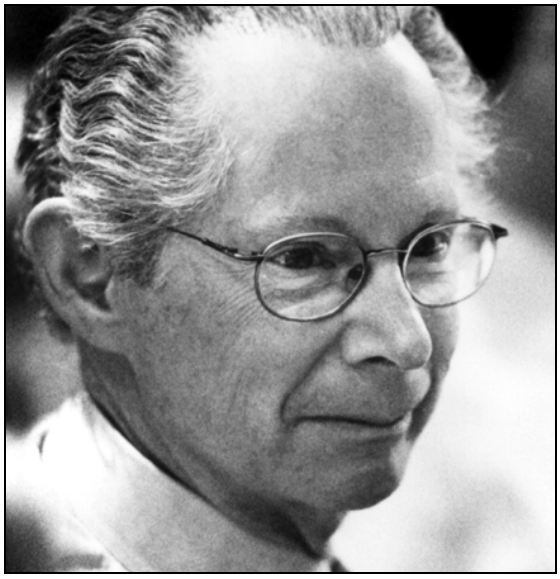

JOHN D. BALDESCHWIELER
(Born 1933)

INTERVIEWED BY
SHIRLEY K. COHEN

January – February, 2001

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Chemistry, chemical engineering

Abstract

Interview in six sessions in January and February 2001 with John D. Baldeschwieler, J. Stanley Johnson Professor and professor of chemistry, emeritus, in the Division of Chemistry and Chemical Engineering. Dr. Baldeschwieler received his bachelor's in chemical engineering from Cornell in 1956 and his PhD in 1959 from UC Berkeley.

He begins by recalling his childhood and early education in Cranford, N.J. His father, an analytical chemist, emigrated from Switzerland and his mother from Manitoba. He matriculated at Cornell in 1951 and enrolled in ROTC during the Korean War. Recalls summer work at Los Alamos and graduate school at Berkeley 1956-1959; his thesis on infrared spectroscopy, with George Pimentel; interest in instrument building. After six months' active duty at Aberdeen Proving Ground, Md., joins the Harvard faculty; becomes a consultant for Aberdeen. Early work with nuclear magnetic resonance (NMR). Invited to Stanford as associate professor in 1965, where he works on electron cyclotron resonance, in connection with Varian Associates.

Joins Army Scientific Advisory Panel; works on "people sensors" during the Vietnam War. Appointed to PSAC (President's Science Advisory Committee); discussion of defoliant Agent Orange. Becomes deputy director of

the Office of Science and Technology in 1970, during first Nixon administration; takes a leave from Stanford and moves to Washington, D.C. Recalls the debates on biological warfare and on whether or not to build the SST (supersonic transport). Recollections of various figures in the Nixon administration. Resigns from government in December 1972 and goes to work at the National Cancer Institute for six months.

Invited to become chairman of Caltech's Division of Chemistry and Chemical Engineering. Arrives in 1973, during Harold Brown's presidency; discusses his close relationship with Brown and his reorganizing of chemistry division, of which he remains chairman until 1978. Meanwhile, consults for Monsanto and Merck and becomes involved in the US-Soviet joint scientific program. Visits the USSR in the early 1970s. Travels with Glenn Seaborg on the first chemistry delegation to China in 1978. Work on binding liposomes to cancer cells in the late '70s; forms company called Vestar to commercialize the technique as a diagnostic tool. Collaboration with the City of Hope. Discussion of patenting and licensing of discoveries made at Caltech and of proposed high-tech corridor for Pasadena.

He concludes the interview by remarking on his children and stepchildren and their work, and he lists the various technology companies he has helped to establish.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH JOHN D. BALDESCHWIELER

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY

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COHEN: Perhaps you can start by telling us a little bit about your parents, where they came from, and then maybe a little bit about what schools you've gone to.

BALDESCHWIELER: Well, my complicated name has its origins in Switzerland. My father was born in the French-speaking part of Switzerland, in the canton of Vaud, in the town of Vevey. But he grew up in the city of Lausanne, which is on Lake Geneva.

COHEN: So Baldeschwieler is a Swiss name.

