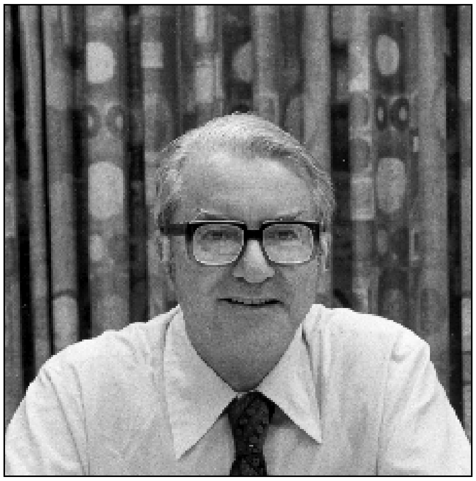

NORMAN H. HOROWITZ
(1915–2005)

INTERVIEWED BY
RACHEL PRUD'HOMME

July 9-10, 1984

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
Biology

Abstract
Interview, 1984, with Norman Horowitz, professor of biology emeritus and former chairman of the Biology Division (1977-1980), who arrived at Caltech as a graduate student in 1936. Recollections of Thomas Hunt Morgan; embryologist Albert Tyler, with whom he did his PhD; Caltech's marine biological station at Corona del Mar. Comments on Biology Division in the late 1930s: Calvin Bridges on *Drosophila* salivary chromosomes; Frits Went and James Bonner in plant physiology; Henry Borsook on thermodynamics of biological compounds. Importance of genetics at Caltech. NRC fellowship, 1939, at Stanford and meeting George W. Beadle; recollections of Beadle, and Beadle's 1941 talk at Caltech on his and Edward Tatum's work on *Neurospora*. Horowitz returns to Stanford as postdoc in Beadle and Tatum's lab, compiling evidence for the "one gene, one enzyme" theory. Returns to Caltech in 1946 as senior research fellow with Beadle, who came as division chairman. Instrumental in getting Max Delbrück back to Caltech from Vanderbilt University. Lee DuBridge arrives as Caltech's president in 1946. 1954 work with Boris Ephrussi on *Drosophila* tyrosinase in Paris. Becomes chief of bioscience section, Jet Propulsion Laboratory, 1965. Comments on history of Mars observations and ideas about microbial life on Mars at time of first Viking (Mars) launch, 1975. Designs Viking instruments with George Hobby and Jerry Hubbard. Comments on Roy

Cameron's search for bacteria in dry valleys of Antarctica and on spacecraft sterilization. Later work with *Neurospora, Aspergillus,* and *Penicillium* on water and iron requirements. Comments on Robert Sinsheimer, his predecessor as Biology Division chairman, and on presidencies of DuBridge, Harold Brown, and Marvin L. Goldberger. Comments on current trends in Biology Division, and on the book he is writing about the search for life on Mars, and his conviction that Earth is the only place in the solar system that supports life.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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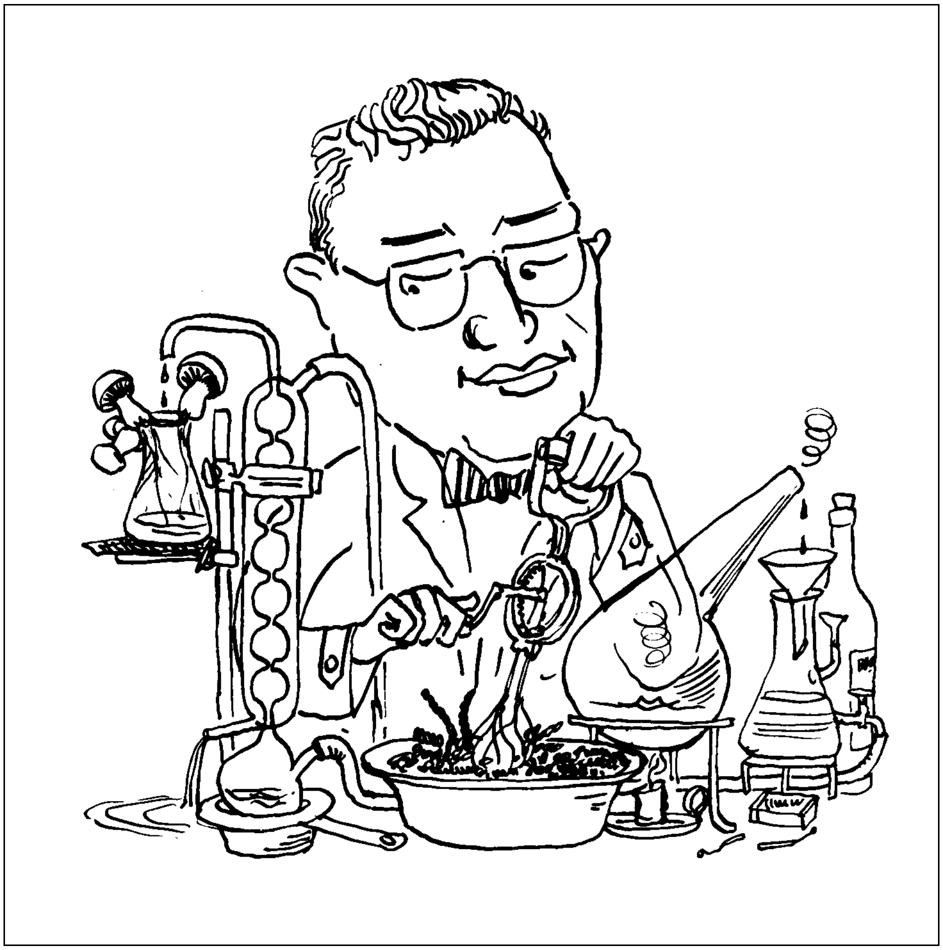

Caricature of Norman Horowitz by Hans Gloor, 1950s.

California Institute of Technology
Oral History Project

Interview with Norman H. Horowitz

by Rachel Prud'homme

Pasadena, California

Caltech Archives, 1987

Errata

p. 14: "…a man named Manglesdorf."—Correct name is [Paul] Mangelsdorf.

p. 15: "He later left Stanford and went to the Rockefeller Institute, where he became assistant director…. That probably happened in the late forties."—Douglas Whitaker left Stanford in 1955 to become vice-president of Rockefeller University and continued in that capacity until his retirement in 1964.

p. 33: "the principle gas in the atmosphere"—Should read "the principal gas in the atmosphere."

p. 35: "…during the International Geophysical Year, around '58." [Prud'homme] "'57 I think it was."—Date of IGY was July 1957-December 1958.

p. 44: "Kenneth Pagen—he's a professor of biochemistry…"—Correct name is Kenneth Paigen.

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Robert Sinsheimer as chairman of division and as president of UC Santa Cruz; Lee DuBridge a beloved and outstanding president of Caltech; Harold Brown's Vietnam connection as a problem and its resolution; controversy over Brown's plan to open Caltech to women through purchase of Immaculate Heart College whose religious orientation is deplored as unsuitable for Caltech; Brown as technician; Goldberger as personable, affable as well as political.

Expansion in neuroscience; primacy of molecular genetics; movement to expand into medical school seen as antithetical to reputation for basic science and as devouring funds; feasibility studies and oppostion of faculty prevented establishment of medical school; complexity of decisions re psychology and psychobiology departments, humanities; sees future absorption of psychobiology into molecular biology; great goal for future of division is building bridge between molecular and chemical biology and psychobiology.

Completing book on search for life in solar system; recipient of many honors and awards including NASA Public Service Medal; member National Academy of Sciences and American Academy of Arts and Sciences; enjoyment of National Academy meetings; most proud of Neurospora work that formed basis of one gene, one enyzme theory; credits Beadle rather than himself as author of theory although writings seem to indicate differently; regards role in exploration of Mars as important; book in progress points out in layman's language the cosmic importance of Earth as only planet in solar system that can sustain life and that life is all related; also writing book to combat irrationalism rampant in world today from Mars myth to creationism; book as reflection of strong feelings.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Norman H. Horowitz by Rachel Prud'homme

Pasadena, California

Session 1	July 9, 1984
Session 2	July 10, 1984

Begin Tape 1, Side 1

Prud'homme: Where do you come from originally?

Horowitz: I come from Pittsburgh, Pennsylvania. The joke used to be "I've come clean from Pittsburgh"; but it's not a joke anymore, because Pittsburgh has been cleaned up.

Prud'homme: What was your family like? Were they interested in science? Were there any other scientists in the family?

Horowitz: My parents weren't interested in science. But my only living brother is a chemist; he works for the United States Department of Agriculture in their laboratory here in Pasadena--on Chester Street. He's an organic chemist. And another brother, who is now dead, was a petroleum engineer.

Prud'homme: So they spawned a group of scientists, then.

Horowitz: Yes.

Prud'homme: Where did you get your early education? Did you go to local schools in Pittsburgh?

Horowitz: I went to local schools--public elementary school and public high school. And I won a scholarship to the University of Pittsburgh.

This was during the Depression, and I was very glad to have that. I graduated in 1936.

Prud'homme: Who did you study under at Pittsburgh?

Horowitz: Well, my major was in zoology, and, of course, I was an undergraduate.

Prud'homme: Did you have any special mentors or professors there that you remember?

Horowitz: There was a professor of biology for whom I did some research. His name was H. H. Collins. I remember him because he provided me with lab facilities. I actually published a paper or two from that work, which turned out to be important for me, for my career.

Prud'homme: How so?

Horowitz: Well, the fact that I had published a paper when I was still an undergraduate turned out to be a recommendation for graduate school when I applied to Caltech.

Prud'homme: Did you apply any place else besides Caltech?

Horowitz: I applied to several other places--I can't remember where--but I wanted to come to Caltech.

Prud'homme: What made Caltech so special?

Horowitz: Well, it was so well known. One of the professors I had urged me to try to get into Caltech. His name was J. M. McKinley and he taught genetics. He had a strong influence on me. Neither of these people--neither Collins or McKinley--published much research; they were teachers. I also had a professor in biochemistry at Pitt named C. G. King, who was quite well known--still is; he's still alive. He isolated vitamin C, ascorbic acid, and his research was quite important. I got to know him, but he was in a different department.

Prud'homme: The work at Caltech was primarily genetics at that point, was it not?

Horowitz: Well, that's what it was most famous for. But there was also embryology going on here and neuroscience. And plant physiology was very important.

Prud'homme: You knew specifically that you were interested in that area then?

Horowitz: No, I didn't. I assumed that I would get into genetics. But I didn't specify ahead of time what I wanted to do. I just applied. I wanted to come out and look around before I made up my mind.

