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History of Science and Technology Program

WILLIAM E. SIRI:
BIOPHYSICAL RESEARCH AT DONNER LABORATORY, 1945–1975

An Interview Conducted by Sally Smith Hughes

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William E. Siri

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Acknowledgment

This interview with William E. Siri is one of several dealing with the development of Crocker and Donner laboratories, within the larger series of oral histories produced by the History of Science and Technology Program.

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

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

James D. Hart
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Introduction

This transcript is based on five interview sessions in April-June 1980 with William E. Siri. The interviewer was Sally Smith Hughes; the interview took place in Mr. Siri's office at Lawrence Berkeley Laboratory. The interview focuses on Mr. Siri's career as a biophysicist at Donner Laboratory. A complementary account of his activities as a mountaineer, conservationist, and president of the Sierra Club (1964–1966) can be found in another oral history interview, *Reflections on the Sierra Club*, conducted by Ann Lage for the Regional Oral History Office (The Bancroft Library, 1979).

The present transcript is part of a series recorded during 1979–1980 with individuals associated with Donner and Crocker laboratories and, in some cases, the Division of Medical Physics and Biophysics. Donner Laboratory is part of the Biomedical Division of what since 1971 has been called the Lawrence Berkeley Laboratory; Crocker Laboratory no longer exists. The Division of Medical Physics, which in March 1980 became a department, is an academic unit of the University of California, Berkeley.

This medical physics interview series, conducted under the auspices of the History of Science and Technology Program at The Bancroft Library, is part of a project funded by the National Endowment for the Humanities to document the history of the Donner and Crocker laboratories, the Division of Medical Physics and Biophysics, and the careers and research of individuals associated with them. These interviews, in conjunction with archival holdings at The Bancroft Library and Lawrence Berkeley Laboratory, will be useful in constructing a picture of the growth and development of the fields of medical physics and biophysics, in which the Berkeley research and academic institutions played an early and significant role. The interview series also complements other History of Science and Technology Program oral histories pertaining to the development of Lawrence Berkeley Laboratory.

Oral history can frequently provide information on subjects not easily retrieved from published sources. These subjects include family history, social and economic conditions affecting research, interactions with colleagues, peculiarities of institutional organization, and perceptions of a scientific discipline. It should be borne in mind that the interview records what the subject remembers during the interview about what happened at a given place and time; typically,

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many years have passed since the events occurred.

The verbatim transcript of this interview was checked against the tape recordings and edited for punctuation, paragraphing, spelling, verification of proper names, and repetition; several sections were rearranged for better topical or chronological sequence. Mr. Siri reviewed the edited transcript. Marie Herold aided in the production of the transcript, and Robin Rider prepared the index.

Literary rights for this interview are vested in the Director of The Bancroft Library. Any quotation for publication of the material included herein requires the advance written approval of the Director. A request to see the transcript constitutes an agreement to abide by this restriction.

Background materials pertaining to this and other interviews in the History of Science and Technology series, together with tape recordings of interview sessions, are available for research use in the Heller Reading Room.

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Sally Smith Hughes
Robin E. Rider
History of Science and Technology Program
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Curriculum Vitae
William E. Siri

1919	Philadelphia, Pennsylvania, born January 2
1942	University of Chicago, B.Sc.
1942–43	Research Engineer, Baldwin-Lima-Hamilton Corp.
1943–45	Physicist, Radiation Laboratory, University of California, Berkeley
1945–74	Physicist, Donner Laboratory, University of California, Berkeley
1950	Member, University of California Peruvian Expedition
1952	Leader, University of California Peruvian Expedition
1954	Leader, California Himalayan Expedition
1957	Leader, Bolivian High Altitude Study Project
1957	Field leader, International Physiological Expedition, Antarctica
1963	Deputy leader, American Mt. Everest Expedition
1964–66	President, Sierra Club
1974	Manager, Energy Analysis Program,

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Lawrence Berkeley Laboratory,
University of California, Berkeley

President, Save-the-Bay Committee

1977 Sol Feinstein National Environmental Award
(with Jean Siri)

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Date of interview: 16 April 1980

FAMILY BACKGROUND AND EARLY EDUCATION

Hughes: Mr. Siri, I read with interest the early part of your interviews with Ann Lage *[Reflections on the Sierra Club, the environment and mountaineering, 1950s–1970s,* Sierra Club series, Regional Oral History Office, The Bancroft Library, University of California, Berkeley], and there were a few questions that I wanted to ask about your childhood and early education. One of them is about your grandparents: where they came from, what their names were, what were their education and professions.

Siri: On my mother's side, my grandfather and grandmother were born in Frankfurt, Germany, and came to the United States at an early age. I believe all of their children were born in the United States, including my mother, Caroline. Their name was Schaedel. I have very vague recollections of my grandmother. The last [time] I saw her, I think, occurred when I was about three. They lived in Pennsylvania near the town of Wilkesboro. My grandfather on my mother's side had died some years before, so I have no recollection of him.

My father, Emil Mark Siri, was born in a village in the Alps in southern France and came to this country, I think, when he was thirteen. I never met his parents, my grandparents on my father's side. However, most of the family, two brothers and a sister of my father, also came to the United States at a very early age, and of course I knew them.

Hughes: What did they come to do?

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Siri: They joined friends here in the United States initially and gradually worked their way, educated themselves, and went into various kinds of businesses. My father became a mechanical engineer—at that time they were called steam engineers—and began work with a large company near Philadelphia, Baldwin Locomotive Works, which at that time was the biggest builder of those magnificent steam machines and also other kinds of large machinery. As time passed, he advanced and finally became the general superintendent of the plant some years before he died.

His brothers went into other businesses. One went into the restaurant business and was quite successful. Another went into real estate, as I vaguely recall, and has had spectacular successes and failures, as I guess is characteristic of that business. That's about as much as I can recall of my relatives.

Hughes: Can you remember what your parents' attitude was towards your education? Or more generally perhaps, what they hoped you would be one day?

Siri: My parents were not in any sense pushy about what my education would be. This may have been because there was no need to be. There was no question in my mind, and it was obvious to them, that I was going to go into a scientific career. It was just a question of graduating from high school and going on to a university, and this is what would happen.

Hughes: When did you decide that?

Siri: The change came, as I recall, fairly abruptly when I was about 12. I can't recall the specific factors that led to it, but suddenly I discovered literature and the sciences. Almost overnight I found myself listening to the Saturday afternoon opera broadcast from the Metropolitan and avidly absorbing

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everything I could lay my hands on in the sciences. I also began building things like telescopes and instruments, including some early electrostatic machines that raised havoc with the neighborhood radios. I succeeded in partially blowing up the chemistry laboratory in high school, as I suppose every fledgling would-be scientist does in going through his chemistry classes in high school.

Hughes: Were you doing all this on your own, or did you have a group of friends with similar interests?

Siri: There were several friends. By several, I mean only two or three who held similar interests in science. I do recall that throughout high school we stuck pretty much together. Our discussions were always about science in one form or another. We had relatively little contact with the kids who were more interested in sports or other kinds of activities that are normal for that age.

Hughes: When you say you were reading at age 12, you don't mean the scientific literature, do you?

Siri: Not the refereed scientific literature. No, I didn't have access to that, but the popular books and texts I could find in the library and in the second hand bookstores in Philadelphia. I began to discover a wealth of material in the local libraries that was fascinating and overwhelming. It included Immanuel Kant's *Critique of pure reason*, which I tried to struggle through [laughter], and Spinoza, and several volumes of Herbert Spencer.

The discoveries one makes at that age about the intellectual world are like first love affairs; they're intense, they're startling, they're new. Suddenly a whole new world opens up. I remember this in discovering that there was such a thing as philosophy. I got clues to this, however, in some of

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my classes in high school where we had a few enlightened teachers—well, several very good teachers. They provided the clues, and I began to follow these. I discovered there were people like Immanuel Kant, Spinoza, and Socrates. The discovery of Jung, Adler, and Freud was also another great event [chuckles]. It opened up an entire new world, and I began to read the works of the early psychoanalysts and psychiatrists.

Hughes: Were your friends interested in the same things? Was there anybody to talk to or with about these subjects?

Siri: Not very much, not really. There might have been only one other person with whom I could discuss these kinds of works at the time.

Hughes: Your parents were not interested?

Siri: They were not informed. My father was oriented to machinery, to engineering, and had had no education in the humanities and pure sciences. My mother had not had much in the way of education. She had not completed high school because of the need to work. So within the family there was little opportunity to discuss these fascinating subjects.

Hughes: What about teachers? Was there anybody with whom you had a special relationship in high school?

Siri: Yes, but only in science. For example, a chemistry teacher who was something of an oddball—this was in junior and senior year in high school. He decided that I knew more chemistry than he was going to teach the class, because I'd already read everything available in chemistry that I could lay my hands on. So he created an interesting project that was rewarding to him and instructive to me. He would

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collect the floor sweepings and the filings from jewelers in Philadelphia and bring them to a high school in Audubon, New Jersey, and had me extract the gold and platinum. I would digest and extract the platinum and gold from all this mess and he would take it back to the jewelers and I assume that he received some kind of reward.

Hughes: But you didn't?

Siri: No, I got the training and practice. Let's see, there were one or two other teachers, but the quality of their training in science was not so advanced that there didn't come a time when I began asking questions they couldn't answer. So I pursued it in the books and other literature I could find and in the local museums in Philadelphia.

Hughes: You mentioned in the other interview, working in the summers at what became Baldwin-Lima-Hamilton Corporation, the steam engine outfit where your father was superintendent. What sort of work were you doing? This was in high school, was it not?

Siri: It began in high school, yes. I was preparing to go to college, and summers were a time to earn money. I was fascinated with machinery, just as my father was, and had some inherent skill, I guess, because I'd played in our basement shop from the time I could walk, making things.

Hughes: Did your father have a hand in that?

Siri: Oh, I'm sure he did. My father was held in fairly high regard by management and by the people in the shop, and if young Will wanted a job, why, fine. But young Will worked his tail off, too, because he was highly motivated. I found it fun as much as often hard labor. The last year or two in

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high school and then when I went to college, [I would] come back and work during the summer in the shops.

Hughes: What sort of things were you doing?

Siri: Well, the first summer I was employed [my job] consisted of menial tasks like wiping out the crankcases of very large diesel engines or doing small jobs that required no particular skill. The second year they decided I could run machines and do relatively simple machine jobs. As these summers progressed, they assigned me to more interesting tasks, ultimately ending up working with an old German craftsman who produced precision gauges that are used in manufacturing locomotives, testing machines, all kinds of equipment. I found that I had a talent for doing this and so I was assigned more and more work of this kind.

Hughes: Well, do you think the summer job had a part in confirming your interest in engineering?

Siri: Oh, I'm sure it did, yes.

Hughes: So by the time you were ready for college, you had settled upon engineering?

Siri: No, oh no. That was not what I intended to get into at *all*. In fact, I guess I regarded engineering at that time as a trade, and I didn't want to get into a trade. I wanted to get into research, into science. My initial thought was to go into astronomy when I entered the University of Chicago. But the more I studied astronomy, the more I realized that it was really physical theory in astronomy, and hence physics, that was interesting.

Hughes: Why were you attracted to astronomy?

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Siri: I don't know precisely why I was attracted to it other than the early exposure to books on astronomy. Part of the commitment no doubt resulted from building my own telescope when I was about 14. I ground my own mirrors and lenses and built a telescope that was fairly respectable and spent a good many evenings observing nebulae, stars and planets. This reinforced the interest in astronomy, of course.

CHICAGO (1938–1943) AND BERKELEY (1947–1950)

Hughes: Why the University of Chicago?

Siri: This was done quite deliberately. I collected the catalogs of nearly all of the large eastern universities—I hadn't considered the West Coast at that time—and went through them to see what their astronomy and physics departments were like. There were a number of factors that I considered, such as faculty and facilities, and the tuition and living costs. I used some primitive basis for judging; the scope of my information at that time was pretty restricted.

Anyway, on the basis of their catalogs, their faculties, and what little I knew about them, I settled on Chicago. It clearly had outstanding physics and astronomy departments, and Yerkes Observatory at that time was one of the great observatories. On the other hand, Harvard and Princeton were also attractive, but they were also expensive; the state of our finances back in the Depression years, while it was adequate, it would have been a strain on the family budget to have gone to Princeton or Harvard. Oh, there was another factor too. At that time both Princeton and Harvard and possibly Yale required Latin for admission, and I couldn't provide the necessary credentials in Latin. [laughter]

Hughes: That makes me think of a question that I should have asked earlier. Was the language at home English?

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Siri: Oh yes. I think my father had forgotten his native tongue. He never used it. My mother knew a few words of German but she could not really speak German.

Hughes: Or Latin.

Siri: Or Latin.

Hughes: At what point did you make the switch from astronomy to physics?

Siri: It was about my sophomore year in college, I guess. At that point there really was no difference in the curriculum for going one way or another.

Hughes: It was all the same department, too?

Siri: Pretty much the same. Yes, that's right. In any event, to get into astronomy, one had to acquire a physics and mathematics background.

Hughes: Were you beginning to get interested in any particular area of physics?

Siri: At that time cosmic ray physics, of course, was the fad, and Chicago was one of the leading centers of cosmic ray research. Arthur H. Compton was deeply involved in early work in cosmic rays.

Three or four other faculty members in physics were also some of the leading lights in the field. So naturally I tended to move in that direction as did, I guess, many of the physics students who were there.

Hughes: Did you have any courses from Compton?

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Siri: No, as a matter of fact, I didn't. He was giving very few courses at the time. He was chairman of the department. It seems to me, he gave only one course and wasn't a very good lecturer. One would take his course only to be able to say that you had taken a course from Compton. There were other men in the department who were superb lecturers, and their courses were the ones we all wanted to take.

Hughes: Were you at all aware of any of the war-related work going on? This would have been towards the end of the last two years? You graduated in 1942? I was a little confused by your biography because it sounded as though you should have graduated in 1942 and worked with Baldwin-Lima-Hamilton Corp. from '42–43.

Siri: Yes, you have it correct.

Hughes: Is there anything outstanding to mention about those years in Chicago, particularly in terms of the people you came into contact with in physics?

Siri: Among the students I encountered undergraduates and graduates, such as Alvin Weinberg and Ralph Lapp. And there are memories, as everyone has, of particular professors, the influence they had on one's thinking, attitudes, and outlook.

The most striking teacher I encountered at Chicago was [Fredrik] Zachariasen who was one of the outstanding men in crystallography. He did a lot of the early work, both theoretical and experimental. I think Zachariasen had a profound influence on all the students who attended his courses. His lectures were so clear, so profound that you went away with a very pleased feeling that you had learned something and learned it well.

One of the tragic figures we encountered—I took several courses from him—was Professor Lund. He was said to

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have developed quantum mechanics before [Erwin] Schroedinger, but had the misfortune of not publishing soon enough. When Schroedinger published the theory of quantum mechanics, it changed the whole of modern physics. Professor Lund missed one of the great opportunities for immortality, and it had a profound effect on his life. But he was an interesting lecturer in theoretical physics, particularly in quantum mechanics.

The most memorable personalities at Chicago in those years were Maynard Hutchins and Mortimer Adler. They were at the peak of their glory and dominated campus intellectual life.

Hughes: I asked you and then didn't give you a chance to answer, if you were at all aware of the work in the Metallurgical Lab which would have been just getting off the ground, wouldn't it, as you were about to graduate?

Siri: That's right. It would have started possibly as much as a year earlier. I had no clue, none whatever, about its existence.

Hughes: Well, tell me what happened after you graduated in 1942. What was on your mind about the next step?

Siri: Well, the war was on everybody's mind at that time. I tried to volunteer [for] the Navy, but since I had to wear glasses, they weren't terribly keen about it. At about the same time I received an invitation from Baldwin Locomotive Works to take a job with them on war work as a research engineer.

I could not continue as a student. There was a war going. We had to do something. The psychology of the time was such that you either went off to fight in the front lines or, failing that, worked at something that would aid the war effort. You could not get around the intense feeling that

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one has in the midst of a war that you felt was not of your country's making but were attacked. Then all the animal instincts emerge. It was not the same thing as Korea or Viet Nam.

In any event, the Navy didn't want me. At that time Baldwin-Lima-Hamilton was engaged solely in war work—producing tanks, diesels for naval vessels, and other machinery of war. So they asked me to come back as a research engineer to participate in developing some new high-speed marine diesel engines. The conventional engineering needed to be supplemented by analytical and research work. So I joined a small team of research engineers to work on theoretical analysis.

Hughes: Were you put in charge of a project?

Siri: No, I worked under the leader of the engineering research team. There were about eight of us. The team leader was an old Swiss engineer—a very fine fellow, very able. That proved to be a valuable learning exercise for the next year. During that time I managed to turn out about a dozen reports on research projects assigned to me.

Hughes: Was it unusual to publish in that volume in that position?

Siri: No, because out of a dozen, maybe three [had] considerable substance in terms of original thought and significance of the findings. The rest were analyses of problems encountered by the engineers. Well, all of my studies were directed ultimately to the solution of problems for the design engineers.

Several reports, however, I thought were of some merit. One was on [the] application of vibration theory to large high-speed diesel engines. Another was on bearing lubrication, to develop an applicable theory of lubrication. A great many problems had been encountered in trying to develop a

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large, high-speed diesel because it constantly burned out its bearings. So I was assigned the task of finding out why that was and how the designs could be corrected. I succeeded, but the diesel project failed for another reason. Another company had a better design.

Hughes: Were you pulling on your background in physics?

