glenn Seaborg, was the precursor to full-scale plutonium production at the Hanford Nuclear Site in Washington.

think this must have been in the early '40s—I guess it was, he went to medical school and got an M.D. [in 1946]. As I recall, he won the "Gold-Headed Cane," because he was the top student in his class.[46] I think he went to UCSF. He came back to the laboratory, and formed a group. Again, you should talk to the scientists, but he was interested in lipoproteins.[47] There were those that thought he was a little obsessed with them. Looking back on it now, it appears that he probably was decades ahead of his time.[48] One person you could talk to would be Tom Hayes.

BERGE: H-A-Y-E-S?

WHALEY: H-A-Y-E-S. Tom was a grad student of Gofman's. He got his Ph.D., and stayed at the laboratory. He did not stay in that field, however. He was deputy division head under Ed Alpen, and retired a couple of years ago, and still comes in a little bit. He lives in Oakland. Thomas L. Hayes was interested in diet, as it related to heart disease and lipoproteins.

Another one of his grad students was Alex Nichols, who is a professor and is still, currently, now on the staff of the Laboratory. Also, Frank Lindgren. I don't know if Frank is still there or not. Frank was the one who really developed ultracentrification[49] as the means for measuring the various classes of lipids.

I think that, in some ways, perhaps Gofman's greatest impact might have been through the people that he trained, and what they did later. That's a judgment I'm not really qualified to make. It strikes me that some of those people, who were associated with him in the early days (I guess Hayes and Nichols got their Ph.D.s just after I got there, the rest of them had them before), had a big impact on the field. Gofman himself, as you probably know, in 1963 went out to Livermore to start up, at the AEC's request, a biomedical program out there.[50] He took some of his group with him; not Lindgren, Hayes, and Nichols. Bob San Souci, who I mentioned earlier, was an administrator who went out there with him.

BERGE: Why did they leave? Were they asked to leave?

WHALEY: Gofman was asked by Charlie Dunham.

BERGE: Who was that?

WHALEY: Dr. Charles L. Dunham. His is a name that would figure prominently in Donner history. He was the head of, what I think they called, the Division

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46 At the UC San Francisco Medical School, the Gold-Headed Cane was given to the senior medical student with the best promise of being, in Gofman's words, a "true physician."

47 any of the class of proteins that combine a lipid with a simple protein

48 In 1948, a paper published by Gofman et al. opened the way for the discovery of the sequence of low-density lipoproteins. For his work on heart disease, Gofman received the Stouffer Prize in 1972 and in 1994 was a guest speaker at the American Heart Association. See "Heparin and Lipoprotein Research With Human Subjects" in the Gofman transcript.

49 the use of a centrifuge that rotates at a very high speed and separates and sediments the molecules of a substance

50 See the footnote on the new Livermore division, on page 3.

of Biology and Medicine of the AEC [between 1963 and 1967]. He was our funding source. There was, and this I remember very clearly—it was 1963, and there was concern about fallout [from atmospheric atomic testing],[51] and the biological impact of fallout: "What was happening to us?" The AEC didn't have as much of a research program as was desirable.

And so, Dunham asked Jack Gofman to go to Livermore and set up a program there to study fallout.[52] Remember, Livermore is a weapons laboratory. And this may have been more than to just study fallout. He went there because Dunham asked him to go there [to Livermore, California] and do it. Gofman was the kind of man who inspired very intense loyalty in his staff. They went with him because they wanted to continue working for him. He had AEC support. The AEC support for Dr. Gofman wasn't going to continue at Berkeley when he wasn't at Berkeley anymore.

BERGE: How did Berkeley react when he left? What happened to the funding?

WHALEY: I don't remember, but I think some of it stayed. Lindgren got some. Nichols got some. Gofman had some NIH grants. I think Lindgren also had one at that time. He may have been more-or-less self-supporting. Nichols, that's an interesting question. I don't think the NIH grants could go to Livermore. This was when these were all on-campus in Berkeley. They couldn't do it at the Laboratory. I think Nichols may have inherited one of Gofman's [NIH grants] as a substitute P.I. [(principal investigator)], and then went ahead and got one on his own.

I don't think that there was any animosity generated by the move. John Lawrence was a very forceful, vigorous individual who was pursuing his vision of the Donner Laboratory. And Jack Gofman was a very forceful, aggressive individual who was pursuing his vision of a research program. Sometimes those things collided. I would suspect, but I don't know, that neither one of them was terribly unhappy that this separation had taken place. Certainly, a program in that scientific area (lipids) continued at Donner. It has certainly been on a long-term basis through Lindgren and Nichols. Tom Hayes, although he had been a grad student of Gofman's, really didn't stay in the lipid area. He ended up in electron microscopy.[53] That's what I associate him with.

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51 radioactive debris from a nuclear detonation or other source. Fallout is usually deposited from airborne particles.

52 For more discussion of the genesis of the Lawrence Livermore fallout studies program, see "Livermore Biomedical Division; Conflicts with John Gofman (1962–72)" in the John Totter transcript (DOE/EH-0481, September 1995).

53 use of an electron microscope, a microscope of extremely high power that uses beams of electrons focused by magnetic lenses instead of rays of light; the magnified image is formed on a fluorescent screen or recorded on a photographic plate.

Cornelius Tobias

BERGE: What about Cornelius Tobias?

WHALEY: Tobias, as I recall, was a graduate student from Hungary, and was trapped here by the war, [a refugee from Hitler]. He became associated with Lawrence at the end of the 1930s. It's awfully hard to say.

Some feel he was the most brilliant scientist in the whole history of Donner Laboratory. That would be very hard to say. But certainly, he would be one of the top contenders for that. He did, really, much of the basic research that led up to the use of the cyclotron and the bevatron, medically. That's what he will most be remembered for. I think, very likely, that's what he would want to most be remembered for. He was very widely respected in the country. He had very good success in generating money; particularly from the National Institutes of Health. He told me, one time, because I dealt with personnel, that he didn't like "technicians," because they always went home at five o'clock.