BALDESCHWIELER: Yes—a Swiss-German name. But it wasn't until many years later that I finally understood all of the perambulations of our family. Each Swiss has what's called a *Heimat Gemeinde*, which is the village of origin. It turns out that ours is the town of Laufenburg, which is a beautiful city on the Rhine in the northern part of Switzerland. It lies roughly halfway between Basel and Schaffhausen. So our family is, in origin, German-speaking. But my father grew up in Lausanne, where the native language is French. He was trained to be a postal clerk in the Swiss postal system. He was born in 1890, and his father died when he was

only seven years old. His mother, who was a very astute survivor, decided that the Swiss postal service would be the place for her son, because it was a federal job and was secure. Of course, in those days, the Kaiser was rattling the sword and Switzerland was not nearly as prosperous as it appears today. And so he was trained as…

COHEN: He was an only son, I gather.

BALDESCHWIELER: No, no. He was the youngest of three. So he was trained as a postal clerk. In his later years he could still recite all of the tiny towns along the Trans-Siberian railway and all the other things that you learned as part of the Swiss postal service. And he was fluent in German and passable in Italian as well. But his fortunes changed dramatically when his sister went to New York as an *au pair* girl—or the equivalent of an *au pair* girl; I don't know what they were called in those days. Her name was Emma, and she found a job with the Flagler family. You may remember that it was the Flaglers and the Rockefellers who built the old Standard Oil Company. She was involved in that family and wrote back to my grandmother. The two boys decided that New York was a wonderful place and they should go there. So they packed up and came over. My father's older brother had been trained as a decorative-iron worker. He was a little bit too old, I think, to make a graceful transformation into the New World. But my father was seventeen, and he had saved the equivalent of $28 by doing various jobs in Switzerland. With that, he landed in New York. He found that this was by no means the paradise that his sister had described. They all worked very hard. He started with various kinds of jobs. He told me that he began wrapping packages in some nondescript place. He of course had to learn English. He told me that from time to time the telephone would ring. He would pick it up and say, "Hello? Yes. No. Goodbye," [laughter] and he thought he was speaking English. That will happen to me sometimes and I'll always think it's some poor immigrant trying to learn English—so I'm very tolerant of such things. In any case, he discovered that there was a night school at Cooper Union, and so he went to Cooper Union at night—for thirteen years. He earned degrees in chemistry and mechanical engineering, and throughout that process he worked at jobs with successively greater pay and responsibility. So that's my father's beginning years.

My mother was born in Brandon, Manitoba, [which was] a tiny prairie town. Her father was a dentist, as were her two brothers. She was a very lively and articulate lady. She taught school in a one-room schoolhouse in Brandon, as a young girl of seventeen. She loved languages. Her uncle, who taught romance languages at Brooklyn College, invited her to come to New York to live with them and study languages, which she did. After a year, she had the opportunity to spend a year in Florence and a year in Paris, and she was so good at languages that she became quite fluent in both Italian and French. She returned to New York and believed, for a time, that she could perform on the stage. This did not work out so well. But in the course of her living in New York, she asked someone if they could find a nice young man who could speak French. And they indeed did.

COHEN: Well, that's very romantic.

BALDESCHWIELER: So that's how Mother and Father found each other.

COHEN: So you are a first-generation American. Did your parents become citizens right away? How did that work?

BALDESCHWIELER: I don't know the exact sequence of events. I think it took a number of years. They were married in 1929, just before the market crashed, and they lived in New York. My father worked for the International Nickel Company as an analytical chemist. He was one of those meticulous Swiss analysts you perhaps have encountered. He was very careful and precise. And in those days one did wet chemical analysis. He had been employed by the company just before the Depression, and he lost his job after the crash. He had wonderful skills, and he wrote many letters and went to many interviews and finally found a job with the Standard Oil Company of New Jersey.

COHEN: So we're back to the Flaglers. [Laughter]

BALDESCHWIELER: Yes. [Laughter] These lines, I suppose, tend to crisscross. He was employed at the Bayonne and Bayway refineries. What is today Exxon was in those days Standard Oil of New Jersey. The family moved to a small town in suburban New Jersey.

COHEN: That's where you were born?

BALDESCHWIELER: I was born in New Jersey in 1933, in the midst of very difficult times.