Siri: Yes, it was entirely applied theoretical physics.

Hughes: And also your extracurricular activities as a youngster? I mean building the telescope.

Siri: I'm sure that was useful because it gave me some feeling for machinery. Familiarity with machinery was invaluable when judgment or insight was necessary in applied analysis.

Hughes: Well, until very recently anyway, you have seemed to maintain that link with the machine pretty much through your career. Would you say that is true?

Siri: Yes. Well, I wouldn't dismiss it even now because in the analytical work on energy that we do now, we have to deal with many technologies, all of them involving machinery as well as economics and other facets of energy. Here some feeling for machinery, what it takes to design, build, and deploy it is helpful. I notice that some of the people doing energy analysis who have no experience with real machinery still don't have a real grasp of it. Here's a technology that looks great on paper, [but] they don't realize that it takes one to two decades to make it work commercially.

Hughes: So you've perhaps been unusual in that regard.

Siri: Well, I don't know if it's unusual. I think the background

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of having gotten hands dirty on machinery and made lots of mistakes while learning about it is sometimes helpful in making judgments in energy analysis.

MANHATTAN PROJECT (BERKELEY, 1943–1945)

Hughes: The next step would be the move to California. Tell me how you learned about the position at Berkeley?

Siri: A friend of mine was aware of the project at Berkeley and knew they were looking for physicists and gave them my name. The [Radiation] Laboratory sent me a wire offering me a job.

Hughes: Do you remember who it was?

Siri: Yes, it was the manager at that time, Mr. George Everson. His wire said if I were interested, come to Berkeley for an interview. After thinking it over for several tenths of a second, I decided to go.

Hughes: Do you think this was part of a large recruiting program? Were they pulling people from all over?

Siri: Yes, yes.

Hughes: Tell me again why you went.

Siri: Oh, it was clearly an opportunity to get back into physics—no question in my mind at all about that. At Berkeley, Lawrence was making history with the cyclotron; this was where the action was in modern physics.

Hughes: So you were very aware of that.

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Siri: Oh yes, and the opportunity to come out here was overwhelming.

Hughes: You mentioned to Ann the attraction of the mountains. Would you say that was probably a secondary consideration?

Siri: That's a good question. Let's say it was not overlooked. [laughter] It was an important factor.

Hughes: What happened when you arrived? What were you doing here? With whom were you working?

Siri: I accepted the job here as a physicist and was assigned to a group working in the Old Radiation Laboratory, which does not exist now. It was the first building that was turned over to Ernest Lawrence by the Regents to house the 37-inch cyclotron. It was an old building. I've forgotten what its original purpose was. Soon after the war it was torn down to make space for the physical sciences lecture hall and a parking lot. It housed the old 37-inch cyclotron, but by the time I arrived the cyclotron had been converted to a calutron, which is a large form of mass spectrometer. Its purpose was to separate uranium-235 from uranium-238.

Hughes: So you had to have a security clearance right away.

Siri: Yes. We were all pledged to secrecy.

Hughes: That was part of the employment process then? They had checked your background before having you out here?

Siri: I presume they checked to see that I was a safe person to have working on the project. Anyway, the project was an eye-opener. It was absolutely staggering because I hadn't known until I joined the group, and the group leader

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explained to me what they were doing, that a mammoth project was under way to develop an atomic bomb. I never dreamed that what I'd learned in nuclear physics at Chicago would be put to this use. It was a staggering discovery.

Hughes: Do you think the people at Chicago really hadn't considered that possibility?

Siri: Obviously they had. But my knowledge of the subject—not the project—came from courses that I took in nuclear physics from Arthur Dempster at Chicago. They had nothing to do with the use of nuclear energy for practical purposes. Fission was just a part of the course in nuclear physics and radioactivity. I wasn't aware that an effort was being made to utilize it in a practical way.

For the next year I worked with the old 37-inch cyclotron, now calutron, trying to improve the technology for separating uranium-235 from uranium-238. Those were interesting days. We were certain the whole of the war that the Germans were ahead of us, particularly since we knew that [Werner] Heisenberg, one of the leading physicists of the twentieth century, was in Germany. There could be no doubt that he and a large team of very able German scientists were working very hard at the same thing and probably were ahead of us. It was this stimulus that kept us going day and night. We would often work 24, 36 hours straight through. I remember curling up by the old calutron magnet to keep warm while I sacked out briefly during these long sessions. Working [long] hours meant nothing. We were driven by the constant thought that unless we succeeded before Germany, the war would be lost and in a horrible way.

Hughes: Who was the group leader?

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Siri: Carl Helmholz, as I recall, but John Backus was the spiritual and intellectual leader. Bernard Peters also provided much of the scientific leadership.

Hughes: And most of these people were physicists?

Siri: Oh, they were all physicists.

Hughes: Did you come into contact with Ernest Lawrence?

Siri: Oh yes. I came into contact with him when he breezed through like a small-scale cyclone from time to time. He was an imposing figure. He would burst through the door from time to time to see how things were going or bent on some mission. It was like God had entered, they used to say. [laughter] No question about who ran the whole operation. It was leadership in its ultimate form. I mean no one questioned it. Oh, you would discuss technical matters with Ernest Lawrence and he would listen, but you'd better have your facts and theory straight because he would quickly ask direct and very hard questions. If your thinking was faulty, you were left feeling very, very foolish and chastised. If he thought your idea was good, he was highly supportive and acted on it immediately.

Hughes: So you felt that he really did have a finger on what was becoming a rather vast project during the war?

Siri: He most certainly did. He had a complete command of, and understanding of, everything that was going on. Nothing escaped his attention.

Hughes: What was the status of the shielding around the cyclotron by that time, or the calutron?

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Siri: There was none, but you have to understand that the calutron did not produce high-energy radiations. It was a mass spectroscope and accelerated the ions a few tens of thousands of volts, which is no more than the energy of electrons in a TV tube. So there were no high energy radiations to contend with. Some x rays may have been produced from sparking, because voltages in the 10 to 20,000 volt [range] could produce soft x rays. We were a little casual about radiation and chemical hazards at the time.

The part of it that I look back on with some horror and amusement was our totally indifferent handling of and exposure to uranium tetrachloride, the chemical form of the uranium we ran in the calutron. Large quantities passed into the vacuum chamber and condensed on the walls, where it formed a thick green carpeting resembling dark green slime mold. It would stay dry and stick to everything while the chamber was under vacuum. The moment the chamber was opened, it absorbed water, forming a gooey mess.

One of the tasks we had was to clean out the chamber after experimental runs. I would crawl into the chamber and scrape down and wipe off tens of pounds of uranium tetrachloride. Moreover, to clean the chamber, we used huge quantities of carbon tetrachloride. Its use is now *verboten* because it is a serious toxic chemical. It wipes out one's liver and has other toxic effects. I can remember coming out of the chamber on any number of occasions literally drunk from inhaling carbon tetrachloride. We weren't aware of the hazards at that time. We thought carbon tetrachloride was safe. It was, after all, nonflammable and presumably benign. That was combined with exposure to quantities of uranium; we were eating it, breathing it, covered with it. One has no right to be alive.

Hughes: I asked the question about the shielding because I understand that the shielding on the early cyclotrons was far from adequate at least by later standards.

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Siri: That's right. But it must be remembered that no one at that time knew how hazardous radiation was at low and moderate levels.

We did know that it was hazardous at high levels, but we thought this meant it was hazardous only at hundreds of rads or roentgens. We knew this from the experience of the medical profession in using x rays. The incidence of cancer and radiation injuries among early radiologists was extremely high, but then so was their exposure. We were well aware of this, so the object was not to get overexposed. By that we meant hundreds of roentgens. We were pretty casual about smaller exposure, compared to contemporary attitudes and practices.

Hughes: Then you went back to Oak Ridge after a year?

Siri: Yes.

Hughes: Why were you sent back there?

Siri: Well, almost everybody was sent....The machine, the calutron, had been fully developed here, and now the big production facilities based on the Berkeley research were established at Oak Ridge and ready to run. So nearly everybody from Berkeley was sent back to Oak Ridge to help get the production machines going. I don't recall how long I was there, probably about a year. We trained crews, supervised them, and helped with some of the technical problems that developed.

Hughes: And there were a lot of them, were there not?

Siri: Yes, there certainly were.

Hughes: I think it's incredible that it worked at all.

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Siri: It really is. The whole operation needed constant wet nursing by physicists and engineers. It was an unbelievable operation.

Hughes: Was that pretty much a nonstop work situation too?

Siri: It wasn't as demanding as the research at Berkeley. We were scheduled on eight-hour shifts, matching or overlapping the regular operating shifts for the facility. If we stayed on longer than that, we were just in the way. Work was much more regularized. It had to be because this was a production operation.

Hughes: How much did you know about the purpose of your work? Were you very aware of why you were doing this?

Siri: Oh, but of course. The physicists who were doing the research had to know, and this was part of my initial indoctrination. I was not told, as part of my indoctrination, about the other approaches, particularly plutonium production. But I knew about this in due time and about the other approaches that were being used to separate uranium-235, and about the possibilities of using plutonium and the reactors that were producing plutonium.

Hughes: Speaking of plutonium, do you know anything about the circumstances under which Crocker Lab received a small sample of plutonium?

Siri: When?

Hughes: As I understand it, it was part of the first plutonium produced in Chicago, where [Glenn] Seaborg was, and he was a friend of [Joseph] Hamilton. Crocker Lab, I presume it was about 1943, received a sample with the idea of trying to find

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out some of the biological effects, didn't it?

Siri: Yes, I have only a vague recollection of this. I was aware that Joe Hamilton had received some of it to look at the biological effects. This is all I can recall at the moment.

Hughes: What had you achieved [at Oak Ridge] by the end of the year?

Siri: The whole object of sending the Berkeley people to Oak Ridge was to assist in getting the production plant operating. We instructed operators, diagnosed problems, and then suggested solutions to problems. This was our function.

By the end of the year's time the plants were operating well enough to produce an appreciable quantity of highly enriched uranium-235. In fact they produced enough uranium-235 to make the first bomb, possibly the first two bombs—that is, the test bomb and the one dropped on Hiroshima. So by that time there was no further need for us at Oak Ridge, and we were shipped back to Berkeley. The operation was going reasonably well and could be conducted by the contractors without our help.

Hughes: So you were troubleshooters, so to speak.

Siri: Just about that, yes.

Hughes: What happened when you returned to Berkeley? Were you still associated with the Manhattan Project?

Siri: For a while, but it was clear that Berkeley's work for the Manhattan Project was coming to an end. Plutonium was the preferred nuclear bomb material and it was now being produced in the nuclear reactors at Hanford, Washington. Most of us were sent back to Berkeley not too long before

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the first test shot at Alamogordo. We spent our time trying to develop improvements in the isotope separation process that might be incorporated in the big calutron production units.

I've forgotten the timing, but I think it was between the time the test shot was fired at Alamogordo and the bombing of Hiroshima, that it was clear Berkeley's role was coming to an end, and Ernest Lawrence was now looking forward to going back to a high-energy, basic research laboratory. So he cut the staff down to a few senior scientists he wanted to keep on after the war. He knew the war was coming to an end; it was inevitable now. I was among the younger people who were dismissed. Later, members of his prewar team such as Glenn Seaborg, [Edwin M.] McMillan, and [Luis W.] Alvarez returned to the Laboratory. Within a few years, under Lawrence's powerful driving force and a collection of brilliant minds, the Radiation Laboratory became the center for accelerator and high-energy research recognized around the world.

Date of interview: 6 May 1980

Hughes: Do you remember approximately the number of people that were let go at that point after the war?

Siri: I don't remember the number, but it was probably two-thirds, perhaps three-quarters of the staff. Ernest Lawrence kept only a core group of professional and technical people about him. I was one of the youngest of the junior professionals who was let go. However, the Laboratory made a conscientious effort to find jobs for all of us. As a consequence, I was offered a number of positions, including one with Chevron Research, which I found tempting. But John Lawrence, Ernest Lawrence's brother, offered me a position

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on his staff in Donner Laboratory.

After interviews for other job offers, I decided that since John Lawrence, Donner Laboratory, offered the least salary, a very indefinite future, and all the hours you would ever want to put in, it was irresistible. [laughing]

Hughes: Seriously, why did you do it?

DONNER LABORATORY (1945–1975)

Siri: It's a little difficult to say. Well, one dominating reason was that I wanted to finish graduate work, and this would give me an opportunity. I would work fulltime with John Lawrence and, when the war ended, return to graduate school.

Hughes: Had you done about a year of graduate work in Chicago?

Siri: Yes, right. So I still had another few years to go. This was really the reason I decided to stay on at Berkeley. I don't recall the date but it was before the bombs were dropped on Japan [August 1945].

Hughes: Do you know how Dr. John Lawrence knew of you and your background? How did he happen to make the offer to you?

Siri: I don't know. I never questioned him about that. It may only have been that he looked at a list of available people, and maybe others turned him down before he got to my name. I don't know. [chuckling] In any event the offer was made, and what the precise circumstances were, I have no idea to this day.

In any event, I did join John Lawrence's group in 1945—June, possibly—and found quite a different atmosphere from the one I had been working in during the war.

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The Manhattan Project was intense, big machinery, lots of secrecy, sometimes working 24 hours a day, fraught with importance and lots of vitality. Donner Laboratory was by comparison a very small operation with a totally different spirit, involved in biology and medicine and not so much physics. Nevertheless, they needed a physicist and perhaps this was the reason I was considered—I'd also had some experience in research engineering. This was helpful for the first projects that I was involved in at Donner Laboratory.

When I joined Donner Laboratory, it was called the Aeromedical Laboratory. It was in the building named Donner Laboratory that had been built just before the war—an awful concrete, completely artless structure funded by Mr. [William H.] Donner. The Aeromedical Laboratory consisted of the basement floor of the building. The upper floors were still occupied by Ernest Lawrence and the remnants of his group. It was still part of the Manhattan Project. That was now closed to me, whereas I had complete freedom of the place before leaving the Project. I was now restricted to nonclassified things and therefore confined to the first floor of the building.

The staff at that time was very small. John Lawrence of course was the head of the Laboratory. Hardin Jones, a physiologist, was the senior person, Cornelius Tobias was the second senior person. Other members of the group were Franklin Henry, a member of the faculty in the Department of Physical Education; Hans Welton, a physicist; Enrique Strajman, a physician from Buenos Aires who had come to this country to spend several years training with John Lawrence; William Berg, a graduate student at that time in zoology, waiting for the war to end so that he could complete his graduate studies.

Among this small group, Hardin Jones became a leading authority in several fields as the years progressed, and advanced to full professor here at Berkeley. Cornelius Tobias was trained as a physicist, and in due time became a

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full professor. As soon as the war was finished, Bill Berg returned to Stanford to complete his Ph.D. in zoology. He then returned to Berkeley to take an academic post in the zoology department. Franklin Henry returned immediately after the war to the Department of Physical Education. Hans Welton left for other parts, I believe to take an academic position in a college, but I don't recall where. Enrique Strajman stayed on for some years before returning to Buenos Aires to organize a Department of Medical Physics at the University of Buenos Aires.

Our small group was engaged in various projects. My first projects, working with Cornelius Tobias, were an automatic parachute opening device, and development of an oxygen warning system for military aircraft. This was before the end of the war. Both of these devices were needed by the Army Air Force to reduce the number of casualties in crews of high altitude bombers when they were damaged or shot down.

The automatic parachute opening device was intended to deploy the parachute automatically at a safe altitude. An injured crew member of a disabled plane could simply be pushed out of the airplane and the device would, at an appropriate altitude, open the parachute even if he were unconscious. The reason for this was that parachutes opening at very high altitudes—at that time 27 or 28,000 feet—opened with a shock so great that it usually caused serious injury. So it was always desirable in bailing out of a damaged aircraft at say 27,000 feet to simply freefall to 10,000 feet before opening the parachute because at 10,000 feet, one is traveling in freefall much more slowly than at 27,000 feet and the opening shock is less severe.

The oxygen warning device was a system that would alert the pilot or co-pilot to a situation where a member of the crew in some remote part of the bomber plane may have lost his oxygen supply, either because of damage from gunfire or because he was injured. If the crew member, say

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the tailgunner, failed to breathe for 15 or 16 seconds, the system would alert the pilot to the fact that he was not breathing and assistance could be dispatched to him. You don't have very long, only a minute or two before irreversible damage is done.

In any event, we developed a system that would indicate when a crew member stopped breathing for whatever reason. These devices were installed in planes toward the end of the war, and we would like to think that maybe they helped. It was interesting work, in a sense, because we could install these devices in bombing planes and then go up for rides and see how they worked. In the case of the automatic parachute opener, the Army Air Corps equipped a dozen dummies with parachutes and our devices, and we went up to high altitudes and kicked out the dummies to see if they worked. I regret to say that one or two of our first designs did not open the parachutes. [laughter]

Hughes: I'm glad they were dummies.

Siri: That's right. So it was back to the drawing board. Anyway, during the same time other work was being done at the Laboratory by Hardin Jones and John Lawrence with the assistance of Bill Berg and Franklin Henry. This was work that was being done in a decompression chamber that had been installed at the beginning of the war. It was large enough to hold 10 or 12 men.

One of the problems faced by bomber crews at that time was "bends," a painful affliction of high altitudes in unpressurized planes. In going to high altitude rapidly bubbles tended to form in the tissues. When this happened it was incapacitating, extremely painful. The question was: how could the incidence of this effect be reduced?