He came out of the central European academic tradition, the: "Herr Doctor, Professor." He expected very hard work from his graduate students, and he much preferred to use graduate students. That was, to put it frankly, the source of some conflict within the faculty. Because many of his graduate students took an inordinately long time to get a degree. And it was the feeling that it was because they kept getting diverted to work on this project and that project of Dr. Tobias's, instead of their thesis. On the other hand, the grad students of Cornelius Tobias usually did very well in the world, because of Toby's reputation. Anybody that got a Ph.D. with him was well-vouched-for when they went out to look for jobs.

He is now retired, and I believe he's living in Oregon. I had a fair amount of contact with him. He was a nice man. He was the sort you enjoyed working with. He too, had his vision of the research that he wanted to do. And I think there was some feeling, occasionally, that perhaps the research that he was doing with money from Source *A*, may not really have been quite the research that Source *A* money was given for. I don't think that was one of his primary concerns: that the funding and the research should be very tightly related. That was not uncommon.

We accomplished a good deal in this country by operating in that mode. But the trend has been quite in the other direction: that of rigid accountability. You can make a good case for that. If money is given for a certain purpose, that's what it should be used for. But you can also make some historic allusions to money that was given for *X* and was used instead for *Y*, and look what *Y* turned out to be. A little gain and a little loss as you go along. He was a very bright and able man.

BERGE: What about on the administrative side, what about Bob San Souci?

WHALEY: I was very fond of Bob. And he was very good to me; to help me to learn the ropes. He had been in World War II—Army officer, lieutenant colonel. I'm not quite sure how he got to the laboratory. I would put his arrival in probably, the late '40s: '47, '48, somewhere in there. If you look at the

administrative history of the laboratory, I think that Donner, or Bio-Med, was the only Division at that point that had somebody specifically hired and designated to do that kind of administrative work. This is really an interesting thing. After all, there was this big administrative structure centrally: Wally Reynolds, and Rex Barton, and all of those purchasing people, etc. Most divisions didn't have people like Bob.

Although I can't speak for the very early times, but a little later on, they in fact did have people like that. But they didn't call them administrators. For the most part, they were scientists. Bill Nolan was, in effect, Louis [(pronounced "Louie")] Alvarez's administrator, but he was classified as a scientist. To some extent, Earl Hyde had an administrative role in chemistry. When I was hired, I was a replacement for somebody else. So, sometime between the time Bob came on, there were now two administrative people in Bio-Med. There was some negative feeling by scientists in other divisions of the laboratory that that was wrong, and that you didn't need that, and you shouldn't have it. Gradually, that [formal administrative] structure evolved in other Divisions. Because, in fact, you did need it—in fact, you did have it. It's just that you masked it [by having scientists do administrative work part-time] and, whether deliberately or not, and you didn't call it that. Long before I left, every division had a division administrator, who was so designated, plus supporting administrative personnel in the division.

BERGE: Why do you suppose Donner was the first?

WHALEY: I think because John Lawrence was trying to carve an independent way. It gave you a little more flexibility; plus the fact that we had a good foot in the campus, which was not true with the other divisions. There weren't really anything like the "odd funds" in the other divisions. They all had ties to campus departments through the faculty members that had joint appointments in Physics or Chemistry. But that wasn't an arrangement at all like Donner. Why was that? I don't know why that was. It just grew that way. Perhaps because, in Donner, they were inventing a discipline that later became the subject of the academic Division of Medical Physics.

I think it's one of the few cases that I can think of where it worked in that direction. Usually, you had a historic scientific discipline, and that spawned an institute or a bureau or a laboratory, whatever.

But this was the other way around. You were creating research on topics that had never before existed. And now you had to have some kind of an academic unit to teach it—it certainly was true. And when Igor and I were both there, there was certainly criticism of there being two such highly paid [administrative] people.

Budgeting and Staffing

BERGE: What kinds of things was Bob San Souci doing?

WHALEY: He looked after the budget—both in terms of costing—that's what budget people do at the division. They cost proposals and they monitor expenses.

They tell you, "Yes, you can afford this kind of person. No, you can't afford that kind of person with that budget." They're really, in a place like Donner, they're an interface between the scientists and the people who are really accountable, who certainly are—the central administration. You need somebody to interface with the plant and facilities people.

It puts a unit at a tremendous disadvantage, if you don't have somebody at a level that can speak to decisionmakers in the departments you depend on for service support—if you're in a situation where you either have to take a scientist to deal with this, which is what they did to some extent on the hill[54] (before they got into the present structure), or you have to have a clerical person. Clerical people don't carry any weight. So if you have a recurring problem with your ventilation system or with the electrical system, you're not going to get very far if you have a clerk calling up— somebody at this level—and saying, "Gee, you have to get out here and do something about it." This guy has a budget to worry about, and he's got people at his level to deal with.

I think it evolved because there was a real need there. It didn't matter what you called it, whether you covered it up, or whether it's as Donner did and was explicit about what these people did: personnel, salary. Bob was very good on the "thing" end of things. On the budget, he was remarkably good. Somewhere, and I suppose the people that followed me threw it away, I discovered and saved a device he used to monitor the AEC budget. This must go back into the '50s. It was sheets of columnar accounting paper, and what was obviously something cut out of a huge sheet of cardboard; probably used for a technical illustration. All of the numbers were filled out by hand in ink: July, August, September, October, November, and all the budget numbers.

Contrast that with the computer-generated spreadsheets today. It's just remarkable what has happened in one person's lifetime, *less than* a whole life.

Bob probably was less strong in the people end of things. That's what I ended up in. I was good in the budget. I had the math background. I wasn't particularly interested in dealing with the plant and facilities. I had to have Igor do that, or hire somebody to do that after he had left.

BERGE: What do you mean you were strong on the people end of it? What did you do?