Prud'homme: When you came out and looked around, what did you think?

Horowitz: Well, I didn't have a chance to look around. I walked into [Thomas Hunt] Morgan's office, which was on the second floor of Kerckhoff in the old days. I went into his office to tell him I was here, and he welcomed me and he said, "Horowitz, you're going to work for Albert Tyler." He told me what I was going to do; and, of course, I never would have dreamed of contradicting him. And I did work for Tyler. Tyler was doing developmental biology, or embryology as it was called then.

Prud'homme: You'd done some transplantation work at Pitt, hadn't you?

Horowitz: Yes. That probably gave Morgan the idea I ought to be in embryology. So he told me where Tyler's office was, and I went there; and I did work with Albert for three years, got my Ph.D. with him.

Prud'homme: Can you tell me something about Albert Tyler?

Horowitz: I guess he was the youngest member of the faculty. He was very ambitious, worked very hard. He was interested in the development of marine animals; he worked on sea urchins and a worm we have on the Pacific coast called Urechis. He published a tremendous amount, and my name was on many of those papers during the years I was working with him. It was very lucky in a way, because Morgan by then had left Drosophila genetics and was working also with marine organisms. That's what he had done before he got into genetics, before he took up Drosophila. So, when I became Albert's student, I got to know Morgan quite well, because we would all go down to the marine laboratory at Corona-del-Mar on the weekends. Tyler had a Model A Ford, and he and Morgan and I would go down to the marine station every Saturday morning, sometimes Friday afternoon, and come home Sunday night.

Prud'homme: *What was Morgan like?*

Horowitz: Well, he was in his late years then. I think he died in 1945, and I got to know him between 1936 and 1939. He was a distinguished old man. Somehow, I had imagined before I met him that he would be the sort of person who makes wise comments, someone with so much life experience that everyone hung on every word he said as he distilled his great wisdom. But he wasn't like that at all. He didn't philosophize much at all. But his attitudes were quite interesting to me and quite pleasing. He was a very sardonic type. And he didn't accept anything at face value. He was quite sarcastic about religion, which pleased me a great deal. He didn't like religion, and neither did I. His attitudes I enjoyed very much.

Prud'homme: That surprises me, and somewhat delights me, because my impression of Caltech was of a rather parochial institution in those days.

Horowitz: Well, it may have been in other departments, but it wasn't here.

Prud'homme: You said that he had left mainline genetics by that time. Why did he leave genetics, do you know?

Horowitz: Well, I think genetics got to be too much for him.

Prud'homme: Too complicated?

Horowitz: Yes. Morgan was a discoverer; he was a romantic figure. And genetics got to be too detailed; he wasn't a bookkeeper at all. At the marine station, he used to amuse me. He was doing work on an organism called Ciona, the sea squirt. It's a primitive vertebrate that is hermaphroditic--that is, each individual makes both sperm and eggs--but it's self-sterile. You can cross individuals taken at random from the population; but if you try to fertilize the eggs of an individual with its own sperm, it doesn't work. And he was interested in that problem. He thought it had a genetic basis, which was certainly correct. He would set up these large experiments at the marine station. We would always stop at the Newport Yacht Club on the way to Corona-del-Mar and pull Ciona off the pilings where they grow. And Morgan would set up these big arrays, big matrices, with sperms and eggs from different animals, and he would make all the crosses. He would have two or three of these, maybe a hundred dishes each, and each one set up on the lab bench. But he never had a notebook; he used to scribble everything on little bits of paper that he found lying around or pulled out of his pocket, so it was a totally chaotic system. One of Tyler's main functions was to keep track of what Morgan was doing and remember it. [Laughter] So besides doing his own work, Tyler would keep an eye on Morgan.

Prud'homme: That sounds as though it was terrific for you. You had Tyler and then Morgan, and you had this very special involvement with the two of them.

Horowitz: Yes, that was very nice. And it was nice in other ways, too. The director of the marine station at that time was named George MacGinitie, he's been emeritus for a long time, retired and living up in

Washington somewhere. He was in Alaska for a long time; but I think he's on San Juan Island now, off the state of Washington. He and his wife Nettie always prepared a big lunch for either Saturday or Sunday, I can't remember which. They would have all the marine delicacies you could imagine--lobsters and crabs and fish of all sorts. It was quite nice for a graduate student; our stipends then were $30 a month, plus tuition--even in the depression, I had to get money from home to live. If my mother hadn't sent me some money, I couldn't have made it.

Prud'homme: Could you compare Pitt and Caltech for me? What was your impression when you came in and you walked around the first time, before you were . . . ?

Horowitz: Well, first of all, I stopped--I guess it was at Throop Hall where the administration offices were--to find out where the biology laboratory was. Somebody gave me directions and I came down here, and I saw Kerckhoff. Now Kerckhoff then was just one-half of the present Kerckhoff. It was the west wing; in 1936 the east wing hadn't been built yet. And we didn't have Alles and we didn't have Church; so compared to the present biology division, it was very small. But it seemed enormous to me. I couldn't believe that this was all biology, but it was. I walked around it first, because it didn't seem likely to me that this building was only biology. And I ran into someone who was working at a greenhouse across the street--it's now gone. It was another graduate student, and I asked him if this was where the biology laboratory was. And he pointed it out to me, across the street; so I realized then that it was much bigger than the department I had come from. And, of course, here the emphasis was on research; teaching was a relatively minor activity.

Prud'homme: Did you teach as a graduate student?

Horowitz: Oh, yes, everybody taught. We taught labs, we taught Biology 1 lab.

Prud'homme: Did you like that?

Horowitz: Yes, I always enjoyed teaching, except when it interfered with something more important. Once I put myself into it and broke away from the other things, I enjoyed it.

Prud'homme: What was the main work being done in the biology department in the late thirties?

Horowitz: Well, there were several. One of the most important things was the Drosophila genetics. And at that time, the big excitement was the giant chromosomes, the salivary chromosomes. These have distinct markings on them; they're banded, and the bands are a constant feature of the chromosomes. The question was the significance of these bands. They seemed to be related to genes, but what their relationship was wasn't clear. There was a lot of interest in that. At that time, [Calvin] Bridges was still alive; Bridges was one of the great cytologists of the time. He had a big laboratory at the end of this hall with enormous enlargements of the salivary chromosomes pinned up all over the place. I got to know him and hear him give seminars. We had a general biology seminar every Tuesday night on the first floor of Kerckhoff. It was always after dinner--now it's at four o'clock in the afternoon on Tuesdays in a big hall--but then it was in what's now a classroom. After dinner Morgan and Mrs. Morgan would come across the street--they lived in a house that was located approximately where the Noyes Laboratory is now--and Morgan always introduced the speaker. He sometimes would read news stories from The New York Times; he got The New York Times daily by train. That was very unusual. Of course, it was about a week old by the time it arrived. He would go through the Times, and if there were any news items with a scientific slant, he would read them aloud and comment on the content and treatment of the stories. He was very amusing. And then he would introduce the speaker, and then he would sit down in the front row and fall asleep. [Laughter] He usually was asleep before the speaker had two sentences out of his mouth. Mrs. Morgan would sit next to him and nudge him and say, "Tom, Tom!"

Prud'homme: Did you have classes? It seemed as though there was only research then.

Horowitz: No, there were classes. Before we get on to that, you asked me what was important. The other thing that I remember, or two other things that I remember, of importance: One was all the work that was going on in plant physiology. Frits Went and James Bonner were very busy in those years identifying new plant growth factors and so on. That was a big operation. There were Went and Bonner and a large number of people. And then [Henry] Borsook had an interesting operation. Actually, I took my minor with Borsook in biochemistry. He was working on the free energies of compounds of biological importance, the thermodynamics of these substances. And that was quite interesting. And those, plus neurophysiology, were the big things that I can recall now that were going on at that time.

Prud'homme: Was Caltech considered a specialist in genetics?

Horowitz: Oh, yes. It was one of the few places that had a really substantial department, a group in genetics. A lot of biologists at that time didn't think genetics would amount to anything. It seemed like some special little area that had something to do with bristles on flies but nothing else.

Prud'homme: Had Morgan directed the department in this direction?

Horowitz: Yes, he was the key. He had started this before he left Columbia; he came from Columbia University. His group at Columbia consisted of himself and Sturtevant and Bridges, and Jack Schultz was at Columbia at the time. And they all came out to Caltech together. Tyler was a student of Morgan's at that time; he came with him. The only one of the early group that didn't come was H. J. Muller, who was then somewhere else. So they had started Drosophila genetics--well, they didn't start Drosophila genetics, someone else did; I don't know the early history of that, but the use of Drosophila as a genetic organism got started in the early 1900s, and Morgan and his group took it up.

They're the ones who first demonstrated that the genes are located--the Mendelian factors are located--on chromosomes. It's a material substance that's located on chromosomes. That was their big, important, discovery. And then they learned a lot about the chromosomes and about exchanges, genes moving from one chromosome to another, and so on. That's what Morgan won the Nobel Prize for. But by that time, he had left genetics and was back in developmental biology, albeit with a genetic angle.

Prud'homme: What was it like as a graduate student? Where did you live?

Horowitz: Well, I started out living in the Old Dorm, which occupied the space where the bookstore is now located. It was so noisy, I couldn't sleep at night. The Old Dorm had been a barracks during World War I, and the walls were paper thin. It was just too noisy for me, so I got a room in a rooming and boarding house up on South Michigan run by a Mrs. Nichols.

Prud'homme: Did she have only graduate students from Caltech as her boarders?

Horowitz: Yes. I still remember several of them. One of them was Charlie Townes, who's now professor of physics at Berkeley. And one was Homer Stewart. Stewart was professor of aeronautics at Caltech for a long time; he retired maybe five or six years ago. Homer Stewart and Charles Townes. Charles won a Nobel Prize later for his work in physics, his work with the maser.

Prud'homme: And you ate at her boarding house?

Horowitz: Yes, I roomed and ate there. And I never did come back to live in the Old Dorm at Caltech.

Prud'homme: When you had free time, what did you do as graduate students? You were working all weekend.

Horowitz: Yes, I was at the marine station on weekends. But on holidays we would go camping. James Bonner was a good friend of mine, and he was a great camper. He had a car, and one or two other people, the postdocs or young faculty, had cars, and we would go to the desert on weekends or go to the mountains. San Jacinto was a favorite place to camp over holidays.

Prud'homme: Did you stay here during the summers?