COHEN: But your father did have a job.

BALDESCHWIELER: Through most of that, Father had a job. He also caught on very early to the stock market.

COHEN: I have a feeling your father was a very intelligent man.

BALDESCHWIELER: He was very astute. He saw things very clearly.

COHEN: So even though the stock market was way down there, he saw it as something…

BALDESCHWIELER: He saw it as an opportunity. And he and his sister—the *au pair* girl, who, by this time, had also done quite well—invested in the market and had a very good time at this. In his later career, my father was both a chemist, and in some sense, a financier. He told me that the interest in finance stemmed from his early childhood in Switzerland. Although his name was of German origin, his native language was French. But in school he was always placed in advanced German [classes]. It was his most difficult subject. The final exam in his course in German—you had to read a piece from the literature and then be examined on the content. The piece he was given was called *Spekulation und Börsenspiel*—"Speculation and Playing the Market."

COHEN: So he learned that very well.

BALDESCHWIELER: He claimed that there's always a silver lining to even the most disastrous situation. In this case, it was a very difficult exam, but he never forgot. He was learning about the stock market. He had both scientific and financial interests. So I grew up in Cranford, New Jersey, which is a small suburban town. The early years were Depression years.

COHEN: You went to school in this little town.

BALDESCHWIELER: We went to school. We did not have a lot, but both Mother and Father were very frugal, and we survived just fine.

COHEN: You finished high school in New Jersey?

BALDESCHWIELER: Yes.

COHEN: Did you think at that time that you wanted to be a chemist?

BALDESCHWIELER: Oh, yes. But as a child, I had most fun, actually, with a set of toys that may or may not exist anymore. The most attractive toy for me was the Erector set.

COHEN: Oh, of course.

BALDESCHWIELER: This was a whole collection of little metal rods and beams, and there was a motor with gears. And it also had a screwdriver and a wrench and a bucket full of bolts. You could build all kinds of things. I loved that, probably far beyond the age when one should have been playing with these things. [Laughter] So I think my real interest was in engineering. You know, when you look back, some of these things are very indicative. I think we would do well to have such a toy these days. I suppose they're all made out of plastic now. [Laughter]

COHEN: Well, there's Lego, but with that you just sort of click things together. It's probably not as much of a challenge.

BALDESCHWIELER: But with this Erector set, you really had the chance to build structures. Anyhow, that was influential. In high school I did very well. I must say I enjoyed languages, and also math and chemistry and physics.

COHEN: What language was spoken in your home? Did they speak French?

BALDESCHWIELER: Yes, French was spoken at home. But then as we became adolescents it was less and less French and more and more English.

COHEN: Did your mother ever work at teaching languages or anything like that?

BALDESCHWIELER: No, she didn't. She ran the household.

COHEN: So you finished high school there in New Jersey.

BALDESCHWIELER: I finished high school in New Jersey, and I applied for college, basically in engineering. I had opportunities to go to a number of places, but I chose Cornell for several reasons. One was that I got a scholarship. It turns out that the Standard Oil Company had…

COHEN: Oh, scholarships for employees' children?

BALDESCHWIELER: Exactly. It was a small—but at that point, critical—benefit. So I chose Cornell for that reason and also because you could start directly with calculus in the engineering program. One forgets that the standard for incoming freshmen at our universities has gone up very substantially.

COHEN: In fact they do calculus in high school.

BALDESCHWIELER: Yes, and we did not. In fact, I think I was a bit ahead in math and science. So I arrived at Cornell in 1951.

COHEN: And you went into the engineering school?

BALDESCHWIELER: Yes, chemical engineering. That seemed to me somehow to be the best compromise between chemistry and engineering. I really enjoyed that. I had the chance to work in refineries. I worked for one summer in the Dupont pigment plant in the old ironbound section of Newark. And I also did a tour at the Bayonne refinery.

COHEN: Now, was that arranged by Cornell or was that something that came from your scholarship?

BALDESCHWIELER: Neither. It was on my own initiative. And the program at Cornell was a challenging one. Also in 1951, of course, the Korean War was going on. And I was of the right age. So I enrolled in the ROTC [Reserve Officer Training Corps] program. Another thread of my career has been involvement in defense-related matters. That in part has its origin in the fact that my father was drawn into the US Army in 1944, in the Second World War, because, again, of his…

COHEN: His expertise in languages…

BALDESCHWIELER: Language expertise and technical expertise.