WHALEY: Not for *scientists*, now. We're talking about support people, clerical people. For years I interviewed every applicant, along with the supervisor—not sitting in the same room, but we both interviewed them. A joint decision: "Who would get hired out of a pool of applicants for a particular position?" I did the staff work for salary adjustments. We had a salary committee that actually made the decisions. But I prepared all of the

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[54] Lawrence Berkeley Laboratory Hill refers to the hillside, overlooking Berkeley and San Francisco Bay, where the Lab is situated.

stuff, and it never really came to a vote. So I don't know whether I would have had a vote or not; probably not. But there are ways that you can influence the committee toward a particular objective.

BERGE: For example?

WHALEY: Sometimes you're not successful. In the very early days, there was tendency not to hire people as "Technicians." There was a tendency to hire people, because it carried more prestige, as "Junior Professionals." There was a series that the lab had for many years. Put in the name of a discipline: Biologist, Physicist, Chemist; with grades P-1 up to P-8 (actually there was P-9 and P-10, off the end of the scale). It was so much classier to be a "Biologist P-2," than to be a "Laboratory Technician." But it didn't really reflect what they were doing.

Gradually, that changed, except in a few cases. These people ended up being very badly treated on salary, because they didn't get general adjustments, and the technicians did. I kept trying to persuade the committee to change these people. They did, but it took me a long time. It wasn't until Dr. Alpen came in that I was successful in doing that.

Another example: if somebody pipes up and says, "I think that *X* ought to get a 15 percent increase," you think, "Now *X* works in *your* group, right?" You don't say that, but you think that, because nobody wants to contradict. You say, "Let me just point out people: that a minute ago, you only gave *B* six percent. And *B* and *X* have been equal [in their annual raises], if you look at the record for the last seven years, and nobody has put up a justification for changing that now." Then you can get people to back off—not always, but often you can do that.

It's not just a question of filling out forms, and sitting there and taking the minutes. You can take an active participation. Even if push comes to shove, [however,] you're not [ultimately] going to make the decision.

What else? That was really mostly what it was. It's interesting, now, looking back over the years. There aren't very many administrators in the division. There was Bob and Igor and there was me, up until my retirement. That is 40 years. Bob became very heavily allied with Jack Gofman.

It's a kind of situation where: yes, you are supporting everybody in the division. Everybody is equal, but some are more equal than others. You do a little more support here. Don't ask me why. I have no idea what started that. But it was certainly true that Bob was heavily associated with Gofman and did a lot of things for him in an administrative way, that other groups had to pretty much do for themselves. Because there wasn't enough of us to go around to do that sort of thing. The same when Igor Blake became associated, in exactly the same way, with Tobias. And he did a lot of things for him that did generate some complaints, every now and then. I didn't really become associated that way with any one group. I didn't think you could point to anyone and say, "Baird's showing favoritism for so-and-so, and doing extra work for

them that he doesn't do for me." I don't think it was because I was good or pure or anything. It's just that that wasn't my vision of the job.

BERGE: What about Dr. Born?

WHALEY: He was a fabulous man. He was an attorney first, and practiced as an attorney for a short time, and then he went to medical school—got his medical degree. He came out here and was working. I'm not sure if he was still in his residency, or if he was actually working for Kaiser [Hospital]. He died a number of years ago. His wife [(widow)] is still living in the area. She was a physician too, and worked in the Student Health Services at Cowell [Hospital] for many, many years. I think, perhaps, she may have even been there when he was at Kaiser. I don't know that.

John Lawrence was looking for somebody who had administrative skills, and was a medical person. Jim [Born] just fit the bill ideally. It was probably through the University, through Bill Donald, Sr. (not Bill Donald, Jr., who was in practice in Berkeley for so long and just retired a year or two ago). Bill Donald, Sr. was the university physician. And I think he may have put John Lawrence and Jim Born in touch.

Jim was very good. He was very smooth. He could interact with all kinds of people on all kinds of difficult problems, and work out resolutions and compromises. Whatever that quality is, he had it. In something like that, I think the medical degree helps; not because of what he learned in medical school, but it carries a certain amount of clout with it. Bob and I were given the nuts-and-bolts, and the routine things. Jim was, for example, good on dealing with the campus architect, and he had a savoir-faire.

He was also very active in both the clinic research program (saw lots of patients), and also, although he had a less direct involvement patient-by-patient, in what we called the Pituitary[55] Irradiation Program. Not Joe Castro's program, but the original one: Cushing's [disease] and acromegaly. He was a really nice man. He was appointed [Donner Lab] Director, as you probably know, after John Lawrence went on the Regents, and it wasn't permitted that John Lawrence continue as director. You can't be on the Regents and also have an administrative position. Although, I think there was the one exception: that was a professor at UCSF.

Jim became director, and I think he was not a very successful Director. It pains me to say that, because I thought the world of him and he was very, very nice to me. I worked very closely with him. During the time when John Lawrence was director, I worked much more closely with Jim than I did with John Lawrence. We had our offices right next to each other, and we were in and out all the time. Maybe he was too nice; I don't know. I think that he lacked the ability to say, "No," often enough. That's one thing about Ed Alpen, he knew how to say, "No"—

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55 the small gland attached to the base of the brain, constituting the master endocrine gland affecting all hormonal functions of the body

for the most part, in a way that people would accept it, although they might not be thrilled.

Jim did have that difficulty. Also, I think there's a difference between *managing* an organization, and *administering* an organization. I think in managing an organization, you are placing a good deal of emphasis on planning, and the future, and the changes that you have to make in order to remain at the forefront of conditions that are always changing. Administering is taking what you have and seeing that it runs smoothly. I think that maybe, in Jim's reign, there was too much administration and not enough management. Because he was forced out, as you may or may not know.

When Andy Sessler became Director, after Ed Macmillan and Andy and Earl Hyde pushed him out, that's what they did. They felt he wasn't the one, because the program was not running successfully. There was a big review by the AEC for three or four days, and the programs were presented. And a lot of programs were held to be no longer strong and viable. And scientists were laid off.

BERGE: How do you think that happens? Do you think that was only because John Lawrence left, or was it something that was developing over a long time?