Horowitz: No. Part of Morgan's deal with Caltech was he could go to Woods Hole every summer. So Morgan went back to Woods Hole. And Tyler always went up with him. And I went with Tyler. [Laughter]

Prud'homme: The great triumvirate.

Horowitz: So that was great. I met my wife back there on one of those trips.

Prud'homme: Was she at Woods Hole?

Horowitz: She was a student at Radcliffe and had come down to Woods Hole to see a friend, I think.

Prud'homme: So then, at some point, you came back as a student.

Horowitz: I went back every summer, and I used to see her in the summer. And when I got my degree, we got married.

You did ask me about classes here. There were advanced classes.

Prud'homme: But were they graduate classes? It doesn't seem, in my research, that they were, except for the seminars.

Horowitz: Yes, I guess there weren't enough students to differentiate between graduate and undergraduate.

Prud'homme: How many graduate students would there have been in the biology department?

Horowitz: I think there might have been a dozen or something of that order.

Prud'homme: So you really got to know each other very well.

Horowitz: Yes. Well, it was very different from what it is now. It was a monastery; there weren't any women around. The only women in this department were Mrs. Morgan and Morgan's secretary.

Prud'homme: Wasn't the secretary something of a holy terror?

Horowitz: Yes. Brusstar was her name. We used to call her Susie; I don't remember what her real first name was. She was a tragic figure; she committed suicide. She was a very masculine type. At night, there was no one in the building except students, and those of us who were on the second floor would use the women's lavatory, because otherwise we'd have to go upstairs or downstairs. One morning, I heard Brusstar going down the hall here, roaring, "Who in the hell left a seat up in the women's john?" She was a very good secretary, and she guarded Morgan like a dragon. But as I say, she was a tragic figure. I never got to know her really well, but I imagine she had problems.

Prud'homme: Did you get to know Mrs. Morgan?

Horowitz: Fairly well. After we were married, she invited us over a couple of times; and she was very kind. Of course, she survived Morgan by a number of years. She was a geneticist, too. She worked with Drosophila.

Prud'homme: Did she work with her husband?

Horowitz: No. Well, he wasn't in genetics at the time, and she worked independently. The worst part of Caltech was the lack of women. Partly that made the atmosphere. I mean, there was nothing else to do but work. It was very informal, and everybody was just as ambitious and just as bright or brighter than you were. Of course, that's always been one of the hard things for students at Caltech. I came out here from the University of Pittsburgh, and I had graduated with highest honors and Phi Beta Kappa and all that stuff, but everybody here is just one of the crowd. I think especially for freshmen that's rough on their sense of self-esteem. I know it's been hard on some of the advisees I've had over the years. Their whole system of self worth is based on their superior intellectual capacity; but when they come to Caltech, instead of being number one, they're maybe number fifty out of a class of a hundred.

Prud'homme: It must be devastating.

Horowitz: Yes, for some it is.

Prud'homme: Can you warn them ahead of time?

Horowitz: I don't think it does much good.

Prud'homme: Why did you go to Stanford? And how did you get your fellowship there when you first went in '39?

Horowitz: Well, I was awarded a National Research Council Fellowship. There weren't many postdoctoral fellowships then, as there are now. Then, science was sort of an oddity that few people went into. No one regarded it as central to the national security or the progress of the nation; that didn't come until after the war. I suppose if there hadn't been a depression, I might not have gone into science myself. As it was, there wasn't anything else to do.

Prud'homme: Was it difficult to get the fellowship?

Horowitz: Well, it would have been, except that I'm sure that Morgan did it for me. Morgan was without a doubt the most influential biologist in the United States at the time. He was a member of the National Academy of Sciences, which operates the National Research Council. He had been president of the National Academy. And when he recommended anyone for a National Research Council Fellowship, it was automatically awarded. And as I said, many things that happened to me were just sheer luck. I worked for Tyler, and I got to know Morgan, and I'm sure Tyler urged Morgan to put in a good word for me when I applied for it. He never told me that, but I'm convinced that's what happened. Normally, one would have gone to Europe in those days, but the war clouds were gathering in Europe, and I went to Stanford. I worked in the laboratory of Douglas Whitaker, who was Morgan's son-in-law and also a developmental biologist. It was an extension of the work I'd done--my thesis actually. I worked on a respiratory pigment, a red pigment from the eggs of this marine worm that I'd done some work on for my Ph.D. thesis. And there again I was very lucky; I met George Beadle. Beadle had been at Caltech in the early thirties. As a matter of fact, he's told me that when I applied to Caltech, he saw my application and he urged that I be accepted because I was working in the field of tissue transplantation. And that's what he was planning to do with [Boris] Ephrussi in Paris. When I got to Caltech in 1936, Beadle had just left for Paris, where he and Ephrussi were going to do what turned out later to be very important experiments. Beadle thought that I couldn't be a total loss if I was smart enough to work in the field of transplantation, so he had urged that I be accepted as a student. But I didn't meet him until after I got my Ph.D. and went up to Stanford in the fall of 1939, I guess it was. He was then there, working on Drosophila. Though I didn't work in his laboratory, I got to know him very well. We became friends.

Prud'homme: What's Beadle like?

Horowitz: Well, what he's like now is very different from what he was.

Prud'homme: What was he like then?

Horowitz: Well, he was very ambitious, very competitive, a very engaging person, very fair. People who are so competitive tend often to be unpleasant and nasty, but not Beadle. He always bent over backwards--still does--to give other people credit. But he can't sit still if there's an interesting problem to work on. I think having a competitor is essential for him. He's still having a battle with a man named Manglesdorf. After he left the University of Chicago he went back into corn genetics, which he had worked on before he came to Caltech. He got his degree at Cornell with R. A. Emerson, the father of Sterling Emerson, who was on the faculty when I was a grad student; Sterling was one of the geneticists, a plant geneticist. Beadle, when he came to Caltech, got into Drosophila, and later--at Stanford--Neurospora. After that he became chairman of the division here and then went to the University of Chicago to be president. Well, after he left the University of Chicago, he found that the field he had started had left him, so he went back to his first love, which was corn. And the problem he became interested in--had been even when he was a graduate student--was the origin of maize. Now maize is a man-made plant. It doesn't exist in nature. It can't survive in nature. Yet it was grown by the Indians, and there's always been a question of great interest as to where the Indians got it. There was a professor at Harvard named Manglesdorf--if you read The New Yorker, E. J. Kahn recently had a piece about him; I think there were two episodes in The New Yorker within the last two months on what he calls the "corn wars." I don't think it's awfully good, but it'll give you some idea of the disagreement between Beadle and Manglesdorf. They had different theories about the origin of corn. I saw Beadle on the Fourth of July. . . . I've never met Manglesdorf, but he must be Beadle's age; Beadle is eighty, and I'm sure Manglesdorf is at least that. And these two old men still disagree. And I think that if Manglesdorf suddenly were to agree with Beadle, Beadle would just be devastated. And Manglesdorf may be the same way. I think they need each other, they need to have someone to disagree with. It stimulates them to have new ideas, and so on. And that's always been a characteristic of Beadle.

Prud'homme: Can you describe Doug Whitaker to me?

Horowitz: Doug died a long time ago, and my memory of him is dimmer than it is of Beadle. Doug had a nice laboratory at Stanford. He didn't do very much work in the lab when I was there; but he was an extremely good teacher and a very jolly person, a very witty man. And he also did administration. He wasn't chairman of biology at Stanford at the time, but he may have been vice chairman, I'm not sure. He later left Stanford and went to the Rockefeller Institute, where he became assistant director, I think. That probably happened in the late forties; I never saw him after that. I heard he wasn't very happy at the Rockefeller, and he retired eventually and went to Texas and died there. I liked Doug very much; I always enjoyed him. He was a witty, smart person who enjoyed talking about research more than actually doing it. Whereas with Beadle, it's just the opposite. Beadle wanted to do it and not talk about it.

Prud'homme: How was Stanford different from Caltech?

Horowitz: Well, it was much larger. Actually, since I was married and wanted to get as much done as I could on my one-year fellowship, I didn't change my habits at all there. I guess it wasn't a culture shock going to Stanford at all, because my habits were pretty much what they had been at Caltech. I'm not even sure I met people from other departments. There wasn't very much mixing, at least at my level. I was a postdoctoral fellow; I didn't go to the faculty club for lunch or attend classes in other departments.

Prud'homme: And you knew you were only there for a year.

Horowitz: Yes. And when I came back at the end of the year, Borsook offered me a fellowship in his laboratory. He had some money from a local orthodontist. And I took that and spent a year or maybe two years in Borsook's lab working on calcification and then went back to Stanford. It was shortly after I left Stanford in 1940 that Beadle decided he couldn't get any further with Drosophila, and he had this great idea of looking for certain kinds of mutations in Neurospora. And he did that; and in 1941, he came down here and gave the most interesting seminar I've ever heard on the findings he and Ed Tatum had recently been making with Neurospora.

Begin Tape 1, Side 2

Prud'homme: Can you tell me about Beadle's seminar? Was it supposed to be just a seminar in biochemical genetics?

Horowitz: No, it was a general biology seminar. Biochemical genetics wasn't a subject yet--he was just going to found it in that lecture. It was the usual general biology seminar, down in 119 Kerckhoff. Beadle got up and he talked for thirty minutes. Then he sat down--I've written this up for the Neurospora Newsletter, so I remember it--and everyone thought he was just pausing for a moment. I mean, I myself couldn't believe that anyone with such discoveries could stop talking after thirty minutes. But he did; he sat down. And it was about a minute before everyone realized he had really quit, and they started applauding. And Frits Went, who was in charge of the general biology seminar that year, was so excited--I remember he jumped up, and he turned around and faced the audience and pointed at the graduate students and said, "You see, biology is not a dead subject. There're still things to be discovered." And Beadle was looking for a couple of postdocs to come up and help him.

Prud'homme: And he had the money?

Horowitz: Oh, he had money. Although the war had already started--it was early '41--we weren't in it yet. It was obvious that this was a serious war, and we were beginning to mobilize, and there was a lot of aid going to Europe. But he had no trouble getting money--never did. We worked all through the war. The work he was doing had nutritional implications and enormously interesting fundamental biological implications.

Prud'homme: Can you describe the work that you were doing at that time?

Horowitz: Let me finish this story. He was looking for postdocs to help exploit these discoveries, and David Bonner and I were the two that went back with him. I've always felt that was the single most important decision of my life, because working for Beadle was just marvelous. He was a great man; he's without a doubt the most important person in my life professionally.

Prud'homme: He seems to have been very generous, giving you credit.