Hardin Jones along with John Lawrence and Franklin Henry and a few others were studying these effects in our

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high altitude chamber. They had acquired some insights into human physiology that were quite valuable, both for this purpose and others, and for a better understanding of certain disease states that were studied after the war.

Hughes: Had that low pressure chamber been made in the Laboratory?

Siri: No, I believe it was acquired from the military. It was installed for these studies in a small building adjacent to the Donner Laboratory building.

Hughes: Were all of these things that we've talked about fairly well advanced at this stage? You must have come in more or less at the tail end of the project.

Siri: That's right. The high altitude studies were just about finished at that time and were being phased out. A good many bomber crew members, either volunteers or those ordered to volunteer, had gone through this study in the preceding two years. The research was beginning to move into other directions, such as more basic physiological and medical studies. We wound up the several studies on the oxygen warning device and the automatic parachute opener. These design exercises and other studies continued for perhaps another year. All of them related in one way or another to problems encountered by air crews in bombers and fighter planes.

Hughes: When you were working on a project with a group of people, such as these studies on the bends that we were just talking about, what was the breakdown of labor? I mean, what was John Lawrence doing and what were you doing and what was Bill Berg doing? Or was everybody doing everything?

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Siri: The group was so small that a formal structure was hardly visible. In an organization with so few people you fit naturally into the scheme of things and each recognizes the others as possessing special talents for specific tasks. There was however a hierarchy.

John Lawrence of course was director of the Laboratory. He was not so directly involved in the day-to-day bench top research. But then he had to handle most of the administrative matters personally. He had a secretary, but that was about the extent of his administrative staff. So he was kept pretty busy.

Hardin Jones was regarded as the senior research person. This occurred naturally because Jones was an unusual person in two senses. He had a very fine mind and everybody respected this instinctively. After you'd worked with Hardin for a short while, you recognized that you were associating with a very fine intellect. The other part of it was that Jones was an unusual—how do I want to put it?—was a very human person in the finest sense. He was genuinely concerned about the people around him. He was always receptive to one's problems, not only to listening to them, but doing something positive about them. Everybody both admired and loved Hardin Jones. So he was naturally the spiritual and intellectual leader of any of the projects in which he was engaged—not because he made himself so but simply because this is the way it was. People regarded him as clearly their friend and mentor.

Hughes: Would you say that he retained that role all the way through? Later he was made research director. Is that going along with that same feeling that here is a superior man who obviously is head and shoulders above the rest of them? I mean, did he continue to stand out?

Siri: The answer is yes, for about two decades. John Lawrence placed great reliance on Hardin Jones, and Hardin was in

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every sense the deputy director, the person who really had contact with the professional staff in a close and effective fashion that for the most part John Lawrence did not. That role changed in the early '60s in a peculiar and sad kind of way. Perhaps we can come to that later. But in those early years, through the '40s and '50s, Hardin Jones was a person who was loved and admired by everyone.

Hughes: Cornelius Tobias came at about the same time and about the same age. I don't know how to put this diplomatically, but why was it so apparent that it should be Hardin Jones who would be the man in command?

Siri: That's a little bit difficult to answer with complete assurance. One of the reasons, I believe, was that Hardin Jones was a physiologist, had good insight on medical problems, and therefore the rapport between Jones and Lawrence could on this ground alone be better, or closer perhaps, than it was between Lawrence and Tobias. Tobias was a physicist, and John Lawrence never really understood physics, although he had great respect for physicists. But he could relate to Jones perhaps better than he could to Tobias.

But as time passed both of them had lead roles in the Laboratory—Jones in the area of physiology and to some extent medicine, at least at the outset; Tobias in the physical approaches to biology and medicine. John Lawrence, of course, maintained a firm hand on the medical studies.

The Aeromedical Laboratory came to an abrupt end at the close of the war, and the Aeromedical Laboratory was renamed Donner Laboratory. It also became part of the Radiation Laboratory. Administratively and organizationally it has been a division of the Radiation Laboratory since then. They were not separate institutions. However, Donner Laboratory became identified in most people's mind as a separate entity directed by John Lawrence.

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Hughes: What was that due to, do you suppose? The subject of research more than anything else?

Siri: I think it was left generally ill-defined, so far as the outside world was concerned, probably by mutual agreement. John Lawrence in those years was an outstanding, internationally-known figure, and it was just assumed by others that he was director of an independent laboratory on the Berkeley campus. As a matter of fact, I don't think anyone in the Radiation Laboratory, including his brother, would have suggested otherwise, that he wasn't the director of Donner Laboratory but an associate director of the Radiation Laboratory and Head of the Biomedical Division. So far as I know the Radiation Laboratory rarely attempted to intervene in the management of Donner Laboratory, although I could be mistaken in that belief. Most of our funding came through the Radiation Laboratory from the Atomic Energy Commission, and this was, of course, reflected in the laboratory's research program and management.

Initially, John Lawrence's biomedical group consisted of a half-dozen professional people and not much support staff, and the bottom floor of Donner Laboratory. As the years passed the staff increased enormously, as did space, budgets, and administrative demands. Then the effectiveness of laboratory direction became a significant matter.

Postwar Research in Donner Laboratory

Siri: Once the war ended, additional space was made available in Donner Laboratory, and the staff began to expand. Nello Pace joined the group but left after a short stay to take an academic appointment in the Physiology Department on the Berkeley campus. John Gofman joined the group right after the end of the war, and of course became an international figure in several fields, particularly in the effects of ionizing radiation. Jean Brandenburg, who came to the Laboratory

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about this time had special significance to me. I married her in 1949. Nathaniel Berlin joined the group. He already had his medical degree but he wanted to earn a Ph.D. in medical physics. Others included Robert Lovinger, Louis Wasserman, Lola Kelly, and Karl Bucher, who came from Switzerland to spend a year with us. Lowrey Dobson and Ernest Dobson, two brothers, joined as graduate students to work for their Ph.D.'s. Elsworth Dougherty came about this time. A year or so later Rex Huff, who figured prominently in the early studies of blood, came to the Lab along with Pat Weymouth and Ray Dunn, both chemists, Jim Robertson, who was there to earn a Ph.D. in medical physics, Paul Elmlinger, and Gordon Fryars from England. And about this time, James Born joined the staff. Born had both an M.D. and a law degree and some time later was appointed Assistant Director [1951–1970]. In any event Jones and Tobias and several others had academic appointments and now began to take on graduate students, and of course, they were teaching regularly as well as conducting research in Donner Laboratory. Some of their students would stay on and become senior members of the Laboratory staff in later years. Others left after a few years at the Laboratory to take positions in other institutions around the country.

Donner Laboratory in the '40s and '50s rapidly gained an international reputation for its work in the application of radioisotopes to biology and medicine, the use of the cyclotron or high-energy particle beams for physiological research, medical research, and irradiation therapy of the pituitary gland and other special forms of cancer that were difficult to treat by surgery. This [latter] was a program initiated by Cornelius Tobias, and expanded over the years. That program is still going.

Hughes: One name that I believe you didn't mention is Hal Anger who came in 1945 [in fact, February 1946], I believe. It seems to me that there were certain parallels in some of the

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work that you and he did. He particularly, of course, has been interested in instrument development. Did you ever work in collaboration?

Siri: We did, as a matter of fact. Soon after the war, we were applying both radioactive and stable isotopes to biological and medical problems. I was involved in some of the instrumentation—counters and other devices for detecting the radioactive isotopes. But I had also proposed development of an instrument that would enable us to use stable isotopes—carbon-13, oxygen isotopes. [I] proposed to Dr. Lawrence the development of a mass spectrometer with some novel features which would enable us to measure these isotopes. Developing this apparatus, I asked Hal Anger to assist with the electronic circuits. He did a very nice job of designing and building the electronic circuits for this apparatus.

Hughes: Was that typical of the way he usually worked? Would somebody come to him with a project and then he would be able to build the instrument and the circuits?

Siri: Yes, that's right. Hal participated in a number of projects in this fashion until he developed his own ideas about instruments for measuring radioisotopes. This led to an extremely successful culmination in his development of the Anger camera. It must be considered a brilliant breakthrough in the instrumentation for using [radioisotopes] in studies of disease in humans. Hal was properly recognized for this important contribution.

Hughes: And at that point did he become more independent of other groups?

Siri: Yes, once he was launched on the development of what

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came to be called the Anger camera, (which is not a name he applied, incidentally; he's a little more modest than that), he was given support and assistants. He then was in effect the principal investigator in that project. After that I had to find someone else [or] design my own electronic circuits, some of which I did. Later, we had an electronics group in the laboratory that could handle this kind of thing for us if we were too lazy to do our own.

Hughes: Can you say something about the transition from the Aeromedical Laboratory to Donner Laboratory, with all that those two names imply. I'm trying to get at the switch from the strictly war-related research to basic research or, in some cases, applied research. Did you get together as a group and draw up postwar plans? Or were these projects just natural occurrences?

Siri: No. Hardin Jones, John Lawrence, and Tobias did discuss their views of where the Laboratory ought to go. My contacts with John Lawrence were less frequent and less profound. I was still a pretty young kid at that time, although I had been advancing some ideas and everyone agreed that I should pursue them. But for the most part, responsibility for the nonclinical research in the Laboratory fell to Jones and Tobias. Then in the late '40s John Gofman joined the group, and that made a profound difference.

As to the transition, the funding from the Army Air Corps and from other military activities dwindled and soon disappeared. At the same time, research on the application of radioisotopes to biology and medicine grew rapidly to replace the war work. Major funding came from the Atomic Energy Commission as soon as it was formed. Considerable support was also received from other agencies and foundations.

Hughes: Was that just a given fact? There was never any question

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[that] this whole operation in Donner Lab, the whole Radiation Lab, would be an AEC-supported institution?

Siri: Yes, because all of the people involved in biology and medicine in the Manhattan Project were well known to each other, and that whole group became part of biomedicine in the Atomic Energy Commission. The transition to new fields of research was smooth and gratifying. We were no longer concerned with bomber crews. We were looking now at fundamental biological and medical problems—developing tracer methodology, applying physical methods, and developing new instruments for biology and medicine. This was a very exciting moment for us. It was a period in which a new discipline or several new disciplines were formed.

I can remember attending the first national meeting of researchers in the juvenile field of biophysics. A number of us were interested in forming a new society rather than being a part of the [American] Physical Society or the American Physiological Society. There were about 150 of us at the first meeting to form the society, and we knew there would be at least 150 different definitions of what a biophysicist was, so the question was avoided.

Anyway, a new society was created, and in due time the field became an accepted discipline. But not without a struggle, as any juvenile discipline has. It was not totally accepted by the biologists and was viewed with some suspicion by people in medicine, and of course the physicists looked on it as an inferior kind of scientific activity. There was a struggle for a decade or more to achieve recognition as a legitimate discipline.

John Gofman

Siri: Jack Gofman's arrival at the Laboratory was without

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pretension, but we soon sensed in him an extraordinary intellect. Even in later years when the controversies surrounding Jack Gofman developed, all of us who knew him over the years always regarded him as the most brilliant mind to come to Donner Laboratory. He, like Hardin Jones, was also a very, very fine human being, extremely thoughtful of the people associated with him. He earned the deepest affection as well as respect of his students and colleagues. On the other hand, he could be impatient with incompetence and what he perceived to be intellectual dishonesty. But Jack Gofman's graduate students thought he was God, and maybe they were right. They thought he was the greatest of teachers, the most brilliant of minds, and the most thoughtful of persons. And he was all of these things. All of us had the highest regard for him. But there was a problem.

Hughes: Was that problem immediately apparent?

Siri: No. When Gofman chose to enter a field of research, his progress and innovative thinking were faster than others could, or were willing to follow. It was as though he stomped on the accelerator of a high-powered racing car. But several times as the years passed, it would end in a great crash. Jack had a tendency to precipitate a confrontation when his ideas were at odds with views generally held by the rest of the scientific community and he felt his work was unjustly criticized. Unfortunately, when a significant difference arose, Jack would take an implacable position and one in which he did not treat his opponents at all generously. Jack could be quite abusive.

This was the case in the lipoprotein studies, which ended in a bitter confrontation between Jack Gofman and the rest of the research community engaged in [work on] coronary artery disease. It became quite an ugly interchange. The result was, of course, that Gofman's funds were cut off. I cannot judge the justness of either side in the

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controversy but it was inevitable that a body of people whose views, research, and persons were publicly attacked would react in kind. In this case, there were many of them, and they wielded great influence in scientific and funding circles. It ended in Gofman's withdrawal from research, academic life, and the Laboratory for several years. Some years later this whole process would be repeated in connection with his work on the effects of ionizing radiation.

Hughes: Did this controversy have a wider effect on the morale or even the operation of the Laboratory as a whole?

Siri: That's a little difficult to say. It did have an effect on the people working with Jack Gofman and his students, of course. Some of them were affected more than others.

Hughes: You're meaning more than just the fact that the funds were drying up.

Siri: Yes, that's right. However, some of his students did stay on. Well, most of them stayed on, and they found other funding, other support for his graduate students. For example, Alex Nichols stayed on as a permanent member of the staff, acquired an academic appointment, and today is full professor. Others left for other institutions. One person who stayed with Jack over the years, Art Tamplin, followed Jack to other places, such as Livermore, and really became a permanent part of Gofman's very small, close group.

Hughes: Do you remember how [Gofman] first became interested in lipoproteins? He came out of chemistry and medicine and, to my knowledge, had had no experience with lipoprotein research until he walked into Donner Lab, which also had had nobody doing lipoprotein research. How did the whole problem arise?

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Siri: I honestly don't know what caught Jack Gofman's attention and so intrigued him about lipoproteins. I have a vague impression of chatting with him about this in those early years but I don't recall after this length of time—more than 30 years now, just what it was that intrigued him.

Hughes: But it's your impression that he didn't arrive at Donner Laboratory with a lipoprotein project in his back pocket, so to speak?

Siri: He may have come here with an interest in this area, but it seems to me that when he first arrived at Donner, he was engaged in other kinds of activity, that the work on lipoproteins didn't start until after he had been at the Laboratory for some time—by "some time" I mean a year or so—because the initial work he did was with Hardin Jones. In this they were interested in some other fundamental medical research and tumor therapy, using colloidal zirconium and some other agents, particularly radioactive isotopes that could be put in the forms of colloids that might concentrate in certain kinds of tumors or could be injected into tumors.

It was only after he had been at Donner for perhaps a year or two that he started the lipoprotein work. That meant starting from way back because he had to develop a whole technique for measuring lipoproteins and set up a very large program. Anyway, his work progressed so rapidly that within a very short time he was being heavily funded for a major research program.

Hughes: Some of his research anyway was not directly related to the use and application of radioisotopes and cyclotron-produced radiations in a laboratory where that was the basic theme running through the research program. Did that ever present any problems?

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Siri: Not that I'm aware of. I'm not aware that the work on lipoproteins and atherosclerosis presented an administrative problem. It may have, but I don't think anyone would have seriously raised the question that it was not related to radioisotope applications and therefore inappropriate for Donner Laboratory. John Lawrence, after all, was director of Donner Laboratory, and if anyone had a question about it, I don't think they were about to voice it. [chuckling] Now there were some early controversies with the UC Medical School; I was not privy to those disagreements.

Hughes: What about the formation of the Division of Medical Physics? The talking stage would have preceded your arrival. That was 1944 when the committee with members from both sides of the Bay were meeting. But were you privy to any of the organizational aspects, when the Division actually began to take on students and courses had to be planned and a standard set?

Siri: No, I had essentially no contact with that. Those negotiations were handled by Lawrence and Jones and Tobias and, when he came, Jack Gofman, and others. I had no real contact with the academic part of the activities.

Personal Research

Siri: When we finished the wartime projects, among them the oxygen warning system [and] the automatic parachute device which sometimes worked, there was, as I said earlier, a shift in direction of the research. It was now peacetime. It was focused on biology and, more particularly, medicine. The thrust was the use of ionizing radiations, radioisotopes. Of course, as I was a member of the staff, I became involved in these projects.

There were several projects that I initiated, and others came out of discussions with Tobias and Jones and

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Lawrence. The earliest ones were the applications of some physical principles to biology and medicine, in the sense of doing a tracer analysis—what happens in a theoretical sense. So one of the things I started doing early was the calculation of radiation doses when radioisotopes were given to animals and humans, the mathematics of tracer behavior in biological systems. At that time, since people in medicine and also the biologists had very little background in mathematics, there were amusing encounters. The biologists just could not understand why we dealt with exponential functions and what all these equations were about. To their way of thinking that had nothing whatever to do with biology.

It wasn't until the new students in biology and medical physics began to emerge in, say, the late '40s and early '50s that they began to have an understanding of what it was all about. The postwar biologists were exposed to more mathematics and physics and began to understand what these processes were about and that biological systems indeed did have to obey physical laws just like the rest of the universe. That came as a very difficult problem for some of the older biologists.

Anyway, it was a wide open field. So it was fun in those days, back in the '40s, to look at biological systems and the application of, say, a radioisotope as a tracer, to develop the equations and calculate the radiation doses, the turnover of chemicals to which radioisotopes were attached. This led to the development of a handbook for the Army Air Corps. I was given responsibility for writing the handbook, which ended up as a monumental tome after two years of work.

Hughes: Did this all come about because of a reputation for expertise in this particular area?

Siri: Well, it could have been that. It could also have been that nobody else wanted it. [laughter] I'm not willing to swear

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to either of these reasons. Anyway, I recognized an opportunity and took it and apparently did reasonably well with it because it ended up with a report some two inches thick that contained almost everything that was known about radiation and radioisotopes at that time.