WHALEY: I think it was developing over a long period of time. Because I don't think in just three or four years that you're going to have a going-down-hill. It's true, I think, that many scientists, looking back on it, that they have a period of intense creativity and then they continue doing the same research for the rest of their careers. And a point comes when you can't continue in that area successfully. I think that's it. Obviously, that's a gradual process. He on whose watch it becomes very apparent, tends to get blamed.

BERGE: You mentioned the pituitary program several times already in the earlier period. Can you talk a little more about that?

WHALEY: That's what—again you really need to talk to the scientists. The pituitary is a nice target for the cyclotron because it's symmetric and you can do the rotation. There were all kinds. It was thought that breast cancer might be dependent on hormones, that were either secreted by, or in one way or another controlled by, the pituitary. There were these other conditions where, in fact, the pituitary was involved: acromegaly and Cushing's [disease]. The technology was such (as much as I understand about it) that they weren't really able to deliver odd-shaped doses of radiation—doses of radiation to odd-shaped tumors that weren't symmetrically located [(centered at the axis of a rotating radiation beam)].

But you could irradiate the pituitary, because of its symmetry. You ended up with the heaviest dose concentrated on the pituitary, and then a double cone[56] on either side. Of course, you are trying to spread the radiation that doesn't go to the pituitary, out over as much area as possi-

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[56] double cone—conically distributed radiation dose to tissues between the beam source and the pituitary, one on each side of the patient's head

ble. That was a major thrust of the program, from 1954 right up until several years before the time I left. The 184-inch [cyclotron] was converted to the ALS. And, I guess, the Bevalac[57] isn't there any more. I hear that Dr. Castro is still doing therapy at [UC] Davis. And Dr. Fabrikant, who inherited the—it wasn't pituitary, because he was working on arteriovenous[58] malformations (AVMs)—has died. I assume the program is defunct. Because, unless they go some distance, there isn't a machine to do it on anymore. It was really a centerpiece for 30 years.

Patient Care in the Donner Clinic Research Program

BERGE: What kind of contact did the scientists have with the patients? And what kind of contact did you or others have with the patients?

WHALEY: The people that had the greatest contact were our physicians. There were a whole flock of them over the years working backwards from the time: Jack Fabrikant, who had the Arterial Venous Malformation program. Before him, John Linfoot was for many years chief physician at the pavilion. Before him, we had a variety. C.C. Wang was there from Massachusetts, and John Constable. I think they may have both been from Massachusetts General Hospital (MGH).

BERGE: Constable, C-O-N-S-T-A-B-L-E?

WHALEY: Yes, just like the famous painter. May even have been related to him. Richard Carlson, Rollin McCombs—that goes way back.

BERGE: Could you spell his name?

WHALEY: McCombs, Rollin—that must go back to the '50s. At that time, it was fairly typical for younger physicians to come in and spend a little time, and then go on to something else. Linfoot was there for a long time.

BERGE: These people—you said they were physicians.

WHALEY: They were physicians.

BERGE: They did research too, didn't they?

WHALEY: It was a research program. At least, it was supposed to be a research program: simply the conduct of the therapy, the evaluations of the patients, the follow-up. That was one of the big key things, was the follow-up; to bring people back. If you don't do that, you're just running a clinic. But if this treatment really is a research program, then if you don't bring the people back and find out how they did, how do you know whether the thing was successful or not? Follow up; and most of these people, essentially all of them, came back at their own expense.

BERGE: How would the follow-up procedures [be] executed?

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[57] a hybrid of two accelerators: a **beva**tron and a super-HILAC or Heavy Ion Linear **AC**celerator

[58] pertaining to or affecting an artery and a vein

WHALEY: Again, you'll have to check with somebody, but it was understood at the outset that they were expected to come back. There were *some*. Not all of them had the money to do that. I honestly don't—I don't think we paid. There may have been a rare case where we paid to bring them back, because of some particularly interesting aspect. Or we would work with their physician. We only took people on referral from physicians. You couldn't call up and the Donner Lab and say, "I'd like to have my pituitary irradiated, because I think it would do me a lot of good."

I think there was a tendency for the referrals to come, not just randomly. But there were a number of physicians who referred more than one patient. And they'd work with the physician to do whatever kind of tests and measurements that they wanted, so the patient wouldn't have to come here. I have no idea what the proportion was. That wasn't something I would have become involved in. Tom Budinger is a physician that comes to my mind—that was involved with patients and did research, in the sense that I think you mean: putting on a white coat and going into the laboratory, and there's no patients there and you've got animals or test tubes or whatever it is. Most of the physicians didn't do that. This probably wasn't true of the Hematological Group. They did some of what you're talking about. But the people with the Pituitary [Irradiation] Program, really, that was the research.

BERGE: The Hematological Group, that was the earlier?

WHALEY: That's the Jim McRae and Myron Pollycove and Tom Budinger and Rex Huff and Nat Berlin, and the whole—

BERGE: I didn't ask you about people like Budinger or Pollycove yet.

WHALEY: Let me just go back. You asked me what—about the contact the patients had? We talked about the physicians, the scientists. The patients did have contact with both. There was a team of people at the cyclotron who ran it. The physicians did not run the machine; the physicians were in charge of making sure that the patient was properly positioned. But the people who pressed the buttons and looked at the control panels were not physicians. Those people, if there was one characteristic about that group—and I get a little angry when I read in the paper about the heartless experimenters subjecting people without their consent, as guinea pigs. I know there was some of that that was done. I don't think there was very much of that that was done down here. But it certainly did happen elsewhere.

The care that people got, the personal attention that they got from not just the physicians, but the administrative personnel . . . I didn't have a lot of contact with patients, but I had a little. It was quasisocial contact. The secretarial people, the nurses, the technicians, the people at the cyclotron . . . And I don't know if they still exist anywhere—the letters that people wrote in probably would be in their charts. Those charts have been preserved, and I suppose they are in San Bruno[, California]. You'd know those people really appreciated the kind of care they got. It was remarkable. If we could convert medicine in the United States to that kind of care—nobody could afford it, but it was fabulous.

