



HERSCHEL K. MITCHELL
(1913-2000)

INTERVIEWED BY
SHIRLEY K. COHEN

December 3 and 22, 1997

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Pasadena, California



Subject area

Biology

Abstract

Interview in 1997 with Herschel K. Mitchell, professor of biology, emeritus. George W. Beadle brought Mitchell to Caltech with him in 1946 from Stanford as a senior research fellow, along with Norman Horowitz, Mary Houlahan, Adrian Srb, and August Doermann. The group worked on *Neurospora*. Mitchell recalls teaching the biochemistry course with Henry Borsook; recalls Beadle's style as chairman of the Division of Biology. Recalls his earlier work on pantothenic acid and folic acid as a graduate student with Roger Williams. Comments extensively (in mid-interview and again toward the end) on the dubious work done by Lawrence Burton and Frank Friedman as research fellows in the mid-1950s, their consequent dismissal from Caltech, and their later careers in highly controversial immuno-augmentative cancer therapy. Recalls instituting athletic activities at Caltech for graduate students and refers to many of his successful PhD students—among them Bruce Ames, who invented the Ames test for detecting mutagens and potential carcinogens; Mogens Westergaard, with whom he devised a medium favoring sexual reproduction in *Neurospora*; and Ernst Hadorn, with whom he worked on *Drosophila* mutants.

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Norman Horowitz and Marguerite Fling observe a chromatopile in a Caltech biology lab, circa 1950. Invented by Herschel K. Mitchell, the chromatopile was a cross between earlier glass column and paper chromatography methods. The apparatus contains a large stack, or pile, of compressed filter papers. The top few layers are impregnated with the solution of the substance to be resolved, and the solvent is poured down through a hole above. As the solvent works its way down, the component zones appear on the paper as the chromatogram.

California Institute of Technology

Oral History Project

Interview with Herschel K. Mitchell

by Shirley K. Cohen

Pasadena, California

Caltech Archives, 2000

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Herschel K. Mitchell
Pasadena, California

By Shirley K. Cohen

Session 1

December 3, 1997

Session 2

December 22, 1997

Begin Tape 1, Side 1

Cohen: Good morning, Dr. Mitchell. I'm delighted to have you here with us. You are going to tell us a little bit about your family background and then we'll get into your work here at Caltech. So, do you want to tell us a little bit about your early education?

Mitchell: I went to Pomona College and majored in chemistry. I then did graduate work in the chemistry department at Berkeley, and later at Oregon State University [Oregon State University is the university's present name; before 1961, it was known as Oregon Agricultural College.—Ed.].

Cohen: Where did you get your PhD?

Mitchell: The PhD was from the University of Texas at Austin.

Cohen: Was all this in chemistry?

Mitchell: Yes.

Cohen: And then what did you do after that?

Mitchell: I was a research fellow at Stanford with George Beadle and his group there.

Cohen: Now, you went from Texas to Stanford?

Mitchell: Yes.

Cohen: Were you ever a student at Caltech before then?

Mitchell: No, only a visitor.

Cohen: Oh, I see. So you hadn't been at Caltech before as a student?

Mitchell: No, I had never been here as either a research fellow or student.

Cohen: I see. So your first real experience with George Beadle was at Stanford?

Mitchell: Yes, although it was George Beadle who invited me to come here, along with Norman Horowitz and Mary Houlahan.

Cohen: Now, was Mary Houlahan a student?

Mitchell: She was a stock-keeper for *Neurospora*.

Cohen: *Neurospora*—that's a mold, isn't it?

Mitchell: That's a fungus, yes.

Cohen: And so the four of you came down to Caltech?

Mitchell: There were two graduate students who came also: Adrian Srb and August Doermann.

Cohen: Was this the whole group then, when you came?

Mitchell: Yes.

Cohen: And no one from Caltech joined this group?

Mitchell: Sterling Emerson worked on *Neurospora* also.

Cohen: Was he here already?

Mitchell: Yes.

Cohen: Could you tell us a little bit about this work that you did with Emerson?

Mitchell: He had, along with Ed [Edward L.] Tatum, proposed the “one gene, one enzyme” hypothesis, which later was changed to “one gene, one protein,” a hypothesis which was correct then and is still correct now.

Cohen: And whose idea was that?

Mitchell: I’m not sure whose idea it was. I think Norman Horowitz had some part in it. He was a confidant of George Beadle’s.

Cohen: Do you mean that Norman Horowitz was closer to George Beadle than the rest of the group?

Mitchell: That’s right, yes.

Cohen: Would you say it was a partnership, or a symbiosis, or some other thing?

Mitchell: A symbiosis, I would put it. When the group left Stanford, Tatum and David Bonner went to Yale. The rest of us came down here.

Cohen: That must have pretty well depleted the group at Stanford.

Mitchell: It did, totally.

Cohen: Was there some problem at Stanford that caused this group to leave?

Mitchell: No. Beadle received an offer to come here.

Cohen: And he felt it was more attractive than what he had?

Mitchell: Yes, I think that was the case.

Cohen: And he just asked certain people to come with him?

Mitchell: That's right.

Cohen: But he didn't ask these other people who went to Yale to come—David Bonner and Ed Tatum.

Mitchell: No.

Cohen: What building were you in when you came? Where did you set up the operation?

Mitchell: In Kerckhoff—I had a laboratory there. We continued with the same kind of work that we had been doing at Stanford.

Cohen: Did you have many students?

Mitchell: I had quite a few. There were thirty-six altogether over the years that I was here, which totals fifty years.

Cohen: That's a long time. Of those thirty-six students, do you remember some of them as outstanding?

Mitchell: Oh yes, there were several. One that everybody knows about is Bruce Ames.

Cohen: He didn't stay here at Caltech?

Mitchell: No, he's at Berkeley.

Cohen: Why was he so notable?

Mitchell: He's the one who devised the Ames test for carcinogenesis.

Cohen: When you moved out here to Pasadena, had you lived in Southern California before?

Mitchell: Oh yes, I was born in Southern California.

Cohen: That's right, of course. You went to Pomona College.

Mitchell: Yes. I had been to the campus here a few times, for American Chemical Society meetings.

Cohen: So this was a familiar place to you?

Mitchell: Yes.

Cohen: How did you feel about the move to Caltech?

Mitchell: I thought it was a good move. I wasn't invited as a faculty member, though—I was still a research fellow when I first came to Caltech.

Cohen: When did you get asked to be on the faculty?

Mitchell: After about a year.

Cohen: I see—it didn't take very long. Could you tell me a little bit about how the group actually worked, that is, the interaction of the group with George Beadle and Norman Horowitz?