Hughes: Did that mean that you were culling the literature?

Siri: Yes, right. And this led to a book. I rewrote the whole thing and published a book in 1949. This book published by McGraw and Hill, *Isotopic tracers and nuclear radiations,* came out of the efforts to produce the *[Handbook of radioactivity and tracer methodology.]*

Hughes: What were they going to do with that report?

Siri: I haven't the remotest idea. [laughter] The report was done for Colonel Gage, who was an old friend of the Laboratory and of John Lawrence's. For whatever reason, they wanted a compilation of everything that was known about ionizing radiation and radioisotopes, and funded us to do it. I found it a fascinating job, and the book that came out of it was one of the first books associated with the Radiation Laboratory, and proved to be a best seller. It was widely used in this country and Europe as a textbook in classes back in the early '50s in radiation biology and biophysics.

Hughes: Then was it your idea to turn the handbook into the book?

Siri: Yes. Unfortunately, I never got around to doing a revision, which the publisher wanted, but I hated writing.

I started several other projects, and one of them was the development of a mass spectrometer that we could use to measure the stable isotopes that we were trying to apply, carbon-13, oxygen-17 and -18, and deuterium, which were at

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that time available as isotopic tracers, although not radioactive. There weren't too many radioactive tracers available to us initially.

The other [project] came out of one of the programs that Hardin Jones was running. This developed into a separate program that I had for the next 10 or 15 years. This was the development of means of measuring gross body composition. That gradually evolved as a major project, at least for me, and involved some new approaches to the determination and significance of gross body composition, that is, total fat, protein, bone mineral, and water. I began to develop the method of measuring total body water with deuterium initially, then tritium when it became available, or the means of measuring it became available.

[I] also developed a new approach to determining the density or volume of the body. Those two measurements, total body water and density, were necessary in order to make an accurate estimate of total fat, total protein, total bone mineral, and water. That became then for me a major activity.

Hughes: And the apparatus which, I guess, [came] along a little bit later, was that totally a conception of yours?

Siri: Yes, this was a helium dilution method. About a dozen of them were built around the world. A commercial company went into business trying to build them, but I guess they were a little expensive for most laboratories for the budgets that were available in those early years.

Crocker Laboratory and Joseph Hamilton

Hughes: I'm interested in the interrelationship between the Donner Laboratory and the Crocker Laboratory which seem to me to go in quite independent paths after the war. Crocker

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Laboratory and Hamilton of course become very much involved in fission product metabolism. But other than that, what about the directorship of the two laboratories which were practically side by side?

Siri: Joe Hamilton and John Lawrence were old friends, had been for many years. It was understood that John Lawrence ran Donner Laboratory and all that was connected with it. Joe Hamilton ran Crocker Laboratory and the work that was done there. There was an understanding that they were two independent, equal men. It wouldn't have worked otherwise. There was a strong mutual respect between the two men and their staffs. We always regarded Crocker as a separate entity run by Joe Hamilton, and Joe's people regarded Donner as a sister or brother organization, depending on one's inclinations. It was understood that we would have close working relationships and friendships, but that we were separate organizations. Joe and John were two different people; don't mix them up. So far as I was aware, it was always a good working relationship.

Hamilton was more concerned with the transuranic elements, but plutonium and strontium particularly, as I recall, and some of the other isotopes. He was conducting a very respectable program, investigating the health implications of these isotopes and also of nuclear radiations because he had the 60-inch cyclotron of course.

Joe had one fault, and that is he did not hesitate to use himself as an experimental subject. This cost him his life. He exposed himself far, far too much. He wouldn't hesitate to inject substantial quantities of radioisotopes to see what effect they would have, and he quite literally martyred himself. This would not be permitted today, of course, but he was a very conscientious investigator.

Hughes: Then he was totally aware of what he was doing.

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Postwar Radiation Safety Standards

Siri: Yes, he was aware of the risk. In the '40s and early '50s no one was quite sure what the risks were of moderate levels of ionizing radiation. We knew the history of the radiologists and, of course, were perfectly aware that high levels of exposure, say over 100 roentgens or rads depending on what unit one chooses to use, caused severe biological damage. We were aware that something between 250 and 400 rads resulted in a 50–50 chance of dying from exposure, that at 600 rad, death was 99 percent certain. We were aware of the signs, the symptoms, and the damage that would be caused. But down at the low end of the dose range, no one was sure, and unfortunately no one is sure even to this day as to whether there is a threshold and what the very low levels would do.

Hughes: Was Gofman really the first to become concerned about the low end of the spectrum?

Siri: No, he was not the first. He was unquestionably the most vocal. That, along with his prestige and position, commanded attention to his views. No, Jack was not the first to call into question the prevailing views on the effects of low level doses of radiation. He was the most effective person in making a national issue of it.

Hughes: When would you say that things begin to tighten up? When is it taken out of individuals' hands what they do themselves, and what they do to others?

Siri: You mean for ionizing radiation?

Hughes: For ionizing radiation and for the use of radioisotopes. I'm thinking of the genesis of human use committees, that whole

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apparatus which is so apparent nowadays. When would you say that that all began?

Siri: I would say the late '50s.

Hughes: Do you know what provoked it?

Siri: Offhand I can't give you a definitive answer. It was not a single event or person. It grew out of extensive research that gradually led to a better understanding of the biological effects of ionizing radiation. Jack, incidentally, was not part of that early evolution because he was at that time involved in the studies on atherosclerosis and lipoproteins. It was not until some time after he went to Lawrence Livermore Laboratory that he became deeply involved in radiation effects.

Throughout the '40s and the '50s limits on exposure and regulations on sources of radiation were steadily tightened as more was learned about ionizing radiation. The several councils on radiation protection standards were constantly reviewing the results of new research in trying to arrive at standards that in their best judgment were appropriate. One can always say that if in doubt, Thou shalt not have *any* exposure to ionizing radiation, and maybe that's what should be done; I don't know. This is of course what Jack Gofman and many others would like to see. The questions are, how much ionizing radiation can a population be exposed to without undue damage, and its companion, how does society perceive "undue." The question of what is acceptable and what is undue damage are matters for society to judge. In the case of automobiles, 50,000 deaths a year may be lamented but are accepted by society.

I don't recall a dramatic change in public attitude toward ionizing radiation in the '40s and '50s. During the '50s and early '60s there was a growing awareness of the seriousness

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of radiation exposure, but it did not evolve into a bitter public controversy until the late '60s and early '70s. And then a number of people, including Jack Gofman, became involved. [Ernest Joachim] Sternglass was another who emerged at about the same time. And then many distinguished scientists and some of the environmental organizations around the country began to perceive it as a form of pollution we shouldn't have to live with.

Hughes: But you weren't aware in the Laboratory of a sudden edict saying, well, now we've got to really concentrate on lowering radioisotope dosages; we may be harming our patients. I'm putting it in very simplistic terms, but that kind of attitude or change in thinking.

Siri: Well, yes. Let me see if I can describe the attitudes that I was part of and saw in my colleagues, both at Berkeley and elsewhere.

In the '40s and early '50s we were aware of the hazards of radioactivity or more correctly ionizing radiation. The question was always how much radioactivity could we safely give a patient or a healthy person participating in a research project. So we were very conscious of this and strictly followed the prevailing rules. As more was learned about the effects of ionizing radiation, the levels were steadily reduced. And of course we as researchers grew more and more concerned as well. Back in the mid '40s we didn't hesitate to inject ourselves with radioisotopes, to expose ourselves to ionizing radiation. We just assumed that reasonable levels weren't all that damaging. We still don't know. But as time passed, more and more people became concerned, as they did about all aspects of human exposure and risk whether it was radioisotopes or other toxic agents.

Some of the research we did in those early years, we now know, certainly warranted lower radiation doses for the patients and normal subjects. It wasn't that we were careless

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or just unconscionable about what we did. We just didn't know. The information we had indicated that this was harmless. We weren't going to harm patients or the normal subjects. I hate to think that any of the hundreds of subjects, maybe it's thousands, that I had to deal with were in any way injured by what we did. That would be pretty hard to live with. I don't know of any cases where this happened.

I don't know if I've fully answered the question about Joe Hamilton and Crocker Laboratory versus Donner Laboratory. I can only add that it appeared to be an agreeable working relationship between two sister organizations. In a sense it was not very different with Melvin Calvin, who set up his own group in molecular biology funded by the Atomic Energy Commission. That was again a separate organization from Donner Laboratory, but part of the Radiation Laboratory.

The Physics Department and Donner Laboratory

Hughes: The other element in this whole scheme, because we've talked about the Radiation Lab a little bit, is the Physics Department. How does it fit into the whole picture?

Siri: Well, originally the Radiation Laboratory was a part of the Physics Department.

Hughes: I'm thinking of the relationships to the operations of Donner Lab itself and the Division of Medical Physics. Was there any sense of loyalty there?

Siri: To the Physics Department?

Hughes: Yes.

Siri: Yes, but it was less strongly felt in Donner than in the rest

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Siri: of the Radiation Laboratory. Professor Birge was sort of the grand old man on the scene at the time, but Ernest Lawrence was the dominant figure, and Donner was a part of the Radiation Laboratory.

Hughes: So everything would come through the Radiation Lab?

Siri: That's right. The Physics Department had little to do with our day to day operations or research program. For most of the Radiation Laboratory staff there was a very close relationship with the Physics Department. Much of the professional staff had its roots in physics. And most of the senior people in the Laboratory, at least here on the Hill, were from the Physics Department—Ernest Lawrence, Ed McMillan, Luis Alvarez and others were professors in the department. The Laboratory was the Physics Department in large measure because the senior people here at that time were all faculty of the Physics Department.

Hughes: The Division of Medical Physics [was founded] in 1945 [and] was a division within the Department of Physics. Did that change anything particularly other than the paper relationship? Did the relationship become any closer with the Physics Department as far as medical physics on the Berkeley campus is concerned?

Siri: I really can't answer that question because I was not involved in the academic affairs of the Laboratory. I don't think I have enough information to say what that relationship was, other than as it had a bearing on those of us who were in the purely research end of things. And it wasn't significant.

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Research on Erythropoiesis

Hughes: Is now a good stage to talk about Rex Huff and his work with radioiron, which I believe figured pretty prominently in your research?

Date of interview: 23 May 1980

Siri: The application of radioactive iron to the problem of erythropoiesis developed very rapidly at Donner Laboratory, and the Laboratory quickly took a lead role in the use of radioactive iron and other radioisotopes in attempting to understand the kinetics of red blood cell formation. This work grew out of Dr. John Lawrence's longstanding interest in cancers of the blood, particularly polycythemia vera. Very little was known then about how red blood cells were formed, at what rate they were formed, and why, in the disease polycythemia vera, the bone marrow simply continued to churn out red blood cells at an enormous rate.

And there was no cure for the disease. John Lawrence had developed a treatment involving the injection of radioactive phosphorus. This suppressed erythropoiesis for a time, but it was not a cure. Radiation from the radioactive phosphorus destroyed some of the precursor cells in the bone marrow so that fewer red cells would be produced. But in due time the marrow would restore itself, and the disease would still be there. The object then was to find out how the bone marrow produced red cells and what the biological mechanisms were.

Rex Huff became involved in this problem and began to develop the radioactive iron technique for measuring red cell kinetics. Radioactive iron given to the patient became

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incorporated in the hemoglobin in the red blood cells. From this one can determine how fast they were formed and released into the circulation and how long they lived. At that time a small group had formed, led by Rex Huff, that included Paul Elmlinger, Tom Hennessy, and a visitor, Louis Wasserman, who was even then one of the country's leading hematologists. Wasserman remained with the group for about a year on sabbatical leave from Mt. Sinai in New York.

This work made substantial progress for several years, but then the group began to fall apart sometime after 1950. The ultimate fate of this small group was a curious one in terms of the fates of the people who were involved. In the early '50s Huff for various reasons—partly dissatisfaction with conditions at the Laboratory, differences of opinion, I think, with John Lawrence—left the Laboratory, went to the University of Washington, and some years later went into private medical practice in the East. Little further was heard from him. Paul Elmlinger had serious emotional and mental problems and was ultimately admitted to a mental institution. Tom Hennessy left to take a job at the University of California at Los Angeles and subsequently died at a relatively early age.

Hughes: When you say that the project began to disintegrate, do you mean that the people began to disperse or do you mean the scientific aspect of it also was not going as well as you'd hoped?

Siri: I mean mainly the dispersal of the group. But as a result, research on this specific problem at Donner lost much of its momentum. It was unfortunate because it continued to be an important field of research.

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Date of Interview: 5 June 1980

White Mountain Research Station (1950)

Hughes: Mr. Siri, I think you wanted to say something about the founding of White Mountain High Altitude Station.

Siri: This is only by way of a comment on, let us call it, the discovery of the White Mountain site for a high altitude station. The idea for a high altitude station was Professor Pace's. I had known Nello Pace from the time he joined Donner Laboratory briefly immediately after the war. He became a member of the faculty in physiology at UCB. It was Nello's idea to establish a high altitude research station somewhere, preferably in California. He had a longstanding interest in environmental physiology, and this was, of course, natural. There was a need for a high altitude station for physiological investigations because the low pressure produced physiological changes that made it easier to study the body's regulatory processes under stress, for example. In any event, Nello Pace and I set out for the high Sierra Nevada in search of a potential site for a station. The obvious first candidate to examine was Mt. Whitney, the biggest mountain in the lower 48 states. The two of us, suitably geared, climbed to the summit of Mt. Whitney. Nello saw that it was, although rocky, relatively flat and a structure could be put on the summit. There was, in fact, a stone hut there already that had been placed there many years ago by a survey party. There were two problems with that site. One, it would have been very difficult, perhaps essentially impossible, to establish a suitable access road for transporting construction material, equipment, and personnel. The other problem I explained to Nello as we sat on the summit of Mt. Whitney.

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At that time I was an active member of the Sierra Club, and I pointed out to him that this was not the place for a structure of any kind, even a scientific laboratory. It was part of the wilderness, and he could be certain that the conservationists, who were at that time primarily wilderness advocates—which included me—would unquestionably challenge this as a site for a high altitude station. And I could guarantee him a vigorous battle. Nello took this in good grace and understood. Moreover, he could also see that it would be almost impossible to build a road to the summit of Mt. Whitney. The alternative choices in the Sierra Nevada were not very good, primarily because he wanted a laboratory, understandably, at the highest possible altitude. A laboratory at moderate altitudes would not serve the purpose.

From Mt. Whitney, we crossed the Owens Valley to the White Mountain range. Nello had made some inquiries about the area beforehand and had learned that there was a road up White Mountain and there was some kind of installation somewhere on the mountain. The two of us drove up the mountain and ultimately found the station that had been established there some years earlier. It had been used by geophysicists, atmospheric physicists, and cosmic ray physicists for some years, and they were, we learned, about to abandon it. Nello saw this as an opportunity for the University of California to take over the existing station at about 10,000 feet and perhaps then extend facilities all the way to the 14,200-foot summit.

The top of White Mountain consists of a series of relatively flat areas, nothing like the sharp crests of the Sierra Nevada. It was also the site of the bristlecone pines, the ancient pines that had life spans of some 3,000 years. While we were there, we borrowed a jeep and drove from the station at 10,000 feet perhaps 8 miles to a few feet below the summit of White Mountain. It was evident that there would be excellent sites for stations at 12,000 feet and at about

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14,000 feet. From what he had seen, it was clear to Nello Pace that this was the best of all possible sites for a high altitude station.

Following that trip, he then proceeded to persuade the University to acquire the necessary leases, and before long had established an excellent high altitude laboratory at 10,000 feet, at 12,000, and one very close to the summit. During the years that followed, a great deal of valuable work came out of those stations from researchers from all around the world.

Hughes: Was there any permanent staff?

Siri: Yes, there was always a person in residence to manage the station and usually several assistants. Ultimately the station manager was a scientist in his own right and maintained the station through most of the year.

Hughes: But Donner Lab never used this station?

Siri: No. For our purposes, we had a high altitude chamber in the Laboratory and, as it turned out, we were generally more interested in altitudes substantially higher than the summit of White Mountain. For example, in the many animal experiments we conducted at Donner Laboratory, we normally operated the chamber at 20,000 to 24,000 feet. Even on the occasion when I spent 4 days wired to all kinds of instruments in the chamber, it was set for something over 17,000 feet. So Donner Laboratory had very little contact with White Mountain other than through our personal relations with Professor Pace.

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Scientific Expeditions: Peru (1950)

Siri: [The research on erythropoiesis led] to several interesting expeditions. One of them was the first Peruvian expedition in 1950. This expedition resulted from contacts that John Lawrence had with Professor Alberto Hurtago at the Medical School at the University of Lima in Peru. Professor Hurtado had been doing studies on blood and high altitude physiology for many years in Peru. He was an internationally recognized authority, had a very good group of people working with him, was a brilliant investigator. He had established by 1950 a high altitude laboratory in the Andes some miles east of Lima, Peru. Through that contact and an invitation from Professor Hurtado, John Lawrence asked the erythropoiesis group, which was still intact at the time, and me to do some studies at high altitude.

The justification for all this was that humans at high altitudes develop polycythemia, that is, a large volume of red blood cells, much like that in the disease polycythemia vera, but in this case to compensate for the low oxygen pressure. Since red blood cells are the carriers of oxygen in the blood, the body reacts on the premise that the more red cells the better when it is exposed to low oxygen pressure. The lot of us—Lawrence, Huff, Elmlinger, Hennessy, Wasserman and I—headed for Peru in 1950, taking with us all equipment we would need, including the radioisotopes to do the studies at about 15,000 feet in Professor Hurtado's laboratory.