BERGE: The general overall feeling was that everybody was really dedicated?

WHALEY: Yes. Dedicated to what they were doing, and really cared about these individuals as people; not this picture that you get out of some of the accounts that they thought they [(the patients)] were ciphers, and thought no more of them than they do of the mice running around in the experiment.

And we had nice people working for us. This wasn't something artificial that was being put on to make the patients like us. These were good people, who were interacting in ways that you want to with sick people. They were dedicated to what they were doing. I think it was a fabulous model for good medical care.

BERGE: Before I get back to the other question, what kinds of collaborations did Donner Lab have with other hospitals or medical facilities?

WHALEY: In the early days, a lot of patients were referred in by local physicians. We're talking, now, primarily about the blood dyscrasias,[59] the polycythemia, and leukemia and so forth. The physicians in the Berkeley–Oakland area: some of whom may have come in and worked half-a-day-a-week in the clinic, or maybe not, or knew about us because they did physical exams on the employees, or knew about us because they read the literature, whatever—it was a much more informal kind of thing. There was no question of any kind of payment to those people.

I understand *now* it's a fairly typical thing, that you make a payment to a physician that refers; because he's losing revenue. Here's my patient *X,* and I could treat them with standard therapy *Y* and I could make *Z* [amount of dollars]. Now I refer him over to you, and I'm not going to make *Z.* You're going to go ahead. I don't know much about payments. But I do know, I remember a discussion just before I left, that they were including that in the budget. And an assertion was made by somebody that that's perfectly acceptable to NIH. It was a payment of 1,000 dollars.

I have gone off the trolley again here.

BERGE: Oh, collaboration with others.

WHALEY: Collaborations. There again, that's a hard thing to dredge up. There wasn't, I don't think, a lot of early collaboration with UCSF. I think there was jealousy involved there. I think there was turf. That tended to dissipate over a very long period of time. Certainly, in the early years, UCSF believe[d] that only at the medical school, and *only* there, can they treat patients. That is a fact, and it crops up; but maybe not in official histories.[60]

However, I think in the later years there was a good collaboration. That's another story: George Brecher and Shirley Ebbe and all those. That's modern history now (of the Department of Laboratory Medicine at UCSF). We had a very close relationship, during Ed Alpen's time. We

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59 conditions characterized by an imbalance of the constituents of the blood

60 See "Conflict Between University of California San Francisco and Berkeley" in the Gofman transcript.

had very good and close relationship with them, and good relationship with the radiology department over there—Alex Margulies.

BERGE: Is that M-A-R-G-O-O-L-E-S-S?

WHALEY: I'm not sure. The name will come—the Radiotherapy Department, the department that Joe Castro was a part of. I can't think of the name—Ted Phillips. We're talking about the time between 1976 and 1986. [When] we go back to the 1940s and 1950s, it wasn't quite such harmony.

BERGE: In what way are they cooperating with each other now, in terms of: what kind of research is Donner doing that they're *not* doing?

WHALEY: I couldn't answer that, I don't know. But Joe Castro—when that program was going full-force down at LBL, Joe Castro was a member of that department in San Francisco who was essentially dedicated full-time to doing this work. He continued to be paid in San Francisco. We paid *them,* but they paid *him.* That's part of the administrative history. We wouldn't have been able to pay him, because his salary's too high. These rules that govern salary caps in a National Laboratory, you wouldn't [have] been able to do it.

The problem that we alluded to earlier about the inability to get, so-called "research professors" of Nuclear Medicine at Berkeley—apart from all the other things, you couldn't pay a physician on the Berkeley salary scale and get them here. What we ended up. . . Also, there's a whole process of hiring professors—and this is a fact, whether we admit it or not—there is a tendency for the Ph.D. Berkeley faculty types to feel that physicians are not quite scholarly: mechanics, plumbers. That's a difficulty in getting them. Even if that didn't exist, you couldn't possibly pay them.

So what we did was: find a friendly department in San Francisco, put them on according to the San Francisco [salary] scale, and then somehow get money over there. That's how we solved the 20,000 [dollar]-a-year, 20-year Donner professorship problem. That turned out to be Shirley Ebbe, who I think is still working there because I see her driving towards the laboratory in an old classic Porsche. That's how we worked that. That involves a good relationship, which I think now exists at UCSF. But it didn't in the early years.

BERGE: What was it like in the early days?

WHALEY: I really don't know, because I wasn't high enough up in the hierarchy to be privy to this; other than they (UCSF) didn't want medical treatment to go on over here in Berkeley—certainly, not any treatment that was being given by a physician who wasn't on the faculty. It was very much of a turf kind of thing, which they may really have believed or they may not have—that this was: they were looking out after the interest of the patient. But the equipment was all over here. And there were people— Robert

Stone was one[61]—in the Radiology Department at UCSF, that came over and collaborated for a time with John Lawrence. We took care to have a consultant: Dr. [Bert] Low-Beer,[62] from some department over there for the Pituitary [Irradiation] Program. But there was a jealousy, a turf kind of thing, going on. I don't know if there's anybody still left around that would be able to give you examples. This was a long time ago.

BERGE: What kind of collaboration did Dr. Lawrence have with Dr. Stone?

WHALEY: I think it was in the—it had to do perhaps with neutron therapy. Again, I don't know. Jan DeMoor is somebody that would know about that sort of thing.

BERGE: What was Low-Beer?

WHALEY: Low-Beer. B.V.A. Low-Beer. I think he was a radiologist; he may have been a radiotherapist. He was a consultant with the pituitary program. I haven't talked about a couple of people. I can't remember who they were.

BERGE: I was wondering about the other collaborative hospitals. Were they only from the areas around [the area]?