Mitchell: We were all asked to do our own business, pretty much. George Beadle epitomized the phrase: "You can take the boy out of the country, but you can't take the country out of the boy." I think every day of his adult life he walked through a corn patch in his mind.

Cohen: Does that mean that every day was a source of wonder for him?

Mitchell: Yes, I think so.

Cohen: Now, about your work here at Caltech: when did you start working on vitamins? I read your articles in *Engineering and Science* about your harvesting great fields of spinach leaves.

Mitchell: As a graduate student at Oregon State University I worked on pantothenic acid and solved its chemical structure.

Cohen: Did you do that as a student?

Mitchell: As a student, yes.

Cohen: When you came here did you start working with Henry Borsook right away? How did that come about?

Mitchell: Well, as soon as I became a faculty member I was told that I should arrange to create a laboratory course in biochemistry. So I consulted with Henry Borsook, of course. I gave a few lectures in the biochemistry course, but he gave most of them.

Cohen: So that was a new course?

Mitchell: The laboratory course was. Beadle gave me about a week's notice, and I did it.

Cohen: How many students did you have in that class? Was it a graduate course?

Mitchell: There were both undergraduates and graduates. The laboratory space was suitable for fifteen students. A couple of years later I had sixty.

Cohen: It must have been a very attractive course.

Mitchell: It was to people who didn't stay in physics.

Cohen: You mean, people who were in physics suddenly wanted to see what chemistry or biochemistry was like?

Mitchell: I think that's what the situation was. Besides, there were not so many positions available in physics, I think. There were more in medical science.

Cohen: Oh, so you think it was a practical move for those students. They realized that if they couldn't get a job as a physicist they would have some chance as a biochemist or as some professional in the health sciences?

Mitchell: Yes, exactly.

Cohen: I see. That's very interesting. What sparked your interest in the vitamins? Or did you just naturally get into it?

Mitchell: Well, it was accidental in a way. I read a paper by Roger Williams, who was at Oregon State College, on pantothenic acid and the isolation of it from sheep liver. In the paper he said that they had only very small amounts of the acid and they didn't know much about its chemistry. At the time I was working with Paul Kirk in microchemistry at Berkeley. And I thought that this would be a good direction for me to go in, because they didn't have much material on it. So I wrote to Dr. Williams, and he took me on as a student. I was able to solve the problem by using microchemistry.

Cohen: Very good. So then what took you down to Texas?

Mitchell: Well, Roger Williams moved to the University of Texas at Austin, and the university established a biochemical institute within the chemistry department. Before going to Texas, I went to an American company in Rahway, New Jersey, because they were interested in production of pantothenic acid and I had done the chemistry. Then I went to Texas. There I was handed a culture of lactic acid bacteria—one of them, there are many—and I was told that this bacterium required something from a yeast extract or liver extract in order to grow. I was asked to find out what the material was that it required. That turned out to be folic acid.

Cohen: So you had to find a source of folic acid.

Mitchell: Yes. I immediately went to the grocery store and sampled a little bit of everything. It turned out that spinach was the best source, and that's a good thing, because in Texas they raise lots of spinach. So I isolated folic acid from spinach.

Cohen: Then you went from Texas to Stanford. That was essentially your first professional job after your PhD.

Mitchell: Yes.

Cohen: And you immediately went into George Beadle's group, is that correct?

Mitchell: Yes. I had known a man in the poultry department at Berkeley who knew I had worked with pantothenic acid, and that's something chickens need to thrive. He knew that I had worked with pantothenic acid at Berkeley. He suggested that I might be interested in the work that was being done by Beadle's group at Stanford. So I went to Stanford to see George Beadle. Beadle gave me a tour of the place and was very nice. I liked the whole set-up, so I asked if there was a possibility of working there. He gave me half a fellowship.

Cohen: How did you manage the rest of your support?

Mitchell: Well, he gave me a full fellowship before long, but at the time I thought I would support myself and my wife by working in the glassblowing shop, as I had done when I first went to Berkeley.

Cohen: Ah, so you are a glassblower also.

Mitchell: Yes. I wasn't much good when I started, but I learned, after the first thousand distilling flasks I made.

Cohen: You learned how.

Mitchell: I learned how, yes. At Oregon and Texas I did some special glassblowing for various people.

Cohen: Well, that's a good skill. To get back to Caltech, George Beadle was then here with his group. How many years was George Beadle here before he left to go to Chicago?

Mitchell: Oh, it must have been fifteen years.

Cohen: So he made a real contribution here.

Mitchell: Oh yes. He was a good chairman.

Cohen: Why do you think he went to Chicago?

Mitchell: He was a very ambitious person and he thought that was moving up.

Cohen: What was his position in Chicago?

Mitchell: He was chancellor of the University of Chicago.

Cohen: Well, in some sense that was moving up.

Mitchell: Yes.

Cohen: Did he have strong ideas about education? Was that one of the reasons that attracted him to the job at the University of Chicago?

Mitchell: That I do not know. I think it was just strictly a personal matter. He was an ambitious fellow.

Cohen: Do you think his second wife had any influence over his move to Chicago?

Mitchell: I don't know that she did. She was also an ambitious person. She was in the same class at Pomona College as I was.

Cohen: Is that right? So you knew her for a long time.

Mitchell: I had known her some in college.

Cohen: I've talked a little bit to David Beadle [son of George Beadle by first marriage—Ed.], and he speaks very highly of her. He said that when they were married George Beadle's personality really came out. Did you find that?

Mitchell: I think she helped him, yes.

Cohen: He became much more—I don't know if "sociable" is the right word.

Mitchell: He was a much better speaker.

Cohen: After he got married?

Mitchell: Yes. I think he took some elocution lessons.

Cohen: And you think that was her influence on him?

Mitchell: I think that was her influence, yes.

Cohen: How about his work with the group? Did that become any more harmonious or anything?

Mitchell: No. That was the same. Each of us worked independently, so it really didn't affect us.

Cohen: So then after he left what happened with the group? Did his departure diminish the quality of work that was going on?

Mitchell: Well, we all continued on our own, doing our own work.

Cohen: So, at that time who held the group together? Who was your division chairman then?

Mitchell: Ray Owen was chairman for a while. Then Horowitz was chairman for a while. [Robert Sinsheimer succeeded Owen as chairman of the Division of Biology, 1968-1973. In 1973 Norman Horowitz became acting chairman and then chairman 1977-1980.—Ed.]

Cohen: But everybody just continued to do their own work?

Mitchell: That's right. Each of us had our own grant money.

