

Photo by Leigh Wiener

HENRY BORSOOK (1897-1984)

INTERVIEWED BY
MARY TERRALL

April 5, 1978

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California



Subject area

Biology, biochemistry

Abstract

Interview in 1978 with biochemist Henry Borsook, who joined Caltech's newly created Biology Division in 1929 and retired from Caltech in 1968, moving his laboratory to U.C. Berkeley. Professor Borsook's major contributions were made in the areas of protein synthesis and nutrition. He recalls Robert A. Millikan's interest in establishing biology at Caltech and the early days of the Biology Division under Thomas Hunt Morgan; Caltech's intellectual life in the 1930s; the establishment of a Health Center at the Institute; his relations with Linus Pauling. In the 1930s, Borsook began applying thermodynamics to the study of biological phenomena, working with bacteria and studying the production of urine and creatine. He discusses his later work on vitamins and his wartime service on the Food and Nutrition Board, including the formation of the Recommended Daily Allowances and the Dept. of Agriculture's opposition to the RDAs in favor of Minimum Daily Requirements. In the 1940s he developed a soybean-based Multipurpose Food (MPF) and in 1946, with restaurateur Clifford Clinton, founded Meals for Millions, a nonprofit organization dedicated to combating world hunger with MPF. Recalls advent of George Beadle as division chairman in 1946 and subsequent changes in the Biology Division. Recalls his postwar work on protein synthesis with isotopes from the Atomic Energy Commission,

and his work on hemoglobin and erythropoietin. Discusses his difficulties during the McCarthy era and his work on heart disease.

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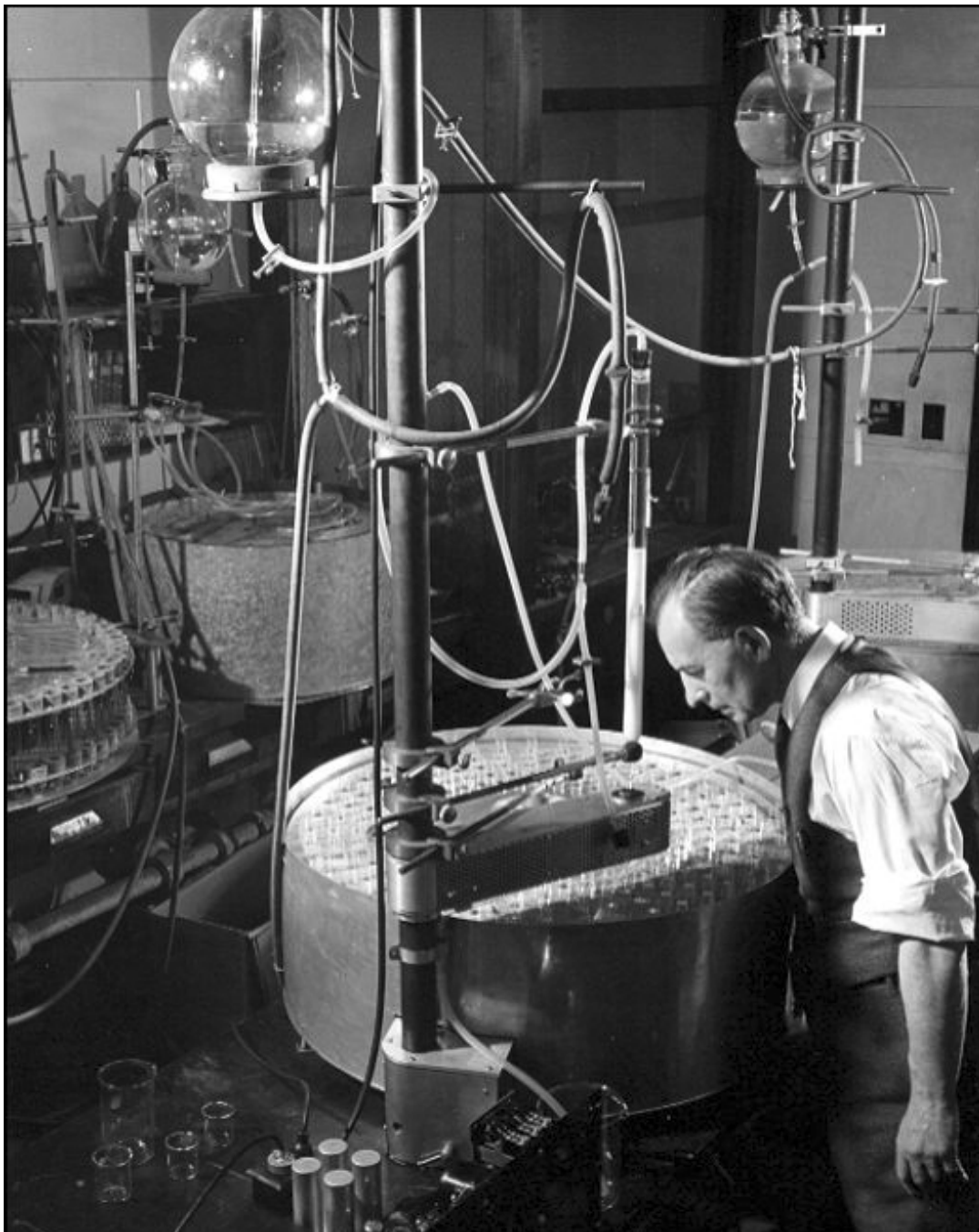
Preferred citation

Borsook, Henry. Interview by Mary Terrall. Pasadena, California, April 5, 1978. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web:
http://resolver.caltech.edu/CaltechOH:OH_Borsook_H

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Henry Borsook with Warburg respiration apparatus, 1933. Photo by Ross Madden.

California Institute of Technology
Oral History Project

Interview with Henry Borsook

by Mary Terrall

Santa Barbara, California

Caltech Archives, 1981

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Errata

- p. 3: “Loeb and Northrop, who later got the Nobel Prize in biochemistry”—John Howard Northrop received the Nobel Prize in Chemistry in 1946. He worked at the Rockefeller Institute with German-American biologist Jacques Loeb (1859-1924), who never received a Nobel.
- p. 7: “he died in ’46....and he was about 81 or 82 then”—Thomas Hunt Morgan died in 1945, at age 79.

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Henry Borsook
Santa Barbara, California

by Mary Terrall
April 5, 1978

Begin Tape 1, Side 1

Terrall: I'd like to start with your childhood and educational background. I have some biographical information already; I know you were born in London. What did your parents do there?

Borsook: My father was a tailor. My mother was a housewife. My father was born in Russia. My mother was born in Rumania. They emigrated to Canada in ... I'm not quite sure. It was either 1906 or 1907 and, of course, I went with them. So my early schooling was in London, and in those days you went to school at the age of three and it wasn't a kindergarten. You started right off learning to read and do arithmetic and such things. So that when I went to school in Canada, Toronto, I was a year ahead of the other children as far as schooling was concerned. But otherwise I had all my schooling, all my education, in Toronto.

Terrall: In public schools?

Borsook: Public school--high school, university, medical school.

Terrall: What decided your parents to come to Toronto?

Borsook: Some friend or other probably said something to them about Toronto, but in those years people living in England ... there was a good deal of emigration, thinking they could better their circumstances, and so that's why they emigrated to Canada. There was a feeling--I could exaggerate this possibly. I think

it's better to say the reason they emigrated, they felt they could better themselves. And I think they did.

Terrall: Now going back to your years in Toronto, when did you first get interested in science? Do you remember?

Borsook: Well, even as a child I had intended to become a doctor. And so I first went to the university. The course I took first was not a medical course. It was called a course in physiology and biochemistry, which was really a kind of pre-med course, in a way, though not necessarily. It was then that I became interested in science and specifically in biochemistry. If you ask me just why, I can't tell you. It was just one of those things. So I went through and then I stayed in the Department of Biochemistry, took my Ph.D. there. But I went on to the medical school afterwards to get a medical degree as a grubstake. That is, I wasn't sure that I could make a go of it, make a living in academic work, but as a doctor, well, the chances were, I could. And so after I graduated in medicine I rejoined the Department of Biochemistry at Toronto for one year. And then the man I had worked with for my Ph.D. had known and was a friend of Dr. [Thomas] Morgan in New York. And so when he learned that Dr. Morgan was going out to Caltech to start a Division of Biology there, he wrote him about me and so then Dr. Morgan offered me a job.

Terrall: You were thinking first and foremost of a career in science but you got the medical degree ...

Borsook: As insurance, just in case I couldn't make a go of it.

Terrall: How much biochemistry was being done, say at Toronto, at that time? Was it a large group?

Borsook: No, it was a small group. There were two professors and maybe three or four graduate students. That's all.

Terrall: So biochemistry was really a relatively young field?

Borsook: Not really. I would say that biochemistry went back to the 1880's in Germany, and then to England, and to a lesser extent France, and then the United States. So the professor of biochemistry at Toronto when I was a student was a Scottish professor* and he had studied in Germany. And then the man I worked with, Wasteneys, actually was an Englishman, but he didn't have a Ph.D. He had been an assistant to two men, Loeb and Northrop, who later got the Nobel Prize in biochemistry, in New York at the Rockefeller Institute. And it was somehow in that connection that he got to know Dr. Morgan. And then at that time the professor of biochemistry at Toronto was an Australian, a man named Robertson. And so, I don't know the details, but it's sort of natural, Wasteneys had grown up in Australia and had worked in Australia so he knew about him. And so he went up as sort of the second in command in the Department of Biochemistry at Toronto. Robertson left and went back to Australia and then this Scottish professor, Andrew Hunter, came and Wasteneys was still the second in command, and that's how that connection was established. But, you see, biochemistry, certainly going back to the beginning of this century, was one of the departments in the medical school, like physiology, and only later did it become partially separated from the medical school, especially with the development of biochemistry, bacteriological biochemistry and plant biochemistry. So biochemistry began to be kind of a discipline in itself.

Terrall: Now what were you working on in those days?

Borsook: Well, for my Ph.D. I worked on the synthesis of protein. It was a subject which always had interested me and in the last years at Caltech I took that up, the last fifteen years when isotopes became available and with isotopes it began to be really possible to study synthesis of protein which it really wasn't before. For my Ph.D. I

*Andrew Hunter

was working under difficulties, the system which I studied was, as we now realize, an artificial system. It wasn't really one that normally operates in animals, plants, or bacteria.

Terrall: Were there many people back then working on this system?

Borsook: No, I was all alone, and I was all alone in that field until isotopes became available.

Terrall: So you said that the professor at Toronto had known Morgan. So you knew of Morgan obviously. What did you know of Caltech?

Borsook: Nothing, except that it was a famous place for physics because [Robert] Millikan was a famous man. To illustrate that, when I was taking my leave, I knew one of the professors of physics at Toronto and when I said I was going to Caltech he said, "Well, you should learn some physics there."

Terrall: So Morgan offered you this job. What was your response to the job offer, not knowing anything about Caltech?

Borsook: Well, I was glad to take it because there didn't seem to be any future for me at Toronto. So I was glad to take it.

Terrall: So they weren't going to be expanding or anything at Toronto?

Borsook: No, no, not at all. It was only much later in 1947 that I was offered a job at Toronto as professor, which I declined.

Terrall: Was Morgan personally interested in biochemistry?