WHALEY: There must have been. But I can't remember. There was a tendency, I think, to form ties to particular physicians who might have sent out one or two patients; and the patients did well under the therapy they got here. And that encouraged the physician, wherever he was, to refer others. The patients for the pituitary program came from all over the country, and some of them from outside the United States. Because, and I'm sure that this is correct, for a substantial part of this time there was only one other place in the country where you could get this therapy. And that was at MIT.[63] The man's name was Kjellberg.[64] I think his first name was Raymond, but I won't swear to it. They did it.

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61 A pioneer in radiation therapy, Robert Stone, M.D., had conducted human radiation studies before World War II. He was an early researcher at the Lawrence Radiation Laboratory and became a major figure in radiobiology research. When Joseph Hamilton began operating his 60-inch cyclotron at Crocker Laboratory, Stone requested that fission products be made on the cyclotron and that their fate in mammals be systematically studied in small animals. That information would be used for radiation protection proposes. In 1942, while chairing the Department of Radiology at UC San Francisco's medical school, Stone was recruited to lead the Medical Division of the Manhattan Project, overseeing all biological, medical, and radiological protection research. Accordingly, he moved to the University of Chicago, where he served as Associate Director for Health under Arthur Compton. In the 1950s, after serving in the Atomic Energy Commission, Stone returned to his post at the UCSF as head of the Department of Radiology. Under Stone, UCSF acquired a 70-MeV synchrotron for conducting therapeutic research.

62 a medical researcher at the University of California, San Francisco who died prematurely of leukemia, probably brought on by overexposure to radiation in the course of his career, which included work with radiophosphorus in England. Low-Beer, a physician, had been trained in his native Czechoslovakia. He served as an associate professor of Radiation Therapy before heading the Radiation Therapy Division of the Department of Radiology at UC San Francisco.

63 Massachusetts Institute of Technology, Cambridge, Massachusetts

64 A neurosurgeon now practicing nuclear medicine at Harvard. As a medical student at Harvard, Kjellberg trained under Cornelius Tobias, who was serving as a visiting professor.

We didn't charge patients. He did it for a fee. We did come, in the later years, to bill their insurance carriers. We would accept whatever the carriers would pay. And if they didn't have insurance, that was okay. We didn't charge the patient. That [policy] was a source for a lot of administrative garbage, too. But that's another whole long story. I can't come up with, and Jan might, the kind of collaboration that I think you're talking about; where it's almost the kind where you might have had a written agreement: "We will participate in your pituitary study and agree to send to you any patients you want to accept of the following sorts." I don't recall anything of that sort.

We were widely known; we published the results in the literature. It wasn't any secret that we were the place on the west coast that did this therapy. [There was one collaboration, I remember, with Highland Alameda County Hospital, where Donner ran the isotope unit for many years.]

Changes in Laboratory Organization Under Ed Alpen

BERGE: There are two people that I didn't ask you about before: Dr. Budinger and Dr. Pollycove.

WHALEY: Myron Pollycove was very early in my time at the laboratory. I would put him right in there in the '50s. There are ways of checking on this. I don't remember how long he was there. Was it 10 years? And then he moved to San Francisco, but kept a, kind of a foot in his research program here for a while. And then, it sort of dwindled away.

He was a hematologist. He was still using the device called the Monster (the Multiple Port In-Vivo[65] Counter). They were scintillation counters;[66] that could be positioned over four or five areas of the patient's body to see where the radioisotope was going. Again, you really would need to talk to the scientists about that. Pollycove was in charge of the Multiple Port In-Vivo Counter.

However, Dr. Lawrence was not one to have a formal group structure like [the one that] Ed Alpen put in. When Ed came, he said, "Okay, I've worked on this, and I want to see what you people think about this. These are the four groups I have identified"—I think there were four—"The group leaders are going to be Joe Blow and Bill Brown and Mary Smith, and whoever the people will be." And he would name them: "This is the research." I think people pretty much accepted that. The group leader was the group leader. He was appointed as the group leader and he had specific duties, in addition to whatever his research obligations were.

John Lawrence would never have dreamt of doing that. He didn't organize things that way. The organization chart *was* John Lawrence. And then a line that went 40 feet in either direction [(branching left and right,

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65 inside the body

66 devices that measure radioactivity by registering the number of scintillations (ionizations of a phosphor struck by a photon or energetic particle) it produces

from a single descending line)] with everybody having a little box underneath that. That's just the way he was. When I say Pollycove was in charge of that—well, John Lawrence was in charge of everything. In terms of what we might think of now, with the kind of structure that Alpen put in; yes, Pollycove was in charge of that.

There were other people working on it—but the names, the time frame . . . There was a doctor named Rashed Fawwaz. I know he was there for quite a while. But without referring to some documents, I couldn't say whether he came while Pollycove was there.

Saul Winchell—that was another name. Saul was a really bright, able guy, who left to go into private industry. I don't know what the heck he's doing now, other than he has a winery, Gan Eden [(Hebrew for "Garden of Eden")], and they make very good wines. There were a series of them. I think Saul followed Myron. Myron followed this (Nat Berlin, Huff, Elmlinger) group. Jim McRae followed Saul Winchell. In a sense, I don't think we can say that Budinger followed Jim McRae. They were there at the same time.

I think Tom [Budinger] came from Livermore. I may be repeating myself here. I don't know what he was doing out there. He was a man that would have been at home in the days of the Regency. He was an oceanographer–physicist–physician, a man with interests in and capabilities in a whole wide variety of scientific fields. I think that perhaps the criticism that some have made is, that his interests are so wide that it precludes some of them from being very deep. Certainly, he was a ball of fire and bundle of energy—interested very early on in NMR [(nuclear magnetic resonance)], which is now called MRI, [(magnetic resonance imaging)] because we don't like to get the "nuclear" name in it. Really, the whole medical end of it, almost the first time there was anybody, below the "Directors" level, that was overseeing the whole thing: the clinic, the hematology, the pituitary, and the Castro therapy thing—exceptionally bright guy.