Horowitz: Yes. He and I always got along. We thought in the same way and stimulated one another. Whereas some of the other people, even in his lab, never thought the work was as fundamentally important as we did--or maybe that's putting it the wrong way. He had arrived at an important generalization: one gene, one enzyme, meaning that the role of the gene is somehow to create a particular enzyme. Some of the people in the lab didn't agree with this and wouldn't accept it; it was too radical, and it wasn't proven to the hilt. But I thought it was very plausible, and I helped compile evidence for it. For a long time, that's what I did.

During the war, we had a lot of applied work going on, actually. For example, one of the things I did during the war was to work out an assay for choline--choline is one of the B vitamins--using mutants that I had worked on, Neurospora mutants. Another thing we had going during the war was trying to find mutants of Penicillium to produce more penicillin. There were two other laboratories in the country doing the same thing. One of those other laboratories, the one at Cold Spring Harbor, did find a mutant that increased the yield of penicillin by a factor of about ten. And later another mutation was found that increased that by still another factor of ten. So these activities were important for the war. I think a lot of our scientific colleagues at the time thought we were wasting our time doing this applied work, but it was essential to do it.

Prud'homme: Did Bonner have the same sort of relationship with Beadle that you did?

Horowitz: No. The lab at Stanford was divided physically into two parts, and Tatum was at one end and Beadle was at the other end. Bonner was working in Tatum's part, and I was working in Beadle's part. Tatum was an entirely different sort of person from Beadle. Tatum was a microbiologist. He was very important for the work because, at first, Beadle didn't know how to handle microorganisms. It was Tatum who set up the medium and learned how to grow Neurospora. But Beadle had the idea to use it as a genetic organism. No one had ever used a microorganism before for this kind of genetics. Neurospora was selected by Beadle because it was one of the few microorganisms that could be used that way.

Prud'homme: In what sense?

Horowitz: Its life cycle was understood. The funny thing is that Beadle had first met Neurospora down here at Caltech in the early thirties. The man who worked out the life cycle of Neurospora and discovered that it has two mating types, that you can cross them and get progeny, and the progeny show inheritance of characteristics of the parents, was a man named Dodge at Columbia University. Morgan knew Dodge. Beadle has told me many times that one of the last things that happened before Morgan left Columbia was that Dodge came up to him and handed him two test tubes containing cultures of Neurospora, the two mating types--they were called large A and small a, otherwise they're identical and you can't tell them apart--and he said, "Take these with you. They're going to be important some day for genetics." Morgan took the cultures and he kept them going out here. Then one day a man named Carl Lindegren, who was working in a gas station up on Lake Street, came in. Lindegren had studied microbiology as an undergraduate--it may have been at USC--and he went into Morgan's office and said he'd like to become a graduate student. He was eventually accepted, and Morgan gave him these cultures and told him to work out the genetics. Lindegren took them, and Bridges got interested and helped him; together they made the first chromosome map. So the first map of Neurospora was Lindegren's map, but these weren't the biochemical kinds of mutations that Beadle and Tatum later discovered.

Beadle knew about this; this was all going on while Beadle was a postdoctoral fellow here in the early thirties and Lindegren was a graduate student. When Beadle finally convinced himself that he had gone as far as he could with Drosophila, he was sitting in on Tatum's course in microbiology at Stanford. Tatum was lecturing on the nutrition of various species of microorganisms: some microorganisms require certain vitamins, and some require certain amino acids. This was a very active subject in the thirties and forties, especially the thirties. Beadle said, "Why don't we look for mutants that have lost the capacity to synthesize growth factors?" You see, he got the idea that the reason some organisms need these growth factors is that they've lost the genetic capacity to make them. He and Tatum agreed that they would induce mutations in a normal culture of Neurospora and examine five thousand progeny. They would irradiate a strain, a culture of Neurospora and cross it with an unirradiated culture. The progeny come as spores--they're called ascospores. They would pick five thousand ascospores at random and test them for loss of essential biochemical capacities. If they didn't find any mutants after five thousand spores, they would give it up. The first mutant has the number 299; that first one required vitamin B6, I believe. From that point on, they knew they had something. . . . I've forgotten what the point of all this was.

Prud'homme: Well, the use of Neurospora.

Horowitz: Oh, yes. Well, that's the reason. It was the only microorganism where the life cycle was understood so that you could actually do genetics, and it also could be grown on a synthetic medium--a medium whose components are completely known. In those days, you couldn't grow most microorganisms that way. They needed yeast extract and nobody knew what was in yeast extract.

Prud'homme: And they could synthesize everything they needed.

Horowitz: That's right. The only vitamin it needed, the only growth factor needed, was biotin. And biotin had recently been isolated and you could obtain pure biotin, synthetic biotin. That was all you to give it. Otherwise, it just needed nitrogen and sugar and salts. So it's an incredible coincidence that Beadle knew about Neurospora and Neurospora was the only organism that could be used at the time for this kind of investigation.

Prud'homme: You published eight papers between '43 and '45 with Beadle. And they're all considered sort of classics in the field, I'm told.

Horowitz: Really? Well, I'm sure I agree with that. [Laughter] I think that's probably right.

Prud'homme: What was the work that you did?

Horowitz: Well, it was hammering the last nails in the theory of one gene, one enzyme. You have an idea like that and you elaborate it deductively. I mean, if one gene, one enzyme is correct, then this should follow. Then the deductions become predictions, and you compare these deductions with observations. And that's what we were doing. And they worked out. Some of the effects were quite complicated. It turned out that there are all sorts of branches in these genetic networks, so that one gene might in fact control an enzyme that gives rise to two different products. At first that looked like a real contradiction of this simple idea of one gene, one enzyme. But when we worked out the pathways, it turned out to be perfect, that it controlled one step in a branched chain. That sort of thing. And I worked on an enzyme, tyrosinase--this may have been after we got back to Caltech; it was in the forties, though, I'm sure--that we found in different forms in different strains of Neurospora. The forms differed in their thermostability, their heat stability. We decided that if the role of a gene was actually to fashion the enzyme, then this thermostability should be inherited as a simple genetic trait, and it was. We found a number of these things.

Prud'homme: How terribly exciting to have all this work out.

Horowitz: It all worked out, but of course it wasn't really settled absolutely until the mechanism of gene action was totally understood, as it is now. And before that could be done, DNA had to be discovered; and then the ribosome mechanism for making proteins. But it all worked out. So our expectation, which was actually a deduction from observations made on these mutants, turned out to be correct. But it was prolonged. And that's what all those years were devoted to: working on Neurospora.

Prud'homme: Beadle returned to Caltech in '46 as chairman of the biology division. And you came along as a research fellow, right?

Horowitz: Right.

Prud'homme: And you had a son by then.

Horowitz: Yes, Joel. And a daughter, too--Elizabeth.

Prud'homme: What was the style and the work of the division at that time? Had it changed much during the course of war?

Horowitz: I don't recall that it had changed much at all. Beadle made a big difference. The division didn't have a chairman after Morgan died. It was run by a triumvirate--Haagen-Smit, Borsook, and Sturtevant, I think--I wasn't here at the time. Sturtevant was the senior inheritor of Morgan. He was Morgan's favorite student and a great scientist, totally hopeless at administration but a man of great prestige.

Prud'homme: Too academic, or too introverted, to be administrator?

Horowitz: Well, I don't think he'd ever done any administration. And I wasn't here while he was an administrator, so I don't know. You have to be interested in keeping books and thinking about money. I mean, money is the thing that a chairman mostly has to think about--and such things as admitting graduate students, and who's going to teach such-and-such.

I mean, for someone who's worked in science his whole lifetime, it's very hard to switch over. Science is so exciting. [Laughter]

Prud'homme: I can imagine, actually, Beadle doing it, because of his wonderful enthusiasm.

Horowitz: Yes, that's one of the amazing things about Beadle. Beadle was so good as a chairman; he was such a charming man. He's still charming, although he's lost an awful lot of his capacities now--he has Alzheimer's disease. But he was so charming; he could charm money out of a . . . And he liked people. And he built up the division; he brought a lot of money into the division, and he enjoyed doing that. That's why he went to the University of Chicago; they got him back there to help them.

Prud'homme: You came with him to Caltech.

Horowitz: Mitchell and I came down from Stanford.

Prud'homme: Ray Owen came about that year, too, didn't he?

Horowitz: Well, Ray arrived that year, but he didn't come from Stanford but from Wisconsin as a postdoc. And Tatum went to Yale at that time; he took Dave Bonner with him.

Prud'homme: Did you like coming back?

Horowitz: Oh, yes. I could have stayed at Stanford. Doug Whitaker came over the night we were packing up our bags to leave and asked me to stay. [Laughter] But I wouldn't have left Beadle's group for anything. And later, [Max] Delbrück asked me to join the phage group. I'd known Max when I was a graduate student here. One of the first things Beadle did was to get Max back here. I think I had something to do with that. He was a postdoc when I was a graduate student; and he used to go on camping trips with us. And then he was at Vanderbilt, doing his phage work, which he had learned to do here. When the division started looking for new faculty after the war, I urged Beadle to think about getting Max back, and he did. That was very good for us. Max later invited me to join his group, but I would not. It wasn't a question of leaving Beadle but the work. I enjoyed it so much, and it was so important. Not that Max's phage work wasn't important also, but I enjoyed working with Neurospora and with Beadle, although Beadle left research after he became chairman here. He tried to keep up with research, but he couldn't, and then he didn't pretend to after a while. He was a very good chairman, and he enjoyed the work of being chairman.

Prud'homme: So he expanded the department.

Horowitz: He expanded the department, that's right. He built the Church lab. The story was that Mr. Church was a very wealthy man--I don't know where he made his money, but he had race horses. And one of his jockeys was accused of having doped one of his horses; something was found in the urine of the horse. Haagen-Smit did some chemistry on the horse's urine and found the accusation was false. And Beadle talked Church out of the Church lab as a result. [Laughter] That's the story, and I can believe it.

Prud'homme: Lee DuBridge also arrived in '46.

Horowitz: That's right. Oh, I remember that well.

Prud'homme: What was your first impression of him?

Horowitz: Well, everybody knew we had a new president, because the first thing that happened after Lee DuBridge came was that faculty salaries went up about 30 percent.

Prud'homme: Because [Robert A.] Millikan was not big on money.

Horowitz: Millikan was very tight, and salaries were very low. And when DuBridge came from MIT, he looked at this and he changed it. And everybody knew we had a new president--there was no question about it.

Everybody liked Lee--not just because of that, but he's also a very charming man, like Beadle in many ways. I've always liked Lee DuBridge.