Borsook: No, and apropos of that, I think I might tell you a story. When Einstein came to Caltech, it was in 1931 or '32, everybody wanted to meet him. But Morgan was a reticent person and didn't seek out people, so he didn't. So Einstein came to see Morgan, and Morgan's office

and my lab were close to each other and so I knew that Einstein was coming to see Morgan that afternoon. After Einstein left, they'd spent most of the afternoon together, Morgan felt he had to talk to somebody about it so he came in and talked to me about it. The first thing Einstein said to him (and this is in answer to your question) [was], "What in hell are you doing in a place like this?" And Morgan said, "Well, my belief is that the future of biology rests in the application of the methods and ideas of physics, chemistry and mathematics." And Einstein shook his head and said, "No, that trick won't work. Look, even in physics we can handle only the very simplest molecules, hydrogen and helium and a few others. We can't do anything about organic chemistry." And he said, "Do you think you will ever be able to explain in terms of chemistry or physics so important a biological phenomenon as first love?" And so I said to Morgan, "Well, what did you say to that one?" And he said, "Well, I tried to explain something about the connection between sense organs and the brain and hormones." And I said, "You didn't believe that yourself, did you?" And he said, "No, but I had to say something to him." Morgan was like that. He was a witty person and he could pick up like that, quickly. So that's why he had me in biochemistry. Of course he brought with him, as you know, his whole genetics group. But he felt that he had to have biochemistry, animal physiology, plant physiology, experimental embryology, and that's why he got me.

Terrall: But you were the only biochemist?

Borsook: I was the only biochemist.

Terrall: Were there plans to expand the biochemistry?

Borsook: No, there were not plans to expand anything in biology. It was, as budgets go, it was relatively small. It was nothing compared to physics or chemistry, for example. You see, the budget in those days was \$75,000 a year. And so you can see how much would be spent on all the others. No. Morgan thought that was enough and, of course, time would tell.

Terrall: So he really wanted to have a sampling of people from different areas in biology?

Borsook: The major areas, yes.

Terrall: How closely did he follow the work that was being done by these different people?

Borsook: He couldn't. He didn't have the background. Morgan really-- he was the greatest biologist of his time and it's an interesting commentary that he knew very little chemistry, he knew very little physics, and he could only do the simplest statistics that he needed for genetics. It's typical of Morgan.... I got to know him very well. We lived one block apart and I used to drop in often, and I said to him once, "Look, why don't you take one of these Caltech graduates who are well schooled in physics, chemistry and mathematics, to work with you for a year. They could do all these things that are hard for you to do and it would be a wonderful experience for them." And he said, "No, my work isn't important enough." So he never had, he wouldn't accept a graduate student. People would come to Caltech to do genetics; they'd go to the other geneticists--[Alfred] Sturtevant or [Sterling] Emerson or [Calvin] Bridges or [Jack] Schultz. But Morgan wouldn't have them with him.

Of course he could follow the genetics, but he couldn't really follow, except in its larger outlines, what the others of us--like in plant physiology, or I, or in animal physiology, were doing. But he was an extraordinarily intelligent person, and even if he didn't know the details he had a very sound judgment in the main about how valuable the work was, and he made very few mistakes on that score. He was really very acute that way. He was really a wise person.

Terrall: I guess part of what you're saying is that he was relatively old by the time he came to Caltech, and he had already done his major work.

Borsook: Well, he had done his major work before then. Morgan's major work had been done between the years of 1911 and 1921. Before then he was doing experimental biology; that is, really developmental biology, and actually he was the first professor of experimental biology in the world. No one had been appointed to such a position before. And then after 1921, as he told me, he had become bored with genetics. He said it was just algebra problems. So he went back to working on what he had worked on originally. He knew very well that it wasn't anyway nearly as important, but that's what he was interested in and what he was going to do. So, let's see, how old was Morgan? Morgan came to Caltech in '28.

Terrall: I think he was 62.

Borsook: Yes, and then he died in '46, didn't he, and he was about 81 or 82 then. Yes, so Morgan continued to work. He used to go every weekend to the Marine Biological Lab at Corona del Mar and he worked during the week. And he taught a course. He and I taught the first general course in biology to all students of science--it was a required course for all sophomores, regardless of what science they were going to major in. He began the course, he gave the first ten classes, and I took the rest. And that pattern went on until 1935 and then we thought it would be time to change and get somebody else.

Terrall: Was he a popular teacher?

Borsook: I don't know. I can't recall now any comment. But he was such an interesting and commanding person that.... And of course they knew he was a great biologist and that's all that was necessary. And what made it particularly interesting is that Morgan in his own career spanned the development of biology from Darwin to experimental biology and he lived through that whole time as well as living through the time of the re-discovery of [Mendel's work]. I don't know whether someone told you this. Morgan, after he had a job at Bryn Mawr, used to go every summer to the Marine Biological Station at Naples to carry on his own research.

The teaching load at Bryn Mawr was too heavy to do much work there. At the end of the summer, first he would spend a week in Siena and then he went to a friend, to an amateur biologist in Basel who kept all the current journals in biology, and that's how Morgan caught up with what was going on, because at Bryn Mawr biology was a relatively backward subject. And it was there that he learned about the re-discovery of Mendel, and this is how his interest in genetics was first aroused. But he didn't work at it until about 1911 and then he worked at it very hard with Bridges and Sturtevant especially and with [Hermann] Muller, a pupil of his at that time, and that's when the great development in modern genetics occurred, until recent times with the relation of genetics to DNA and all of that, which Morgan didn't understand and had nothing to do with.

Terrall: Well, Morgan was also on the Executive Council.

Borsook: Morgan was on the Executive Council right from the time he came. Naturally I know nothing about that, but it was one of the remarkable features of Caltech that it was run, as it were, with such a loose administration, with almost no administration. If anybody on the Executive Council wanted any information they would never think of going through channels. You'd just call up the person who had the information. He could be a graduate student or a professor, and you would go over and talk to him. And that's the way Morgan was in relation to the administration of the Biology Division. But I'm sure that his contributions to the proceedings of the Executive Council were important because he was such a wise person. He had such good sense and he was quick to understand even things that he wasn't really schooled in, and I think they appreciated that.

Terrall: Was he the kind of person who was easy to get along with personally?

Borsook: Yes, oh yes! But if you put on any dog or any pretense he was very quick to puncture it, and sometimes he would do it anyhow. And this

is where Millikan found it difficult to understand Morgan, because Morgan was such a tease. He liked to tease. Now let me give you two stories about the relation between Morgan and Millikan. At the time when the Biology Division was founded, Millikan had been writing a number of articles in the Atlantic Monthly on the relation of science to religion and Millikan was making the point that there was no necessary conflict. In those days at Caltech there was a Friday morning assembly where all the undergraduates came and different people talked to them. This morning Morgan talked to them, naturally enough, about biology as a career. So he started out by saying, "Well, there's this kind of a job that you might be qualified for and so on, but that is of secondary importance in your taking a course in biology. The important thing when you take a course in biology is that you will lose a lot of superstitions." And Millikan was sitting right in the front row. Morgan said, "One of the superstitions that you will lose is that there is no conflict between science and religion." [Laughter] Everybody appreciated what was going on.

Another time the National Academy [of Sciences] was meeting in Pasadena. Morgan at that time was President of the National Academy and so he presided; papers were being presented in chemistry and biology. And he said, "Now I'm going to turn the chair over to Dr. Millikan because the next are a group of papers," and then he looked at Millikan and said, "on celestial rays, and Millikan is a lot nearer to heaven than I am."

I like to talk about Morgan because he is the really important person--he was a humble person. As I say, he wouldn't take any graduate students. He was lovely with children. For instance, they had at their house, on the front lawn, always an Easter egg hunt and he just reveled with the children and took part in it. The morning after the announcement that he had won the Nobel Prize he happened to be standing outside Kerckhoff. And there were a lot of boys playing around on the outside there, and a newspaper photographer came along and wanted to take his picture. And he said, "I'll let you take my picture if you'll take all the boys with me."

Terrall: I think I've seen that picture.

Borsook: To finish that off, this side of Morgan, I had published a book on vitamins; it was published by the Viking Press*. The president of the Viking Press, a man by the name of Ben Huebsch, was coming out to see Upton Sinclair. They published Upton Sinclair. So he wrote and we asked him to come to dinner, just himself. And I surmised that Ben Huebsch and the Morgans must have had a number of mutual friends in New York, so I thought it might be pleasant, and since they only lived one block down, I called the Morgans up and said Ben Huebsch was here and wouldn't they come up and I told them why. So they came and it developed that they did have a number of mutual friends. So they had a very good time together. Then the Morgans left and Huebsch said to me (I had told him about Morgan and his career), "I would like you to put a proposition to Dr. Morgan." He said, "We would like to publish his memoirs," because he had lived through this crucial period in biology. More than that, he had been a boyhood friend of the Flexners; they used to run around the streets of Lexington [Kentucky] together. And so naturally enough, with the Flexners, Morgan was advisor or a member of the board of a number of important medical and biological institutions in the United States. So he said, "We would like to publish his memoirs, but now, will you put this proposal to him? Let him choose a secretary and she or he could come and he could talk, and she would type it up, and he could check it over and have the secretary send us the bill, to make it as little bother as possible." So I said I would and I thought I had better speak to Mrs. Morgan first. And Mrs. Morgan shook her head. She said, "He won't do it, but I'll tell him anyhow." So a few days later we met in the corridor. (She was still working in genetics herself then.) And she saw me and she shook her head, no. Well, Morgan was already getting on and since I had medical training and I was a friend and I used to come in when he was ill and see what was up and what I could do. So I was with him in the hospital in his last illness, and he sensed this was the end for him, but he was a brave man and he was witty even though he knew he was dying, and he said to me (he only called me by then by my first name, but I always called him

* Vitamins: What they are and how they can benefit you.
(New York: The Viking Press, 1940).

Dr. Morgan; I couldn't do otherwise) and he said, "Henry, you get yourself a good secretary and you write my biography, but you must make me a promise. It won't be published for a hundred years." And that gives, I think, something of the humility of the man. Because he remembered the offer for his memoirs, and that was the only remark that he had ever made about the offer of Mr. Huebsch.

Terrall: What about the other geneticists, the younger people who had come with Morgan; Sturtevant and Bridges?

Borsook: The two principal ones were Sturtevant and Bridges and they were distinguished geneticists in their own right and they were working on their own. Bridges, of course, died while at Caltech. So did Sturtevant but Sturtevant lived much longer. By the time that they came to Caltech, they were working independently. Morgan wasn't working in genetics anymore, but Morgan felt that he should continue the genetics group and a younger man who came along with them was Jack Schultz, and he also brought along Albert Tyler whose field was, like Morgan's, experimental embryology on invertebrate forms, and Tyler kept on working on that but independently of Morgan. Morgan wouldn't work with anybody else and, of course, from the very beginning he insisted that Mrs. Morgan should work independently and not with him. She had learned genetics from him but after that she worked by herself, and published by herself.

Terrall: Did you, the group of you, did you have discussions about the direction the Biology Division should go in? Was there any discussion about changes?

Borsook: Each of us did what we liked, but we had certain teaching responsibilities, so the geneticists divided up the genetics, I taught biochemistry, [Cornelis] Wiersma and [Anthonie] van Harreveld taught animal physiology, [Frits] Went taught plant physiology. That's the way it went, but that's all. And our staff meetings consisted really only of the approval of applications for graduate students. We all went over them, and we all had a say in who was chosen and who not, as well as new

appointments.

Terrall: Did you recruit graduate students?

Borsook: We didn't try. They just came.

Terrall: You got as many as you wanted?

Borsook: Just about.

Terrall: You had graduate students working with you?

Borsook: Yes, two or three at a time as well as some postdoctoral people.

Terrall: What about contact with faculty in other divisions?