We spent about six weeks in Peru conducting studies on residents at high altitude and on people brought from sea level, that is, from Lima, to the laboratory. Out of this came several papers which were fairly fundamental in demonstrating the effect of oxygen tension in the blood on the rate of production of red blood cells and the manner in which this happened. That was a useful expedition and in the space of 6 weeks, we accomplished a great deal.

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Not all of the members of our team were equally comfortable at 15,000 feet, and some of them felt the altitude rather severely. I had been assigned a specific set of tasks, [as] each of us was, in carrying out the work. I guess one of the additional purposes in my being on the expedition was to fill in if others fell out. I had had considerable experience at moderate to high altitudes by then and found myself completely at home in this high mountainous terrain in the Andes.

Hughes: Did those that were not so comfortable stick it out for the 6 weeks?

Siri: They did. Wasserman felt it strongly, but he courageously stayed on, feeling absolutely miserable most of the time. John Lawrence was the most severely affected and did not spend a great deal of time at the high altitude laboratory, but went back and forth to Lima.

Hughes: Do you think it's significant that in the high altitude studies during the war he was also seriously affected?

Siri: I do not know. Not everybody accommodates to high altitudes equally well, and Lawrence seemed one of those who did not tolerate high altitudes. This is purely a matter of chance and the particular physiology of the person involved. I happened to tolerate it quite well. Part of that of course may have been psychological. I felt at home in the mountains in that altitude. To the others it was a novel experience. One can understand that they were not wholly comfortable. In any event, the work was done, and I think it can be said that it was extremely useful in advancing the understanding of red blood cell formation.

Hughes: Who paid for the expedition?

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Siri: Dr. Lawrence raised funds from several sources to cover the costs. He also obtained the assistance of Brigadier General Otis Benson, Jr., commandant of the USAF. School of Aviation Medicine, who arranged transportation for us to Peru on the regularly scheduled military air transport flights.

Sometime after we returned (I don't remember the specific time but it may have been a year or two, possibly more), the group began to break up, although some of us continued to work on the use of radioactive iron in the studies of erythropoiesis. In the meantime, others had come on staff. There was Nathaniel Berlin, particularly, who became quite close to Lawrence and carried on some of the work in erythropoiesis.

Hughes: Could you say something about why Lawrence seemed to have such faith in Berlin?

Siri: Well, Nat Berlin is an extremely able person. He is a first-rate researcher. He had a very fine sense of organization and administration, and this was evident to Lawrence as it was to others. He was a firm, no-nonsense sort of person, but completely reasonable and always helpful. Lawrence learned to lean on him more and more, both for counsel and for handling administrative matters. This was natural because Berlin would handle them so effectively and skillfully that Lawrence found him, if not indispensable, at least extremely valuable. At the same time Berlin was turning out very good work, publishing frequently, and had a growing reputation in the world of hematology. So it was natural that Lawrence would rely on him as a very dependable person, both for his scientific skills and achievements and his management skills.

There was a problem, however; it's one that is not completely clear to me even today. As Berlin's reputation grew internationally, it was natural that he would want an

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erythropoiesis, but no one provided the kind of leadership that had initially been given by Rex Huff and then by Nat Berlin.

Hematology was one of John Lawrence's primary interests, but he was far too preoccupied with management of the Laboratory and numerous clinical studies to provide the detailed, day-to-day leadership that Huff and Berlin provided. But beyond this I think there was a prevailing feeling among his colleagues, both in and out of the Laboratory, that he was not a gifted researcher or research leader. He had to depend, as most directors must, on particularly gifted people. This was perhaps a weakness, or at least it was perceived as a weakness in John Lawrence, in that during the last three decades some very fine people came to Donner Laboratory, would like to have stayed, but for one reason or another Lawrence seemed unable to provide them with the incentives to keep them. Huff was one of these; Nat Berlin and Saul Winchell were others. There are any number of other scientists who should have been kept on at Donner and who would have contributed to a very strong research organization. As the years passed, the Laboratory grew weaker and weaker.

Hughes: Do you have any explanation for that reluctance on Lawrence's part?

Siri: I don't know the answer to that. I can only offer conjectures. But there was a pattern that many of us seemed to sense. Academic appointments in physics, physiology, and possibly chemistry were obtained by Donner staff but this never seemed possible for people in medicine. I believe John Lawrence tried to obtain appointments for outstanding Lab M.D.'s but it was our impression the Medical School was unreceptive. People like Nat Berlin and Saul Winchell were not given academic appointments which they fully merited. I don't really know the answer. But it seemed to

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center on brilliant people who came to the Laboratory in medicine. Lawrence did not have this problem with people in other fields, physics particularly—a field that he really did not understand but seemed to have a great deal of respect for.

Peru (1952)

Hughes: The 1952 Peruvian expedition—is that the next step?

Siri: In my personal experience at the time, it was the next most memorable event. But a good many things had happened in the meantime. I'd gotten launched on several research projects, one of them involving the metabolism of tritium in the human body—its relationship to a variety of diseases that the Laboratory was beginning to take an interest in, particularly endocrine relating to the pituitary. I'd been involved in the development of the application of radioisotopes, particularly tritium, to medical and biological problems. By that time I guess I had also started developing a method for determining the gross composition of the body, which we felt was a useful addition to the studies on the diseases the Laboratory was beginning to examine—the endocrine diseases, the blood diseases, other forms of cancer, and regulatory malfunctions. In any event, I was also partly involved in the iron turnover studies.

About the middle of 1951 I went to John Lawrence with a proposal to do studies at much higher altitude than we had done them at Peru. The reason for this was that the 15,000-foot altitude of Professor Hurtado's laboratory where we had done the earlier studies was marginal in terms of certain critical physiological effects. What I proposed to Lawrence now, however, was to do these studies at altitudes up to 20,000 feet. I had evidence that we would then be at or above the critical altitude for which the body could accommodate itself. I thought this was important because

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we would then be putting the hematopoietic system and other regulatory systems in the body at their limit. Stressed in this fashion, we could learn something more about how these regulatory systems worked and, most particularly, what happened in regulatory systems associated with the production of red blood cells, erythropoiesis. I presented John Lawrence with a proposal for a second expedition to Peru. On this expedition, I proposed that we take a group of climbers who could get to 20,000 feet and do the studies on them at [this altitude].

John Lawrence was quite enthusiastic about this proposal, and we proceeded then, with his blessing, to organize it. I got a group of climbers together, in addition to Nat Berlin, who would not attempt to get to 20,000 feet but would go as high as he could, and of course John Lawrence. Huff, Elmlinger, and Hennessy had all left by this time; so had Wasserman. So Berlin and Lawrence were really the only two, in addition to myself, who could perform the studies.

I would have to carry out the studies at the very high altitude because it was unlikely that either Lawrence or Berlin would go that high, both because of the altitude and because of the technical difficulty of just getting there. It meant climbing and hauling up all of the scientific paraphernalia. With Lawrence's assistance the project was funded, and again we had the support of the Air Force in transporting us to Lima, Peru. There were six of us who could climb, so I had six subjects including myself, [for] whom I expected to be able to do blood volumes with radioactive phosphorus, iron turnover with radioactive iron, and a host of other tests. All of these I had packaged and would be portaged by us up to the highest altitude; at least that was the intention.

Hughes: But [there were] none of the endocrine tests that figured in later expeditions? I'm thinking of the work on the adrenals during the Everest expedition.

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Siri: No, work in that field came somewhat later. The Laboratory had not yet developed a strong research program in that area. So this [expedition] centered on the hematology of extreme altitudes.

That was an unfortunate expedition because one of the members of the expedition by the name of [Oscar] Cook, who was a graduate student at the Davis campus, died on the way up the mountain. This had a demoralizing effect on the whole expedition and of course distressed John Lawrence considerably. [Cook] had died at about 16,000 feet, and died within 12 hours from the onset of the first symptoms. On post mortem in Lima, it appeared that he died from something akin to bilateral pneumonia. Both lungs were full of liquids, and nearly all of his tissues showed numerous small hemorrhages, including his brain; all signs of oxygen starvation. No one was certain about the cause of his death. It could only be assumed that it was an infectious process, probably pneumonia.

We did not realize until some years later that what we had seen was a classic case of primary pulmonary edema, which was not described as a disease entity until some years later. Had we been more observant, we might perhaps have guessed that this was a primary process due only to hypoxia and unrelated to infection or prior existing conditions, but we didn't. Several years later Dr. H. Haltgren, of Stanford Medical School, and Dr. Charles Houston demonstrated by their studies in Peru that some people are susceptible to primary pulmonary edema. Their lungs simply fill with fluids when they go to moderate to high altitudes. Since that time, I've had several cases on later expeditions, and we recognized it straight away and were able to save the climbers.

In any event, we lost Oscar Cook on that expedition. By the time we got back to high altitude, it was just too late to carry out all of the experiments. The impact of Cook's death left everyone in a rather troubled state of mind. We also had a schedule to meet, so the work we had intended to

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do simply was not accomplished. That ended the expedition. There were some observations that were made, but we did not succeed in achieving our primary objective and this had to await our trip to Everest some 11 years later.

Hughes: Did you find climbers willing to participate in scientific experiments? Were they willing to be guinea pigs?

Siri: The answer to that is yes. They didn't like it, but for the opportunity to go to the Peruvian Andes, they were willing to put up with anything. It required a great deal of needle poking. That is, they would have an indwelling needle in the vein for many hours. I would be taking blood samples fairly frequently throughout a full day. They would have to collect urine for 24 hours at a time. All of these things were inconvenient for a climber, but they were willing to put up with it. They certainly didn't like it, but it was a price they were willing to pay to go to the Andes.

On our return, settling the affairs of Cook took a great deal of time and caused a great deal of distress, particularly for John Lawrence, who felt very responsible for the loss of Cook—perhaps took it harder than anyone else. Lawrence was very sensitive about personnel matters of this kind.

Hughes: Was there anybody working intimately with you on the body composition studies?

Siri: There were from time to time. A man by the name of Theodore G. Prentice came to the Laboratory for about a year and worked very closely with me on tritium before returning to his own institution. Gordon Fryers, who was a visitor from England, worked with me for some time, and there were others from time to time. For the most part, however, I was assisted by a number of technicians and occasionally students.

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Hughes: Were you seeing or studying a string of volunteers?

Siri: We did have normal volunteers. Among them were students from the University, Laboratory personnel, others whom we acquired through various sources in order to establish normal values for adults, that is people from 18 to 75, to see what the population of healthy adults looked like, i.e., the variations in total water, total fat, protein, bone mineral, and the rate of turnover of water in the body. This had secondary significance related to the metabolism of radioactive fallout from nuclear testing, and to the dose of radiation that would be received by anyone inadvertently exposed to tritium.

Hughes: Did Hardin Jones show any particular interest? I'm thinking of his theory of aging which seems at least peripherally related to what you were doing.

Siri: Yes, he was interested. He did not take an active part. In fact, I think I have to say that Jones really set me onto these studies, suggesting sometime earlier that it would be valuable to know what people consisted of—are there better ways of determining this, particularly in patients. As my project grew, it was clear that it was applicable to some of the other studies that were beginning to expand now on cancer, heart disease, and the endocrine dysfunctions, particularly pituitary adrenal cortical diseases. And so there was from that time on a steady stream of patients—cancer patients, endocrine dysfunction patients, acromegalics, patients with Cushing's disease and heart disease. We also looked at groups of obese people to see what the differences were in their bodily functions and composition as compared to athletes, and as compared to other normals in a wide range of ages. So there gradually accumulated a mass of information on body composition, the turnover of water, the rates of formation of protein, residence time of tritium in the body.

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This also involved the development of the methodology. It was during this time that I developed the helium dilution method for determining body density or rather true tissue volume. That combined with the use of radioactive water enabled me to make a far more accurate measurement of body composition than had been possible earlier. It was also a method that was applicable to people who were quite ill. Some of the earlier methods involved dunking them in water and weighing them under water. This was rather difficult for people with advanced diseases, particularly heart diseases, and with young children. Our method was passive, somewhat more accurate, and could be used on all of our patients. That work continued till the day I left Donner in 1974.

Himalayas (1954)

Siri: Well, let's see, the next expedition was to the Antarctic.

Hughes: No. Well, at least in my notes, I have the California Himalayan expedition, which was 1954.

Siri: That expedition was wholly independent of the Laboratory and the research work we were doing. That was organized as a Himalayan climbing expedition.

Hughes: So there was not a scientific component?

Siri: There were several scientific components to it. But it was primarily conceived and organized as a mountain climbing expedition. The object was a peak called Makalu, the fifth highest peak in the world, located about 12 miles from Mount Everest. It had not been climbed. There had been no prior attempt to climb it. In 1954 that part of the Himalaya was very poorly known. There were no reliable maps, and this was to be the first major American attempt on

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Siri: a big peak. In fact, it was the first American expedition to Nepal following the opening of the country in 1949. Nepal had been closed for several centuries to all foreign visitors, and this was now some five years after the country was open to people from the outside, which followed a bloodless coup in Kathmandu and a change in the politics.

We did, however, have several research programs. As organizer and leader of the expedition, I was not in a position to attempt to carry out research projects simultaneously. I found that organizing and leading an expedition of this kind is a fulltime job. However, Nello Pace, professor of physiology and director of the White Mountain Laboratory, did come with us to carry out studies that he had begun at the high altitude station at White Mountain. And there were several other scientists. We had a botanist with us, and one of the climbers was a glaciologist. Pace did his studies, which mainly involved at that point collecting urine for later analysis of hormone levels. The other studies were botanical, zoological, and glaciological.

Hughes: What happened to your laboratory when you took off like this? Were you able to keep your assistants working at something?

Siri: Yes. The expedition lasted five months. I had set up a plan of work for the technicians, and they were able to run the tritium and other measurements as a routine matter on those patients who were sent to us by the medical research programs. So that work continued while I was gone.

Hughes: Dr. Lawrence was perfectly willing to have you disappear for five months?

Siri: Well, not completely. He could not say no, of course, and he did give me his blessing and tried his best to keep my

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salary going as long as possible. But it was not possible to cover my salary for that whole time, which meant that I was without income for several months and used up whatever leave I had. But there were some adjustments that he made, and for this I was grateful to him. I think he got some satisfaction out of knowing that a member of his staff was involved in what was the first major American Himalayan expedition. He was quite sympathetic and interested.

Hughes: When you came back, what happened? Was there any particular shift?

Siri: None that I can recall specifically. I continued to be involved in the studies that had been launched. In the meantime, the pituitary irradiation program had advanced. So had some of the other medical research programs. I found myself more and more involved in them in performing the studies on water turnover, body composition, and some work related to blood.

Hughes: In 1955 the first breast cancer patients are treated with protons. Did that mean an influx of business, so to speak?

Siri: Yes. I believe we saw all of the patients who came through the Laboratory in the several programs on heart disease, breast cancer, and pituitary irradiation. We were running from two to sometimes as many as six patients a day, several times a week. So there was a steady flow of patients to keep us quite busy. In the meantime, I was still developing the methodology and instrumentation and taking a peripheral part in several other projects.

Hughes: Had your association with Hal Anger ceased by this stage?

Siri: That's correct. He was, of course, deeply involved in his

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own project now, developing the Anger camera. It was proving to be a highly successful project and was receiving wide recognition as a completely new approach to the problem of visualizing what was going on inside the body without entering it. His project was now well under way.

Hughes: For a man whose background was largely physics, it seems to me, you are becoming more and more a physiologist. Was this requiring outside reading? Or were you essentially picking it up as you went along?

Siri: There appeared to be little opportunity to take courses in physiology. What it did require, however, was intensive reading in physiology and some areas of medicine, particularly hematology and some aspects of cancer, not only in the texts, but the current literature. But it nevertheless was an effort in self-education. And of course I was in daily contact with the rest of the staff, most of whom were physiologists or M.D.'s.

Hughes: There was a lot of medical and scientific exchange?

Siri: Yes. So [I learned] partly by osmosis, partly by intensive reading and trying to stay current with the literature, at least in those areas that had any relevance to what I was doing.

Hughes: But there weren't formal occasions set aside where the people concerned with these areas of research got together? It was a more happenstance kind of exchange?

Siri: It wasn't altogether happenstance. Those of us who were working on common problems were in constant touch, that is, either in meetings on specific problems or research efforts, or daily conversations, whether in the hall or in our offices or laboratories or wherever. So there was this steady

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exchange of information, as there should be in any institution, particularly among those who have a common interest, or are working on common problems or projects. Over and above that, there were more formal things, like seminars. I found myself in time running the seminar, bringing people from around the country to talk about current medical and physiological research.

Hughes: Why was it primarily physiological and medical when you were basically biophysicists?

Siri: Oh, because that was the thrust of research in the Laboratory. This was the Laboratory seminar, and it had to cover all the interests and particularly the major programs of the Laboratory. Most of the people attending the seminars and therefore the speakers were in medical and physiological research.

Bolivian High Altitude Study Project (1957)

Hughes: How did the Bolivian expedition come about?

Siri: As I recall, this was an idea that came from Dr. John Lawrence. A good friend of Dr. Lawrence's, Dr. Charles Webster, and Dr. Lawrence had been discussing their mutual interests in blood and circulation. Dr. Webster was a cardiologist. And apparently the idea emerged that it would be useful to conduct further studies on blood at a high altitude station in Bolivia. This station had come to Dr. Lawrence's attention and he pursued the idea of sending a small team to carry out the studies on erythropoiesis that we had not completed in Peru in 1952 as a result of Oscar Cook's death.