Cohen: I understand you were quite successful at getting grants. You worked with Dr. Carl Niemann?

Mitchell: We had some collaboration, yes. He was a very fine fellow.

Cohen: I see you got an NSF [National Science Foundation] grant in 1956 for research on phospholipids.

Mitchell: Yes.

Cohen: In '57 you were working on vitamins. Is that when you were working with Henry Borsook?

Mitchell: Yes.

Cohen: Okay. You might say a little bit about mini-flies and substances that alter the genetic controls.

Mitchell: Well, the mini-fly business was about bee venom. One of the two proteins that are in bee venom, if injected into *Drosophila* larvae caused the larvae to continue to develop all right until they became flies, but they were only half the size of normal flies.

Cohen: Oh, so that really stunted their growth in some way.

Mitchell: Yes, but this had already been done really, in a sense, by Beadle and [C. W.] Clancy.

Cohen: Clancy?

Mitchell: He was a student here, I think. [C. W. Clancy was a research fellow at Caltech 1934-1936.—Ed.] They had tried to starve flies. They found out that if they didn't feed them they would go ahead and develop all right, but they would be small.

Cohen: And you never found out why?

Mitchell: No. And I don't know what the bee venom did. It didn't just paralyze them.

Cohen: Because they continued to develop?

Mitchell: They continued to develop.

Cohen: What was the purpose of this research? Why did you want smaller flies?

Mitchell: We didn't have any reason for wanting smaller flies—it was just a matter of curiosity. At the time I had the injection apparatus, which was easy to use. This came out of a mistake that George Beadle made, in my opinion. He brought to Caltech two fellows from New York University who had claimed that they had been able to transfer a tumor-inducing substance in one stock of flies to another stock, which would then get tumors. He brought those two guys here to Caltech as research fellows.

Cohen: Do you remember their names?

Mitchell: Yes, [Lawrence] Burton and [Frank] Friedman. They wanted to purify this substance and they asked me to advise them on how to do it, and I did. The trouble was that the whole thing was a fraud.

Cohen: You mean, they really couldn't do that?

Mitchell: Right. They made it all up.

Cohen: Why did they do that?

Mitchell: Just to make a splash and get some money, I guess.

Cohen: So what happened with these guys?

Mitchell: Well, they worked here, and I advised them. But after a while I got suspicious of them, because everything I told them to do worked.

Cohen: They told you it worked?

Mitchell: Yes.

Cohen: You didn't go into their labs to see it?

Mitchell: Eventually I went into the lab to see it. And I observed one thing at once that was totally revealing. They presumably did controls on all their experiments by injecting a salt solution instead of this preparation. Well, I knew from experience that whenever you stick a needle in larvae there will be a black spot which develops within an hour or so, and it will appear on the pupal case. I found that in their controls the larvae had no scars from the injection.

Cohen: Did they ever publish any of this?

Mitchell: They did, and they put my name on it.

Cohen: How about Beadle's name?

Mitchell: No, they did not put his name on it. I had a letter not long ago from Linus Pauling, not long before he died, asking about this, and I told him what had happened.

Cohen: Did you publish anything saying that this was a fraud?

Mitchell: I did, yes.

Cohen: Because that's not nice for fraud to go on at Caltech.

Mitchell: I wrote an article for *Science*, and it was published.

Cohen: Do you recall what year that was?

Mitchell: Well, I know I wrote a letter to the editor of *Science* while I was in Switzerland, which was in 1959.

Cohen: So did you tell George Beadle this? I mean, how did it finally play out?

Mitchell: Beadle and Renato Dulbecco prepared some coded samples for them to inject. And Burton and Friedman couldn't tell the difference between distilled water and their best preparation—this so-called tumor-inducing factor.

Cohen: How long did these guys stay here?

Mitchell: They were here about three years. [Burton and Friedman were at Caltech in the academic years 1955-56 and 1956-67.] After we discovered that it was all a fraud they were asked to leave, and they went back to New York.

Cohen: Back to their other jobs?

Mitchell: I don't know what they did there, but they found some private support. Burton moved to the Bahamas and set up a clinic. He claimed that he had found something in serum that would cure cancer. It created a big hullabaloo, and he was interviewed on *60 Minutes* because of it.

Cohen: That sounds like quite a story. Does anybody here know that story?

Mitchell: Yes.

Cohen: Because I certainly have not heard it.

Mitchell: The American Cancer Society went after them. The television people did too, because when he was interviewed on *60 Minutes*, they made it sound like he was a poor scientist who had discovered something marvelous and nobody would believe him.

Cohen: Did anyone ever come and ask you anything about him working here?

Mitchell: Yes, and I sent them a copy of the letter I had written to the editor of *Science*. When I came back here after I had been on sabbatical in Switzerland, George suggested that I try to repeat what they did. So I did.

Cohen: And it didn't work.

Mitchell: No, and I did controls. That was included in my statement. Their system of injecting larvae was poor, so I invented a machine to inject known quantities. I still have one. I've used it quite a bit for other reasons.

Cohen: Is that your old glassblowing experience that came into use?

Mitchell: Yes. All the way along it has come into very good use. But I can't do it anymore.

Cohen: Do you have any observations to make about the development of biology over the years, because it's really in the forefront now. Our new president [David Baltimore] is a biologist.

Mitchell: Yes, although I don't know him. Beadle was an unusual chairman. I've not seen anybody else who did the job like he did.

Cohen: What was unusual about him?

Mitchell: He walked around to see what needed doing. I think that was the farm experience coming out.

Cohen: Do you mean that every so often he would go into everybody's lab?

Mitchell: He did, yes. He asked people what they needed, and he'd try and get it for them. And if there was a spill of water or something, he would be the first one in there with a mop and a mop bucket.

Cohen: So he was a real hands-on chairman.

Mitchell: He was a real hands-on chairman, yes.

Cohen: So, even though you were all going your independent ways, what you're saying is that George Beadle always knew what everybody was doing.

Mitchell: He had a very good idea, yes, and he tried to help in every way he could.

Cohen: And if he thought the idea wasn't good was he on top of that also? I mean, occasionally people have ideas that aren't so good.

Mitchell: Yes. He spoke his mind whenever it was needed.

Cohen: So he was really a strong person.

Mitchell: He was, yes.

Cohen: Is there any other observation you'd like to make about life at Caltech at that time, or changes you saw over the years?

Mitchell: Well, I liked it very much here.

Cohen: That's a common theme. People really are happy here at Caltech. How about outside of the biology division? Was there anybody that you were particularly friendly with or enjoyed talking to?