Borsook: Oh well, that was one of the great features of Caltech when I first came. See, it was a community and everybody knew everybody else and in those days we often had lunch together in what was called the greasy spoon, before the Athenaeum was built. One of the striking features that struck me was that everybody was a really intelligent person, quite apart from their professional competence, with wide-ranging interests in other people's work and in politics or literature and in art.... Well, the chairman of the Humanities Division, Clinton Judy, was very good. He used to run once a month in his house a seminar open to everybody and where everybody took turns at giving a review of a book or an author and there would be active discussion and so on, which is indicative of the smallness of the place and of the wide-ranging interests of everybody. You probably have heard this story but it's worth retelling. As an example again of the mutual interest in what everybody else was doing, Charlie Lauritsen had just finished building the first what we used to call the "million-volt X-ray tube," the high voltage X-ray tube. And that morning there had been a piece in the Los Angeles Times that Joliot in Paris had created artificial radio-

activity. So we were talking about that at lunchtime and around the luncheon table a long telegram was drafted to be sent by Charlie to Joliot asking for more details and the next day Joliot's reply came. That was discussed, and then that afternoon Lauritsen went and did an experiment to check up on Joliot. That's the kind of place Caltech was in those days. And although we all respected each other there was no deference. Millikan was there, and if he'd get into an argument he had to take his chances like anybody else, and it was the same all the way through. And Millikan often came. You sat down wherever you could. Of course when the Athenaeum came, with separate small tables, that relation was broken. It was really better when we were in the old greasy spoon with a long table and you just sat down.

So we had a pretty good idea of what just about everybody was working at and what kind of people they were like, and the nice thing was we had no sense of administration. About the only administration that we knew was Ned Barrett* and he would come too and we all knew him, and the administration was there to do things for us but one didn't sense it was administration. And this was one of the wonderful things about Caltech, this closely knit community; faculty, Executive Council, Board of Trustees, we all trusted each other and knew each other and it went very harmoniously.

Terrall: Did you know the Trustees as well?

Borsook: Some of them.

Terrall: Did you see them socially?

Borsook: Oh yes, you see, for example I was a member at first of the Library Committee of the Athenaeum, and a member of that committee was Jim Page and we occasionally met at his house. And we knew the Ruddocks and some others, and so on. And others knew other trustees. I know Roger Stanton who was in the Humanities Division and was a good friend of mine--he used to come to Santa Barbara on occasion; when the Ruddocks

*Comptroller

would go away for a stretch, he would use their house weekends and summer for longer periods and so on. This was characteristic of Caltech in those days.

Terrall: So the people you knew socially were people from all parts of the Institute then?

Borsook: Yes.

Terrall: Was there a reaction to having biology at the Institute as a new division?

Borsook: No. The reaction was they were interested, and they wanted to know what it was about and they could easily understand--well, the connection wasn't so hard for them to grasp because they grasped at once the medical connection, and of course they grasped at once the importance of genetics. You see, these people were interested in lots of things. They were interested in understanding, and sometimes they would drop in and talk to one another. We were friends, you see. They'd keep asking, "Why?" And I must tell you in this connection, and it's indicative of the relation--our daughter was very small. I used to walk to school with her, and like young children she would always be asking questions, and one day she said, "What does Mr. Millikan do?" So I said, "He's a physicist." And she said, "What's physics?" I began to talk about the relation between energy and matter and she became impatient and she said, "Is it asking 'how' and 'what' and 'why' back and back and back?" And I said, "Yes. That's as good an answer to all science." Well, that's the way they were. They were always asking 'how' and 'what' and 'why' back and back and back. And so biology was just another field, just as geology, and astronomy was there before Caltech was there--that is the modern incarnation of Caltech.

Terrall: I was just wondering; physics and chemistry and math were so established, and here were these new people coming in.

Borsook: This was Morgan's reputation. They knew of him by reputation and that made it easier for all the rest of us.

Terrall: And that was why Millikan brought Morgan?

Borsook: Oh, of course, yes. I know how the story went. The Executive Council before Morgan was, as you know, Millikan, [George Ellery] Hale and [Alfred A.] Noyes, and they had decided they should have biology because it was to be an institute of science more than an institute of technology. Well, and this was a typical Millikan question, "Who's the best biologist in America?" Well, it was Morgan. Hale and Noyes said, "Oh, you can't get Morgan. He won't leave Columbia. He's established there." They had luncheon at the Century Club in New York, the four of them, and they asked Morgan to come and Morgan, to their surprise, said, "Sure I'll come. I've always wanted to retire in California. In any event," and he said then the answer he gave Einstein [about] why he would like to come to Caltech because he felt that the future of biology was in using chemistry, physics, mathematics, and so he came.

Terrall: So he was the perfect person for the job really.

Borsook: They couldn't have got a better person in the world for that job. Now I should say one other thing before we go on. It was Morgan who started the Health Center. It was his idea that there should be a doctor and that the students should come in. The visiting hours were a couple of hours every day. The first doctor was Dr. Edward Kremers. Now of course there had to be a faculty committee, and naturally he chose me for that because I was the only M.D. on the campus. Well I was chairman of that committee for over thirty years and I am proud of one thing about it. It did what it was supposed to do, but possibly mine was the only committee on the campus where every decision had to be unanimous. If there was one dissenting voice we put the question aside and we discussed it again and maybe we changed it and it worked. It worked very well and everybody was harmonious and everybody felt two

things: they were free to speak up, but there would be no frivolous objections because they knew if they objected the whole thing would be held up. So it went very well. It was only toward the end of that time that first we had on the medical staff a psychiatrist who came one day a week, and then some years after that a clinical psychologist who was full-time. And the deans especially felt that the clinical psychologist was a great help to them. It helped them with many of the personal problems that undergraduates and graduate students, too, had. And as part of that school Health Center we charged \$2.00 for medical insurance so that if boys went to the hospital it would be paid for out of that. The students were well looked after from a medical point of view, and the real initiator of that was Morgan.

Terrall: Well, maybe we should break now.

Borsook: Yes, we have to go out to lunch now.

Begin Tape 1, Side 2

Borsook: Morgan was the first non-medical person to get the Nobel Prize in medicine. I happened to drop in one Sunday at Corona del Mar and watched him working. He was working on a certain invertebrate that produces both eggs and sperm and yet they could not fertilize themselves, the sperm of one animal could only fertilize the egg of another. And he had found out that if he suspended the eggs in an acid solution, that would break down their resistance to self-fertilization. He told me the acid did not always work. And how was he making the acid solution? He had an eye dropper, and he would have a dish of sea water, and he would drop a certain number of drops from the eye dropper into this dish of sea water. And I said, "Well, no wonder it doesn't always work." I didn't say anything but went back to the lab and made him a set of standards, so he could measure the acidity colorimetrically. So I brought the whole set to him and he said, "Goodness gracious! Nobody has ever done this to me before in my whole life." And I thought he'd

be offended, as he might very well have been because I was interfering in his affairs, and I said, "Well, really, Dr. Morgan, you know yourself you were getting variable responses, and that's the reason, but if you will use this, then you will know." And then the following week, he'd been down another weekend, and he came down--my lab was in the basement then--and he said, "If you will promise me that it won't interfere at all with your work, I would like another set of those standards."

Terrall: He didn't want to impose on you.

Borsook: Oh, of course. No, no, he was most diffident about that. And that was when I asked him why didn't he take one of our young graduates and, of course, what I did would be nothing for them. But he wouldn't do it. But children often would come into the lab--his door was always open--and of course they would come in and want to know what he was doing. He would lift them up on his lap and have them look through the microscope and show them. He was wonderful with children.

Terrall: Who were the children?

Borsook: My daughter, somebody else's daughter and so on. Just kids six, seven, eight years old would be strolling in.

Terrall: What about Millikan? Was he interested particularly in biology?

Borsook: No. He was only interested in it because--when I first came, of course, I was introduced to him and he said he thought physics was finished and that the future of science was going to be in biology. He wasn't interested in the details of what anyone was doing but just as, shall we say, a statesman of science. That was his interest in biology. Of course he was wrong, but so was many another. They couldn't see the future of things.

Terrall: When you came to the Institute in '29, what were you working on?

Borsook: I felt I had to leave behind what I was doing in Toronto, and knowing how strong the Institute was in chemistry and physics, I began to teach myself the kind of chemistry that the undergraduates were getting. They knew much more chemistry than I did, especially physical chemistry, and I was interested especially in thermodynamics, and they were very strong in that. And then I began to apply thermodynamics to biological phenomena and that turned out to be very interesting. Before then the concept of an animal organism was that it was like an engine. It burned the fuel that you poured into it and there was only a minimal amount of wear and tear. Then by using thermodynamic data I was able to show first that this wasn't so; that even the waste products which you would think would just be degradation products weren't that at all. They were synthetic products. There was urea, the chief waste product of nitrogen, that took a good deal of energy to build up. Before then it was taught that when protein was broken down to amino acids--there are twenty-one amino acids--then when they built up, the reaction was reversed. Well, I was able to show from thermodynamic data that this was impossible. It was too far uphill. To rebuild them into protein you had to put energy into the system. You had to couple an energy-donating system, like burning sugar or something like that, with the synthetic apparatus; and it was entirely different from the breakdown process. Now the important thing of that was that it removed from physiological thinking what we call teleology. The organism was not a machine and we were able to show then that of the urea which is excreted daily it was not, as it was taught, two or three percent of the food you ate, but that 55% of the urea that was daily excreted came from breakdown of one's own tissue protein. So proteins were continually breaking down and continually being rebuilt, and this was a much more biological concept. When you had a system that was in flux like this, then if you needed more it was relatively easy. So for growth, for recovery from disease, for antibodies, all of these things for which we needed protein, with a system going like this where it could be increased or decreased according to the supply of energy and protein, it was much more readily understandable and it was that that I think really changed the outlook in biochemistry in these matters.

Terrall: Now when you were doing this work applying thermodynamics to biology, was this being done also at other places?

Borsook: No, I was the only person doing it. It's not a virtue, but if anybody began to work in something I was working at I would drop it and turn to something else. The big advances in all science are made in the fashionable branches because lots of people are working at it, but I couldn't do that. I have to do my own things in my way. In a way it was an amateur's way of looking at science rather than a professional's, but that's the way I was, you see.

Terrall: Who were the nationally respected biochemists at the time?

Borsook: There wasn't one. There were a good many good biochemists all over the country and I don't think there was any commanding figure. And now this thing that I did, you see, using thermodynamics and certain experimental devices--I had no isotopes and this was in '32, '33, published in '33. It was only in '39 when isotopes became available that my idea then was proved with isotopes. It was easy to prove this with isotopes then. And at first the old men of biochemistry, a man named Folin at Harvard, and a man named Hamilton, I think, at Illinois, when I published this idea of mine, oh, they pooh-poohed it and said, "This is all wrong, it can't be," and I just didn't understand how to look after animals and what not. They were so set. And in a sense it was eight or nine years too soon, if you like. They weren't really ready and of course I couldn't prove it as conclusively as it was later proved with isotopes.

But from then on this led me into thinking about and trying to understand how energy is transferred. It's all very well to say, well you have to take the energy to use it to build up, but how was this done? and this led me to put forward that there are two ways. In the first place energy is transferred by burning something, oxidizing it, and when something is oxidized something else is reduced. The reducing mechanism is an electron that moved from a high potential to a lower potential. So this was one way in which energy was transferred. And the way we did

it, we took bacteria, that was easier to work with, and I could show what it took to have [energy transferred]. A substance was oxidized on, say, one side of the bacterium and another was reduced on the other side. Well, how was the electron transferred? I showed this could happen in two ways. If I disturb the surface, let's say, by washing it with ether or toluene the enzymes that did the oxidizing and reducing were still intact, but they had lost their connection. And so you had to put a substance in that would carry the electrons from one to the other. But in the intact bacterium the energy, by electrons going in and out, traveled through the wall without ever being free, and so these were the two ways in which energy was transferred. So that was also an outcome really of thermodynamic thinking.