He asked me if I would undertake the studies with Dr. Webster assisting. I was hardly reluctant to turn him down and very quickly sketched out a research project outline

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involving a number of studies on erythropoiesis, and this was accepted by Dr. Lawrence. So Charles Webster and I then arranged to fly to Bolivia and spend about a month, both in La Paz, Bolivia, and at the high altitude station at Morococha at an elevation of about 17,500 feet.

The station had been established by Professor Escobar, who was a cosmic ray physicist and built the station for high altitude atmospheric and cosmic ray studies. It was well equipped and quite comfortable and sat very near the summit of the 18,000 foot peak some miles outside of La Paz, Bolivia. The name of the mountain is Morococha. It was an easy drive from La Paz, which lay at about 12,500 feet on the Altiplano of central Bolivia, to the station about ten miles east in the Condillera Real.

In any event, I used radioactive iron, radioactive phosphorus, and tritium to make a variety of measurements on iron turnover, blood volumes, and sundry other physiological measurements. The net result of all of this was quite interesting since this is the highest altitude, or alternatively the lowest pressure at which humans had been measured for erythropoiesis. At this altitude they were producing red blood cells at one and a half to two times the rate at sea level. The turnover of iron in the blood was much more rapid than at sea level. It was interesting to note in the x rays that the Altiplano Indians were obviously better adapted to these high altitudes than people from the lowlands. They had larger right hearts and relatively large lung volumes compared to the rest of us.

In the course of these studies we met many of the research people and medical faculties at the university and the La Paz hospitals. They were all eager to participate in the studies, and five of them took an active role in helping with the studies.

Hughes: Was high altitude physiology a preoccupation of Bolivian science, would you say?

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Siri: It was in some quarters, yes, and understandably so, because a major part of the population of Bolivia resides at moderate to high altitudes on the Altiplano. The airport at La Paz is over 13,000 feet high. Many of the Altiplano Indians live at altitudes between 13,000 and 17,000 feet. There are some problems associated even with the native population at these altitudes.

There are other problems associated with people of European descent. Our own embassy in La Paz had an ongoing problem with its staff. The ambassador invited me in on one occasion to discuss their problem, which they saw as a medical problem, and from their point of view a very practical one because many of the people who came to the embassy from the States were simply ill. They could not tolerate the 12,500 foot altitude and had to be returned to lower altitudes. I quickly discovered they made the mistake of flying people in directly from the States without allowing time for acclimatization. I recommended some courses of action they could take to reduce the incidence of "soroche" or altitude sickness.

Hughes: The people who were distressed by the high altitude, was this a chronic complaint? It was not something they would eventually adjust to?

Siri: It varied. With some persons it was simply a matter of acclimatizing, and that would take a week or two at most. For a few people it was an inability to make an adjustment to that altitude. There were not too many such persons, but there were enough to concern the embassy.

Hughes: Was there any common parameter?

Siri: None that we could find, no. We could not judge from physiological or medical tests whether a person could or could

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not acclimatize well to altitude. We still have no reliable tests for this. But there is also a high altitude disease, known as Munge's disease, in which even the natives lose their capacity for acclimatization and must be sent to lower altitudes. The nature of the disease, at least at that time, was not very well known, but fortunately it was uncommon.

Hughes: Can you say something about the breakdown of labor? I mean, what were you doing specifically, and what was Charles Webster doing specifically? What part were the Bolivians [playing], who were presumably there on a fairly permanent basis at the station?

Siri: Well, we had a number of tests to perform. I provided the immediate direction for those involving the radioisotopes and in fact conducted them, but with the assistance of three or four members of the staff of the university medical school and the local hospital. Dr. Webster, who was not familiar with these techniques and the use of radioisotopes, nevertheless participated, did the physical examinations, and did some of the routine blood tests and made some examinations of his own related to the heart and circulation.

International Physiological Expedition (1957)

Hughes: Are we ready to move on to Operation Deep Freeze?

Siri: All right.

Hughes: I was wondering how you became involved, who [else] was involved, and what was the funding?

Siri: For the Antarctic expedition?

Hughes: Yes, the International Physiological Expedition.

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Siri: Yes, the expedition to the Antarctic, which became known as the International Physiological Expedition to Antarctica—INPHEXAN was the acronym—the idea of Nello Pace. He had been with me on the expedition to Makalu in the Himalayas. We had a common interest in environmental physiology, and this seemed to Nello an excellent opportunity to go into an area to examine the effects of stress. It would involve measuring regulatory mechanisms and more particularly, the regulatory hormones in men under stress in an isolated and stressful environment.

Hughes: Do I understand then that in this particular expedition the blood turnover studies had a lesser role?

Siri: Yes, that's right. We had little reason to believe that exposure to cold, isolation, and the other elements of stress would affect the blood system, but there was ample evidence that it would affect other regulatory systems in the body. How do men acclimatize to cold? What happens to them when they're exposed to these conditions for an extended period of time? So the object was to carry out a variety of physiological measurements that would hopefully disclose what the body does when it's put under adverse conditions. For Pace and myself this involved principally collection of blood and urine for analysis of adrenocortical hormones, which are among the principal regulatory hormones that respond to stress.

Hughes: From everybody? How many individuals were you dealing with?

Siri: Well, we obviously had to be selective; we couldn't collect urine and blood from everybody in the Antarctic. There were a dozen nations represented, and many stations, even in 1957. This was the Geophysical Year and so there were geophysicists facing all over the continent.

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Siri: Nello Pace and I were joined by Charles Meyers of the Naval Biological Laboratory, Oakland, Peter Hildebrand of Nello's staff, and by a British team consisting of Griffith Pugh and Charles Adams. Griffith Pugh was an internationally known environmental physiologist. He had been with the British Mount Everest expedition, and we had carried out some studies with him on swimmers in an English Channel race, again looking at stress reactions. The team headed for the Antarctic in the fall of 1957 and planned to return in March 1958. We would not stay over the Antarctic winter on this trip. Transportation was provided by the military services, and we arrived in the Antarctic completely equipped, including tents and food.

Our base station was at McMurdo Sound where we were assigned to one of the huts. Nello and I arrived first; the rest of the team would be following from New Zealand in another week. The first night there, Nello Pace and I discovered that the huts were kept at such a high temperature, there was no possibility of finding someone stressed by cold exposure at the base. The next day we abandoned the overheated hut we had been assigned to and moved into a canvas-covered affair that had all the benefits of hot and cold running drafts and snow blowing in through the ill-fitted plywood door, and finally felt comfortable in our sleeping bags. In due time our equipment arrived by ice breaker from New Zealand. We then set about carrying out the studies that we had planned.

Hughes: Who now is composing the team? How was the team assembled?

Siri: Well, Nello Pace, Peter Hildebrand, and I performed those studies concerned with adrenocorticoids, that is, those hormones that were known to be involved in adjustments to stress and were normally involved in the regulation of the body's functions. Griffith Pugh and Adams were concerned

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primarily with another battery of physiological tests that they had designed. Charles Meyers was a bacteriologist, and his primary mission was to survey the Antarctic for microorganisms, at least in places we visited. That in itself makes an interesting story.

In order to carry out the studies that we had planned, we needed to go out to the remote stations, because that's where we would find the men who were exposed to cold and isolation and other stresses. Hence, in the months that followed we found ourselves in some of the remotest parts of the Antarctic, visiting field teams and staying with them for about a week at a time as they traveled across the Ross Barrier, the polar plateau, or living in remote camps. For the most part this involved collecting blood and urine and doing some preliminary chemistry in the field. [This] meant that we set up our laboratory on the snow and performed the initial chemistry to prepare samples that we would take back to our laboratories for analysis. If nothing else, this at least made curious photography. Our ventures took us far out over the Ross Shelf on extended tours to remote posts where geophysicists, glaciologists, and atmospheric physicists were at work. It took me to the South Pole itself.

The ostensible reason for that trip was the arrival there of Sir Edmund Hillary. He had traveled to the South Pole by tractor to meet Sir Vivian Fuchs, who was arriving from the opposite side of the continent. I flew to the South Pole about the time Sir Edmund Hillary arrived there, with the intention, of course, of collecting blood and urine from Hillary and his men, which I did. It was particularly interesting because Sir Edmund Hillary, who I knew slightly from earlier expeditions, particularly in the Himalayas, is a man whose courage I admire immensely. However, at the sight of a 10-milliliter syringe and a lengthy needle, even Sir Edmund Hillary's courage seemed to evaporate, which probably speaks for my technique.

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In any event, we did get to the South Pole, and that was one of the two memorable adventures I experienced in the course of this expedition. On our way back, the airstrip on the Ross Sea ice was covered with fog, and it was uncertain that the plane could land. The navy pilots were old hands at this kind of thing. They had the airstrip, we discovered later, lined with every piece of crash equipment they had available, fully expecting that they would be picking up a lot of pieces. The pilot put the plane in a gentle glide and we all waited for the shock, which we expected to be our last. He happened to hit the right place, otherwise it could have been disastrous.

However, the most interesting part of that expedition in terms of adventure was the trip we made to the Victoria Plateau, which was part of the upper ice plateau of the continent. It had never been penetrated as far as we were going, so we were exploring new land. It took more than a month to persuade the station commander at McMurdo to permit us to go, and even the navy pilots were reluctant to take us there in the belief that they would have a great deal of difficulty landing and then ever finding us again. In any event, we did go. We accompanied two glaciologists who were there to study the ice cap. What we did learn from them was that we were camped on top of two miles of pure ice and that this area was the origin of all the bad weather that circulated into the southern hemisphere. The navy pilot was able to recover us, I guess to everyone's surprise, and by virtue of a large corner radar reflector that I constructed the day before they started their search. It was made from window screen wire and bamboo poles that I had picked up and thrown into the plane just before we left McMurdo Sound. This was to prove to be one of the most valuable objects in the Antarctic, according to the pilot, because he bundled it all up and put it in the plane to take back with us in the event he had to do this kind of nonsense again.

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The results of that expedition were rather curious in this sense, for although we had obtained adequate specimens from men who presumably had been under stress, cold stress particularly, in some cases for many months, we could find little evidence of physiological stress in these persons. So far as the adrenal cortex and the regulatory systems were concerned, everything was normal.

One of the interesting findings in our explorations was not physiological, but bacteriological. Everywhere we went we collected with sterile implements samples of snow, ice, and air. These were brought back to our base camp where Charles Meyers cultured the samples. Among the first of these samples were those that I picked up at the South Pole itself, some of them from a tunnel that went 60 feet deep into the ice. When Meyers cultured these three days later, he raced in to show us his findings. Th petri dishes were covered with growths of microorganisms, which he then proceeded to identify. It was obvious then and later that the Antarctica is not a sterile place. There are microorganisms—bacteria, fungi, yeasts—just about everywhere, including 60 feet under the ice at the Pole.

Hughes: The next step, as I see it anyway, is a big one, namely Everest. Is there anything that you care to say about those intervening years, between 1957 and 1963?

Siri: Nothing of great significance. I was engaged for the most part in the studies that I had been doing on gross body composition, and discussed earlier.

American Mount Everest Expedition (1963)

Hughes: Now Everest?

Siri: The Mount Everest expedition was initiated by Norman

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Dyhrenfurth. Norman was the son of a famous Himalayan explorer and geologist, and Norman himself had been to Everest several times in the past. He now wanted to put together an American expedition to make the first serious American attempt on Mount Everest. Sir Edmund Hillary had climbed it some years before, and we entertained the possibility of not only climbing Everest by the now normal route, but also [of exploring] another route to the summit. Norman contacted me soon after he had conceived the idea, and together we began the planning and the efforts to raise funds. That was back in 1960.

Hughes: Did he contact you as a climber or as a scientist?

Siri: Both, and the fact that we had known each other for some time. I can't say that we were close friends. We had run across each other on some earlier occasions, and for whatever reason he sought me out to help organize the expedition.

Hughes: Was it fairly common in those days to have a scientific component in an expedition of that nature?

Siri: It wasn't uncommon. The British had always done this on their expeditions. The Japanese also. But not so much physiology or human studies, but rather glaciological, botanical, zoological, and so forth.

In any event, Norman and I set about organizing the expedition, first putting together a nonprofit corporation with a board of directors to meet IRS requirements and assist in fundraising. We then qualified as an organization to which tax-exempt contributions could be made. In the several years that followed, fundraising was the primary preoccupation. Ultimately we secured funds from a great many institutions, half a dozen federal agencies, and of course, had

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surprisingly generous support from the general public and equipment manufacturers. Everest was still big in the public mind in those years; not too many people had tramped on its summit. It was still an exciting adventure and novelty to most people.

Hughes: What nations had gotten to the summit between your trips and Hillary's?

Siri: The Swiss had succeeded in putting four men on the summit, and the Chinese claimed three of their team reached the summit, but their claim was never substantiated convincingly. I believe that most people felt that it was a slight exaggeration. In any event, the total was either six or nine, depending on if one believes the Chinese claims.

Hughes: Six or nine countries?

Siri: No, six or nine persons.

Hughes: So it was still a considerable challenge.

Siri: Well, everyone believed so, yes. And it still is today, although it has lost some of its glamour because so many people have now successfully climbed Everest. It is now possible to buy a ticket to its base at the local travel office. Some of its former glamour has faded.

In any event, a significant part of our expedition program was to be research. This involved my own studies in physiology of high altitude, and studies by four others in psychology, sociology, glaciology, and geology. All of them were very carefully planned. All of the research investigators were professionally competent in their fields and quite serious about the work they were going to do.

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Hughes: It would seem to me that it would be very difficult to find the necessary combination of competence in the scientific field and the ability to climb.

Siri: Well, it's not as difficult as one might imagine. With one exception, all the scentists on this expedition, and that was about half the expedition, were also very skilled climbers, and this is not uncommon at all. The sociologist, Richard Emerson, was not only a professor of sociology, he was a first-rate climber with a reputation well established in this country and Europe. The glaciologist, Maynard Miller, also a professor, was a well-known climber as well as a glaciologist, as was his assistant. The only exception was the psychologist who accompanied us. Although he was certainly physically fit, he'd had no significant prior climbing experience, and climbing was not really his bag, but he enjoyed the mountains and was not expected to do any serious climbing on the mountain. As for the rest of us, support of the climbing effort was expected as well as the execution of our research projects.

We had, all told, six research projects—in physiology, glaciology, geology, psychology, and sociology. The last two of these would seem curious fields of research to pursue on Mount Everest, but it was quite a logical one. I would be measuring, or trying to measure, the physiological impacts of stress, and in this case the grounds for believing that stress was severe, we felt were quite good. They involved not only the environmental questions, that is, very low oxygen pressures, extreme cold, exposure, poor diets, but also a very high level of anxiety and goal orientation. The question is, what happens to a group of isolated men under these presumably stressful conditions? What changes take place in their behavior and how do they behave as a collection? That is, for the sociologist, how do they behave as a collection of people in isolation and under stress? Those studies in their own right I think are fascinating

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reading because these two investigators could tell us something about ourselves and how we behaved under conditions of this kind. And the results are rather intriguing.

My own findings in the physiological responses, we thought were interesting, particularly in the changes that took place in blood, which were rather staggering. The evidence of stress as reflected in the hormonal systems was not so striking, except at the very beginning during the early stage of acclimatization: the body was clearly stressed, and this was shown in the adrenal corticoids and in other hormone systems as well that we measured.

In any event, coming back to the expedition, Norman Dyhrenfurth and I having put together a tax-exempt corporation, proceeded to raise the funding necessary to get the expedition into the field. All told, the cost was about half a million dollars, and about half of that was in support of the scientific studies.

Hughes: Was each scientific discipline responsible for arranging its own funding and equipment?

Siri: For the most part, yes.

Hughes: And who was saying, Yes, you may participate, or No, you may not?

Siri: The selection of the team members was a decision made primarily by Norman Dyhrenfurth and myself. We advertised the expedition to invite qualified people to apply. We had some candidates of our own in mind, and these we sought out individually. But we did give everybody interested an opportunity to apply. Ultimately, we settled on a group of best qualified candidates.

Hughes: Were you basically making the scientific decisions?

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Dyrenfurth had no specific scientific background, did he?

Siri: That's right. I served as research coordinator as well as deputy leader for the expedition. So one of my principal functions was coordinating the research efforts.

Hughes: Did you also formulate the general scheme? I know, for example, that a month or so before the expedition you established a baseline for every participant in terms of physiological parameters. Were you making that kind of decision as well?

Siri: Yes. My role as coordinator was to have the principal investigator for each study prepare his proposal, setting forth the purposes of the study and the details on how it was to be conducted and a budget. They submitted their proposals to me for processing, and I then submitted the formal proposals to the agencies expected to fund them. In the meantime, we had been negotiating with the agencies to be sure of their interest. So that all proceeded in a fairly orderly fashion, although it called for unending negotiations and trips to Washington.

However, in all of this effort we had the backing of some very influential people, including Stewart Udall, who was at that time Secretary of Interior. With his assistance, we had access to offices that might otherwise have been difficult to enter. The *National Geographic* was an enthusiastic backer. They had in mind, of course, a film and full magazine coverage of the expedition upon its return. The film incidentally was to be the beginning of the *National Geographic* TV special series. And it turned out it was, in fact, the first in that series. Several book publishers were eager to have the book rights, and we had with us James Ramsey Ullman, who had written many books on mountaineering, some of them bestsellers.

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Hughes: With the specific idea of writing another book?

Siri: That's right.

Hughes: Which he did?

Siri: Which he did, right.