COHEN: By then he must have been a citizen.

BALDESCHWIELER: He was a citizen. He had never actually properly completed his Swiss military service, so he made a deal with the US Army whereby he would join an intelligence mission into occupied France and Germany if they would straighten out his affairs with the Swiss. This turns out to be important later on. So he went to the Swiss Consulate in Washington and the matter was settled for $50 and a nice dinner. [Laughter] So that was a good deal. He had gone with the US Army into France, Germany, and Belgium. His job was to figure out what in fact the Germans were doing and had been doing with regard to…

COHEN: Is that part of what they called the OSS [Office of Strategic Services] in those days? It was a precursor to the CIA.

BALDESCHWIELER: Yes. But his job in particular was to find out what they had been doing with regard to synthetic fuels, synthetic organics, propellants—a whole variety of matters in which the Germans had of course amassed considerable technical expertise.

COHEN: Yes. They were the chemists of that decade.

BALDESCHWIELER: Yes. At any rate, during the time of the Korean War I was in the ROTC at Cornell. I was quite enthusiastic about the ROTC. I was in the ordnance, and quite interested in matters of weapons, small arms, artillery, tanks, tactics.

COHEN: Now, this was not instead of your university work?

BALDESCHWIELER: This was part of it. But it did manage to keep me out of the Korean War directly.

COHEN: But then, when you finish, aren't you a reserve officer or something?

BALDESCHWIELER: Yes. So I have a lot of military in my career. So those were the years at Cornell. A significant thing happened in my last year at Cornell. It was a five-year program in engineering in those days, so you had, I guess, the equivalent of a master's in engineering. For some reason, which was more or less accidental, I stopped in to talk with Paul Flory. Paul Flory you may remember. He ultimately became a Nobel laureate [1974]. He had had his early teaching career at Ohio State, and then he worked with my father, at Standard Oil of New Jersey, during the war, on the synthetic rubber program. So he was a friend of the family. Later he was at Cornell, in polymers. But I never took a course from him. A physical chemist of great distinction, of course. So I talked to Paul. I was of course, ready to be a captain of industry. I was prepared to go off to a career at Dupont or Exxon or wherever. And Paul very quietly described to me something about the structure of molecules. Now, the chemistry that we took was very classical. I had just done it—I had done it well, but without a lot of inspiration. But just that conversation with Paul Flory…. I remember he talked about the structure of CO_2 and the interesting fact that this wasn't a rigid structure but a dynamic structure.

COHEN: It moves around.

BALDESCHWIELER: Well, the electrons move, but it also has vibrational modes and bending modes.

COHEN: I imagine your Erector set…

BALDESCHWIELER: Exactly. [Laughter] I can remember saying, "You know, I really can understand this—that the molecules in fact have a structure and dynamics and that you can in fact interrogate these vibrational motions, for example, with infrared spectroscopy, and literally determine what those frequencies are." I understood classical mechanics. You can work out with great precision what the structures are.

COHEN: And this hit you just from this conversation?

BALDESCHWIELER: Yes. It really made sense.

COHEN: Now, why do you think he did this? You might think he was encouraging you to go on with school.

BALDESCHWIELER: Yes. And it may be that I asked him whether I should do a PhD. Yes, I think that's right. The reason I did was because I had met some people with PhDs and mostly I didn't think they were all that much smarter than I was.

COHEN: They probably weren't.

BALDESCHWIELER: [Laughter] Maybe they weren't. So perhaps that was the reason I went to talk to him—to see if it made any sense. I think you're quite right. And that's how the conversation began. It's one of those turning points in your career. I really felt that this was something I could understand. The mechanics were not only like the Erector set but they were also a little bit more interesting, because of the quantum mechanical aspect. The quantum mechanical side was even more interesting. In fact, on that scale, things aren't exactly like the Erector set; they're more refined. In those days, the mid-fifties, infrared spectroscopy was just starting, and it began to reveal—I mean, you could literally look at molecules. I thought about this some. You know, if you really could look at them closely and measure them with a sufficient number of frequencies, you could in fact understand a good deal about them.