Prud'homme: It sounds like a sort of golden era.

Horowitz: It was, oh, yes, it really was, it was a golden era.

Prud'homme: What were the post-war students like? Did they differ?

Horowitz: Well, they certainly became different later. I don't recall that the immediate post-war students were very different from the students before the war. But I know that in the sixties, and perhaps still--I don't keep up with it--we got more and more students with extremely good records who came out and spent a year and then decided they made a mistake, and they would go to medical school--or that's usually what happened. I don't know whether it's the students or the nature of the science.

Prud'homme: They decide they don't want to do basic research?

Horowitz: That's right. They decide basic research is not for them; that happens more and more now. Opportunities are so different now. And incentives are so different.

Prud'homme: Well, I think the attitudes are different. I was in college in the very early fifties. And you didn't fiddle with your college career; you went and you stayed and you graduated. There was no questioning the authority of the institution, in a sense.

Horowitz: Yes, that's right. It was quite different.

Prud'homme: What did you teach at that point? You became an associate professor in '47 and a full professor . . . .

Horowitz: I taught biochemical genetics for years. In fact, I guess I taught biochemical genetics until what's called molecular biology became a really new subject. And then I gave that up and I taught evolution, which has always been one of my interests. Also, I was at JPL [Jet Propulsion Laboratory] starting in 1965. I spent a lot of time in the space program, and that took me away from teaching, although I think I've taught every year, even when I was at JPL.

Prud'homme: So between '46 to the mid-fifties, you were still doing Neurospora work, in effect, doing a lot of the work that Beadle had stopped doing because he was chairman.

Horowitz: Yes. I never did give up Neurospora. Even when I was at JPL, I had people down here working on Neurospora, and when I retired two years ago, I was still doing Neurospora.

Prud'homme: Did you have any favorite students at this time? I was thinking of S. C. [San-Chiun] Shen.

Horowitz: S. C. Shen, yes, he's one of my favorite students. I invited him back for our division's fiftieth anniversary. He was the last mainland Chinese student we had at Caltech before the communist revolution; he left in 1950. He was taken off the ship in Tokyo or Yokohama--MacArthur was occupying Japan at the time--and held against his will while all of his notebooks were sent back to Washington to be checked through. He had been a close friend of [Hsue-Shen] Tsien, an aeronautical engineer who is now an important figure in China. For this reason, MacArthur and his organization became suspicious of Shen. And Shen never did get his notebooks back, or the cultures he had taken--the Neurospora cultures--and a lot of chemicals I'd given him. So ridiculous! Shen was eventually released and went to China and has become an important figure in genetics. He's at the Institute for Genetics in Shanghai. He came here to be a graduate student; he left a wife and a child at home and he didn't know anybody here. Amazing. All the Chinese were that way. He did very well. I remember a letter I got from him after he got back to China, telling me about this horrible experience in Japan on his way home. The thing that bothered him most was that they hadn't informed his parents. He wasn't so much worried about his wife, but his parents in Peking--or Shanghai--were expecting him and he didn't arrive. Just infuriating!

Prud'homme: Any other students you can remember?

Horowitz: Yes, there was a Japanese student, Noboru Sueoka. He's now at the University of Colorado. He was also a very good student, so bright. He's working in the United States, although the Japanese want him back, on animal cell genetics.

Prud'homme: Back to Neurospora. How do you use the mutants? What do you use them for?

Horowitz: Well, you use them to get knowledge, mostly. Some of them can be used for applications of one kind or another, but mostly they're to get information about how nature works. One application I've already mentioned was to use them for assaying vitamins, for example. These bioassaying methods I'm sure are out of style now; they're too slow. Now everything is done chemically. But in the forties and fifties, it was an important way of finding out, for example, how much vitamin B1 is in a food. Another way the mutants are used, as I've also mentioned, is to produce substances of value, like penicillin. We discovered that if you knock out a gene that controls a particular enzyme, the stuff that's made before that in a biosynthetic chain often accumulates behind the block, just like water behind a dam. So that was the idea that led to the proposal to look for mutants that accumulated penicillin; and that turned out to be useful, of course. Still is.

Prud'homme: You went to Paris in '54 on a Guggenheim fellowship. What did you do there?

Horowitz: I was working on Drosophila then. I started doing experiments on tyrosinase in Neurospora, which I've already mentioned; and I wanted to see what I could do with Drosophila, because Drosophila has such a tremendous genetic background. So much was known about it, but very little about its biochemical genetics. In fact, Beadle and

Ephrussi had done almost the only biochemical genetics that existed at that time on Drosophila. I found that there was a tyrosinase in Drosophila that you could extract and study. And I thought, well, let's see what we can do with that; maybe we can do some more biochemical genetics on Drosophila. And Ephrussi invited me to come to work in his laboratory in Paris. That's what I did there. I got some interesting things out of it, but it didn't lead in the direction I was hoping it would, so I gave up. Published a couple of papers on it.

Prud'homme: Beadle left in '61, and you stayed. Were you tempted to go to Chicago?

Horowitz: No. I wasn't even asked.

Prud'homme: You continued to work on your study of the tyrosinase.

Horowitz: Yes. And I was getting more and more into the space program.

Prud'homme: And the origin of life. Well, it's all wrapped up together.

Horowitz: I never really did work in the lab on the origin of life, but it had been an interest of mine. But in '57, I guess, Sputnik I was launched, and by '59 it was definite that the Jet Propulsion Laboratory was going to be a planetary science lab, and people began coming down from JPL to see if there was any interest here in planetary explorations.

Prud'homme: Why did you think that there was a possibility of life on another planet?

Horowitz: Well, at first it was a plausible idea. Everything that was known about Mars at the time later turned out to be wrong, but everything suggested that there was a good possibility of life on Mars. And I was beginning to run down, was getting tired of working on the same things. I had a choice of going into something new here in the lab or taking this golden opportunity to get involved in a new program. And that's what I did. And it turned out to be very exciting. Of course, we didn't find life on Mars, but I'm glad I did it.

NORMAN H. HOROWITZ

Session 2

July 10, 1984

Begin Tape 2, Side 1

Prud'homme: I've been asked to ask you a question I wouldn't normally do. Some people believe that Morgan was very anti-Semitic. Is this true?

Horowitz: I never felt it. I did see Kevles's book review in Science, where he quoted a letter from Morgan about appointing Michaelis. All I can say is that I never noticed it.

Prud'homme: You never felt that he always gave Jewish graduate students to Jewish professors? There was some brouhaha about that.

Horowitz: Well, Tyler was Jewish, that's true. I can't remember whether there were any other Jewish graduate students.

Prud'homme: But you didn't feel that at all.

Horowitz: No, not at all. I mean, as I told you yesterday, I thought he was a good friend of mine. I'm absolutely certain he was responsible for getting me the postdoctoral fellowship, which was a very unusual thing to have in those days.

Prud'homme: So he did his all for you.

Horowitz: I always thought so. I was annoyed when I saw that review of Kevles's. Of course, you can't argue with it; he's obviously quoting a letter, but it's not the whole story.

Prud'homme: We started to talk a little bit about JPL yesterday.

Horowitz: I went up there in 1965. JPL became the lead center for planetary exploration.

Prud'homme: You'd also been a consultant to the Bio-Sciences Committee of NASA from 1960.

Horowitz: That's right. I got involved very early. The exploration of Mars became the key idea for a planetary program, for obvious reasons, and JPL set up a bio-science section to plan for the biological exploration of Mars, with an eventual lander. They asked me to come up and be chief of that section, which I did, in 1965. I agreed to go on a half-time basis. Actually, I spent most of my time up there, but half my salary came from JPL, and half from Caltech.

Prud'homme: What did you do as the chief of the bio-science section?

Horowitz: I did a number of things. There was a lot of work going on up there in trying to design instruments to fly to Mars for a biological search, and I got involved in that planning. Two of the instruments that eventually flew on Viking came out of that group. The gas chromatograph mass spectrometer, which was probably the most important single instrument on the lander, was designed at JPL, though it was not built there.

Prud'homme: Did you get involved in the actual design process?

Horowitz: No, not of the gas chromatograph mass spectrometer. When I went up there, that was already in process--it had been anticipated that this would be a useful instrument to have on Mars. What I did get involved with in connection with that instrument was making sure that there was a lot of ground-based experience with it. The instrument is based on empirical patterns of breakdown of organic compounds. You take an organic compound and you heat it until it pyrolizes--it breaks into smaller fragments due to the heating. These fragments can be identified by a combination of analytical steps called gas chromatography and then mass spectrometry. The only thing you have to identify the original compound you started with is the pattern of its breakdown products, and you try to infer the nature of the original compound from these breakdown products; and you try to infer the nature of the original compound from these breakdown products. There's not much general principle or general theory you can go on; you just have to have a library of results you can compare your actual results with. We did a lot of that during the years that I was there.

Another thing I did was to get the idea for the second biological instrument that JPL had on the Viking lander. NASA called it the pyrolytic release experiment; we used to call it the carbon assimilation experiment. It was an experiment that I developed with two collaborators, George Hobby and Jerry Hubbard. The point of this experiment was to carry out a biological test on Mars under actual Martian conditions. It's hard to convey in a few words the total commitment people had in those days to an Earth-like Mars. This was all an inheritance from Percival Lowell. It's amazing: In the pre-Sputnik I days--in fact, up until 1963, well into the space age--people were still confirming results that Lowell had obtained, totally erroneous results. It's simply bizarre!

Prud'homme: And life on Venus, too.

Horowitz: Yes, a lot of people thought Venus was covered by an ocean. But that was speculative; in the case of Mars, they were making measurements and coming up with the wrong answers--I mean, these were supposedly objective measurements. Measurements were made on the 200-inch telescope by a young man at the time--he's now a professor at the University of Hawaii, a well known astronomer--and they were completely wrong. This is just one example. And this was all based on the desire of people to believe that Mars was an Earth-like planet. It wasn't until 1963 that this began to unravel; the first step in the de-Lowellization of Mars occurred in 1963.

Prud'homme: What was that?

Horowitz: It was one infrared photograph taken at Mount Wilson. It was an unusually excellent photograph, showing the infrared spectrum of Mars. It must have been a very dry night above Mount Wilson, a very calm night. They got this marvelous single plate, and it was interpreted by Lew Kaplan, who was at JPL, and Guido Münch, who was professor of astronomy here--he's now gone to Germany--and Hyron Spinrad, a young postdoc working on Mount Wilson at the time. They showed, first of all, the total atmospheric pressure on Mars, which back around 1900 Lowell had estimated was 85 millibars. All through the 1900s, up until about 1960, people were making new measurements of the surface pressure that averaged out around 85 millibars. And these were by respectable people! So when the space program started, it was generally accepted that the surface pressure on Mars was 85 millibars, and that carbon dioxide was a small fraction of this; the rest of it was assumed to be mostly nitrogen, as on the Earth.