Mitchell: Well, I got to know Carl Niemann very well.

Cohen: Now, he was a chemist.

Mitchell: He was a chemist, yes. Actually, I'm a chemist. I had never had any genetics when I came here.

Cohen: So you came just because of George Beadle?

Mitchell: That's right. Because of the things you could do with *Neurospora*.

Cohen: And you just gradually got over into the "bio" part of the work.

Mitchell: That's right.

Cohen: Do you recall any other people here? Did you used to go to the round table or anything like that at the Athenaeum and meet people?

Mitchell: Not much.

Cohen: So it was mostly the biologists that you were involved with?

Mitchell: That's right, yes. In about 1953 a fellow from Texas named Robert Wagner came to work with me as a research fellow. He had taught a course in genetics in Austin, and he asked me to collaborate with him on a book. So we wrote a book together called *Genetics and Metabolism*. He wrote the genetics and I wrote the biochemistry part.

Cohen: Was that book widely used then? It must have been an interesting book.

Mitchell: It was translated into Japanese, Russian, and Spanish, so I guess it was useful.

Cohen: Did you enjoy writing the book?

Mitchell: I enjoyed what I learned from writing it.

Cohen: It's hard work writing a book.

Mitchell: Yes.

Cohen: Is there anything that you particularly remember about the years here at Caltech that you'd like to talk about?

Mitchell: The one thing I enjoyed was a lot of the graduate students I got to know. When I came here there was no provision for graduate students in the athletic department, and I had been used to some activity of this kind all my life, baseball or basketball. When I came here there was nothing. I inquired about it and found that there was a group of boys in the chemistry division who had a baseball team and who played under the Pasadena Recreation Department, so I joined them for a year. Then the manager graduated and left me with the bats and balls. So I carried that on for the next twenty-five years.

Cohen: Not at the Pasadena Recreation Department—you brought that to Caltech then?

Mitchell: Yes. The athletic department here was mostly very helpful and cooperative. Soon after that, somebody wanted to enter into the Recreation Department's basketball league, so we did that too.

Cohen: Now, whose recreation department was this? The city of Pasadena's?

Mitchell: The city's, the municipal league.

Cohen: So the graduate students were having their recreation by being a part of the city program.

Mitchell: That's right.

Cohen: But you were the conduit for this. Did you ever get these programs moved over to the Caltech athletic department?

Mitchell: Well, they are now, I believe.

Cohen: But while you were involved with them they were part of the Pasadena Recreation Department?

Mitchell: That's right. They participated with teams from the police department and the fire department.

Cohen: Where did you play your games?

Mitchell: At the various schools around town, in their gyms.

Cohen: Did Caltech have any facilities at that time?

Mitchell: No, it had nothing.

Cohen: Did the undergraduates have physical activities?

Mitchell: They did, yes.

Cohen: But there was nothing for graduate students.

Mitchell: Finally the gym was built with alumni contributions, I believe.

Cohen: It's called Alumni Field, so I guess that's why.

Mitchell: I participated several times in track here—at Pomona I was on the track team.

Cohen: Were you part of the regular Caltech team?

Mitchell: I was playing manager of the municipal teams.

Cohen: I think there's still a tradition for faculty, if they want to, to be on the physical education department teams.

Mitchell: Yes.

Cohen: So you brought more than just your idea of *Neurospora* and flies here—you also brought a bit of recreation for the graduate students.

Mitchell: Yes.

Cohen: That's a good legacy.

Mitchell: I enjoyed it. It was my main social activity really. I wouldn't have known all the people in the other departments I got to know otherwise, and I enjoyed that.

Cohen: Anybody in particular?

Mitchell: There were a couple of people on the faculty who participated, like Fred Anson, Thad Vreeland and Roger Sperry.

Cohen: They were basketball players.

Mitchell: Yes, Fred Anson played basketball as an undergraduate here.

Cohen: I have interviewed Fred, so I know a little bit about that.

Mitchell: And Thad Vreeland in engineering also participated.

Cohen: Well, that's good. So, is there anything else you'd like to add to this?

Mitchell: No, I think that's about all I need to say.

Cohen: I didn't ask you about your relationship with Dr. Pauling. Did you have anything to say about that?

Mitchell: Well, I liked him. And I knew of his work and his little book *The Nature of the Chemical Bond*, which I think was an extremely important book.

Cohen: I used that in my chemistry course when I took chemistry.

Mitchell: It's the most important book in biology, I think, that there is.

Cohen: *The Nature of the Chemical Bond?*

Mitchell: Yes, because we depend totally on it.

Cohen: Did he interact very much with, say, George Beadle?

Mitchell: Yes, they did some. Linus was a good friend of Roger Williams, with whom I worked for a long time.

Cohen: Was Pauling a good friend of Williams?

Mitchell: Yes. Pauling appeared in the lab at Texas now and then.

Cohen: So was he aware of the work that all you people were doing? Did he come and look at it very much?

Mitchell: He was aware of it, yes.

Cohen: So he knew all this vitamin work was going on?

Mitchell: Yes. [Tape Ends]

Begin Tape 1, Side 2

Cohen: So he was interested in all this vitamin work that was being done.

Mitchell: He had nothing to do with Vitamin C, actually, at that time. It was a man named [Charles G.] King in New York who synthesized Vitamin C.

Cohen: But Pauling later felt it was very important. It's interesting how much Pauling interacted with the people in biology.

Mitchell: He did, quite a bit.

Cohen: In what way? Did he come to meetings?

Mitchell: He was on the PhD committee for one of my students.

Cohen: So you would have talked to him at that time about the work that was going on in order for him to be on the committee?

Mitchell: Yes.

Cohen: Well, there were a lot of interesting people.

Mitchell: Yes.

Cohen: Which is the strength of Caltech.

Mitchell: Well, I've had a good life. It was very enjoyable, up until the time that I got into this wheelchair.

Cohen: Well, let's leave it on a good note. [Tape Ends]

Herschel K. Mitchell

Session 2

December 22, 1997

Begin Tape 2, Side 1

Cohen: Good morning. I'm really glad to see you again. Today we're going to talk about some of your students.

Mitchell: Not so long after we came here from Stanford, we had an application from a young man from the University of Nebraska—that of course is where Beadle went as an undergraduate. Included in the applicant's folder was a note from the chairman of the department saying that Mr. Haskins [Francis Arthur Haskins] was the best student he had ever had. Beadle had responded to this by saying that he too had taken this course from this gentleman, so we all figured that this young man had to be a better student than Beadle. We promptly admitted Mr. Haskins by unanimous vote and he came and worked with me, and he was indeed a good student. They wanted him back at Nebraska after he got through, and so he went back. For three or four years he was chairman of the department and then he quit.