Again, sometimes hard to work with, sometimes short fuse. An absolute, a certified genius, and the sort of person that drives you nuts when you're trying to do any kind of orderly management planning when you have to have in mind, "Who will succeed *X*, if *X* should step in front of a bus or decide to move to Florida?" or whatever. I don't think that anybody has ever come up with a person that could succeed him; there isn't anybody. The mold got thrown away. [Had he left,] we really would have to reorganize that whole thing. There's a whole flock of stuff in there, instrumentation—the name escapes me now. It's very sophisticated. It's beyond CAT scans.

BERGE: Imaging?

WHALEY: Yes, it's an imaging technique. They were, when I left, trying to get the resolution down to 3 millimeters, or 1 millimeter. It is in use now, clini-

cally, in several places. I can't think of the name. [Positron Emission Tomography, PET][67]

BERGE: You mentioned that there wasn't very much organization, for quite a long time, until Dr. Alpen came on. What was it like for you, working as an administrator in unorganized fashion?

WHALEY: I became used to it, right off the bat. I suppose, everybody else became used to it, right off the bat. A certain amount of it continued even after Alpen reorganized things. Let me come back to that. Looking back on it, it might seem it would be unusual and it would be difficult, because there wasn't a chain of command. But I just dealt with everybody. If Dr. Lawrence[68] was in this [organization-chart] box up here *(holds his hand above his head, palm-down)*, here's this 80-foot-long line with everybody. *(spreads his arms apart)* Now I'm off at the side somewhere. I just dealt with all those people. If somebody said, "My technician has left and we have to get a new one and I need your help," that's fine. I didn't say, "You better go to your group leader and come up through that direction." I said, "Fine, sit down and we'll do that, and I'll make sure that you have approval to replace that position." Or they would say they talked to Dr. Lawrence and that was fine, or talked to Jim [Born].

We weren't perhaps always consistent in what we checked up, and what we decided was so obviously okay that we would go ahead. The group leaders—when Alpen came, a lot of the way the groups functioned depended on the personality of the group leader—some of them followed the academic model: the first among equals, the spokesperson model. But in no sense was Dr. *X* the supervisor of Doctors *A, B, C, D, E, F,* and *G,* who were in that group of which Dr. *X* was the leader. Dr. *X* just happened to be the unlucky one that got named to head the thing up. He was out of the room when they said, "Who's going to head this thing up?" That's the way an academic department runs.

Then there was the industrial model. There were group leaders: whereby George, the group leader, was in fact the supervisor of all the people in that group. Certainly, Dr. Budinger's group—he was very clearly the supervisor of, and he had some very high-level scientists in there. That's the way he organized his group.

There were other groups. I would say Dr. Tobias's group was probably like that. It's hard for me now to remember who was in which group, but there was a group that Dr. Gertrude Forte was the group leader of at that moment. I would say that's very definitely the academic model.

In dealing, as an administrator, with people of that sort—if somebody from the later group came up, I seldom would go and talk to Dr. Forte

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67 the process of producing a PET scan, a medical image obtained by examination with a PET scanner, a device that produces computerized three-dimensional images of biochemical activity in the brain or other organ through use of radioactive tracers that emit positrons and twin 0.511-MeV gamma rays

68 Mr. Whaley added later: "As long as I have known him and even though he's gone, I can't call him John; I absolutely cannot do that."

about it. You just went ahead and dealt with it. But I would be very sure that anybody from Dr. Budinger's group had checked with Dr. Budinger before they came up to talk about hiring a replacement. There is that kind of a difference. The difference really came in when it got organized, not before.

That's what happened. This [need to be] conscious of which group people were coming from and [to know] whether that's a group where they had to go through the group leader, or whether it was one in the academic model, where that wasn't. That's what I meant. The change came when we got organized, when Alpen came in 1976. Before that time, it was really a question of dealing with these people individually, because the group structure was very very loose, and without formally designated leaders. Simply because Dr. Lawrence didn't operate that way.

Where are we now?

The Donner Pavilion

BERGE: I have a couple more questions. They're going back to things you talked about before, that I didn't quite follow up on them. One of them was: you mentioned the Donner Pavilion a number of times. I was wondering, how did that work? And, was there any follow-up from people there?

WHALEY: Yes, very definitely. The clinic patients, I think, were more likely to be local, although that isn't universally true. The clinic was physically a clinic. For a long time, it was located on the second floor of the old wing of the Donner Laboratory; remodeled with money from the Donner Foundation. And it had little examining rooms and a secretary's office and glass windows, just like you see in a regular doctor's office.

Then there was a clinical laboratory, with licensed clinical laboratory technologists. Patients would be referred, many of them by local physicians, some of them from greater distances. I think the reason there weren't perhaps so many from greater distances is that we weren't unique. We weren't one of two places in the country where you could get treated for polycythemia or leukemia, at this point.

We had a group of thyroid patients, because we had a physician, who was interested in thyroid, who actually had been a member of John Gofman's group. He stayed. And he went into private practice in San Carlos, but he came over one morning a week. That was Don Rosenthal. The patients would return, and they did actually get treated with whatever therapy. They continued to see their own physician for other problems, and we would send summary letters to the physicians on what had been done. That was a research project.

That work would be written up and published in the medical literature, the *JAMA* [(*Journal of the American Medical Association*)] or the *New England Journal of Medicine,* etc. The clinic moved to Building 55. I don't remember when—towards the end of its existence. Then it got phased out. There just really wasn't more research content. It would be a very difficult

thing to know when to stop a research project. When the performance is over and you've taken your bows, and now it's time to turn up the house lights, sometimes there's a tendency to come out for another bow.

Changes in Research Funding

BERGE: Another question I had was—I know that you and your responsibilities lay with the "odd funds." What particular interest did the AEC or later the—

WHALEY: ERDA and DOE.

BERGE: What particular interest did they have?

WHALEY: Lots. They were the big—let me see if I can remember. We're talking about a proportion here of 80 to 90 percent AEC funding, and 10 percent the other funding. I don't swear to those numbers, but the ratio was very large. The vast, vast bulk of the money . . . That ratio changed, as time went on, so that by the time I left it was nearly 50–50. By that point, all of the other monies were coming in through the laboratory, not through the campus. In the early times, the AEC was funding the lipoprotein work, they were funding the clinic, they were funding the pituitary program, and they kept that right up to the end; along with many other programs.