Then we worked out how much work is involved, let's say, when the kidney makes urine. With us, that is animals, mammals, the urine is concentrated relative to the blood. In frogs, it's dilute relative to the blood. And one could show by analysis that in both cases it takes energy. It doesn't take energy only when the fluid is the same on both sides. Well, that was a kind of exercise in physiology. And then afterwards we moved, shifted over. I began to wonder how a substance called creatine which is involved in muscular contraction is made. I was interested in that because we make an awful lot of it, relatively. We make one or two grams of it a day and it is excreted as creatinine. There again there are certain tricks. I found that if I tried to make creatine by grinding up a tissue and extracting the enzyme nothing happened. You had to have the intact cell structure, but I couldn't work with a whole animal or I'd learn nothing. So what I found was that if I made thin slices where they were three or four cells thick so the outer two layers might be damaged but there were three or four layers inside, that would do it. And then we found that synthesis of creatine took two organs. The first part which is a condensation product of arginine and glycine was made in the kidney, then the product, which is called glycoyamine, was transported to the liver and the liver converted it to creatine. And then from there it went to the muscles where it was phosphorylated and then was the source of the energy for muscular contraction. That took us pretty well--there are one or two other things we did

like that--into the war.

Terrall: What kind of contact did you have with other people working in biochemistry around the country? Did you travel to meetings?

Borsook: Yes, I traveled to meetings and it was only afterwards that I realized that to be financially able to travel to meetings I, like all the other people of Caltech, owed it to Millikan. You know about that, that all of the fees he got for lectures was our travel fund. We didn't know that. And so I went to meetings. I didn't have many contacts, a few. Because as you see I didn't work in the fashionable fields and so . . .

Terrall: This wasn't a problem for you obviously.

Borsook: I knew some of them, of course, and it was only when isotopes became available to everybody that I was then working in the same areas as people at Harvard and MIT and at Columbia and so on. But before then I was all alone.

Terrall: Did you feel isolated?

Borsook: No, I preferred it, because you see then I didn't have to keep up with the literature. I did read what had been done before but nothing had been done like what I was doing so it was easy in that respect.

Terrall: What about funding when you first came?

Borsook: It just came out of the Institute budget.

Terrall: It was just for your salary?

Borsook: My salary and whatever expenses the work called for. There was no funding at all. It was only after the war that government funding came into it.

Terrall: So you didn't do any application for outside funds before the war?

Borsook: No, that was discouraged at Caltech. And I must say, I was going to say we didn't feel the need of it, if we needed equipment we built it instead of buying it.

Terrall: Well, what about salaries for, say, research staff, lab assistants?

Borsook: Salaries were low. I had one assistant and a couple of graduate students. The graduate students worked on their own and it was understood that I would not put my name on any piece of work done by a graduate student, even though I may have told him what to do and guided him. That is, whatever I put my name to, I had done the work myself. And I don't think I was alone in this respect, in the Biology Division at any rate. I don't know about the other places. But I never felt the lack of money. I may have felt the lack of ideas but not of money.

Terrall: I know that some private foundations were giving Caltech money before the war, like the Rockefeller Foundation.

Borsook: Well, they were famous people and we weren't. And I think biological research, biochemical research wasn't in those days the kind of thing that attracted money. I didn't really need it. Teaching wasn't heavy. I just taught for one term and there was a seminar, of course, and so with one assistant, and I had a very good assistant, [Jacob] Dubnoff, for many years, about ten years, we could do all that we wanted to do and everybody else was working by himself, Morgan was doing his own work and washing his own dishes, let alone the other people. So that this was the style of the place.

Terrall: In my research in the Archives, I found that in the late thirties and forties there were some things that were written by Linus

Pauling proposing that the Chemistry Division get more into biological problems and biochemistry.

Borsook: And Pauling himself did, too.

Terrall: Was that something that was discussed with you? Was there to be any collaboration?

Borsook: No.

Terrall: Was that a problem?

Borsook: No, I wasn't working in the kinds of things Pauling was, and Pauling wasn't particularly interested in [my work]. I think he might have latched on if he thought he could use it, but he didn't see how. And he had his own interests. No, I don't think Pauling discussed this with anybody. He just went ahead and did it, just as the rest of us, we would go ahead and do what we wanted to do.

Terrall: Well, I think that there was a proposal that he wrote for getting into medical applications.

Borsook: Yes, he--molecular medicine, and I was asked to . . . That was with Beadle. Beadle was there then, I think.

Terrall: Yes, it got more specific when Beadle came, but Pauling had had it in the works for several years.

Borsook: My feeling about that was, but there's nothing new in this, medicine has always been molecular medicine. We knew that antibodies, antitoxins, poisons, these were all substances, so if you want to call that molecular, medicine as it advanced has always been molecular. Medicine had long past gone beyond considering a disease in terms of the whole organ. It was considering in terms of chemicals affecting individual cells and Pauling didn't like that. No, Pauling and I were

on reasonably good terms; we weren't at all close friends, but I know his face dropped when I pointed out that molecular medicine is nothing new. But of course Pauling's great contribution was in the sickle cell anemia. Itano and he did it really and then, of course, I think the sickle cell anemia was a more important discovery than Pauling's proposal for the structure of proteins, the helical structure, because the sickle cell anemia has had, shall I say, wider ramifications which the helical structure of proteins has not had.

Terrall: In connection with his wanting to get into biochemistry I think he found money somewhere to hire somebody, and they brought [Carl] Niemann to the Chemistry Division.

Borsook: He did. He hired several people; in fact he hired one of my assistants. This was after the war though, when isotopes became available. That's when he was interested in phenylketonuria as a cause of insanity. It had been known long before Pauling that phenylketonuria was associated with mental deficiency and so on. But there was never any resentment or even any sense of competition. I know it came up once in a Biology staff meeting. One of the other members of the staff, a geneticist, was branching into biochemistry and Beadle asked me what I thought. And I said, "Well, surely every member of the staff has a right to do whatever he likes, wherever his interests go and he doesn't have to ask anybody to do it." I said, "I want that--it isn't a privilege, it's a right--and I think everybody else has it." And that was the attitude toward that.

Terrall: Well, we'll come back to the war, but when Beadle came he and Pauling did a lot of work on this huge proposal.

Borsook: Yes, he did and I know Chuck Newton who was then DuBridges's assistant showed me the proposal and I told him what I just told you. And I said, "I don't think this is right." And it got back to Beadle and Pauling and they rather resented my telling Chuck this. I thought if they said they wanted to study this disease or that disease, well and

good. But to think that they've got a new idea in molecular medicine is wrong and this is what I pointed out.

Terrall: But part of their idea was to get more medical research done at Caltech than was being done.

Borsook: Well, I don't know so much about Beadle as I am sure it was the case with Pauling. That is, he wanted medical research, but the medical research he wanted done he wanted to have under his own thumb. I don't think he would have cared to have other people doing medical research in the Institute. And he came over once and spoke to me about certain notions I had and I told him it was none of his business, and that I would do what I liked, and that was the end of that.

Terrall: Was there any collaboration between members of the Biology and Chemistry Divisions?

Borsook: No, as far as I know, I don't know of a single instance where there was any collaboration though there were very good relations however between . . . Who was the man who died a few years ago who worked on hemoglobin? [Robert Corey] worked on the structure of hemoglobin and so on. We were on very friendly terms, although there was no collaboration, and he was on similar friendly terms with other people in the Biology Division. No, that was fine. When he had a graduate student I was usually on his Ph.D. committee and so on.

Terrall: So there was a certain amount of back and forth.

Borsook: Oh yes, there was a good deal and we were interested in what he was doing and he was interested in what we were doing.

Terrall: I think one of the people that they tried to hire was Todd from the Lister Institute.

Borsook: No, he was Professor of Chemistry at Cambridge. He was a

famous chemist.

Terrall: But he had been working on vitamin E, I understand.

Borsook: And he'd worked on vitamin B₁. He was one of the people involved in working out the structure of vitamin B₁. He didn't do it all but he did part of it. That was Alexander Todd.

Terrall: Do you remember when he visited? Did you talk with him about his vitamin work?

Borsook: No, he came to the Institute and he gave one or two lectures and that's all. He talked then about vitamin B₁.

Terrall: You were already working on vitamins, weren't you?

Borsook: In another way. The work on vitamins, as I say, it was extracurricular and it began really when I first came to Caltech, or shall I say better when I first came to Pasadena. I found that the doctors around town really didn't know anything about nutrition and certainly didn't know anything about vitamins and I felt that the vitamins were among the most important medical discoveries of the century. So I used to do a lot of public lecturing in the evenings to groups of doctors and nurses and teachers and so on, and then we did a little medical research on the effects of this vitamin or that vitamin, but although I had a medical degree I wasn't licensed to practice in California, and it wasn't necessary because I could always work with doctors who were licensed to practice. But that was not very important really. Then the war came and I was asked to serve as a member of the Food and Nutrition Board on which I served all through the war, and the most important work that the Food and Nutrition Board did was the drawing up of the table of Recommended Daily Allowances of proteins, vitamins, calories, and so on. This was of practical importance, yes, but it also had a profound ideological importance because it said that a good diet doesn't consist of food but consists

of providing adequate amounts of certain essential nutrients, if you like, and it doesn't matter where you get them from. And so they could be synthetic, they could be grown, and so on. And this was really a revolution in the whole nutritional concept. It was resisted by some people on the Board and it was resisted by the Department of Agriculture. At first because of their great political influence, the Recommended Daily Allowances in some respects were tailored to what a person could get from what was considered to be a good diet, rather than what you need. But on the whole we succeeded in making this important distinction between what is needed to prevent or protect against a disease, like beriberi or rickets or scurvy, and how much would be desirable for what we would say was approximately an optimal diet. Because it had been shown with two vitamins that--let's take vitamin B₁. Give them a certain minimum amount and they won't have beriberi or B₁-deficiency disease but the growth curve was like this. If you give them three times that much the curve is like this, and more importantly senility was postponed. That is, they didn't live longer actually, but senility was postponed and this was, we felt, very important. So this is why they were called Recommended Daily Allowances, whereas the Department of Agriculture and the Food and Drug Administration held out for some years for what they called Minimum Daily Requirements, which was the minimum required to protect you against disease.

Terrall: So the Recommended Daily Allowances would be much higher.

Borsook: Yes. For instance it's known that for an average-sized man or woman, six tenths of a milligram of vitamin B₁ is what you need to protect against getting beriberi, but the Recommended Daily Allowance is one and a half milligrams. Ten milligrams of vitamin C will prevent anybody getting scurvy, but the Recommended Daily Allowance is fifty milligrams, and so on all the way through. Two milligrams of niacin, nicotinic acid, will protect against pellagra, but the Recommended Daily Allowance is twenty milligrams. And this wasn't just, you know, wishful thinking or doing good works, or playing safe. There was evidence from experimental animals that it was worthwhile to do this. And then, of

course, one had to take into account some individual variations, so this was a kind of safe figure for most people in the population, to give them what amounted to what we thought was an optimal diet. But the Department of Agriculture had said, "But you can't get these allowances from any reasonable good diet." This led to enrichment. Before we enriched flour, we enriched milk and so on.

Terrall: There was a lot of opposition to that, wasn't there?

Borsook: A great deal of opposition, but I think that opposition is largely gone now. But there was then for a number of years a continual argument, continual fight on that. And then we pointed out that if people are going to get all their vitamins and minerals as well as protein and calories from their food they'd have to eat altogether too much, much more than what is really good for them. That is, you couldn't get the Recommended Daily Allowances of vitamins and minerals short of 3000 calories a day, and for the kind of sedentary life that most of us live, that's altogether too much. The field of nutrition that's been most neglected is the calorie requirement. There has been only one good determination of what the calorie requirement is in humans and this was in young men, 25 years of age on the average, doing 400 calories of physical work a day. And then it was shown with them if you make sure they get all the vitamins and minerals and protein their calorie requirement with about 10% surplus is only 2200 calories. Now when you think we've taken on a kind of responsibility to prevent, I won't give it a fancy name like starvation, but calorie malnutrition in Third World countries, the difference between 2200 calories and 2500 calories represents hundreds of ten-thousand-ton ships of food when you're dealing with millions of people. It's a very important consideration.