Prud'homme: With those as givens, the logical assumption of life . . .

Horowitz: Yes. At least it was plausible. The Martian environment appeared to be Earth-like, but a very cold and dry Earth-like environment, an extreme form.

Prud'homme: But with many of the same elements.

Horowitz: With all the same elements, with water available and enough pressure so that liquid water could exist at least transiently on the surface. This was a difficult point, to get enough liquid water to support life. With 85 millibars, there was a possibility that you could have liquid water, at least for part of the day.

Prud'homme: What about Kaplan's and Münch's . . . ?

Horowitz: Their findings showed that the surface pressure could not be 85 millibars. It looked more like 25 millibars to them. They also identified water vapor in the spectrum; that had never been seen before. They found very little water. And it was obvious that carbon dioxide was a big portion of the atmosphere and not a minor portion. Well, this turned out just to be the first step. The next big step came in 1965, when Mariner 4 flew by Mars and found that the surface pressure was more like 6 millibars. And that is the average pressure. And carbon dioxide is the principle gas in the atmosphere. Well, with 6 millibars, there's virtually no chance of having any liquid water. And now, after Viking, we know there is no liquid water on the surface--there can't be any liquid water.

Prud'homme: But you were still out to prove that there was some possibility for life.

Horowitz: There was. The main point up until Viking was water. And there were enough theoretical mechanisms for getting some water on the surface of Mars to maintain the remote possibility--although by the time we launched Viking, it was very remote--that there were either pools of brine or, after snow or frost there might be enough meltwater at sunrise to sustain a population of microorganisms. By then, no one except Carl Sagan was talking about higher forms of life on Mars; the real interest was in the possibility of having microbial life. And there are organisms on Earth that will actually grow slowly on just water vapor; lichens can do that, though they need quite a lot--they need 80 percent relative humidity at a warm temperature to do that.

The point is that in spite of all these new discoveries, people were still building instruments to fly to Mars that were based on the terrestrial environment, and they were eventually approved by NASA. NASA was supporting these efforts. Around 1960, I got involved in one of them, one that actually flew later on Viking. We called it Gulliver at the time. It was invented by an engineer in Washington, named Gilbert Levin. It depended on an aqueous medium. Two other experiments that were being supported by NASA also involved aqueous solutions into which you would put the Martian soil and then use various ways of measuring the metabolism of the organisms. But after 1965, after the Mariner 4 flyby, it was obvious that the chance of liquid water on Mars was so remote that one had to plan for the contingency that there was no water--that if there was any life on Mars, it was living under conditions that were in no way terrestrial. So we designed an experiment that would work under Martian conditions and that involved no liquid water. That was another thing I did at JPL.

Then something else I got involved in at JPL—and when I say involved, I don't mean I worked in the laboratory; I sat at a desk—I had to go to a million meetings every week. . . .

Prud'homme: How much of NASA's willingness to go along with the possibility of life on Mars after, let's say, '63, do you suppose was because of the firm conviction, since Lowell, that there was life; and how much do you suppose it was because this was a way to get money, because it was easier to get money if you could say you were looking for life?

Horowitz: Well, I think there was some of the latter. But I think most of it was that people didn't want to give up the idea. And I agreed that, now that we had the capability, we would never solve the problem by just looking at Mars from the Earth. This was a classical problem, part of Western culture, the idea of life on Mars has been around for three hundred years. And here was the first time we had the ability to test it. I think if it hadn't been for Mariner 9 . . . Mariner 9 found an objective argument for flying to Mars, because Mariner 9 saw that Mars once had water on it. There are dry stream beds, obviously cut by water. All the geologists agree they're water cut; there was water on Mars at one time. And you could say that, if there was water on Mars, then there may have been an origin of life, and that life may still be surviving. Now Mariner 9 was an orbiter, it orbited Mars in 1971; and up to that point, up to the time Mariner 9 took its photographs, I would have said the a priori probability of life on Mars was close to zero. It would have really been an irrational act to fly to Mars before 1971 to look for life. But, you know, I think it would have been done anyway, because people were irrational about Mars; some still are. Not only that, but these big space enterprises are planned and paid for long before they're launched.

Prud'homme: So the machinery was chugging along.

Horowitz: That's right. You know, we would have had a spacecraft, or at least parts of a spacecraft, and a whole big apparatus set up to build the spacecraft and fly it--and no place to go.

Prud'homme: And in a sense, scientifically, you really had to get there and prove that there actually wasn't any life.

Horowitz: That's right. And there are other reasons, too. I mean, planetologists are interested in Mars, whether there's life on it or not. There are a lot of interesting questions about Mars and about all the planets. So it wasn't as if it was only a matter of looking for life and doing nothing else. But after the Mariner 9 orbit of Mars--it was in orbit for a year--there was no question, we had to go to Mars to look for life, because it was clear that Mars once had rivers. And so that's how it happened.

I want to mention while I'm thinking about it that another important thing I initiated at JPL was studies in the Antarctic. I never went to the Antarctic myself, but there was a microbiologist at JPL named Roy Cameron who studied microbial life of the world's deserts--he was traveling all the time. Just before I went up to JPL, I read a report of biological work that had been done in the Antarctic during the International Geophysical Year, around '58.

Prud'homme: '57 I think it was.

Horowitz: Okay. Anyway, it came out that the Antarctic is not entirely covered with glaciers as I had always assumed, but there are dry areas called the dry valleys, actually ice-free areas. A team of microbiologists, the Boyds, got in there during the International Geophysical Year, and they found that a lot of their soil samples were sterile; they couldn't find any bacteria. These dry areas are as Mars-like as you can find on the Earth. They're very cold and they're very dry. And I thought that Roy ought to be spending his time down there instead of in the Sahara and the Mojave and Atacama and so on. So Roy took people from the lab and students from Caltech with him, and he went down there for six or seven seasons. And that turned out to be quite interesting. He found that these areas are not totally sterile, but some 10 to 15 percent of the soil samples contained no bacteria, and the rest had very low bacterial counts. This was in the driest parts of the valleys. We used this as an argument against the sterilization of the Mars landers. The sterilization was very controversial. First of all, it added about 10 percent to the price of the Viking landers. Then, we were always afraid it would damage the instruments. They were going to assemble the spacecraft and then put both of them in ovens and cook them to kill all the bacteria. I, and a number of other people, argued against this on the grounds that if the Antarctic dry valleys can't support terrestrial bacteria, we don't have to worry about infecting Mars.

Well, we didn't accomplish anything, because there was an international agreement that we would not contaminate Mars--although the Russians, I think, did contaminate it. They landed a number of spacecraft on Mars and they certainly didn't do terminal sterilization like we did. But we did sterilize them, and they worked almost perfectly--almost all the instruments worked perfectly. And the mission was a success.

Prud'homme: It must have been a terribly exciting time.

Horowitz: It was, but it was also very nerve-racking, because we never knew when it was going to fly. In 1970, when I decided to come back to campus, Nixon had just announced that the Viking mission was going to be put off two more years because of budgetary problems, which was just as well, because I don't think we would have been ready to go. I don't think it would have been as successful if we had tried to launch it in '73. But that kind of thing was just too nerve-racking. And besides, everything that I had gone up there to do was finished: the instruments were fixed, they were in the final stages of their design.

Prud'homme: Did you enjoy the administrative part of it?

Horowitz: Not particularly.

Prud'homme: Did you have to get money?

Horowitz: Yes. But the machinery at JPL is so big, and there was never any question but that JPL would be the agent to carry out this mission. But within JPL, every sub-unit had to make its own case, so that I had to go back to Washington and talk about the work we were doing and why NASA should allocate to our biological group all the money it asked for and deprive others. NASA had decided it was going to have its own biological laboratory up at the Ames Research Center. They had an enormous group of people up there, a very big laboratory and very well equipped, so they always competed with us for the pool of money within NASA. I was very critical about some of the things NASA was supporting. I mean, all those terrestrial experiments being designed for Mars! They actually appointed a committee of scientists to select the Viking biological experiments from submitted proposals. Well, NASA knew what all the submitted proposals were, because they were supporting the development of all those things. I guess I know how they put it over on those scientists. The people were all competent biochemists, professors at universities and NIH; but they knew nothing whatsoever about Mars. In those days, if you talked to someone about looking for life on Mars, it just seemed the most natural thing in the world to send a medium with a yeast extract, an aqueous medium, and plant it down on Mars and put some Martian soil in it and watch everything grow. I mean, it never occurred to them that this was totally inappropriate.

Prud'homme: Can you describe some of your colleagues there? Bruce Murray, for example?

Horowitz: Well, Bruce wasn't at JPL when I was there; he was down here.

Prud'homme: Hubbard?

Horowitz: Yes. Jerry Hubbard was someone I brought in. He was a microbiologist from the University of Texas. We needed a good microbiologist, and I asked a friend of mine down there, Jack Foster, to recommend someone. He recommended Jerry. Jerry came; he was a very well trained laboratory microbiologist. He's now a professor at Georgia Tech. And George Hobby had been at JPL before I went there. He's an old JPL biologist. He and Jerry did all the laboratory work connected with our experiment on Viking. You have to get a background of experience so that when something comes down from Mars, you don't have to sit around and decide what it means. You have to be able to react automatically.

Prud'homme: Did you get to know Sagan at all?

Horowitz: Oh, yes. In fact, I got Melvin Calvin to appoint Carl Sagan to the Bio-Science Advisory Committee. Calvin was first chairman. It may not have had that name, but it was the first NASA Biology Advisory Committee. I've forgotten where I met Carl, but I can remember Carl visiting me in this office.

Prud'homme: "Bio-Sciences Committee of NASA" is what I have down. Then there was the Extraterrestrial Biology Committee of the Space Sciences Board.

Horowitz: Well, maybe that was it. Anyway, Carl was then a postdoctoral fellow, I think, at Berkeley. And Melvin Calvin, who's a professor at Berkeley, was chairman of the Committee. And I suggested to Calvin that he appoint Carl, which he did. Carl really took off. He had an awful lot to do with NASA's plans for Mars after that. He was very influential in getting this spacecraft sterilization program. Lederberg was also deeply involved in it at that time. And he fully accepted all of that, too. I think he was hypnotized by Carl.