I think the reason that the clinic was phased out was: there wasn't that much more research content; or at least, research content that they were interested in. By this time, it was DOE. I was very heavily involved with this major funding. Even though Bob San Souci and later Igor Blake were actually doing the budgeting and the managing of that, all of the personnel were heavily funded out of this. When I became the only administrator, I was responsible for that as well as everything else. That was certainly was one of the trends in the funding: that the DOE funding was essentially level, for I don't know how long. It would wiggle up and down a little bit. That, of course, is a decrease, when you're talking about purchasing power. I can't remember the numbers to tell you. It got [to] 55–45 [percent DOE–other], maybe.

BERGE: Why was the AEC and Department of Energy interested in these particular programs?

WHALEY: That's an interesting question. Why were they funding them? The National Laboratories had been created, in significant measure, during the war as part of the Manhattan Project.[69] And the AEC inherited them when civilian control of atomic energy occurred right after the war. In most of these places, there had been a biology and/or medical component to the research. So, it was history. There wasn't anything like the National Laboratories at that time. I'm not qualified to comment on what it's like now.

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[69] the U.S. Government's secret project, launched December 28, 1942 by the U.S. Army Corps of Engineers' Manhattan Engineer District, to develop the atomic bomb. Headquartered in Washington, D.C., the Manhattan Project was the Office of Scientific Research and Development Section on Uranium and was codenamed S-1 (Section One of the Office of Scientific Research and Development).

There are those that say that Ernest Lawrence's greatest contribution to science wasn't the development of the cyclotron; it was the creation of big science:[70] the multidisciplinary approach, the large institution—the mechanics, and the engineers, and the glass blowers, and the scientists of every conceivable discipline. I think the National Laboratories were really the only places of that sort, back in that time. Once you get rolling on something, it's inertia: a body keeps on going unless something stops it.

What is the Department of Energy doing running a biology research program? I can't answer that question. Why are they doing it? I think it's because it has always been that way. Should they be doing it? That's another issue. But, if they don't do it, will there be in this country the capability of doing big science, such as the [Human] Genome Project?[71] Many feel it can only be done at the National Laboratories. There isn't any other place that has this massive engineering and computer capability to bring to bear on it. I don't know whether there are other places. There certainly aren't many of them.

BERGE: You've pretty much answered most of the questions I've got on here. There's one that I'm looking down on my list that I didn't follow-up onto. You mentioned that John Lawrence had a very strong personality and Hamilton had a really strong personality. Do you have any—

WHALEY: I don't know that I, more than once, met Dr. Hamilton. That is an impression that I gleaned. I don't know. I don't really know that they clashed. It's just that: as Dr. Lawrence didn't like to go through intermediate people on the way to the president of the university, if he wanted to get a new building, or whatever; I don't think Dr. Hamilton liked to go through Dr. Lawrence on his way to the director of the Rad Lab. In that sense, I think that the powers that be at the Rad Lab (and this is speculation on my part) sensed that it would probably be better if they left those two separate parallel lines of communication alone; rather than trying to scrooch a group under there—to make one of them boss of the other one, figuring it wouldn't have worked.

There's an awful lot, as I have seen over the years, in organizational practice. And it's not supposed to be, according to the theory. But that is, in fact, derived from the personalities of the people who are involved. If you have an assessment of those personalities, and they are people that are making valuable contributions to the organization, you certainly are a damn fool if you take the organization theory (that professor *X* at Harvard says is the only way to go) and cram those people into that thing. You will have a much better, more productive, organization if you forget professor *X*'s theories and organize it in a way that allows those people to contribute to the maximum extent, and not provide lots of opportunities for them to clash.

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70 science whose research activities typically require large, costly facilities and hundreds of individuals

71 a broad-scale program sponsored by the National Institutes of Health and the Department of Energy to map the location of every gene of all 47 human chromosomes

BERGE: I'm sort of running dry. If you have anything else that you'd like to tell me about . . .

WHALEY: It was a marvelous place to have been. It was full of people with all of the plus-and-minus attributes that people have when they interact with each other. It was a marvelous thing. Even though I wasn't in the scientific or medical end, it was a marvelous thing to be associated with. I take great pride in what the laboratory accomplished during those years. I have a feeling—and one has to be terribly careful as you get older, that you don't say, "It was so much better in the old days." But I do think that, as time has gone on, we have glorified structure and process to the detriment of accomplishment—that things were not highly structured and highly organized in the earlier days; and we did achieve an awful lot of wonderful results at that point.

You never can prove what your results would be if you could go back 20 years and organize it differently. People who say, "It would have been better," or, "It would have been worse," are only speculating. Of course, I'm only speculating, too.

I think one of the best illustrations (this is not going to be popular with the lab management) is the five-year plan. Now, every person or organization that's intelligent has a plan of some sort. It seemed, to me, that the description of what the plan was to be, when it was first created, is a far cry from what it has turned out to be. That it has in fact, to some extent (instead of a guide to what the laboratory plans to do), has become an end in itself. The creation of the plan, the modification of the plan, the review of the plan, and the meetings about the plan are, to a certain degree, divorced from what the plan says the laboratory is going to do.

I think that there are a lot of changes in administration, in bureaucracy in the very best sense of what it really means (not in the negative sense), that have also moved us in that direction. Maybe we used to get seven dollars' worth of science for ten dollars' worth of appropriation. And I wonder, now, if we're not getting three dollars' worth of science for ten dollars' worth of appropriation. It's not a trend that's going to be reversed. I think it's not a positive thing, for the laboratory or the country. At that point, I'll end if you have no further questions.

BERGE: I can't think of any except—maybe, if I have any in the future, if I can call you up?

WHALEY: Sure.

BERGE: Thank you very much. ❑