Terrall: Let me go back just a bit to before we were in the war, when you wrote that long document, the Memo on National Defense. Do you remember that, [June 15] 1940?

Borsook: Yes. It was that memo that had me invited to serve on the

Food and Nutrition Board.

Terrall: Now, what prompted you to write that memo originally?

Borsook: Just from my observations of patients, reading the literature of malnutrition and from studying the results of feeding different levels of vitamins, particularly, and of minerals too, to experimental animals. It was clear to me that most of us who weren't using vitamin concentrates were not getting enough. This is what led to the writing of that memorandum.

Terrall: Now, what response did you get?

Borsook: It was resisted, of course, but nevertheless there was a really enterprising and liberal member in the Department of Agriculture who had a lot of influence; his name was Wilson. And he read it and although he was a little uncertain about whether he would agree with me or not, he felt that I would be a good influence on the Board. And so for that reason I was invited to serve on the Food and Nutrition Board. They listened to me but this [memo] didn't mean they paid any attention; but I was not a lone voice. There were other people who felt as I did that we couldn't rely on diet to provide all the vitamins and minerals we needed, and we did need more vitamins and minerals in our diet. And so, as you can see I was really eventually a member of the majority, because we did put through the Recommended Daily Allowances.

Terrall: One of the things that you said in that memo was the importance of a national nutrition program.

Borsook: Yes, well, because the troops should have a good diet and people working in munitions factories, airplane factories, should have a good diet and should get enough vitamins and minerals. And I think that, too, contributed toward acceptance of the Recommended Daily Allowance, the principle, because they couldn't get enough. This was so easy to do even in the 1940's, to add synthetic vitamins and minerals.

People saw that, so we're not going to take any chances with the troops, so all the bread and flour that the troops got was enriched and vitamins A and D were added to the milk they got, and so on. They were playing it safe. Why not, you know? Then I did a study among aircraft workers at Lockheed. Considering what we were doing it with, with human beings, the plan of the experiment was all right. There were two groups and no one knew; they were at random. Half of them got placebos and half of them got the vitamins and we kept track of their records and at the end of the year it was clear that there was no dramatic effect of the vitamins because after all they were not vitamin-deficient people, but the performance of those who got the vitamins was better than that of those who got the placebos.

Terrall: On the job, you mean?

Borsook: On the job, yes. Their production, absenteeism, morale in general, however you measured it, was definitely better, but when the war was over, all of that stopped. Though vitamins now are staples, everybody buys them, they get them in the supermarket, and so on. So the cumulative effect of all of that I think has remained. For me personally I think the most interesting development was really in connection with the Meals for Millions Foundation and Multipurpose Food. That story hasn't been told. The major figure in that whole story is Clifford Clinton.

Begin Tape 2, Side 1

Terrall: I just have a few more questions on the war business. Was it directly the result of the NDRC committee that the food for the troops was fortified?

Borsook: Yes.

Terrall: That was a direct effect?

Borsook: Oh yes. This was the Food and Nutrition Board of the NRC, the National Research Council, not the NDRC.

Terrall: I have a note here that relatively early someone on the NDRC asked you to come to Washington to talk to people in the Army and Navy about this.

Borsook: Someone did, yes, and I did talk to people in the Army and Navy, but I would say that was not significant. That didn't cut much ice. It was later when the Board promulgated certain things, but it was partly that Memorandum on National Defense that led to my being invited to come to Washington to talk to some Army officers and naval officers and even to the Army base in Los Angeles. I did talk to Army officers and naval officers, in terms of what they needed and in terms of how they could get it. And of course I am still, I think, among nutritionists, with the minority who insist that the easiest, cheapest, best way to get your vitamins is in a pill. Then you're sure, it's much cheaper, and you can then eat what you like. You know it's curious, sometimes when I talked to women about nutrition and I'd tell them this, and I said, "You get an additional freedom then," and they didn't like it. They felt, I suppose, it was a reflection on their housekeeping and their preparation of meals.

Terrall: That you were supposed to plan your meals a certain way?

Borsook: Yes, and that they chose the right food and all of this. And I said, "You can just give your husband and children vitamin pills for breakfast. Then all you have to be sure of after that is they get milk or cheese. They can eat what they like after that, as long as they get enough to eat and there's enough variety." And they didn't like that. They don't like it yet, I think.

Terrall: How much of your time did the NRC Food and Nutrition Board take?

Borsook: During the war a good deal of the time.

Terrall: Were you actually in Washington for that?

Borsook: Very often. At least once a month, sometimes oftener.

Terrall: So you were going back and forth a lot?

Borsook: Yes, all the time during the war. One of the people who became a close friend was the man who was the last WPA commissioner, Howard Hunter. He was Harry Hopkins' political hatchet man. He and I hit it off and so--he was connected then with war food production--and so he had me going round among munition factories, shipyards, inspecting food arrangements, the foods that were offered and so on, and then I would report back to him. So during the war all my time was devoted to food in one aspect or another.

Terrall: I notice that you were also on several things out here, the California Nutrition Committee and the Los Angeles War Council's Committee on Nutrition in Industry.

Borsook: Yes, but that was nothing. They didn't do anything. That was of no consequence at all.

Terrall: This must have been a relatively new thing though, for the Armed Services to be worrying about nutrition.

Borsook: Yes, that was really quite new.

Terrall: And who were the other people who were on the NRC board?

Borsook: The other people who thought as I did, one of them is now dead, Norman Jolliffe, and Wilder.

Terrall: Were they chemists?

Borsook: They were doctors.

Terrall: They were M.D.s?

Borsook: Yes. There was one man from MIT whose name I have forgotten now and he was cagey. He was a politico. He was trying to sit on the fence, you know, and he wouldn't take a stand one way or the other. So he didn't count for very much as a result, except that people were friendly with him. I should say the two most important people were Jolliffe and Wilder. Elvehjem later joined the Food and Nutrition Board. He was from Wisconsin. But he was there chiefly to protect the agricultural interests coming from Wisconsin, and I would say he was responsible for having the riboflavin in the Recommended Daily Allowances too high. Well, I'm maybe mean about that. I think it was amusing to us. Every man who worked on a given vitamin like Glen King on vitamin C, or Elvehjem on riboflavin, or Jolliffe on B₁, they always wanted the Recommended Daily Allowance for that vitamin way up and they probably were right. They knew the benefits from giving a lot, especially when it was cheap. And yet the resistance . . . I remember one time during the war there was an outbreak of scurvy in Maine in the winter. So we said, "Well, that's easy. You just send a truck up there with a truckload of ascorbic acid, it's synthetic, it's made in the United States, and you'll wipe that thing out in a day." And would you believe it, one of the members of the Board got up and said, "No. I would rather see them have scurvy than not get their vitamin C from food."

Terrall: That was from someone on the Board?

Borsook: Yes, it was a member of the Board.

Terrall: So what finally happened?

Borsook: What they did, they dragged up a lot of concentrated orange juice and lemon juice and got that up there. I said, "Well, that's all right. It's a lot more expensive but it's all right with us."

Terrall: You had worked on vitamin B₁ hadn't you?

Borsook: Yes, I'd worked on vitamin B₁ in two respects. One of them was for the treatment of a disease called trigeminal neuralgia which is a disease of the nerve that affects the face. It's very painful.

Terrall: Is that the same thing as tic douloureux?

Borsook: Tic douloureux, yes, and it's very painful. I thought I'd like to try out whether giving these people massive doses would help them. And the clinic, as it were, was at Caltech, and it was right opposite Morgan's office so he took a great interest in it.

Terrall: What was the feeling about having clinical experiments going on at Caltech?

Borsook: He didn't care. Morgan was very easygoing and, you see, this was done under the nominal auspices of Dr. Kremers who was the Institute physician and I was sitting in on it too. I think the outcome of it all was that about 25% of the patients were definitely benefitted. The rest were not.

Terrall: Was this with very high doses?

Borsook: Yes, for then we were giving big doses--fifty milligrams by injection daily and they would come five days a week.

Terrall: Were you producing these vitamins?

Borsook: No, I managed to get a gift of them from friends in the industry.

Terrall: In the vitamin industry?

Borsook: Yes. I didn't pay for it and they didn't pay for it. This is what I did and we made up the solutions and Dr. Kremers injected them. I wouldn't take a chance on a malpractice suit. And, as I say, we were satisfied that about 25% of these people were definitely benefitted by

that. And maybe that's all you have a right to expect, because the fundamental lesion is a restriction of the blood supply to that particular part of the brain.

Then earlier on--you see chronic gastrointestinal stress, I don't know how it is now, but then was very common in ordinary clinics and I thought that might also be due to a deficiency of a whole group of the B vitamins that they call the vitamin B complex. And the easiest way to get that was from bran, from the wheat. So I was able to get sack-loads of that and handed out packages to the people and that helped. But you know now I'm not so sure whether it was the vitamins that helped them or the bran.

Terrall: Just the roughage you mean?

Borsook: Yes, because they had to eat quite a lot and there was a lot of roughage in it and it may have been both together, I don't know. Now I'm not sure what really helped those people.

Terrall: When you were talking about that one clinical experiment that you did at Caltech, was that just done within the Institute? It wasn't funded from outside?

Borsook: No, it wasn't funded from anybody.

Terrall: Was this just a short-term thing?

Borsook: It lasted about a year or more.

Terrall: And people just heard about it?

Borsook: Through their dentists. People with trigeminal neuralgia would commonly go to a dentist because it looked like a toothache. And there was one dentist by the name of Wiggins, he is now dead, I think, in Pasadena, who had a special interest in that disease. He used to inject alcohol into the nerve and destroy the nerve that way, and that

destroyed the pain but they usually got some paralysis as well. He was very good about it and he sent us many patients, so it spread around.

Terrall: But he wasn't working with you?

Borsook: No, no. But he was very interested and he saw the patients that had come back. Luckily, one of the beneficiaries was one of the police inspectors in Pasadena--a great big fellow. It used to really knock him out and he really got help. Cropsey was his name.

Terrall: This is a chronic condition?

Borsook: Yes. It gets worse and worse.

Terrall: You have to keep taking the vitamin B₁?

Borsook: You have to keep taking it. After that they could go back to Wiggins, and Wiggins would inject vitamin B₁ into them, or other people.

Terrall: At this time were there other people at the Institute working with vitamins?

Borsook: No, I was the only one.

Terrall: Before we continue with Meals for Millions, I would like to ask you about the Anaximandrian Society on the history of biology.

Borsook: This was for biologists and they would meet once a month during term at our house, and one of the students would read a paper on some topic out of the history of biology going back to the Greeks, and if once in a while there was some person who figured directly or indirectly in medical history, I would invite him over. For example, Dr. Minkowski who was at the Mount Wilson Observatory, his father [Oscar Minkowski] was one of the people who with another one, von Mering, did one of the experiments

of fundamental importance that pinpointed the cause of diabetes. I spoke to him, he was a good friend, and asked him if he remembered his father doing that work. He must have been a youngster. He did, so I had him come over and talk to the Anaximandrian Society about his father and the work he did. But for the most part--see Anaximander was the first Greek physiologist and that was discovered--I didn't know that much Greek--by Horace Davenport who was a very learned guy. He was a student, and so the society was called that.

Terrall: Was it your idea originally?

Borsook: Yes, to have that society was my idea.

Terrall: Was it something you had always been interested in, the history of physiology?

Borsook: Yes. Well also, if you like, it was an extension of the principle of the humanities at Caltech. I was always interested in history and the history of medicine, and there were a number of obvious good reasons for doing it. The students get together on an informal basis at the home of a professor and we get to know each other much better. We usually served coffee or tea or something, and we would spend an evening talking about that. It was a good thing to do and it ran for a number of years.

Terrall: It was just biologists; it wasn't interdisciplinary?

Borsook: It was only just biologists because it didn't spread around. No one was excluded. Anybody could come. Occasionally a member of the Biology staff came, but not often.