Prud'homme: He has that power, I gather.

Horowitz: Oh, yes, Carl is very hypnotic.

Prud'homme: What was the environment like at JPL?

Horowitz: I enjoyed it. Of course, I was in a very special position, because I didn't depend on JPL; I was really a Caltech professor taking some time off.

Prud'homme: Did NASA pay you, or did Caltech continue to pay you?

Horowitz: Well, half of my salary came from Caltech and half from JPL.

Prud'homme: Did the graduate students have any opportunity to work at JPL?

Horowitz: Well, graduate students in geology did, but there really weren't any problems at JPL for biologists, at least there weren't in my time. There may be a little now. The only thing that we had graduate students doing was going on those field trips to the Antarctic. But as for doing thesis work, there was nothing at JPL.

Prud'homme: So the ties between the campus and JPL were fairly limited.

Horowitz: At least in biology. I'm the only member of this division who got really deeply involved with JPL. James Bonner had an interest, and he did have an association with one of the people in the bio-science section but only in connection with an orbital experiment. I'm the only one that really got involved in the planetary exploration program. For many schools, what was going on at JPL would have been fine for a Ph.D. thesis in biology, but not at Caltech. Here, if you aren't doing something very fundamental and very important, you don't get a Ph.D. The kind of work, say, involved in doing desert microbiology, for example, doesn't hold enough interest for anyone here to have a graduate student do a Ph.D. thesis on. There are schools where you could, but not at Caltech. There are some things going on now at JPL that I could imagine a graduate student from Caltech getting involved in for a thesis. There has been sort of a new birth in biology at JPL in connection with their energy program. JPL wanted to get some money from the Department of Energy, and biological sources of energy is something there's an interest in. And they did start a program there, one of their good ones, involving microbial production of methane. Some good basic biology is going on in connection with that, but it hasn't expanded at all. If that work hadn't started off so well, I think they probably would have stopped it by now, because they don't depend on the Department of Energy so much. They're getting as much as they need from the Department of Defense and NASA.

I enjoyed my years there, even though I did have to go to more meetings than I would have liked.

Prud'homme: Did you continue teaching here?

Horowitz: I gave my course, one quarter per year; and I kept my lab going. In the first place, I had students and I had technicians. I wasn't going to just say, "Well, goodbye, I'm going to JPL for a few years." And I didn't know whether the darn thing would fly. You never knew . . . The NASA budget had to be approved every year. It's not as if Congress gave NASA a billion dollars--which is what it eventually cost to fly Viking. They gave them a $100 million dollars a year for ten years. And there was always that uncertainty, so I never did give up my lab here, or the course. But I will say I put most of my thoughts and energy up at JPL. But I came down here almost every day.

Prud'homme: Did you notice any change in the students in the sixties?

Horowitz: There wasn't any at JPL, and I wasn't here all that much to say what was going on here. I know that it had a big effect on my daughter, who was at Berkeley, not Caltech. I know that something went on at Caltech, but it didn't touch me.

Prud'homme: Let me get back to your Antarctic program. That started out, really, in conjunction with your JPL work.

Horowitz: Right. I think it was almost the first thing I did when I went up there. I talked to Roy Cameron, and he liked the idea. The program in the Antarctic was run by the National Science Foundation. The logistics were all Navy: the Navy took people in and fed them and flew them around to their field stations, and so on. It turned out to be a big success. Roy brought back tons of soil. It's still being stored up at the Ames Research Center. They have a big freezer up there with all these soil samples. They were used for a long time as standards during the testing of the Viking instruments.

Prud'homme: Is work still being done there?

Horowitz: At Ames? Yes, it is. One of their main programs is the origin of life, pre-biotic chemistry. I think they're still talking about the possibility of life on the planets, but I think it's really this cynical attitude. I think they feel that their existence is more certain if they take the position that there's still a possibility of finding life elsewhere in the solar system.

Prud'homme: What do you think is going to happen to the space program? The exploration of planets and so on?

Horowitz: I think it has slowed down enormously. The only really exciting thing that I can think of now that's coming up is the Galileo mission. But it's slowed down. I think the shuttle is a great mistake. It's taken all the funding that should have gone to planetary exploration.

Prud'homme: It seems such a pity to develop an institution such as JPL, which is set to do this and has a certain momentum, and then to divert it into another channel that's less academic and more commercial.

Horowitz: Yes, it is a disappointment.

Prud'homme: What did you work on when you came back?

Horowitz: Well, when I came back, I became an executive officer of the division, so part of my working days were again administrative. But <u>the</u> thing, the new thing that I started as a result of my interest in the space program and as a result of reading about the Antarctic and of

Roy's first years down there and seeing what came out of there, was a program in biological water requirements that NASA supported. The original idea was to see if we could find mutations in Neurospora that would enable it to live with less water. First we had to find out how much water it needed and then devise ways to look for mutants that could live with less water. In the course of this, we discovered some interesting things. We discovered that when you lower the water activity--that is, lower the water concentration in the medium--some essential growth factor is lost from the spores. So we now had two problems. One was, could we get mutants that would grow with less water, and if so, how far could we push this? There were clear limitations in nature. If you look in nature, you'll find that there appear to be real limits beyond which no species has discovered how to live in a dry environment. So that was one problem. Then the other one was to try to isolate and identify this mysterious growth factor that was lost under the condition of low water activity.

Well, we never did find any mutants that could live on less water, and that ended. We put a lot of effort into that.

The other problem we did succeed in solving. The growth factor turned out to be three different factors, all related, and they turned out to be quite interesting compounds. They're cyclic peptides; they're involved in the uptake of iron from the outside medium. And this got us into iron metabolism and iron uptake. The whole question of biological iron requirements is quite interesting. Iron is essential for almost all cells. Although there's a lot of iron on the surface of the earth, it's very hard to get any of it because it's so insoluble; it's found as iron oxides, and these are extremely insoluble. So all organisms produce organic compounds that chelate iron. And there's an enormous encyclopedia of these things. If you lower the water activity, Neurospora loses these elegant compounds it synthesizes that can solubilize iron. They're quite marvelous chelating agents, and very specific. But when they are lost from the cell, then the cell can't germinate. We got the idea that maybe this is an alarm response, that this is selected for by natural selection to prevent germination under unfavorable conditions. We got deeply involved in this when we moved over to Aspergillus and Penicillium and found that the same thing happened in these other species. But every fungus has its own private set of these chelating agents. They all need iron, and they all produce chelating agents of one kind or another. But then, it's also beneficial to be able to use the chelating agents made by other species.

Begin Tape 2, Side 2

Horowitz: All species need iron. It's very hard to acquire, and they compete for it in the sense that, if you're making a chelating agent for iron and secreting it into a medium, and I can take up your iron chelating agent from the medium, then I can save myself some trouble. But obviously, then, your response will be to make a different one that I haven't learned how to use. So it turns out that among these fungi there is a whole set of private chelating agents. They're called siderophores--"sidero" being Greek for "iron" and "phores" Greek for "carriers." Every species has its own set of siderophores. And the advantage of this seems to be obvious, that you don't want anyone else to be using the siderophore that you've gone to all the trouble to make. And they produce enormous quantities of these things, especially fungi grown in culture. Sometimes they'll secrete more siderophores into the medium than the dry weight of the culture. It's very important to get iron. There are a lot of fascinating aspects of iron metabolism that you never think about; I'm sure it's true of any other metabolism, too. You don't think about it until you get deeply into it. So that kept me busy even after the failure to make any progress on the water requirement. This iron thing was quite fascinating, and that was one of the last things that I worked on before I retired.

Prud'homme: You were also executive officer for the biology division when Sinsheimer was chairman, between 1970 and 1975. What did you do as executive officer? You were more or less chairman . . .

Horowitz: I was acting chairman when he was on leave for one year. Oh, gosh, there are an awful lot of things that a division the size of biology has to do. We have courses; and new appointments; and applications for funds; and going to meetings of the Institute

Administrative Council, the IAC; and keeping the library going: what journals you're going to keep and which ones you're going to cancel, and what to do when the price of journals go up. Endless detail. And then we were expanding in the neuroscience area, so there were building plans--the Beckman Laboratory--and new appointments, as I mentioned, and people to interview all the time.

Prud'homme: It doesn't sound as though you enjoyed it very much.

Horowitz: I tolerated it. I mean, I accepted the chairmanship when Sinsheimer left; I agreed to be chairman. It wasn't intolerable; but it certainly was not as much fun. But then I had been out of the lab for so long. After I left the chairmanship, I did try to come back to the lab; I guess I retired from the chairmanship in 1980, and I didn't retire from the faculty until '82. And in those two years, I did try, but I couldn't do it. My wife had had a terrible stroke. I don't know whether I would have been able to do it even if she had been well.

Prud'homme: Why do you say that?

Horowitz: Well, because I had been away for so long, and genetics had moved so far from where it was when I left it in 1965.

Prud'homme: Sounds as though it's just sort of exploded.

Horowitz: That's right. A few months ago, we had a memorial service for Dr. Borsook. I organized it actually. And one of the people who was here and gave a talk was Kenneth Pagen--he's a professor of biochemistry at Berkeley. Pagen was one of Borsook's graduate students. He's quite a bit younger than I am, and he's now working in molecular genetics. And I asked him if he'd had a hard time getting himself educated in this subject, and he said that it took him two years after he started working in this field to really feel comfortable. I was just too old to start, so I finally decided I would retire.

Prud'homme: Tell me about Sinsheimer; what kind of a person is he?

Horowitz: Sinsheimer is a very intelligent man. He's very courageous. He doesn't mind speaking out about his ideas, even though they're often unusual and not accepted, especially by his scientific colleagues. But he's also a very private sort of person. He's not someone who's easy to talk to. He tends to figure things out for himself without discussing them with other people. He makes decisions and when he tries to put them into effect, it often gets him into trouble. He's quite an isolated, I would say a lonely, type of person; I don't think he feels lonely, but that's the way he is. You don't just go in and sit down and chatter with Bob Sinsheimer. He writes very well. He seems to be in an awful lot of trouble now as chancellor at UC Santa Cruz because of his lack of political skills. I mean, a man in the position he's in has to know how to get people to go along with him, and Bob is just inept at this. So he's got himself in a lot of trouble that probably he could have avoided if he'd had the right skills. I still see him from time to time. When there's a University of California board meeting down here he often comes over to Caltech and drops in. But I don't envy him. [Laughter]

Prud'homme: It's a terrible job, being the head of a college.