Cohen: What did he do?

Mitchell: He's still on the faculty there, but he quit being chairman.

Cohen: Did he do anything significant as far as the field went?

Mitchell: That I don't know. I don't know agronomy.

Cohen: So you just took him as a student because he came from the same place as Beadle had come from and because he was recommended?

Mitchell: No. When the new students came they were expected to find their own locus. We had the previous year's biology report that they were given so they could find a place they wanted to work. And then they were to talk to the faculty and to the other graduate students.

Cohen: And then they decided themselves who and what they wanted to work with?

Mitchell: Yes, but they weren't necessarily tied down. They could move if they wanted to.

Cohen: So there had to be a meeting of interests between the student and the professor?

Mitchell: Yes.

Cohen: And that was the scheme by which they got allocated? They had a choice?

Mitchell: Yes.

Cohen: I have a list here of names and addresses. I was talking to Manny Delbrück [wife of Max Delbrück—Ed.] and she mentioned the Tissières [Alfred Tissières]. He was from Switzerland, right?

Mitchell: Yes, Geneva.

Cohen: And he came as a PhD student?

Mitchell: He was a postdoc, sort of. I had three different professors from Europe in my lab at one time or another. He was one of them.

Cohen: So he already had an academic position in Switzerland at the time of the postdoc?

Mitchell: Yes, he did.

Cohen: Who were some of the others?

Mitchell: There was Hadorn [Ernst Hadorn], who was chairman of the zoology department at the University of Zürich. And then Westergaard [Mogens Westergaard], who was in Copenhagen.

Cohen: What sort of things did they do when they were here? Did you work with them in a group or did they just do their own research?

Mitchell: No, I worked directly with them.

Cohen: What sort of research were you doing with them?

Mitchell: Well, with Westergaard we devised a medium which favored sexual reproduction in *Neurospora*, which we hadn't had before. We used coconut milk with the idea that it should be a spore medium.

Cohen: I don't understand what you're saying. You used coconut milk—what was this medium for?

Mitchell: To produce fruiting bodies in *Neurospora*.

Cohen: I see. Okay. And so you tried other things, or developed other things?

Mitchell: We developed a medium which did this. And it turned out that the mold had to reduce nitrate in order to produce fruiting bodies.

Cohen: So, how come the coconut milk worked? Did it just happen to do that?

Mitchell: It just happened to do that, yes.

Cohen: But it wasn't as effective as what you developed?

Mitchell: No.

Cohen: Ah, so what you really did, you're saying, is you and your postdoc found out what it was that was needed to produce the medium that was needed to produce the spores? That's correct, isn't it? Because it's yeast that we're talking about?

Mitchell: *Neurospora*.

Cohen: *Neurospora*. And so would that be spores? Am I saying the right thing?

Mitchell: Well, sexual spores, yes.

Cohen: Now as I look at this list here I see so many names. There is somebody named Chen [Pei-Shen Chen], also from Switzerland.

Mitchell: Yes. He was a professor of biochemistry there. He came for a year from Zürich. I had gotten acquainted with him when I was over there for a year in 1959.

Cohen: That was a sabbatic leave, after you were already a professor here.

Mitchell: That's right, yes.

Cohen: What was the connection with Switzerland? Were they just doing the same kind of work?

Mitchell: Well, Hadorn came here for a specific purpose—to look at the biochemistry of many of the *Drosophila* mutants—and we did that.

Cohen: He worked directly with you?

Mitchell: Yes.

Cohen: And so you then went to Switzerland?

Mitchell: Yes. He invited me to come.

Cohen: Was that a good year?

Mitchell: It was a good year, yes. I came back with a Swiss wife, for one thing—a very, very good thing. She's great.

Cohen: Is that your present wife?

Mitchell: Yes.

Cohen: When I talked to her I heard a slight accent and I wondered where she was from. And she's a biologist too? Is that correct?

Mitchell: Well, she had been to lab technician school in Geneva. Her mother had a PhD in biology, but she didn't.

Cohen: And did she work with you when you came back here?

Mitchell: The first year she did, yes.

Cohen: Well, I would say that was a successful year.

Mitchell: Yes, very.

Cohen: After that you proceeded to have more students from Switzerland. You had students from Denmark also. Is that correct? Tissières and Jean Mauron were both from Switzerland.

Mitchell: Jean Mauron was from the French part of Switzerland.

Cohen: Another one from Switzerland was Hans-Rudolf Geiger. Did they come at different times?

Mitchell: Yes, they came at different times.

Cohen: At one point you said that you had three people here from Switzerland.

Mitchell: No, I had three people from Europe. There were four ladies, incidentally—two of them are married to present staff members in biology.

Cohen: They came to study here and met their husbands here?

Mitchell: Yes.

Cohen: Who?

Mitchell: Attardi [Anne Chomyn Attardi] is one and Revel [Gallina Moller Revel] is the other.

Cohen: I see. So that was very good for the men, I would say. And where did these women come from?

Mitchell: Mrs. Revel is Russian. She came and at first taught Russian in the humanities division.

Cohen: Oh, so she wasn't a biologist?

Mitchell: Well, she had training in biology before that.

Cohen: And where did Mrs. Attardi come from?

Mitchell: She was from Pennsylvania, I believe. I'm not sure.

Cohen: But she was a biologist also and came here to do that?

Mitchell: Yes.

Cohen: Of all these students or postdocs that you had, are there some that were really outstanding, that have stayed in your mind?

Mitchell: Well, I have three former students who are on the faculty at Stanford.

Cohen: That's actually what's gotten us started on this. Dr. Baltimore was up there and met them and they asked about you. Who were those students?

Mitchell: Hogness [David Swenson Hogness] is one—the best known one. And Herzenberg [Leonard Arthur Herzenberg] is another. He started in immunology. Metzenberg [Robert Lee Metzenberg] is the third. He went first to Wisconsin, where he continued to work on *Neurospora*.

Cohen: Well, I would say that that's a big record. To have three former students at Stanford—I think that's pretty good.

Mitchell: And there's one at Berkeley, Bruce Ames.

Cohen: These people were PhD students?

Mitchell: Yes.

Cohen: Well, I would say that's more than a feather in one's cap.

Mitchell: I'm happy about it, yes.

Cohen: Have they continued the same kind of work that they did here?

Mitchell: Yes, although Hogness has changed his venue somewhat.

Cohen: What is he doing now?

Mitchell: He's more into gene splicing.

Cohen: Are there any others at other colleges or universities?