Terrall: It was mostly undergraduates then?

Borsook: Yes.

Terrall: Well, how did it happen to die out then?

Borsook: The people who were interested moved on, and then the war came. It was the war that finished it. And after the war it was a new deal. It was really the war or it would have continued on.

Terrall: When Morgan retired in 1941, the Biology Division was run by the Biology Council?

Borsook: Yes. There was a Council consisting of Sturtevant, Went and myself.

Terrall: Now was this meant to be a temporary . . . ?

Borsook: Oh, yes, until we got a successor to Morgan.

Terrall: So were you actively looking for a successor?

Borsook: Yes.

Terrall: Was there any difficulty in finding a new chairman?

Borsook: Some, finding someone whom they felt was good enough. It was Sturtevant who was mainly responsible for bringing [George] Beadle. I don't think Morgan was very keen about it. I think partly because he didn't understand what Beadle had been doing and intended to do.

Terrall: Did Morgan have anything to say about what was going on then?

Borsook: No. He deliberately kept out of it. But I could talk to him about it because we could talk about lots of things. He was scrupulously careful not to interfere in any way and if sometimes somebody who was not on the Council would go over and complain to him he would tell them, "It's your own fault. You go ahead and you work it out yourself. I'm out of this now."

Terrall: Were the other biologists doing war-related work during the war?

Borsook: Not that I know of. I think I was the only one.

Terrall: Other people were continuing with their . . .

Borsook: Yes, with their regular work.

Terrall: And teaching? Were you still teaching during the war?

Borsook: Teaching stopped. There were no students.

Terrall: There were the Navy students.

Borsook: We didn't teach them.

Terrall: They didn't take biology?

Borsook: No.

Terrall: So besides yourself, people were just working on their own?

Borsook: Yes.

Terrall: I guess it was also around that time that you wrote the book that you mentioned before, the vitamins book. [Vitamins: What They Are and How They Can Benefit You]

Borsook: Yes.

Terrall: Tell me how you happened to write that.

Borsook: As I say, for ten years I'd been lecturing on vitamins to medical audiences, nurses, and I had written it up thinking that it would

be a good thing to get it published. It was turned down by one publisher. At one lecture, by chance Upton Sinclair was there, and he listened, and afterwards he came up and he said, "Have you published this?" I said, "No. I've tried and failed." He said, "Would you mind, if you've got a manuscript, if I sent it to my publisher?" "Well," I said, "I'd be delighted." So we sent it to Viking Press with his recommendation and they accepted it. That was very lucky because it was a kind of best seller. We sold about 40,000 copies. For those days for that kind of a book that was really remarkable. And it was very lucky in another respect because the royalties enabled me to send my daughter to college which might have been difficult otherwise.

Terrall: So that was a direct outgrowth of your lectures?

Borsook: Yes, and it was the first non-technical book on the subject written for the general public.

Terrall: Did you enjoy doing that sort of thing?

Borsook: Well at that time I did, yes. Since then I've been asked to bring it up to date, and I don't feel like it. But it's still selling as a paperback. Just a month ago I got a \$144 royalty check for that. I was surprised.

Terrall: You've never revised it?

Borsook: I've never revised it and the reason I haven't--oh, it needs to be brought up to date in a number of respects, but there's nothing wrong in it even now. Whatever is said is true, but I could say much more about vitamin B₆, about vitamin B₁₂, about folic acid, than I did then, but it would have to be a book twice as thick. Since then there has been so much writing about nutrition I don't feel the same need any more.

Terrall: So you really felt that you were filling a void?

Borsook: Oh, yes, there was clearly a need then, especially when Upton Sinclair said this to me. And I was very glad, because as a result of that we used to visit back and forth on other things and he used to send me copies of all his new books, so it was very pleasant. And as I told you, when his publisher came to Pasadena, primarily to see him, he also came to me and we had dinner together.

Terrall: Now, after the war, when Millikan really retired and DuBridge took over, can you point out any changes? You were talking before about the loose way the Institute was governed by the Executive Council.

Borsook: No, that didn't change much. It changed very little. It had to change some because of our funding. Bookkeeping had to be much better, much tighter. After the war I got first an Office of Naval Research grant, and then an NIH grant, and so did everybody else. And there was Institute overhead and tight bookkeeping. So that inevitably changed things. But as far as the actual administration of the Institute we didn't feel it very much. I served a term on the Faculty Curriculum Board and DuBridge would come, but he was just one other member. He was very good; we all liked him.

Beadle introduced more administration in the Biology Division than we had when Morgan was running it. Beadle wanted to be the boss whereas Morgan didn't. It wasn't too bad, but once I had to tell Beadle I was going to do what I liked and that was that. And he took it. He liked to do that--well, I suppose that was in his nature. But it wasn't really serious, and all my time at Caltech I felt the administration was there to help and it wasn't something by itself. I learned how different it can be when I went up to Berkeley because there you almost have the feeling that the University is run for the benefit of the administration. The rest of us just worked for the administration. Caltech was the reverse.

Terrall: Did that continue to be that way even after things expanded? I mean after the war.

Borsook: Yes, it stayed that way as long as I was there, I should say. As I say, one could explain away what changes there were because of the way we were funded and we had to have much tighter bookkeeping, and also much closer supervision on what you spent your money on. It had to be justifiable in terms of your contract. But it wasn't serious interference.

Terrall: Did you have more than one grant going at once?

Borsook: Yes, I had two grants going at one time. I had an NIH grant, the National Institutes of Health, and I had an Atomic Energy Commission grant all the time I was there.

Terrall: Did you have more people working under you?

Borsook: Yes. Research was more expensive. Equipment was more expensive. Isotopes were expensive. I had more people working with me. I had four people working with me because compounds had to be synthesized instead of bought and so on. And so we needed more money.

Terrall: Did Beadle have ideas about how things should be changed in the Biology Division?

Borsook: No, he didn't interfere in that respect.

Terrall: Let me ask you about women in the Biology Department, because I know from looking at old Bulletins that there were always a number of assistants, sometimes research associates even.

Borsook: Technical assistants. One research associate.

Terrall: Did anyone ever apply for a faculty position, a woman?

Borsook: Not so far as I know.

Terrall: Any graduate students? That was much later.

Borsook: No, not even graduate students.

Terrall: Was this an explicit policy?

Borsook: I think it was, and I don't know whether it was a good reason or an excuse. As far as undergraduates were concerned it was: "We don't have any housing facilities for women. There are only dorms for men." But in fact it didn't come up until the development of the women's movement, and then it came up.

Terrall: So you didn't have any cases of people coming to you and wanting to get into your graduate program?

Borsook: No. I don't think any of them did.

Terrall: What happened to the women who were assistants? Did they work indefinitely?

Borsook: They stayed as assistants. I had two women assistants for quite a long time. One of them left when I left Caltech*, the other went up to Berkeley and set up the lab for me and then she went to Texas and got a Ph.D.. She is Dr. Dian Tergler. She is now working as research director for a pharmaceutical house in South Pasadena. She's a fine person. The other girl married, and when I left she left too. Before that I had another girl who was working for me who had her Ph.D. as a matter of fact, but she was only a technical assistant.

Terrall: Did she want to go on and be a faculty member?

Borsook: Not as far as I know. Her sister--Deasy was her name--her sister was a writer, a novelist; they lived together. Neither one of them married and then they both decided for reasons of their own to move to Cincinnati, so she left with her sister. But the question of her becoming

* retired June 1968

a faculty member never came up.

Terrall: The reason I asked is that Biology is one field where women were in such jobs.

Borsook: Yes, there were even women professors at various places, but not at Caltech though we did have some visiting fellows who were women-- Barbara McClintock, she came--but they were postdocs.

Terrall: Let's talk about the isotope work a little bit. You've referred to it a few times. This was a direct outgrowth of your earlier protein synthesis work?

Borsook: Well, it was an outgrowth of my interest in the problem of the synthesis of protein and this was the way to do it. And so as soon as the war was over, and I was back in the lab, you could buy isotopes from the Atomic Energy Commission. They were made at Oak Ridge and they got a private company to handle the business for them. But it was at cost. And we bought the isotopes from them and then--for instance, carbon-14 was as carbonate. We had to convert that into an amino acid before we could use it. I had Peter Lowy, who was an organic chemist, to do that part of the work. And then Geoff Keighley, who was an old friend from Toronto--I had him come out, as a matter of fact years before--he was very good at mechanical things. So before we could buy Geiger counters and such like, he built them and we could use them. Later we could buy them and so on. The same with other things.

Terrall: Were you one of the first groups to start using isotopes?

Borsook: No, I was not. The first groups to use isotopes in this general area was at Columbia, using nitrogen-15, thanks to Harold Urey who had made heavy hydrogen and then heavy nitrogen. And then a man named [Rudolf] Schoenheimer and another man who was a pupil of Urey's, [David] Rittenberg, joined Schoenheimer. They were the first. And then people at Berkeley were doing it on isotopes made with the cyclotron. We

got some sulfur from them, an isotope of sulfur, to study the metabolism of vitamin B₁ using sulfur. But they were doing it on protein synthesis and so on and then the people at Harvard were doing it before I got back into it. Loftfield and others were doing it, Hoagland and so on were in on it. In terms of priority, I would say I was probably fourth or fifth in the time at which I entered this field.

Terrall: But this was an area that was developing specifically because the isotopes now existed?

Borsook: Yes, exactly.

Terrall: And someone realized you could use them in this way?

Borsook: Of course they were used for other purposes, but it was precisely because the isotopes were available that the field was developing.

Terrall: You got the isotopes from the AEC?

Borsook: We bought them from the Atomic Energy Commission through this firm that they licensed. They'd make them and hand them over, and then we would buy them at cost. But they weren't cheap.

Terrall: Was this also vitamin-related work that you were doing?

Borsook: No, not really. The vitamin work was finished then, apart from the Meals for Millions Foundation work which is something else. The outcome of the work on the synthesis of protein had an unexpected development. I was studying the synthesis of hemoglobin and I found that if I added the serum of an anemic animal to the culture medium that I was using, it greatly speeded up hemoglobin synthesis, and this led to the discovery of the hormone erythropoietin which is the hormone which regulates the production of red blood cells. That discovery was a direct outcome of the work with isotopes, studying the synthesis of hemoglobin. And that

of course quickly spread all over the world, because it was easy to make. All you had to do was to make a rabbit very anemic by bleeding him or giving him a poison, and once his hemoglobin was down to about a quarter of the normal figure then his blood contained this hormone, which you could then use crude for whatever experiments. Since then it has been isolated. I had nothing whatever to do with that--purified and so on.

Terrall: Well, you worked with hemoglobin for quite a long time then?

Borsook: Yes, and I have been ever since, as a matter of fact, because it seemed to me the best protein to work with for a number of reasons. There is the obvious intrinsic interest, it's a protein easy to isolate, and it had a clinical interest in anemia and such things, so it was a good thing to work with and we worked out the general method of experimentation, what kind of cells to use, what to do, and that has spread all over since then.

Terrall: The medical applications are one of the attractions.

Borsook: The curious thing is there are no medical applications except by way of interpretation. For the reason that there isn't enough pure erythropoietin in the whole world to treat one patient. It's so hard to come by and it's a protein that hasn't been synthesized. And so its chief value is in understanding certain diseases, and the analysis of the blood for erythropoietin is one of the standard things that clinical labs now do. But to use as a medicine as was at one time hoped is still not possible. How can they get enough erythropoietin? You collect the urine of people who have hookworm, because you have to have an awful lot of urine and that means an awful lot of people, and then you extract it from the urine, purify it and so on. The government still has a contract with the Children's Hospital in Los Angeles for making the concentrates. And then you apply to NIH for a grant of this erythropoietin and they'll give you a few thousand units. But a few thousand units is enough only to treat some mice. That is not enough even to treat rabbits. It would

take too much. And that's the situation at present with erythropoietin. But as I say it is useful in the interpretation of certain diseases involving anemia of one kind or another, and then the blood is analyzed for erythropoietin.