COHEN: So this was all very intriguing.

BALDESCHWIELER: Yes, really *very* intriguing.

COHEN: Did he suggest a specific graduate school for you to go to?

BALDESCHWIELER: Yes. I asked him what one might do along these lines, and he suggested a younger person at Cornell at the time whose name was Bob Hexter. I think he made some other suggestions. One was a good young person at Berkeley whose name was George Pimentel. He mentioned a few other people in this field. Then what happened? Ah, yes—I set out for a summer job.

COHEN: You worked every summer you were in school.

BALDESCHWIELER: Oh, yes. I had arranged a job at Los Alamos, because, again, I was…

COHEN: You still were interested in the military.

BALDESCHWIELER: Yes, the military and the weapons side of things. I drove out to Los Alamos. Before I got there, I saw this strange character. I was driving my parents' car at this point. Father, with his affairs corrected with Switzerland, had left to go back there, with Mother. So I had the car.

COHEN: They went back for the summer?

BALDESCHWIELER: Yes, and that's how I had the family car. I was driving across the barren stretches of west Texas and New Mexico. There on the side of the road was a funny-looking fellow with a suitcase and a fishing pole. And I thought, "There are no fish here." [Laughter] I stopped. It turns out he was also a young student, Larry Piette, who was going to spend the summer at Los Alamos.

COHEN: [Laughter] You just saw him there and stopped?

BALDESCHWIELER: Yes. He was hitchhiking.

COHEN: Those were the days when you picked up hitchhikers.

BALDESCHWIELER: Oh, yes, and he was articulate. It turns out that he was finishing a PhD program at Stanford in chemistry. He said, yes, that the best programs in physical chemistry and molecular structure would be at Berkeley. He didn't suggest Stanford. He did suggest Wisconsin and Harvard. And so, here was a real live peer—a compatriot—who knew what was going on.

COHEN: Where was he going fishing? Did you find out?

BALDESCHWIELER: Well, he was as uninformed about New Mexico…. For me, this was the trip to the great beyond. There weren't too many fish. But that's what he thought he was going to do.

COHEN: So you worked that summer at Los Alamos?

BALDESCHWIELER: Yes. They didn't know quite what to do with me, it turns out, so they took me around to several different groups at Los Alamos. It was still a closed city, with lots of security and so forth. But in one of the labs a fellow looked at me and said, "Ah-ha! We finally have a person of the right caliber." And I thought, "Oh. Well, they recognize all of my talents." It turns out that what they meant was caliber from front to back. What they needed was a thin fellow. [Laughter]

COHEN: OK. [Laughter] Explain.

BALDESCHWIELER: They had been building a reactor, which was going to use a molten salt of uranium, which was corrosive. The reactor was a high-pressure stainless steel vessel, but it was lined with gold. My job was to be pushed into this thing, to see if there were any imperfections in the gold.

COHEN: I see. And so you were of the right size. [Laughter]

BALDESCHWIELER: I had a little bottle of nitric acid with an eyedropper, and ferri-ferrocyanide. And I would put a few drops of nitric acid on what looked like a defect, and then the ferrocyanide. If the crack went through to the steel, then you'd get a blue color. But this was a very claustrophobic…

COHEN: I would think it was hazardous also.

BALDESCHWIELER: Yes. And I had to be pushed into this. It was like being loaded into a gun barrel. I don't know how I did this job. But there would be flecks of gold on my coveralls when I emerged.

COHEN: I see. For some reason, I don't think gold was bad, healthwise. [Laughter]

BALDESCHWIELER: But one did all kinds of things in those environments. The standards with regard to health and safety have changed. We would literally carry reagent jars with uranium samples. I had fun waking up the people at the monitoring stations.

COHEN: Were you intrigued with the landscape in New Mexico?

BALDESCHWIELER: Oh, yes. Stupendous! And we had time. It was not such a high-pressure job, so we had time to hike and climb and so forth.

COHEN: You were there for the summer.

BALDESCHWIELER: Yes. But I came home by way of Berkeley.

COHEN: To look at it. And you talked to Pimentel and some of his people?