Mitchell: Well, there's one at Duke.

Cohen: What was his name?

Mitchell: Greene [Ronald Greene].

Cohen: Ronald Greene of Durham, North Carolina. Does he also continue in this same kind of work?

Mitchell: I don't really know what he does.

Cohen: Are you in touch with these people much?

Mitchell: Well, somewhat, yes. We send Christmas cards to all of them, and we get some cards back.

Cohen: I see. So this is the time of year one really thinks about them.

Mitchell: Yes.

Cohen: Would you say that the majority of them went into academic work?

Mitchell: Yes, although there's one at the City of Hope, who is now, I think, the director of research [Arthur D. Riggs].

Cohen: Here's somebody from the Scripps Institute, John Edward Geltosky.

Mitchell: Yes, that's where he is now—he was at Dupont for a while.

Cohen: And then he went back to Scripps?

Mitchell: Yes.

Cohen: Here's Harris Bernstein, who is at the University of Arizona. Is he continuing the same work?

Mitchell: Yes.

Cohen: I bet many of them are probably chairs of their departments.

Mitchell: Some of them are, yes.

Cohen: Now, here is a John Roberts Simmons from Logan [University of Utah].

Mitchell: Egbert [Larre N. Egbert] is also at Logan—both on the faculty there.

Cohen: Why does Simmons have a Eugene, Oregon, and a Logan address? Does he split his time between the two places?

Mitchell: I think he does, yes.

Cohen: Here's somebody from Canada, [W. D.] Seybold.

Mitchell: Yes. He went back to Canada. He got an MD degree and he's practicing somewhere in Canada.

Cohen: So he's a doctor.

Mitchell: Yes.

Cohen: And here's a William Thomasson from Oak Park, Illinois. That sounds like an academic setting.

Mitchell: It is, a small school. He was one of the poor ones.

Cohen: One of the poor students?

Mitchell: Yes.

Cohen: And what is he doing?

Mitchell: I believe he's teaching there.

Cohen: At what school? The University of Chicago?

Mitchell: No, not Chicago—some small school.

Cohen: Here's somebody from Manhattan, Kansas, Larry Williams.

Mitchell: Well, he's at the University of Kansas at Manhattan now.

Cohen: And here's a lady, Sandra Winicur, from South Bend, Indiana.

Mitchell: Well, when she got through here she married a graduate student in chemistry and he got a job at Notre Dame, so they moved there.

Cohen: And did she continue her career?

Mitchell: The last time I saw her she was teaching in a junior college, or something of that sort.

Cohen: Okay, we've talked about the people from Stanford—Herzenberg, Hogness, and Metzenberg. What about Roy Harding from Indiana, Pennsylvania?

Mitchell: He went from here to the Smithsonian Institution in Washington, DC, but now he's teaching somewhere in another school in Pennsylvania.

Cohen: How about George Ellman from Glen Ellen, California?

Mitchell: George Ellman is president of the Audubon Society.

Cohen: Oh really? That's very interesting. What has he done professionally?

Mitchell: That I don't know.

Cohen: I think that's very interesting that he's president of the Audubon Society. There are a few people on this list without addresses, such as Jake Maizel.

Mitchell: I don't know where he is.

Cohen: How about Paul Meltzer?

Mitchell: Meltzer went to a commune somewhere in Pennsylvania.

Cohen: Antonio Reyes?

Mitchell: He's involved in gene splicing somewhere commercially.

Cohen: And we have somebody from La Verne, California, Arthur Riggs.

Mitchell: He's the one that's at City of Hope.

Cohen: Is he a physician there or just a PhD researcher?

Mitchell: He's the research director of human biochemistry.

Cohen: This is very impressive.

Mitchell: I've had a lot of good students, as you can see.

Cohen: I'll say. Oh, we've got more on this page—

Mitchell: There are two who are deceased.

Cohen: Well, I haven't asked you what they're doing.

Mitchell: They're pushing up daisies.

Cohen: And here's Anne Chomyn [Anne Chomyn Attardi].

Mitchell: She's the wife of Giuseppe Attardi.

Cohen: And Gallina Moller [Gallina Moller Revel]. So they were actually students of yours?

Mitchell: Yes.

Cohen: So you really provided a lot of services for your colleagues here, I must say. [Laughter]
How about William Scott from Newton, Pennsylvania? Did we talk about him?

Mitchell: Bill Scott went to Rockefeller from here.

Cohen: Is he still there?

Mitchell: As far as I know he is.

Cohen: Did we get everybody here on the student list? This is really very, very impressive, I have to say. Okay. You've had, of course, a lot of postdoctoral fellows—probably more postdoctorals than graduate students. Besides Bruce Ames, the professor at Berkeley, there's Dennis Barrett. Where is he?

Mitchell: He's on the faculty at Colorado State, I believe. [Dennis Barrett is associate professor of biology at the University of Denver.—Ed.]

Cohen: And Richard Bondar?

Mitchell: I have no idea what happened to Bondar.

Cohen: And Alan Blumenthal?

Mitchell: He went to work with Hogness at Stanford.

Cohen: Is he still there?

Mitchell: No, he's not there now.

Cohen: So you don't know what he's doing?

Mitchell: I don't know what he's doing.

Cohen: I see that James Boyd is deceased, but I see his address was Davis, California. Was he at the university?

Mitchell: He was on the faculty at Davis, yes.

Cohen: And Thomas Cole was at Wabash College?

Mitchell: Wabash. He's still there as far as I know. He was in rather bad health the last time I saw him. [Cole died ca 1998, per HKM—Ed.]

Cohen: Here is George Dubes from Omaha, Nebraska. Is he an academic also?

Mitchell: Yes. He was also the catcher on my baseball team.

Cohen: Oh, very good, another distinction. How about Richard Eakin?

Mitchell: He's at the University of Texas in Austin, part of the Biochemical Institute.

Cohen: And here's Larre Egbert.

Mitchell: He's at Logan.

Cohen: Another Utah person.

Mitchell: Egbert and Simmons went back there. They were both good.

Cohen: Well, I'm sure they are doing good work there too. Now I'm going to go through this list of postdocs with you. We talked about Westergaard, who is deceased.

Mitchell: Yes.

Cohen: And about Nyc [J. F. Nyc] you have no information.

Mitchell: It's pronounced "Nich."

Cohen: What kind of name is that?

Mitchell: Czech.

Cohen: Was he from Czechoslovakia?

Mitchell: Yes.

Cohen: And he's gone back there as far as you know?

Mitchell: No. He died. He went to UCLA, then he died, I don't know what of.