Hughes: There were some incredible logistic problems. I'm thinking of getting all the expedition members over to Donner Lab within a one-month period, then sending them off again perhaps, and then reconvening them in time for the expedition.

Siri: Well, that's part of the problem of organizing any kind of activity, particularly an expedition; it's a substantial management and planning effort.

Hughes: That was shouldered by you and Dyhrenfurth.

Siri: Yes. The two of us had to carry out essentially all of the planning and management.

Hughes: What was happening to Donner Lab during all this time?

Siri: Well, Donner Laboratory was still there and functioning, and it did take some of my time, I admit, to tend to the details of the expedition. But I had consulted Dr. Lawrence about this well in advance, when I first knew that I would be involved and that it would take time. I explained to him the nature of the studies that I proposed to do, and that I would also be involved in the climbing and organization of the expedition. He found this acceptable and approved.

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Hughes: What was the arrangement as far as the actual use of Donner Lab facilities in those baseline studies?

Siri: Well, that was a legitimate part of ongoing Laboratory research programs. For this purpose, we were using climbers because it provided us with an unusual opportunity to study people at altitudes where we knew we would get profound physiological responses, particularly in the erythropoietic system. This was an area in which the Laboratory was well known for its research. We had made three expeditions to the Andes pursuing this aspect of erythropoiesis, and this provided us with an opportunity to study people at altitudes where we knew there would be extremely profound effects.

Hughes: The numbers of climbers that had to be studied within a given period, namely about a month, did not stress the capacities of the Laboratory?

Siri: Oh, no, the number was not all that great—it was only 15 of us, so that number was relatively small. We brought the entire team to Berkeley, and I spent two days doing the baseline measurements on the climbing team, that is, the blood volumes, iron turnover, the adrenal cortical steroid tests, a battery of other tests, and physical examinations. In this I had the assistance of other members of the Laboratory staff.

The entire team then spent three days in the Institute for Human Studies on the campus, where the 15 of us were examined by about 25 psychologists and psychiatrists. I think most of the climbers found the experience somewhat amusing. They had not been exposed to anything quite like it before. I think the climbers who were not scientists viewed the psychological testing with some skepticism and engaged in a lot of bawdy humor about the whole experience. [laughter] In any event, we had the entire team here

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for a better part of a week in an intensive effort to establish baseline values. Most of these tests would then be repeated several times in the field during the course of the expedition, although the sociologist and the psychologist would be conducting their observations continuously.

Hughes: Were you actually able to complete the whole battery of tests on the climb?

Siri: Yes. There were one or two things that proved unworkable. One of the important tests that I wanted to make was on cardiac output. This was a piece of information that I thought we needed quite badly. I had our electronics people prepare portable equipment with which I could measure cardiac outputs with radioactive isotopes. It was the only practical way to do it in the field. But unfortunately the equipment did not work out very well in the field, and so the information I got on cardiac output was either poor or unusable. A minor chemistry test also proved not to be effective. But for the rest, it worked out very well. I did get the iron turnovers and blood volumes and measurements of the adrenal corticoids, blood chemistries, and cardiac response and recovery in a standard work test.

Hughes: How are climbers under stress? How do they react to a needle going into their vein or whatever? I can imagine it would be very easy to say when you're down at ground level, Well sure, let's go ahead and do this. But under the stress of the [expedition], I would think perhaps people's reactions would also change.

Siri: To some extent this is true, but the attitudes varied from individual to individual. Those who participated calmly in the tests at sea level here at Berkeley weren't in the least distressed by them on the mountain, including an indwelling needle for 8 hours and numerous venapunctures. But those

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for whom the sight of a needle induced a measure of terror at sea level found it no less distressing on the mountain, especially in a small tent in a snowstorm.

Hughes: But you proceeded nevertheless?

Siri: Yes. Only in one case did I have to abandon the effort. I don't recall exactly, but it may have been because he could run faster than me. But the others were cooperative.

Hughes: What about your own incarceration for four days in the high-altitude chamber? Why did you think that was a necessary component of the trip?

Siri: I felt that that experiment was essential for this reason: we do know that during the early stages of acclimatization, right after exposure to low oxygen pressure or high altitude, that profound changes do take place in the body. The question is, how profound and what are they? A large number of experiments on these acute effects had been measured in Europe and the United States, but they were all at low altitudes. The highest had been at about 12,000 feet in Switzerland, and that's too low an altitude to see really profound changes indicating severe level of stress, or a substantial alteration of the regulatory mechanisms in the body. We had to get higher than 15,000 feet, because I knew from past experience that this was about the altitude at which significant differences began to appear. Somewhere between 15,000 and 18,000 feet was a critical level for most people.

But on the Everest expedition, or anywhere else for that matter, it's almost impossible to make these measurements during that critical period of acclimatization, which may last only a week or less. It took four weeks to get to our base camp at Everest, and all that time we were ascending slowly so that by the time we got to base camp, we were already

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pretty well acclimatized, at least to 15,000 feet or thereabouts. Consequently, I felt it was necessary to see what the changes were on acute exposure and during the first, let's say, four days. This is when most of the changes take place. And the only way I could do that was in our own high-altitude chamber. While I intended to go in as a subject, after having set up with the rest of the staff all the measurements that were going to be made, I found it impossible to persuade anyone to join me. [laughter] After the first eight hours in the chamber, I could see why. It was an absolutely disagreeable experience.

Hughes: Was it the isolation more than anything?

Siri: No, nothing like that, because I had a team in attendance constantly, looking through port holes and talking to me. Every few hours one or two of the doctors would come in for a quick examination and to take blood specimens and do other tests that had been set up. So it wasn't isolation. No, it was the reaction to low oxygen pressure.

During the first eight hours it was quite all right, but at about eight hours, and this was an interesting observation too, the effects of altitude began to set in, and the physiological changes were beginning to accelerate. From then on it was a matter of bearing with totally intractable headaches, nausea, dry retching, sleeplessness, loss of appetite, muscular weakness, feeling of apprehension, and just being sick, unable to get off the cot or unwilling to. But by the fourth day a large measure of acclimatization had taken place—the headaches subsided, I was able to get up, and at one point I even tried a little reading. All this time continuous measurements were being made. I was wired to an EKG and to other instruments outside the chamber by an umbilical cord, and of course the chemical measurements were made every four hours.

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Hughes: And did those findings coincide with....?

Siri: They did. The effects we found there were quite significant, and we picked up a few that had not been reported before. All the adrenal cortical steroids were elevated, and we could follow the elevation of those at the outset of the exposure. They more or less corresponded with the symptoms: that is, the increase in the separate corticoids. Other changes in chemistry occurred, and all of these fluctuated in some wild manner during those four days, but by the end of the fourth day they began to settle down into some kind of pattern. The adrenal corticoids tended towards their normal values, but had not gotten down to normal values. Changes in erythropoiesis were quite pronounced, and levels of erythropoietin were high. All of the things that you would like to see in an experiment occurred.

Hughes: The fact that such physiological changes were more gradual or less pronounced on the actual climb, was that explainable solely on the grounds of the gradual nature of the acclimatization?

Siri: Yes. The body can readjust itself gradually instead of disruptively, as if panicked. Ordinarily, here at sea level we operate in a state of homeostasis, that is, everything is balanced and operating normally, the wheels are turning at the right rates, there is something up there regulating the whole physiological machinery, and it is adjusted to the environment—that is, the average temperature, the oxygen level, the whole list of environmental factors.

A change of oxygen pressure to, say, one-half of what you're normally adjusted to means a profound change in the body. Now it means that the body has to reorganize its whole regulatory system in order to operate on half an atmosphere of air. It can do that, but in order to make that readjustment and reestablish homeostasis, the regulatory

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mechanism has to readjust nearly all the body's systems. But they don't all adjust at the same rate, so some of these things happen rapidly, others change slowly. For example, the moment the bone marrow, or whatever is in our bodies, senses that we're under half the normal oxygen pressure, it almost immediately speeds up erythropoietic mechanisms, that is, it accelerates the rate of red cell production, and hence the rate of iron turnover and other things. But since the normal replacement of red cells is only about a percent per day, the effect on blood volume, and its benefits, are not evident for several weeks.

On the other hand, cardiac output, blood pressure, and respiration changed very rapidly. And so what occurred in the chamber was a complex set of rapid changes in the body trying to adjust to the new circumstances. After acclimatization the body's systems have readjusted, but they now operate at different rates, and sometimes in different modes than they did at sea level.

Hughes: In your chapter in *Americans on Everest*, you of course talked about all of your physiological findings. I was interested in the problem of feeding, nutrition. I believe you said there were few signs of extreme physiological stress. But you went on to say that the climbers were probably "victims of semi-starvation and we do not know why." Had you set out with the idea that this was going to be a problem?

Siri: Yes, it was a familiar one and always has been with high-altitude climbing. One of the effects of high altitude is a reduction in appetite, but it gradually comes back. The real problem with nutrition in high-altitude climbing is the difficulty of preparing food and getting it there in the first place. Because of the nature of the food—that is, it has to be concentrated, light, and therefore highly processed—the stuff is barely palatable. It is a very different diet in terms

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of taste, texture, and flavor from what we're normally accustomed to. There is no way that I know of in which one can make a pot of powdered potato look like roast beef. But there is something beyond that and the logistics problem. In addition to some inhibition of appetite, there may be a problem in absorbing fats at extreme altitudes. There is some basis for believing so. At least two members of our team force fed themselves. This took a little doing with the kinds of foods we had at high altitudes. They were under the impression that they were maintaining their body weights. I was keeping close track of body weights on the expedition because this was an important piece of information. At our base camp at 18,000 feet, we could just barely maintain our body weight. Above 20,000 feet everyone lost weight, including the two climbers who force fed themselves. One of them was Tom Hombain, an M.D., who knew that nutrition was critical to their wellbeing and their capacity to reach the summit. Whether the weight loss was caused by severe hypoxia or extreme cold is not clear.

Hughes: What about the question of brain damage that you mention in the article?

Siri: There was evidence of cerebral effects—no question about that. At least there's no question in my mind and the minds of some of the people who were there. Whether any of it was permanent or not is another question. Probably not. We'd like to think so anyway. Those persons who were at very high altitudes and especially those who ran out of oxygen in the summit climbs had well-defined effects for as long as a year, in one case a year and a half or more later. There were spotty losses of memory, inability to recall certain familiar information, such as names of persons well known to them, long periods of depression—in a couple of cases, quite severe—and then repeated episodes. Others experienced episodes of depression, perhaps not quite so

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severe. Whether the disruptions in their family lives had some bearing on this, I don't know. Most of the men who were married before they went on the expedition, subsequently broke up with their families afterwards and in a couple of cases abandoned careers. So if these are related to the effects the exposure to severe hypoxia had on their brains, I don't know, but it is highly suggestive.

Significance of a Ph.D.

Hughes: Mr. Siri, you have been "awarded" a Ph.D. on a number of occasions, which, I understand from talking to you, you have not always appreciated. Could you tell me a little bit about how it has been to have held for many years the role of a person with a doctorate and certainly the prestige of a person with a doctorate, but without the actual piece of paper?

Siri: Yes. I don't recommend this for anyone who chooses to take a job in a setting where the document is generally regarded as an absolute requirement. It has been a source of ongoing embarrassment, at times, dismay.

At the conclusion of the war I registered as a graduate student and took two and one half years of courses in the graduate school in physics. As I recall, I had all of the graduate courses that were needed for a Ph.D. This was done while I was working fulltime at the Laboratory, and it was permitted by the Laboratory.

In the middle of all this, I wrote a book that was well received, both here and in Europe, and was used for courses in biophysics for a number of years. But I never got around to writing a thesis, and the book hardly qualified for that purpose.

At the Laboratory, [the lack of a Ph.D.] appeared to make little or no difference. John Lawrence never raised the question. I was a principal investigator in the projects that I

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devised and pursued. I appeared to be treated at the same level as other members of the staff who had their M.D.'s or Ph.D.'s, and was left pretty much alone to pursue research as I thought most productive. I had discussed this with John Lawrence and Jones and Tobias in those early days. It made no difference whatever to them. The embarrassment came at times as I was introduced as "Dr." Siri and on several occasions when it appeared in print. This was a cause of considerable concern to me. On one occasion I brought it to the attention of the director of the [Lawrence Berkeley] Laboratory at that time, Ed McMillan, and also to John Lawrence's attention, in writing. The replies I got from them were in essence, "Don't worry about it; if they call you 'Dr.', forget about it. It's not important."

As time passed, the likelihood of doing a thesis to satisfy the requirement for a Ph.D. became dimmer. Time, the press of events, and work thrust that into the background. For about ten years I had every intention of completing it, but never got back to doing it. For ten or perhaps fifteen years, I lived with this personal anxiety about not having a degree and feeling an obligation to correct anyone who thought I did. There came a time when this became a fruitless kind of defense, and on John Lawrence's advice and Ed McMillan's, I just let it ride. It appeared to make no difference, except in one respect. And that is at one point, sometime after I published my book, John Lawrence attempted to seek an academic appointment for me. Because I did not have a degree, the University would not go along with it, and no further attempts were made. In looking back, I'm not terribly sorry about that. I really didn't have an ambition for an academic appointment so long as I could pursue research in the Laboratory. And this has been the history since.

I don't advise this for others. It can be a cause of stress because it's a deviation from the norm in this environmental setting.

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President of the Sierra Club (1964–1966)

Hughes: Mr. Siri, you were going to make a comment about the years 1964 to 1966 when you were president of the Sierra Club. I was wondering how you managed to keep your research position going and also the presidency.

Siri: By 1964 I had served about nine years on the board of the Sierra Club, and in 1964 the members of the board asked if I would serve as the next president. At the meeting where this was informally put to me, I had to tell them that they couldn't have an answer immediately, that I would have to determine whether it would be possible to serve as president of the Sierra Club and still hold a job at the same time.

One of the first things that I did after a few sleepless nights trying to resolve this problem in my own mind, was to send Dr. Lawrence a memorandum telling him that I had been offered the candidacy for election to the presidency of the Sierra Club. If I accepted, this would entail a good deal of time and would clearly take up some time that I should be devoting to research in the Laboratory. I then met with Dr. Lawrence, and he was quite agreeable. He understood that I would have to spend quite a bit of time on Sierra Club affairs and that it would indeed mean a reduction of time and effort spent in the Laboratory. So it was with his approval that I agreed then to serve as president and was subsequently elected.

In bringing to Dr. Lawrence's attention the fact that it would take time was not an exaggeration. [Being] president of the Sierra Club is really a fulltime job, although I didn't make it that. I did have assistance in the Sierra Club office and consequently was able to do something less than work fulltime for the Sierra Club and also keep my own work

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going at an obviously reduced level. In any event, those two years were busy ones, trying to keep up with work in the Laboratory, and not wholly successfully, at least as compared to what it might have been working fulltime.

Hughes: Were you able to delegate some of your responsibility in the lab?

Siri: Yes. At that time I had a number of assistants, an occasional graduate student, and from time to time an associate. So the work continued, particularly on patients, whom we ran in rather routine fashion at that time. The patients continued to come through, and the technicians and an occasional assistant would manage that part of the program quite well.

I suffered through the two years of presidency of the Sierra Club at a time when change was coming rapidly in environmental awareness and action. It was an exciting moment in the history of the environmental movement.

Hughes: You're speaking now of the heightened awareness of the environment in general?

Siri: Yes, that's right. It was during those years that general awareness of environmental issues gradually permeated a good part of the population. Whereas the Sierra Club had traditionally been concerned only with wilderness and occasionally with wildlife, one of the thrusts I tried to bring to the club as president was an expansion of its outlook—to become involved in such questions as air and water pollution, energy, land use, and human resources. Things of this sort were resisted initially. But within a year or two, many of the Sierra Club members, particularly the younger ones, began to have a broader view of what the world ought to be like.

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MANAGER, LBL ENERGY ANALYSIS PROGRAM

Hughes: Would you like to say something about your move to the Hill in 1974—how that came about?

Siri: That was a long process that didn't happen suddenly. The work I had been doing in Donner Laboratory was gradually growing stale. For that matter, so was the Laboratory. There were problems that had been growing gradually over the years. By the early '70s or even before that, the aging process in Donner Laboratory as an institution was becoming painfully evident. Funds were also, perhaps, coming with greater difficulty. At 25 Donner was becoming an old institution with all the pains of growing old, without real direction as an institution, and with a lot of contending forces within it.

In the early '70s, about 1970 I guess it was, Andy Sessler and Jack Hollander approached me, primarily because they were aware of my activist role in the environmental movement and familiarity with the problems and contacts with people, both on campus and in government. [They] asked for suggestions and advice for research work that Lawrence Berkeley Laboratory could undertake. They did not at that time have in mind a new division, but rather instituting an environment and energy program in one of the existing divisions of the Laboratory. In any event, I introduced them to some people on the Berkeley campus and in other institutions. After a number of these meetings, they gradually evolved a proposal for a research program, which they took to Edwin McMillan, who was director of the Laboratory at that time. Between the time I first met with Sessler and Hollander to discuss an energy and environment program and the time they went to McMillan, they had formed in their own minds the concept of a new division, and this is what they proposed to McMillan.