BALDESCHWIELER: Exactly. So that sealed the bargain. After finishing at Cornell in 1956, I went out to Berkeley.

COHEN: As a graduate student.

BALDESCHWIELER: Yes. And I was able to defer my military service, although I had a commitment to serve two years active duty and eight years of reserve. It was a substantial commitment. By that time, the Korean War had wound down, so I wasn't needed. So I did my military reserve training in California—actually, at the Presidio.

COHEN: Was Pimentel your professor?

BALDESCHWIELER: I worked with Pimentel at Berkeley. Those were just wonderful years. Berkeley was the place. The Radiation Lab was up on the hill. They had a large nuclear program. I was interested in and informed about such matters. There was a huge amount of direct support which flowed to Berkeley through the Radiation Lab, the forerunner of Lawrence Berkeley Lab. The tradition from the war was there. I had classes from Glenn Seaborg and Ken [Kenneth S.] Pitzer. It was really a wonderful time to be there. It was lively. And of course it was a hotbed of physical chemists. And Pimentel was a young Turk at the time, interested in infrared.

COHEN: So how many years were you there?

BALDESCHWIELER: Three years—actually, less than three years. I went through very fast. I went through too fast, actually.

COHEN: Well, those were years when people did do that. They wanted to get on with it, and there were jobs.

BALDESCHWIELER: Yes.

COHEN: So you enjoyed living in California.

BALDESCHWIELER: Yes. I found some friends. I lived first at the International House at Berkeley, and then with a group of Frenchmen. That was great fun, because my language skills in French were good enough to talk and get along—although I spoke the French of an adolescent. But still I was…

COHEN: Well, I guess any French is better than no French.

BALDESCHWIELER: That's right. It was adequate. So we found a lovely place up in the Berkeley hills.

COHEN: Now, you say "we." Were you married already at this time?

BALDESCHWIELER: No, no—just with various students. I can remember my first chemistry seminar at Berkeley, in which both Ken Pitzer and Glenn Seaborg were highly visible participants.

COHEN: That must have been very exciting.

BALDESCHWIELER: Yes, these were all people—like Paul Flory—who I thought were dreadfully senior. But of course they were essentially in the most productive parts of their careers. I can remember the arguments—I can remember them covering the board with Greek letters, the symbols of various electronic and molecular states. These were all things I had not really studied.

COHEN: What did you actually do your thesis on?

BALDESCHWIELER: Infrared spectroscopy. But I had to learn all of the quantum mechanics and so forth. This was a great mystery to me, but in fact, as I say, wonderful.

I finished. And, as I say, probably too fast, because I went through the courses and other requirements very quickly. I did research that was very successful. As it turns out, I was very good at building instruments. They hadn't seen many people with my mix of skills in chemistry—somebody who really liked to put things together and build things. After about a week in the Pimentel group, I had disassembled the infrared spectrometer, which was a treasure. It was the only one they had. I had the parts all over the floor. Pimentel himself walked in and saw this. [Laughter] I suspect he could have had a cardiac arrest. But I was curious as to how it worked. I got it all back together.

COHEN: And maybe it worked better.

BALDESCHWIELER: I'm not sure it worked better, but…

COHEN: But you knew how it worked.

BALDESCHWIELER: I certainly did.

COHEN: So then, were there postdocs in those days? Did you go on and do something like that?

BALDESCHWIELER: There were postdocs, but I didn't do that. I don't know if you want to hear more about the goings-on at Berkeley.

COHEN: Well, if you have some names to mention that were influential in your life.

BALDESCHWIELER: Well, let's think. Of course, Pitzer and Seaborg were both…

COHEN: They were professors.

BALDESCHWIELER: Yes. Pitzer had been active with the forerunner of the Atomic Energy Commission, the AEC. But he was back at Berkeley full time. He was certainly a mentor and an inspirational person. He had been Pimentel's advisor. So there was kind of a hierarchy there.

COHEN: Who were some of your fellow students? Did some of them go on to be notable people?

BALDESCHWIELER: Yes, but I would have to think about that. Again, it was a wonderful group. There was Bill [William D.] Gwinn, and his student also became a faculty member—Rollie Myers, in microwave spectroscopy. And Sunney Chan.