Cohen: And Benjamin Volcani?

Mitchell: He's down at Scripps.

Cohen: How about Joseph Lein?

Mitchell: He went into industry and then he quit that. He lives on some island up in Washington.

Cohen: And David Sullivan from Syracuse?

Mitchell: He's still there, I think.

Cohen: At the university?

Mitchell: Yes.

Cohen: And William Drell?

Mitchell: He originated the Cal Biochem Company, which produces a variety of chemicals.

Cohen: There is a Drell who is a famous physicist. Is that a relative?

Mitchell: That I don't know.

Cohen: Felix Haas is deceased. And Frank Harold of Fort Collins, Colorado? Is he an academic also?

Mitchell: Frank Harold lives in Denver.

Cohen: We've talked about Tissières already. I think we talked about Hugh Forrest already too, and Jean Mauron, who is another one of the Swiss postdocs, right?

Mitchell: Yes.

Cohen: What about Roland Davis from Laguna Beach?

Mitchell: I don't know what he is doing. I think he's at Irvine [University of California] part of the time.

Cohen: What about Elizabeth Turner—another lady without any address.

Mitchell: Well, she's in England.

Cohen: Is she still working as a biochemist?

Mitchell: No, she is a mycologist. She came here because we worked on fungi.

Cohen: And James Friström?

Mitchell: He was chairman of the biology department at Berkeley.

Cohen: So that's another Berkeley connection.

Mitchell: Yes.

Cohen: Is he still there?

Mitchell: He's still there, yes. I don't know if he's chairman now.

Cohen: We talked about Geiger [Hans-Rudolf Geiger] and Chen [Pei-Shen Chen]. Then there's George Saul from Middlebury, Vermont? That's certainly an academic setting.

Mitchell: Yes, that's right. He was at Dartmouth and he moved many years ago.

Cohen: He must enjoy teaching.

Mitchell: Yes. He was in Zürich at the same time I was—that is where I learned about him. And he came within a year to Caltech.

Cohen: To work with you?

Mitchell: Yes. His work is nice—he works on a wasp that stings various kinds of larvae.

Cohen: A wasp that stings larvae?

Mitchell: Yes, he's got a very good control system.

Cohen: Here's somebody from Twentynine Palms, Kenneth Lunan.

Mitchell: When he left here he went into industry. He is retired.

Cohen: Looks like desert living.

Mitchell: Yes, I guess so.

Cohen: I think we talked about Tom Cole. What about Nancy Petersen?

Mitchell: She is on the faculty at the University of Wyoming.

Cohen: Well, you certainly have people all over the place. I wanted to ask you about the fraud case with Burton and Friedman. I got home and started to think about this and realized that the dates weren't making any sense to me. So I think you have to tell me this story again, because when it happened there was no *60 Minutes* and I somehow couldn't pull this all together.

Mitchell: Well, these two fellows were brought here because they thought they had something they could transmit that would produce tumors.

Cohen: Some chemical solution or something?

Mitchell: Yes. There was a mutant of *Drosophila* called TUE. They called it a tumor, but it isn't. There is a black body inside of the animal inside of the hemolymph. They called it that just because it was black.

Cohen: So they called it a tumor just because of the color? It really was not a tumor?

Mitchell: Yes. So these guys claimed that they could take an extract of this mutant and inject it into normal animals and give them the same thing.

Cohen: I see. They claimed they could transfer this.

Mitchell: Yes, and Beadle saw them in New York.

Cohen: They were from NYU [New York University]?

Mitchell: Yes. He met them there, and he told them to come here.

Cohen: What year was that?

Mitchell: 1958, I believe. [Lawrence Burton and Frank Friedman arrived at Caltech in 1955.—
Ed.]

Cohen: So they were invited to come to Caltech?

Mitchell: Yes, so they came as postdocs. They had both gotten a PhD, for this work. When they came here to work, Beadle asked me to assist them in purifying this stuff. I told them how to do the research, and everything worked, so I didn't believe it.

Cohen: You advised them, they went into the lab, and then they told you they had a result?

Mitchell: Yes.

Cohen: And you were suspicious. Not just suspicious; you didn't believe it.

Mitchell: Yes.

Cohen: So what did you do then?

Mitchell: Well, we had Beadle and Dulbecco, who was here as a postdoc with Delbrück, coat some samples of distilled water and salt water and an extract of this mutant. And they couldn't tell the difference in their assay.

Cohen: Whether it was fresh water or salt?

Mitchell: They couldn't tell the difference. I was also sure that there was something wrong because they had what they called controls with water in them. Whenever you stick a needle in a larva you get a black spot, and that carries over into the pupae. I looked at their controls one day and there were no black spots on their pupae.

Cohen: So they were just lying?

Mitchell: They were just lying. It was just deliberate fraud from the beginning. I think there's no doubt of it.

Cohen: What did they think they were going to get out of this?

Mitchell: They thought they were going to get support, but they were kicked out of here. Beadle told them to leave.

Cohen: Had they ever published anything while they were here?

Mitchell: They published one paper on the purification of the extract, and they put my name on it without my permission, which irritated me.

Cohen: Particularly since you knew it was fraudulent.

Mitchell: I knew it was a fraud, yes.

Cohen: So did that ever go into the upper echelons of the institute or did it stay quietly within the department? How was that handled?

Mitchell: Well, they were just thrown out—just told to leave.

Cohen: And Beadle did this?

Mitchell: Beadle did this, yes.

Cohen: And they didn't give him any problem? They just left?

Mitchell: They just left.

Cohen: They didn't reach up into the administration for support?

Mitchell: They went back to NYU.

Cohen: And NYU took them back?

Mitchell: Yes. [Burton and Friedman returned to New York in 1957 and became research assistants in the Department of Pathology at St. Vincent Hospital.—Ed.] Then they found some private money and did about the same thing with mice.

Cohen: They claimed that they were transferring an extract?

Mitchell: Yes. They also claimed they had a serum compound which would cure cancer.

Cohen: Did they publish anything on this?

Mitchell: I don't know.

Cohen: Because I would think that they would come to one of you other guys to referee a paper. I don't know how the journals work, but papers have to be read.

Mitchell: Yes.

Cohen: So, what happened then?

Mitchell: Well, Burton found some private money and went to the Bahamas and set up a clinic for curing cancer, to which people would pay so much to come and he would cure them. Of course, they all went home to die.

Cohen: What years did he run this clinic?

Mitchell: I don't know the dates.

Cohen: You said it was '58 when they were here. They must have been here earlier.

Mitchell: Maybe it was '57. I don't really know for sure.