Terrall: Did you then get into other areas of hemoglobin research?

Borsook: Yes. Hemoglobin is in the red cell, but it's not made in the mature red cell. The mature red cell doesn't make any protein. It's made in the bone marrow. The red cell is preceded by six nucleated cells and one cell which is not nucleated, so there are seven precursors, and I was interested, and am still, in that process. In which of these precursor cells is hemoglobin made? It's not made in all of them. It's made in the middle of the series. It's not made before; it's not made at the end of the process. What regulates the rate? There are still a lot of questions about that not answered, that is, the method of regulation of going from one stage to the next. You see, one of the great advantages of studying this process, it's a way of studying embryology. It's very like embryology, only in an embryo you've got a whole embryo; here at every stage in the process you've got a separate cell which you can recognize, that you can do experiments with and so on. So this is why I became interested in that, and still am, as my research project is still going on up at Berkeley. I have assistants working on that up there and this is what they're working on.

Terrall: To get back to the '40s again when you started developing the multipurpose food [MPF].

Borsook: Well, I was going to say that is really the story, in its beginning, of Clifford Clinton. Clifford Clinton was born of missionary parents and when he was with them as a child in China, he had seen famine, and he'd made a boyhood resolve that if he could ever do anything about helping hungry people, he would. So it was not surprising that when he grew up, he went into the restaurant business. Clinton's Cafeteria in Los Angeles is his. He has a couple in Los Angeles and I think one in

San Francisco.* During the depression, '30, '31, '32, he advertised that anyone who would come to his cafeteria between 2:00 and 4:00 could get a free meal. And then he felt it was being taken advantage of, so he charged a nickel for the meal. The last time I ate there, which was more than ten years ago, in that particular one the manu said, "You can order what you like," it was a regular cafeteria, "and you can pay what you like." When the war came with the enormous expansion of the armed forces, from a couple of hundred thousand to twelve million, the Army needed advice on how to feed a lot of people in camps quickly. And so they called in consultants from the restaurant business and Clinton was one of those consultants. We never met during the war but we knew of each other. And then, it was in early 1945, one afternoon--he'd phoned first--he and his wife and his public relations man came to see me. And before he began to talk his wife intervened and said, "I want to apologize because my husband is coming here with a perfectly ridiculous proposal, and I tried to dissuade him from coming but he's very stubborn and so I want you to know that I feel a bit ashamed." So I said, "Well, you needn't be. You're here. Make yourselves comfortable and let's listen."

Begin Tape 2, Side 2

Borsook: So, Clinton began to talk, and he said that anybody who thought about it could see the war was coming to an end, that in the countries where the war had been going on, a lot of people were going to be hungry, there's going to be a shortage of food in Europe and in Asia and he wanted to do something about it. And he said he wanted me to do this. I was to devise a food where a meal would provide one third of the Recommended Daily Allowances of everything, but it was to weigh not more than two ounces and it was to cost not more than three cents, that it would not offend any religious taboo, that there should be an abundant supply of all the ingredients, that it wouldn't draw on the kinds of foods Americans are accustomed to eat. And I said, "You needn't worry,

* Clifford Clinton is not alive now, but the restaurants bearing the name "Clifton" are still going.

at that price." And have an indefinite shelf life, it could be eaten in a variety of ways and wouldn't take any equipment to cook--a can of water and a stick of wood underneath should do it. And so he went on. And I turned to his wife and I said, "Well, it's not so wild as you may think, but there are two restrictions I would put on it. It's not possible, it's scientifically impossible to provide a third of the Recommended Daily Allowance of calories in two ounces of anything. It is just not possible. We need about 700 or 800 calories as a minimum and the most that two ounces would supply would be about 140 or 150 calories." I said, "Secondly, I don't think it would be practical to put vitamin C in because we don't really know how to keep it." At that time we didn't. But I said, "Otherwise, we could provide protein that's as good as meat or milk, we could provide all the vitamins and all the minerals that they need in two ounces and I don't think it need cost more than about three cents a meal." So he agreed and gave Caltech the sum of five thousand dollars which I was to use to develop this food.

The food itself was no problem. I had a pretty good idea how to do it. It had to be a vegetable protein; animal proteins were too dear. The best vegetable protein was soybean protein and I knew that during the war we--the government--had greatly expanded the growth of soybeans for its oil. And after the oil was extracted, what was left was thrown down the sewer, it was just wasted. So I knew this would be very cheap, and I knew it was available, and I knew it was available in large quantities. The vitamins and minerals I knew were very cheap and there's no problem about adding that. What I wanted the money for was to hire a cook who would learn and draw up recipes on how to use this, and how it should be cooked in different ways. And that was done. So at the end of a year it was all ready, and we had a whole lot of recipes and one of the fruit and vegetable dehydrators in Los Angeles undertook to make it according to the recipe that I had drawn up, and that was done. We decided to call the food "Multipurpose Food (MPF)." Neither Clinton nor I wanted to patent this food and we agreed that we would give the information to anybody who asked for it. But that wasn't going to get the food to hungry people. Clinton was a religious man--his parents were missionaries--and he set up, and I was one of the cofounders with two

or three others, of this Meals for Millions Foundation, a nonprofit organization. He hired a woman by the name of Florence Rose and his publicity man, Ernest Chamberlain, was the other, who really were the Foundation. They sent out the appeals for money, they raised the money and bought the food and so on.

Terrall: So they raised money from other sources. It wasn't just his money?

Borsook: No, it wasn't his money. No, I don't think he put in much money after that but it was certainly his idea, both to get such a food and then to set up the Foundation to give the food to people who needed it. Both those ideas were really his. And I know when we had the food ready we thought it would be useful if the Food and Nutrition Board would approve of it. We sent it to them and they said, "No. We believe that a good diet consists of meat, milk, fresh fruit, fresh vegetables and this is not that." So for more than ten years, we had no government support, in fact government opposition, because when we [the U.S. government] were giving food away we wanted to give food away, not something like this. We [the U.S. government] even went to the absurd lengths of trying to set up a dairy industry in a poor Asian country. Well, they don't have the technology for pasteurization, for keeping milk and all of that, and they don't have enough cows anyhow. But they opposed our idea just the same. So the money was raised from private groups. But two large charitable organizations very soon began to give us large sums of money, fifty thousand, a hundred thousand dollars at a time, and we would send the food where they told us to. They were Catholic World Relief and Church World Service.

Terrall: So the other organizations made the arrangements with the countries where the food was going?

Borsook: Yes. Since we had the food, they arranged for the shipping but we were to attend to its being shipped to the places where they said they wanted it to go. And it went on that way for a long time.

We went along that way for a number of years. There was one very gratifying experience. At one time my wife, my daughter and I were traveling in Germany, and were in a restaurant, you know how people talk to each other, and a man asked, "Where do you come from?" "We come from California." And he said, "Oh, in 1946, '47 I was an orphan in an orphanage in Germany and we were fed this Wonder Food from California." And it was our Multipurpose Food. So it was very gratifying. And there were other incidents like that.

From the very beginning I had insisted, and in our publicity made it clear, that the formula I had put together was only one example, and I was confident that in many backward countries where there were many malnourished people, that a food as nutritious could be put together from what they had, if they would only use what they had.

Terrall: If they didn't have soybeans, for example.

Borsook: Yes, if they didn't have soybeans. And in south Asiatic countries, India, they don't have soybeans. The director [of the Foundation], the actual director was Florence Rose. We sent her around the world. She had no trouble in getting the Japanese to make a Japanese equivalent. They did a very good job. And then in India, she got the head of their Central Food Technological Institute to put together an Indian version of Multipurpose Food, as it was called, and he did a first-class job. She was a very good director. She would make commitments for money and then come back and ask for permission afterwards, but that was all right since she did most of the work in raising the money. I think we gave this man about five thousand dollars, and he put together, out of peanut meal and chick pea meal, adding vitamins and minerals, a food that was every bit as nutritious as ours. He did a very good job of testing first on mice and rats, then on children who were malnourished, and then he persuaded an Indian businessman who had some money to put up a pilot plant near Madras, and the city of Madras undertook to buy the whole production for use in their school lunch program. They got going, and the Minister for Agriculture for India from New Delhi told people from the provinces to come down to Madras, look, and copy it. Then along

came some representative from CARE and said to the City Council of Madras, "Look, we'll give you all the dry skim milk you want for nothing. Why do you want to spend your money on this food?" That killed the project, and it killed the project for quite a long time, but it has started up again. Two missionaries in northern India, where they have soybeans, have arranged for the manufacture and they're beginning to get it used and widely distributed.

Terrall: Now?

Borsook: Yes. They did this entirely on their own, without any connection with us at all. But now, to come back a little, it was clear that charity would only go so far, and it would be much better if these people learned how to help themselves. So we set up a school at the Meals for Millions Foundation, it's in Santa Monica, where we teach three classes a year. It runs for twelve weeks with about a dozen students from different parts of the world, on scholarships of one kind or another, and they learn how to make food like MPF from what they have. We do more than that. I say "we"--I have really nothing to do with it. The idea of the setting up of the school and so on was all the work of other people. We devise machinery which they can use in their villages, and some of it they could even make themselves, so that they really can help themselves.

Terrall: Where do the vitamins come from?

Borsook: They have to be added.

Terrall: Are they imported?

Borsook: Yes, they would have to be, but that's no problem. In the first place they don't cost too much, and secondly, out of all the AID money the amount for vitamins is negligible and so there's no problem about that.

Terrall: When this program was being set up, how involved were you in the

get a small businessman to set up a multipurpose food plant, you know, and hand the food out. But I suspect these people are so poor that it would have to be subsidized.

Terrall: Even at three cents a meal?

Borsook: Oh, that's much too much. When you think of a man, his wife, with five children on an annual income of \$100 a year. They can't afford to spend twenty-one cents for each meal daily. So it's got to be much cheaper than that. This is where we are now. To overcome malnutrition is no technical problem. We know how to do that, and the means are available. But these people can't afford it.

Terrall: Over the years did you ever get any more favorable response from our country?

Borsook: They never admitted it, but they now agree that the thing to do is to do what we have been doing. We are now beginning to get government money.

Terrall: Do you try to get foreign governments to support the program?

Borsook: What they are interested in first is the transfer of a low level technology, which is what ours is, and also in nutrition education to go with the low level technology, and we're getting money for that purpose now from the government. And they now agree that the thing to do is to use by-products, industrial by-products like soy grits which is made for the oil, or peanut meal in India, where peanuts are used for the oil and they were throwing the meal away, and Subramanjan took it up for that purpose. They also agree now, which they didn't before, that the thing to do is to fortify it with vitamins and minerals. It took fifteen years [to get them to agree].

Terrall: Has MPF ever been used in this country?

organizational part of it in terms of contacting foreign governments and getting programs started? Were you involved in that?

Borsook: No, I had nothing to do with that. The missionaries did that for us. Church World Service and Catholic World Relief.

Terrall: They were in it from the beginning?

Borsook: Very early, very early. At first, Clinton with his church connections had connections with missionaries, and so we would send it out to them. I had nothing whatever to do with that, though later on I was in correspondence with Subramanjan in India who did the job I mentioned in India, and with the Japanese people. But that was technical really, not much more than that, and also kept some people from making mistakes. You see, the soybean people had tried something like this, but they made the mistake of trying to make soy flour compete with wheat flour. Well, it doesn't make bread. It's not like wheat. And the best way, and I persuaded them, was not to make flour, but grits, small particles so we could use it as soup, as a meat extender, in lots of ways. And that is the way to use it. As a matter of fact there's a Texas hamburger chain that uses it in their hamburgers as a meat extender, and it's quite a good meat extender, has the right flavor and so on for that purpose. The Meals for Millions Foundation was founded in '46 and miraculously is still going, stronger than ever really.