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By 1974 conditions in Donner Laboratory had reached a stage where the Laboratory for the most part was static. Oh, some small projects were moving along well enough, but there was a growing demoralization in the Laboratory and anxiety concerning its future. It had none of the excitement of earlier days. There were strong differences, both within Donner Laboratory and between Donner Laboratory and other parts of LBL. My work had almost come to a standstill simply [because of] lack of motivation, and I didn't see a useful future in the directions that I was taking. I guess Laboratory management didn't either. And my funds were being cut. About that time, Andy Sessler asked if I would come to the Hill and join the E and E Division which he and Jack Hollander had created a year earlier. After a session with Andy Sessler, it looked like an exciting opportunity. It was in an area that I was familiar with by virtue of many years in the Sierra Club [and] could feel very comfortable with. In any event, in 1974 I moved to the Hill and joined the new E and E division and after a short while was put in charge of the interdisciplinary group that we subsequently called the Energy Analysis Program.

The program was quite small at that time, consisting of small projects with the ERDA, the Army Corps of Engineers, and a number of other federal agencies. The object then was to develop the Energy Analysis Program into something with larger goals and a more comprehensive look at the major energy issues. So from 1974 when I joined the division here, we have expanded our program, and I think now we have a fairly good interdisciplinary group, looking at energy policies, accessing the impacts of technologies and energy policies, and have a fairly good ongoing program.

Hughes: Did you find that your experience in the Sierra Club was sufficient to get you started? How much on-the-job learning did you have to do?

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Siri: Well, let me say that my experience in the Sierra Club gave me a very fast running start at it. The kinds of problems that we dealt with in energy analysis were quite similar to many of the issues that concerned the Sierra Club and for which we had to conduct the necessary economic and technical research to document and support a position we might take. Many of the analyses we did in the Sierra Club did involve technical, economic, and institutional analysis, which is just the sort of thing we do here, although in the Sierra Club, I must confess, we did this with a slight bias. We were trying to prove a point. The difference here is we cannot afford to indulge in personal biases. We have to let the findings fall out where they will.

DONNER LABORATORY AS AN INSTITUTION

Hughes: You spoke of demoralization and contending forces in the Donner Laboratory in the early '70s. Could you speak a little bit more about that? What were the contending forces?

Siri: To explain this I would have to go back a few years. In fact, I'll go back to the beginning, since I was there almost at the beginning, shortly after the building was erected. At that time the staff was quite small, and the personal relationships were intimate and cordial. The number of projects was very small, and the whole operation had the appearance of a sort of family affair. After the war, in the '50s and particularly in the diversity of programs it had. Everybody was developing and pursuing his own projects, and the staff grew. Lawrence had his own programs going in medicine. Tobias had a full-blown program in developing the heavy ion beams for medical uses. Hardin Jones had his programs that branched into subprograms. Jack Gofman, when he was with us, also evolved very rapidly his programs on heart disease and, more particularly, atherosclerosis.

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As some of Jones' and, more particularly, Gofman's and Tobias' graduate students acquired their certificates, a number of them stayed on at Donner Laboratory and some of them obtained faculty appointments and gradually moved up the academic ladder. Consequently, as the years passed, the Laboratory became more segmented—factionalized as well as fractionalized—in a diversity of ways. The interactions were generally what we would expect in an institution that was aging, that had lost some of its early excitement, and became large enough so that people down the hall didn't know what you were doing any more than you knew what people down the hall were doing.

For a long time I ran the Donner Laboratory seminars, and one of the perpetual problems there was to get staff people and graduate students to attend the seminar. Even on direct orders, this was not possible. Groups kept pretty much to themselves.

Anyway, I'm coming to the point of your question, and that is what happened in the late '60s and early '70s. John Lawrence in the early days was a figure that everybody respected because he was John Lawrence. And he was the director of Donner Laboratory. Donner Laboratory in the '50s and early '60s became recognized around the world as one of the leading centers for biomedical research and biophysics. But as time passed, this changed, as I said, and by the '60s, Dr. Lawrence was not providing the kind of leadership that a complex mixture of biological, medical, and biophysical studies would require. That would require strong scientific leadership, but also someone who was particularly skillful at management of an institution of that kind. Regrettably with all Lawrence's fine qualities, he was not the man to provide the strong scientific leadership in such a collection of diverse activities. It was well known to all of the staff for many years that Dr. Lawrence was not the greatest administrator. And he operated in a way that suggested to many that he leaned on the significance of his name, on John

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Lawrence. I'm not suggesting that he wasn't a brilliant person or a person with great insight. He had both these qualities. In conversations with him from time to time, I was impressed with what appeared to be a very accurate assessment of both people and their work—a remarkably perceptive person. But somehow this was not consistently translated into really effective scientific leadership or the most effective form of administration.

Now in the late '70s as funds became a bit more difficult to secure and the number of projects continued to proliferate, it was evident that it was becoming progressively more difficult to contend with the problems that accompanied both the diversity of research and the growing factions within the Laboratory. I suppose there were other reasons as well. One problem we always had with Dr. Lawrence was that he spent an enormous amount of time traveling. He was frequently on the road in one part of the world or another, lecturing and attending conferences, and wasn't the easiest man to see, partly because of his absences and partly because when he was at the Laboratory, he just appeared to be extremely busy and would often be quite short with people—including the senior people in the Laboratory.

Hughes: How much was he leaning on the assistant directors, Born and Jones, in say, the late '60s?

Siri: Well, he leaned heavily on both Born and Jones and, for that matter, Tobias as well, in the administration of the Laboratory. Born could not provide scientific leadership. Hardin Jones and Tobias were the two people who I think provided whatever guidance there was, and this wasn't a great deal in terms of what is required for a laboratory of that kind. But it was Jones and Tobias primarily who provided the scientific leadership until the younger people started to develop and moved up the academic ladder, particularly

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Alex Nichols and Bob Mortimer. And so Born handled the administrative details; he was in effect an administrative assistant to Lawrence.

Hughes: Was that not a strange choice in 1970, when Dr. Lawrence stepped down to become a Regent, to appoint Born to succeed him?

Siri: Well, to answer your question, yes. I think everyone was surprised.

Hughes: Was the decision made strictly by Dr. Lawrence?

Siri: I can't be sure. I assume it must have been made solely on his recommendation. We all had expected Hardin Jones to be appointed. He would have been the logical successor. And I think almost without exception we were astonished that he wasn't picked and that Born was. At that point the Laboratory was in desperate need of strong intellectual leadership.

Hughes: Was there a general awareness in the Laboratory itself that something rather drastic had to be done to pull things together?

Siri: Yes.

Hughes: Why do you suppose that Jones was passed over?

Siri: I can't say with any certainty. Part of the reason may have been Jones's growing preoccupation with issues that had nothing, or very little, to do with research—his deep involvement in social issues during the late '60s and early '70s. These may have prejudiced his opportunities to be director of the Laboratory. Jones was a person whom

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everyone in the Laboratory respected, both as a scientist and particularly as a human being. He was to all of us a father figure, although he wasn't that much older than the rest of us. He was a benevolent person as well as a scientist we respected. But he now seemed totally dedicated to certain social issues, particularly his crusading efforts against the use of drugs, which was not an unreasonable cause, and his anti-communist crusade, searching for them, figuratively, under the bed or wherever communists were supposed to be.

Hughes: We've got the low-level radiation business too.

Siri: That's right. That was with Gofman. His position on low-level radiation may have been a factor in his not being appointed. I can only guess at that; I don't know for certain.

Hughes: I know he came out rather early—as I remember now it was in the '50s—with a paper stating that from a statistical standpoint, current cancer therapy was worse than useless, that in fact it was perhaps detrimental to the patients. [This] is an astounding thing to say as a member of the Laboratory, in which cancer therapy was a major area of research.

Siri: Right. Funded by an agency whose main mission was to generate radiations, one way or another. That's all true. Hardin had become interested in population problems and more particularly what affected life spans of humans. He did some very fine work in the first years that he became involved in the problems. I think he made some valuable contributions and provided some new insights into what kept people alive and what killed them.

Hughes: Did he maintain a particular interest in your research? It seems to me that body composition and the changes therein would relate rather directly to his theory of aging.

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Siri: That's true, but he was never that close to what I was doing. He was more concerned with, well I'll call them, environmental or exogenous factors. He seemed to take such an extreme view on all of these matters that I'm afraid he lost credibility, particularly when he became so deeply involved in the student disruptions on the Berkeley campus, and actually took quite an active role in opposition to movements of the time. He didn't lose our respect for him as a man and a scientist, but we were puzzled by his extreme and total preoccupation with fighting communism and drugs. I'm speaking of everybody in the Laboratory, because we discussed this among ourselves often. We never stopped loving—I use the word in the fullest sense—Hardin Jones. He was a *very* fine human being, and we all were indebted to him. But we saw him as time passed as a tragic figure who had somehow become totally obsessed and preoccupied with causes that now obstructed an otherwise brilliant scientific career.

Hughes: So in those years of activism from the mid '60s on into the early '70s, two key figures were fairly inaccessible to Laboratory staff members. John Lawrence was traveling a lot of the time, and then of course in 1970 he became a Regent. It seems to me that if you consider a core of four individuals at the heart of the Laboratory, at the heart of the institution, two of them were virtually not accessible. Surely that must have had an impact on the operation of the Laboratory?

Siri: It did. If anything, those two persons largely accounted for whatever happened in the Laboratory. Hardin Jones could have been a very effective leader, but he became isolated by virtue of his own activities, and he lost credibility outside of the Laboratory. People across the country I met at meetings asked about Hardin Jones but asked in such a way as to indicate, How do you explain what's happened to him?

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Hughes: How much do you think that overflowed to the Lab itself?

Siri: Well, I think it had a profound effect because Hardin no longer provided the intellectual leadership that he did provide in earlier years. Hardin was the kind of person you could always discuss your research problem with. He always had valuable suggestions, and this was true for everyone in the Laboratory. We saw less of this as the '60s passed and, of course, going into the '70s. Lawrence was now a Regent. He was more and more preoccupied with other matters than the scientific programs of the Laboratory. Most of the research staff rarely saw Lawrence, and few had a chance to talk with him about their work. There was just no substantive contact for most of the staff.

Hughes: Even if you had, do you think it would have been profitable with all that discontinuity?

Siri: No, only to the extent of involving an administrative matter. But more and more Jim Born took on the administrative tasks. Even I found myself helping on occasion to decide which projects were to be funded and which project work statements needed to be rewritten. Born knew the clinical studies in the Laboratory well but seemed less certain of his knowledge and judgment of the numerous non-clinical research projects.

Hughes: How effective was Born in his relationships with LBL?

Siri: I can't really say. My impression is that Born was not regarded by staff as a scientist, but rather as an administrator. However, he participated in a number of projects. He's an M.D. and therefore was steadily involved in the clinical projects. But he was not a lead scientist, and I don't think that he ever pretended that he was. He seemed quite content

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to be there, in the role that he held.

Hughes: How would you explain Lawrence's choice then?

Siri: Well, you're now asking me to play the role of either an intimate or perhaps a psychologist, and I am neither. It was our collective impression that Lawrence probably felt Jim Born was a person he could count on. And Lawrence, while not director of the Laboratory, would then have a presence that he wanted to maintain with Born serving as his proxy. If this were the reason, one can sympathize with Lawrence's reluctance to abruptly remove himself from an institution he created and nurtured. There is also the possibility that Lawrence simply concluded that Born was the best qualified member of the staff for the job.

In any event, the transition to James Born was seen as a transient move, because the Laboratory now needed strong, new leadership to rejuvenate its programs and restore its status as an outstanding biomedical research institution.

Hughes: How does one determine that?

Siri: Partly by hearsay, partly by the quality of scientists who want to come to Donner from other outstanding institutions around the world, and mostly by virtue of the quality of the work that's produced and recognized elsewhere. Donner had lost much of its early glamour. It was no longer a strong leader in the world of biomedicine.

Hughes: It's difficult for me to separate out how much—and it's probably impossible to do so, too—how much it was a case of an institution aging and how much the field itself was aging. Nuclear medicine, the whole radioisotope radiation field from the mid '30s to, say, roughly into the '50s anyway, was a new and exciting field with relatively limitless

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boundaries. Was not, by the mid '60s anyway, the field more mature? The limits were known. It was at least beginning to be an accepted scientific and medical field. How much of this was a factor in the maturation or even the senility of Donner Lab? Do you see what I'm trying to separate out?

Siri: I understand what you're saying, and I'm trying to find a way to answer it. What you've just described is a fundamentally important factor. The field hadn't been exhausted, but we'd all grown up with the field during its exciting early growth and much of it was now becoming commercial. We could now buy radioisotopes. We didn't have to make them any more. You could buy all kinds of sophisticated instruments, whereas in the early years we not only built them ourselves, we had to invent them. That was all past now. The field was now open to new ideas and more sophisticated research.

But the point is that a good scientific leader directing an institute would sense these changes and try to stay in the forefront of the research. You can to some extent rely on the older, experienced researchers to shift with the times and sense new opportunities, but you can't count on this all the time, and Donner Laboratory was a classic example. An aggressive director would bring in fresh new minds and see to it that the best stayed. Lawrence and Toby tried to do this but were not wholly successful. I think I said earlier that some of the best people that Donner had, particularly in the medical sciences, people who should have been kept around, among them Nathaniel Berlin and H. Saul Winchell, were not given sufficient incentive to stay on. Ultimately the offers they received outside were substantially better. If these and other bright young people had been kept on, the Laboratory might have maintained its vigor instead of slowly relaxing into a state of malaise.

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Hughes: What about the tendency to hold on to one's own, namely graduate students? There is an extremely high proportion of Berkeley medical physics graduates [in the Division of Medical Physics and Biophysics and in Donner Laboratory] even to this day.

Siri: That's right. I regard this as a serious mistake. As time passed it led to divisiveness and parochialism. They weren't always the first choice that one would have made if you wanted to keep the Laboratory alive and in the forefront of the field. But this was determined primarily by Tobias and Lawrence and I guess to some extent by Jones.

Hughes: Was this done even before the so-called decline of the Laboratory, while there still were good outside candidates for these positions?

Siri: Yes.

Hughes: Was this an overblown loyalty to one's own? How do you explain it?

Siri: I don't know. I don't explain it. I was always surprised when Tobias' graduate students, for example, or for that matter Gofman's, stayed on and were offered academic appointments. Some of them should have gone to other institutions to broaden their experience and test their competence.

Hughes: You spoke of the lack of research direction, which you blame on Hardin Jones when he became involved in political issues. Did Tobias step in to any degree at that stage?

Siri: He may have, but I wasn't particularly aware of it. I wasn't aware that he filled a vacuum that was left by Hardin

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Jones—at least not with so much emphasis that it became apparent that he was now the intellectual leader of the Laboratory. He was having problems with his own programs about then. He had spent many years with the accelerators, developing heavy particle beam irradiation for use in medicine and biology. Those programs were gradually beginning to be viewed with some doubt by people outside the Laboratory, particularly by the National Cancer Institute. I can remember, going back quite a few years now, people in the Cancer Institute, whom I knew quite well, expressing doubts whether that work should be funded at present levels or gradually phased out.

Hughes: You're speaking about the heavy particle therapy?

Siri: Yes. Running the cyclotrons was an expensive business, and it wasn't clear that it was offering breakthroughs so significant as to warrant heavy emphasis on that approach to cancer therapy.

Hughes: And did the National Cancer Institute cut back its funding?

Siri: I believe it did but not completely. The programs are still going on, and there are now applications of the heavier ions in therapy. But at the time—the early '70s—the program was supported just to exhaust the possibilities of heavy ion therapy and to explore certain other uses of heavy ion beams.

The appointment of Ed[ward] Alpen as director brought new hope for Donner. Alpen is a very different kind of person from his predecessors. He's had extensive and successful experience in organizing and managing laboratories. He also has the intellectual equipment to act as a research director. Some of us were quite pleased to see him appointed to that job. He's a tough person. Perhaps not lovable like

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Hardin Jones, but he is clearly an able director.

Hughes: You mean in a scientific sense as well as....

Siri: That's right. In a scientific as well as administrative sense. Having known Alpen for many years, I was pleased to see him come on. I know there are differing views, but I think they relate more to Alpen's management style than to his effectiveness. He is a no-nonsense director who demands performance.

Hughes: What was John Lawrence's response to Alpen's appointment?

Siri: I don't really know. I had left Donner Laboratory by then.

Hughes: Do you think the Laboratory will resume its place in the forefront of nuclear medicine, radiation research?

Siri: Yes. I don't know what form it will take, but Alpen is astute enough to sense where the best research opportunities lie and to pursue them. At least he's demonstrated this in the past. Whether it's in radiation biology or new fields, I don't know. But I think that he's quite capable of directing the Laboratory into an avenue that will be productive and perhaps again outstanding.

Hughes: Do you think the multidisciplinary idea has been a viable one?

Siri: I think so. It depends on the staff and the quality of the leadership. It's not a question of absolutes at all. So much depends in the long run on the director of the Laboratory. Donner in the beginning was a small operation, and a small operation can run sort of as a family affair. You can't do

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that after it's grown to maturity. I've never seen an institution yet that didn't have an established hierarchy and able director to see that it went someplace. Otherwise it falls apart, a pot full of bits and pieces, all floating in different directions. Somebody has to provide aggressive leadership, even in research, in spite of what researchers like to think about their independence. They can go downhill like everybody else.

Hughes: Well, I'm at the end of my questions. Is there anything that you would like to add at this point?

Siri: I think not. If you hear from me in a state of anxiety, then you'll know it's something I grossly misrepresented in my impromptu answers. But at the moment I don't quite know what that might be.

Hughes: Well, thank you, Mr. Siri.

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1972	Ph.D. (history of medicine), Royal Postgraduate Medical School, University of London
1966–1969	Postgraduate Research Histologist, Cardiovascular Research Institute, University of California, San Francisco
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