Cohen: So Burton wasn't at NYU very long before he got the money and went to the Bahamas?

Mitchell: That's right, yes.

Cohen: Maybe they asked him to leave at NYU too. What happened to the other one—Friedman?

Mitchell: Well, they split up apparently. I don't really know what happened to Friedman. [Burton and Friedman continued to work together at St. Vincent's Hospital and later, in the mid-1970s, at a clinic called the Immunology Research Foundation, in Great Neck, New York. That facility closed in 1977 and Friedman ended his affiliation with Burton, whereupon Burton went to the Bahamas and established the Immunology Research Centre there.—Ed.]

Cohen: What happened after that?

Mitchell: Well, then there was a great furor on the East Coast, because this “cure” was advertised on television and radio on the East Coast. So the Cancer Society got after Burton. I didn't hear the *60 Minutes* interview with Burton, incidentally—I was in Switzerland at the time.

Cohen: What year would that have been?

Mitchell: 1959. Norm Horowitz wrote me about it. [The *60 Minutes* episode on Lawrence Burton aired in May 1980.—Ed.]

Cohen: That they were on *60 Minutes*?

Mitchell: Burton was, yes. He apparently made it sound like he was a poor, misunderstood scientist who had found something really good.

Cohen: So it was touted as a good thing?

Mitchell: Apparently, yes. As I said, I didn't hear that interview myself.

Cohen: But Norm Horowitz heard it.

Mitchell: Norm Horowitz heard it and wrote me and told me about it. When I got back Beadle said I should do something about it, so I did. I followed their procedure to prepare this stuff and I repeated the experiments. The controls were exactly the same.

Cohen: So you repeated the original experiments that they had done?

Mitchell: I did, yes. It took me all summer. But in order to do it I invented an injection apparatus, which was reproducible in volume. What they were using was not reproducible in volume. It was an apparatus that Beadle and [Boris] Ephrussi had used many, many years before. It just blows the larvae up.

Cohen: So what you are saying is something positive came out of your summer's work, even though you nullified their work?

Mitchell: Yes. I used the machine a great deal myself.

Cohen: So then did you write to *60 Minutes*? What did you do?

Mitchell: I wrote a thing for *Science*—a denial of it with the data that I had.

Cohen: What happened?

Mitchell: Nothing.

Cohen: So *60 Minutes* didn't backtrack and retract their piece. Did Burton continue then to run his clinic in the Bahamas?

Mitchell: Well, the authorities finally kicked him out of the Bahamas, and he said he was going to Mexico. Maybe he's there. I don't know.

Cohen: So that's the last you heard of him.

Mitchell: Yes.

Cohen: He must have had a pretty thick skin. He was kicked out of an awful lot of places.

Mitchell: Of course, I figured that the whole thing was a lie from their behavior here, that their cure was nothing.

Cohen: But it never reached the point where it was an institute scandal of any kind?

Mitchell: No, there was no scandal.

Cohen: It was handled quietly?

Mitchell: It was just a departmental scandal.

Cohen: That was probably because they never sued the institute or questioned the fact that they were asked to leave. And the only person whose name appeared on any of their papers was yours?

Mitchell: That's right.

Cohen: I see. So you were the person that had to defend yourself in some way. Now, speaking of this apparatus you developed—which probably comes from your being an expert from your old glassblowing days—your wife said something about maybe wanting to give a piece of it to put up at the Archives, because it is probably of some historical value. Is it still being used?

Mitchell: I'm not doing anything with it now. I'm doing something, but not much. I have one left—I had two of them.

Cohen: Have you thought about your papers coming into the Archives?

Mitchell: I don't know what's really in my collection.

Cohen: Well, you might think about that. Maybe a piece of this apparatus would be of historic interest to have here also. Is there anything else you wanted to tell us before I turn off the tape?

Mitchell: Well, the other apparatus I still use is the glass needle puller, which was designed by Beadle and built in the shop at Stanford. It was for isolating ascospores.

Cohen: Isolating what?

Mitchell: Ascospores. In *Neurospora*, when you make a cross you get a parathecium, which is a round or pear-shaped body. Inside there are probably about one hundred asci. Each of these has eight spores. So what one does is, on a four-percent auger you break open the ascus (the pear-shaped body) and then you push the spores out one by one. Then you get the segregation. It's a beautiful thing for genetics, because it's a haploid organism and all the mutations show.

Cohen: So Beadle developed an instrument to pull glass needles?

Mitchell: Yes.

Cohen: And is it still used?

Mitchell: No.

Cohen: So you have a piece of this equipment also?

Mitchell: I do, yes. It's the only one that exists.

Cohen: I think it would be very nice to have those here. Is there anything else you'd like to say, Dr. Mitchell, for the record?

Mitchell: Let's see. My wife put in this other thing—the folic acid story.

Cohen: Yes, okay, we can talk about that a little bit.

Mitchell: As you see, there are three authors on the title page, but I did the work.

Cohen: You did this work, okay. Were Snell and Williams postdocs at the time?

Mitchell: [Roger J.] Williams was my major professor at Oregon and Texas. I like to put it in a military context. That is, if you say Williams as the director of the laboratory was a general, and [Esmond E.] Snell as a postdoc was a first lieutenant, then I as a graduate student would be a buck private. So you have three guesses as to who did the work.

Cohen: I see. Sometimes we say “in the trenches.”

Mitchell: Yes.

Cohen: So this was some work that you did when you were still a graduate student?

Mitchell: Yes.

Cohen: But you are listed as the first author on it.

Mitchell: Yes. Williams thought of that—he was a very honest man.

Cohen: Well, I'll certainly make a copy of this and put it in with these papers. Would you say this was the piece of work that made your reputation, and then you went on to do other things afterward?

Mitchell: Well, it helped, I'm sure. Because when I went to Stanford, Dr. Beadle wanted me to work there. He wired to Williams for a recommendation and I knew Williams would give me a good one.

Cohen: Did you work with Henry Borsook?

Mitchell: We did work somewhat together when I came here.

Cohen: Is that why you got into the folic acid business?

Mitchell: It was Williams who put me on to folic acid, when I was at Oregon State University, and then I did the work later in Texas—this was all before I went to Stanford to work with Beadle.

Cohen: Is there anything else that you'd like to talk about that I haven't thought of?

Mitchell: No.

Cohen: I have to say this is very impressive—this list of your former students. You must be very proud of them.

Mitchell: I am. I have enjoyed it.

Cohen: Not many people can boast of so many people that are in influential places in their field. Well, thank you very much.

Mitchell: You are most welcome. [Tape Ends]