Terrall: Is Clinton still around?

Borsook: No, he's dead. The family has nothing to do with it anymore. I think I'm probably the only one of the original group who's still alive. I'm a member of the board still and on one of their committees. And now--and I must say I don't know the answer to this, maybe there isn't one--the most important cause, serious cause of malnutrition in the Third World is poverty. We can overcome the ignorance if we can overcome the poverty. And you see, now maybe we can overcome the effect of the poverty to some extent, if we could persuade the governments to

Borsook: It was used a little, not much now. In some places after the war when the GIs came back, as you know, many of them were married, there was a baby and the amount of money they got wasn't much. So we set up a store near Columbia University in New York and sold quite a lot, and we kept it going until that generation of GIs had passed through. It's still being used, for instance, on Indian reservations near Tucson. There's a remarkable woman there, Mary Diamond, who's working with those Indians, and she is using it there. There are a few other places like that, but for the most part there's no need for it in the United States, not even in prisons or places like that. As you know, we're too rich and why should we do it? If we are ever hard up, for any reason, then we will have to revert to something like MPF.

Terrall: I'd like to talk to you a little about the fifties as our next time period. What was the effect of the McCarthy hearings on the Institute?

Borsook: They had some, not a great deal. For instance, I was one of those whose grant was temporarily suspended, then it was reinstated, for reasons that Dr. Bacher told me. He didn't fight them on the principle, but he had the business administration tell the government, "This is a breach of contract," and that went down and so my grant, with some others, was reinstated. Now why was my grant suspended? I learned that afterwards from some people I met during the war in Washington. I was down in the books as a "Premature Anti-Fascist," and that was because in the middle thirties, late thirties, during the Spanish Civil War, I had been active in raising money and moral support for the Spanish Loyalists. So the FBI, I suppose, had me down as a "Premature Anti-Fascist." Therefore I was a suspicious character.

This had another effect. During the war a procurement officer for the Army came out to see me one day, and showed me a telegram from Washington, to ask me to be the nutrition officer for an Army group, so I said, "Well, sure, we're at war, and if they go to this trouble I will do it." As my wife knows, I was making all arrangements to wind up my affairs, and then came another telegram thanking me for my offer to

enlist but they felt I was needed more in civilian life. Well, I'm sure that was related to my being on the blacklist.

Then, I was offered the directorship of the Naval Medical Research Institute in Washington. I was offered it, and then it was withdrawn for security reasons.

Terrall: And it was all based on this one entry in your record?

Borsook: Yes. Then there were others, but I must say the trustees [of Caltech] and the administration, the president, were very staunch about this. We never had any trouble like the loyalty oath at Berkeley. The trustees wouldn't even think of it. It's true that those who were discovered to be active members of the Communist Party were let out. One of them was my assistant, Dubnoff, and another one was Sidney Weinbaum, and not even the Institute with its government grants and so forth could afford to oppose such pressure.

Terrall: You're saying that the pressure came from outside the Institute?

Borsook: Oh, yes. There was none within the Institute whatever, none whatever. It was all the other way.

Terrall: In a specific case, like your assistant, how would that come about?

Borsook: Somebody found out, and I can't tell who, that this man was an active member of the Communist Party, and I presume the FBI found it out and that's how it came out.

Terrall: Then the FBI would have informed Caltech?

Borsook: The Institute administration. But you see [Linus] Pauling at one time for a short time had a passport refused because it seemed that he was favorable to the Russians. The trustees stood behind Pauling, everybody stood behind Pauling. There was no pressure within the Institute.

Terrall: What about a more general feeling, like in the case of Pauling? It must have been discussed.

Borsook: It was discussed and we felt this was wrong, that nothing that Pauling said warranted such treatment. We had no reason whatever to suspect that he was anything other than a good American and loyal, and so on, and this was just part of the McCarthy hysteria.

Terrall: There wasn't a feeling that what he was doing was inappropriate?

Borsook: No, no. That never came up at all. As far as that whole period, the record of the Institute is a very good one, an excellent one, and the support went all the way through from top to bottom.

Terrall: In terms of that incident with your grant, when it came to be the next grant period, did you . . .

Borsook: It was renewed. They didn't say anything. It was renewed and that was that. I did nothing unusual. I filled out the usual papers and so on, and the grant came through just as usual.

Terrall: But when you were told that the grant was revoked they didn't give you any reason?

Borsook: No. They wouldn't give any reason at all. But it was hinted somehow, not to me, that it was for security reasons. This woman, Oveta Culp Hobby, was at that time head of the Health, Education and Welfare, HEW, she was the Secretary, and she caved in under the McCarthy pressure.

Terrall: And so she was making a policy of checking into . . .

Borsook: Yes. She had people doing it, and then they had to back down when this argument of Bacher's was put to them and then after that there was no more trouble, at least I didn't experience any more trouble. A number of people outside came to my defense. I didn't ask them to, but

it got noised around, like [John] Edsall at Harvard. So for about three or four months my grant was suspended and a good friend, Gloria Gartz, a woman of some means, put up the money so I could go on working, and gave that money to Caltech and then my grant was renewed.

Terrall: So you didn't have to fire anybody?

Borsook: No, as a matter of fact even if she hadn't come through I think the Institute would have found the money so I wouldn't have had to fire anybody. I don't think anybody had to fire anybody for that reason at Caltech.

Terrall: But you know of other people in other divisions who had similar problems?

Borsook: I was told there were. Bacher told me that but I don't know who they were.

Terrall: Well, it must have been kind of shocking.

Borsook: It was. It was shocking; it was rather sickening to think that our government would do anything like that. But as I saw, after that I had the experience with the Naval Medical Research Institute directorship.

Terrall: That was after this?

Borsook: I'm not sure now whether it was after or before. It may have been after. It was very near; it was in that period that the offer was withdrawn with no reasons given, but I heard from a friend of mine in the Navy who really was responsible for the offer being made to me, that it was for security reasons.

Terrall: Was that something that you wanted to do?

Borsook: I wasn't sure, but I really wanted a chance to consider it. It

had its pros and cons. I didn't look forward to living in Washington, but there were opportunities there that were attractive.

Terrall: There was something at Caltech called the Committee on Academic Freedom and Tenure.

Borsook: Yes, there still is, I think.

Terrall: Were you on that?

Borsook: No, I was never on it.

Terrall: Did they get involved when your grant was revoked?

Borsook: Well, I can't answer that because this business of how my grant and other grants were reinstated, how that was done I have only heard recently from Bacher himself.

Terrall: He didn't tell you at the time?

Borsook: No, he didn't tell me at the time, so I can't say. There's no use in guessing. You can find out from other people who were on that committee, but I don't know.

Terrall: And then in the late '50's you got into work on heart disease.

Borsook: Yes, and that was a failure. We had found out how creatine is made, and since phosphocreatine is the immediate source of energy for muscular contraction I had the idea, I thought it would work fine, that if we give a person large quantities of two ingredients from which creatine is made, they would be able to make more, even a diseased muscle, heart muscle, could make more and that would be helpful. And again I worked with a number of doctors. I say it was a failure because we couldn't prove anything. One of the cardiologists was convinced that it was really helpful, but the results weren't encouraging enough to

warrant pushing it. And also there were difficulties with the suppliers of these materials, because the patient was given this for nothing. They were out to make money too quickly and I disagreed with that. As far as my endeavor was concerned it was a failure, but whether the idea really has no merit at all I'm not yet ready to say.

Terrall: I think I saw a press release or something that this could be a new treatment for heart disease.

Borsook: Certainly nothing to that effect from me.

Terrall: No, but something was picked up by the news office.

Borsook: Yes, and I disapproved of that.

Terrall: Who were you working with on this?

Borsook: The man I worked with was my brother, who was a surgeon really, and a cardiologist by the name of Dr. Van Zant. I didn't have much to do with my brother. We disagreed and he was one of the reasons I got out of it. The man I worked with well, we had the same outlook on the problem, was Dr. Van Zant. He's a cardiologist in Los Angeles, and it was he who felt there was really something in this. He was sorry when we had to stop.

Terrall: Where were the patients?

Borsook: In his office downtown, other people's offices, and so on.

Terrall: So that wasn't a question of patients coming to Caltech?

Borsook: No, no, not at all.

Terrall: What about other medical research at Caltech?

Borsook: I had nothing whatever to do with it. I don't know. Pauling may have had some going on but I had certainly nothing whatever to do with it. After all there was a lot of medical research at Caltech in the high volts lab, where they were treating people with cancer. And this was one of the disappointments that Charlie Lauritsen had, that Morgan wouldn't lend the Biology Division to collaborate with him on the treatment of cancer. I think Morgan was right. It was really out of our line. So Charlie, I suppose with Millikan's help, persuaded Seeley Mudd to put up the money that was needed.

Terrall: That went on for quite a while didn't it?

Borsook: Yes, for quite a long time.

Terrall: But even the work that you were dealing with was funded by the more medically oriented government agencies.

Borsook: Yes, but the only clinical work was what I've told you, because I was pretty busy and it was a matter of choice what I would do, and I thought I had done enough if I, as it were, implanted an idea. Then it's up to the medical people to make what they could of it, or as much as they wanted to.

Terrall: I guess that decision was made about not having a medical school at Caltech.

Borsook: Yes, way back, way before my time actually.

Terrall: In the 1960's, did your work continue along the lines of hemoglobin?

Borsook: Yes, along the lines of the hemoglobin, and along the lines of the regulation of the process in the bone marrow. We had found, for instance, under certain pathological conditions abnormally large cells were produced. How were they produced? At what stage in the process?

This is the kind of thing. It shifted from the mechanism of protein synthesis to the mechanism of the regulation of these various stages of the process by which the red cells of the bone marrow and final red cells are produced. That was the shift that occurred in the sixties.

Terrall: Was this work funded by the Public Health Service?

Borsook: Yes, and the Atomic Energy Commission. All the way through those two grants went on.

Terrall: And they were both for the hemoglobin work?

Borsook: Yes.

Terrall: That was really a long period of funding.

Borsook: Yes, it was a very long period from right after the war until I left Caltech. As a matter of fact, both those grants, I took with me when I went up to Berkeley.

Terrall: When you retired and went to Berkeley, how did that come about?

Borsook: I had reached the retirement age at Caltech and the rule was, and I still think it's a good rule, that when you reached that age you no longer can work in the lab. Caltech is a small place. Well, I didn't feel like it, and my friends at Berkeley put up a lab for me and equipped it and so on and then I just simply transferred my grants, took them with me up there and that went on for about ten years and it's coming to an end now. The end of April, the end of this month my grants will come to an end.

Terrall: You just felt you weren't ready to retire?

Borsook: I didn't feel like it anyhow. Maybe I should have, but I didn't feel like it and so I carried on up there.

Terrall: Were you teaching there too?

Borsook: Yes, I did a little. I taught one special course, but I needn't have done it. It was my choice. One class a week for one term.

Terrall: You mentioned a bit about the comparison between Berkeley and Caltech.

Borsook: They are very different places. The administration, as I said, figures very much larger at Berkeley. Furthermore, the impression I have still is that Berkeley is not a corporate entity. Caltech is a community. Berkeley is not. Berkeley is a number of individual departments, small groups, and their vision doesn't really extend beyond that group. The hand of the administration is very heavy. This is not to say that Berkeley isn't a great university, as judged by the number of really first-rate scholars that they have there, but Berkeley isn't a place that you can feel you belong to or are loyal to. Caltech is. Or Caltech was, and I expect still is. After all, there are altogether too many people at Berkeley to feel that you know everybody, whereas at Caltech you knew everybody.

Terrall: I think I am at the end of my list, but maybe you'd like to look over the notes that you made and see if we missed anything.

Borsook: No, I've looked through my notes and I think I have everything.