Horowitz: Yes, especially if the college is a small one that needs money. And Bob is a scientist, and the tradition at Santa Cruz is not scientific.

Prud'homme: On the subject, can you compare DuBridge, Brown and Goldberger for me, as presidents?

Horowitz: Well, everybody loved DuBridge, as far as I can tell. He was extremely popular, and still is. Whenever he shows up he always has a crowd around him, shaking his hand and wishing him well. When Brown came in, it was quite controversial because he had been Secretary of the Air Force during the Vietnam War. And there were really serious debates among the faculty whether he should be invited to be president of Caltech.

Prud'homme: Did the faculty have much say in whether he was?

Horowitz: Well, I don't know whether they had a formal say. I do remember that there was enough question about him so that Christy--I guess it was Christy who was Acting President at the time--arranged to have meetings in the different divisions for Brown to go to and explain himself. I remember his coming over to biology for people to find out what he was like and how he responded.

Prud'homme: Was he successful at that?

Horowitz: Well, he was successful enough so that he was appointed. And I got to like him. I knew him when I became executive officer and especially the year I became acting chairman and later as chairman. I remember, the first thing he did when he came in, he really started a controversy. I guess a new president wants to do something to announce his presence. Like DuBridge increased faculty salaries the day after he got in. [Laughter] Harold Brown got the idea from somebody--I don't know from whom--that Caltech needed women students. And the quick and easy way to get them was to join forces with Immaculate Heart College. Have you heard about this?

Prud'homme: No, I hadn't heard that.

Horowitz: Oh, it's historic. [Laughter] Immaculate Heart is a small Catholic girls school over in Hollywood. It's amazing how much support he had for this among the faculty. I know Christy supported it, . . . But of all the schools to buy! Some of us were just absolutely outraged. I can remember spending a day over there with a group of other faculty interviewing the president of Immaculate Heart and some of the teachers to find out what kinds of people they were. Anyway, it turned out to be sufficiently unpopular for Harold to realize it wouldn't fly, although he did have much more support than I would have ever dreamed possible. It's just amazing to me, how naive some of our colleagues are. Anyway, I thought that was a bad sign. But he's very smart. I like him, and I don't want to criticize him, but he was never a Lee DuBridge. I think basically Harold is a problem-solver; he's a technician. I can't think of anything he especially did for Caltech, except to run it. He's extremely smart, but I don't think he has vision. And I think that's why he didn't mind being Secretary of the Air Force during the Vietnam War, or later Secretary of Defense. Because for him, these are socially useful activities, or at least socially approved activites, that need smart people to solve problems, and he's good at that.

Goldberger is more like DuBridge; he's very personable and easy to get along with. I must say I like him very much. He's gotten himself into a few tight spots. I think the trouble he's gotten into is that he says yes too readily. He's very informal; he's really not built to be in the kind of position he's in, where you have to be careful of what you say. Now Harold was extremely careful. And I think Lee was careful, too, about what he said. He didn't often get himself into trouble.

Prud'homme: But you probably weren't aware that he was being careful, whereas with Brown you were.

Horowitz: That's right. Brown just wasn't as talkative; and he isn't as well-spoken as Lee or as Murph Goldberger either, for that matter. But Murph tends to ignore the institutions that are built up around him to make decisions and give him advice and not make missteps. He tends to say yes to whoever goes into his office and talks to him, because he's a nice guy. He's much more of a professor than an administrator; I think that's why he gets into trouble. But I like him very much, and his attitude on disarmament is extremely good, in spite of this trouble that he's had over the Arroyo center and this latest . . . .

Prud'homme: He's not lacking in courage, which is nice.

Horowitz: He's courageous. He's probably a little too relaxed in going ahead with things, so he should be more careful.

Prud'homme: How much would you say the biology department has changed in the last twenty years? And what have been its ups and downs?

Horowitz: Well, the big change is the expansion in neuroscience. And the changes in genetics--molecular genetics is now ruling the roost. Molecular genetics and neuroscience are the two big things going on now.

Prud'homme: Did Caltech ever want a medical school?

Horowitz: Oh, yes, there was a move to have a medical school, and the present chairman of biology, Lee Hood, was very actively involved in that. It was during Sinsheimer's chairmanship. Sinsheimer, I believe, was very much in favor of it. But there was a lot of opposition from the biology faculty to it.

Prud'homme: Why?

Horowitz: Well, because it departed from the tradition of Caltech as a school for basic science. And medical schools are cannibals when it comes to money. They consume all the money. They require enormous faculties, and you need nurses and hospitals and technicians--a vast support system. And it would be a distraction. I remember Max Delbrück was strongly opposed to it. I would say most of the faculty were against it. Lee [Hood] was in favor of it and Sinsheimer was in favor of it. That's all I can remember now, but it was an issue. . . Let's see, there was at least one trustee--it might have been Norton Simon--who funded a study that made it possible for the faculty to invite people in and talk to them on this subject, get advice, and so on. But eventually it petered out.

Prud'homme: What do you think of the current state of the Institute? And where do you think it should go?

Horowitz: I think it's fine the way it is. I think as long as it stays on top of the most exciting developments in science, I will be happy with it.

Prud'homme: You don't feel that it slid in prestige?

Horowitz: No, not at all, I don't think so. I think as long as it maintains high standards for the admission of students and high standards for appointment of new faculty we will be fine. . . The questions are complex: things like should we have a medical school, and there was also a move afoot to have a psychology department--Roger Sperry was involved in that and Murray Gell-Mann. And there was a group of rebels in the faculty that thought that psychobiology was such a unique and separate field of science that it deserved its own division. And there was also a move to expand the humanities. Max Delbruck was very strong on that; he thought we should have more humanities, anthropology, especially. I looked at all these things over the years, and I've never been convinced that Caltech would be better with them. Well, I'm sure that in the case of psychobiology, it would have been a terrible error, because psychobiologists who become divorced from basic science tend to become mystical. I think the great development of the future in biology is going to be the absorption of psychobiology into molecular biology. I think that to build a bridge between molecular biology or basic chemical biology on the one hand and psychobiology on the other is the great goal for the future of this division. I think that's where this division should focus. It'll be very important when it comes.

Prud'homme: You just finished a book.

Horowitz: I'm finishing the book. I actually have drafts of all the chapters now.

Prud'homme: What's it about?

Horowitz: It's about the search for life in the solar system. And Mars is the main character. I have a publisher, W. H. Freeman. I should be working on the glossary, which is easy, but doing the illustrations--I have some ideas for illustrations--I don't look forward to that. But the basic work is done. My contract is to have it finished by the end of this year, and I'm sure I'll meet that deadline.

Prud'homme: You have received many honors and awards. You're a member of the National Academy of Sciences. You received a NASA Public Service medal.

Horowitz: Yes. And the American Academy of Arts and Sciences.

Prud'homme: Do any of them mean anything special to you?

Horowitz: Well, I did enjoy getting into the National Academy of Sciences. When my wife was able to go with me, I used to enjoy going to the meetings; she enjoyed it as much as I did. Socially, they're wonderful. [Laughter] You meet all your old friends and have a good time. But I haven't been to an Academy meeting since she fell sick.

Prud'homme: What are you most proud of in your work?

Horowitz: Well, I think I'm most proud of two or three things. The Neurospora work that I did at Stanford in the early forties--or let's say through the forties, first at Stanford and then down here--that put the underpinnings in the one gene, one enzyme theory, I thought was important. Incidentally, the question is often raised whether it was my theory or Beadle's theory. I believe it was Beadle's. In fact, I think he used the words one gene, one enzyme first. But Dan Kevles has been asking me this now for a year, and I can't find any proof that Beadle said it first. And Dan says that it first appears in something that I wrote. He's been through all the papers and I accept his word for it, but I'm sure the idea came from Beadle.

Prud'homme: Well, you're very gracious to give him credit.

Horowitz: But the idea was so appealing to me and so obviously correct that it really unleashed me. I needed something like that to give some direction to my laboratory work. It was a marvelously stimulating idea for me.

Prud'homme: It must have been wonderful to have something like that and know that it's right and know that that's what you wanted.

Horowitz: But no one else agreed. [Laughter] But anyway, I think that was scientifically the most important thing I've been involved with in my career.

And then I think the Mars exploration is quite important. The conclusion of this book that I've nearly finished is that the only inhabited planet in this solar system is Earth, that there is no other life in the solar system. And this is all explained and put down in language that I hope is comprehensible to the general reader with an interest in science. I think this is a really important conclusion.

Prud'homme: Because it's a myth that dies hard.

Horowitz: It's not only that, but beyond that. If we are the only inhabited planet in the solar system, and there's only one form of life on Earth--I mean, when you look at the composition of living creatures and see that they all have the same genetic system and they all operate on DNA and proteins composed of the same amino acids with the same genetic code, it's clear there's only one form of life--then we're all related. The origin of life may have happened only once, and it happened here and no place else in the solar system. Or if it happened elsewhere, it didn't survive. I think this is a conclusion of really cosmic importance. If people become aware of this, then maybe they'll be less inclined to destroy the planet.

Prud'homme: . . . Might assume some responsibility for themselves.

Horowitz: Right. So I think when it sinks in, when people become conscious of the fact that on all the planets around us, there's no other life, and that the Earth is really unique. Most importantly, we're the only planet that has liquid water on its surface--that's the real key, I think. They may look at one another differently--not only treat the planet differently, but think of their foreign policies in a different way. So I think that is important. But this was less a personal accomplishment than the Neurospora experiments, where I was doing a lot with my own hands. But of course, even in the Neurospora work, I was by no means the only one; there were other people, too, very important.

I've enjoyed writing the book, I must say. And another reason I wrote the book was not only to tell the story of the Mars explorations, although the history of this is so fascinating--I mean, there's nothing that I know of in modern science like the history and ideas about Mars because they're so crazy. But aside from this, there's so much other irrationality, there's such a strong tendency toward irrationality in the world today. I sound like an old man complaining, but when you look at creationism in our country and fundamentalist religion everywhere, all over the world, and all these horrible religious wars, these bloodlettings over ridiculous religious convictions in the Middle East--the birthplace of religions--and Northern Ireland. . . . And the whole idea of exploring the planets to look for other life is a fundamentally rational kind of activity. And the attacks against the idea of evolution--I mean, our president is a self-declared creationist. I just felt that it was an added stimulus to write this book, to point out in language that I hope can be understood by educated people why it's absurd to think that evolution has not occurred or that the Bible can be taken as literal truth. The book I've written is very rational. There are two chapters on the origin of life. So the whole thing enabled me to say a lot of things I feel very strongly about.