COHEN: Ah, he's here at Caltech. Was he a student the same time as you were?

BALDESCHWIELER: He was a couple of years behind me. So he would be a notable acquaintance I made at that time.

COHEN: So you did finish up, even though it was faster than you wanted. Then what did you do?

BALDESCHWIELER: Well, I went to war.

COHEN: Oh, you actually went into the army.

BALDESCHWIELER: The time had come to do my active duty. I had done a number of years of reserve training. So I got swept into the army at that time. There were various events that had happened, which changed the demand for young second lieutenants.

COHEN: There was no active war going on.

BALDESCHWIELER: I was very fortunate—at that time there was no active war going on. There were a few other emergencies that changed things. I was sent to the Aberdeen Proving Ground.

COHEN: Now, this is in Maryland?

BALDESCHWIELER: In Maryland. That's right.

COHEN: OK. I think maybe we're going to stop now. [Tape is turned off]

JOHN D. BALDESCHWIELER

SESSION 2

January 16, 2001

Begin Tape 2, Side 1

COHEN: You wanted to say a little bit more about your army service.

BALDESCHWIELER: Well, there are one or two other critical details. Let me throw in a few more things about Berkeley. I went to Berkeley with an NSF [National Science Foundation] fellowship. At that time I had my first chances to follow up on some of the investing interests that came from my father's side. It turns out that I lived very frugally at Berkeley and was able to save money from my NSF fellowship, [laughter] if you can believe that. So I had the chance to make early investments in some high-technology companies of the time.

COHEN: Now, was this something you discussed with your father and decided together?

BALDESCHWIELER: No. It's just that his philosophy was that you should invest in things you understand. So I simply took my first trial steps in that direction, investing in some of the interesting technology companies of the 1950s.

COHEN: Were any of them very significant? Do you want to mention what they were? Were they new companies?

BALDESCHWIELER: Well, it turns out that the most exciting one was Varian Associates. It was brand-new at the time, based on technology that came both from Stanford and from Harvard. From Felix Bloch at Stanford, and at Harvard it was [Edward M.] Purcell. Anyhow, Varian was one of my first investments.

When I left Berkeley—actually, I had finished my thesis and the final examination and so forth—I first had one of the secretaries start typing my thesis, but she wasn't able to finish it, so I had to finish the typing myself. And those were the days before Xerox machines, and you had multiple carbon copies, so it was quite a chore. Every time you made a mistake, that was a

significant problem. Anyhow, when the time came for me to leave Berkeley, I said my farewells to the faculty and fellows. On the day I was leaving, my last act was to turn in the thesis to the administrative center, the graduate dean's office. A lady behind the desk took down my name, and so on and so forth. And then she flipped through the thesis and noticed that there were two different fonts of typing. She said, "I'm sorry. We can't accept this." [Laughter] I said, "What do you mean you can't accept this?"—because my car was packed, I had left my apartment. And I was getting married in two weeks, and then I had to go into the army. There was no way I could reproduce the thesis. She said, "Oh, no. You can't have two different styles of type in the thesis. That's against the rules." Fortunately I had the presence of mind to say, "Is there anyplace where that rule is written?" She went through all of the documents on theses margins and all those other kinds of things, and nowhere did it say you couldn't have two different fonts in the thesis. [Laughter] Just at that moment, the dean walked by, and I said something like, "Hey, Dean, she says that this is unacceptable, but in fact there's no written rule about this," and he said, "Well then, in that case we can accept it." [Laughter] So I left the office with great dispatch and departed Berkeley.

My research at Berkeley was focused on infrared spectroscopy. I think I told you about dismantling the spectrometer. My research centered on the structure of molecules that are trapped in matrices made of rare gases at extremely low temperatures, cryogenic temperatures. Under those conditions, one can make very energetic species, trap them, and look at their structures. It was a successful piece of work. I often tell people that if I had known at the beginning what I knew at the end, it could have been done in three weeks. [Laughter] But I'm sure that's the case with anyone's research project.

So I left Berkeley. I had been courting my future wife. She was someone I had met at Cornell, and she came out to Stanford. During my Berkeley days, we had been courting. We had decided to get married in June of 1959. We had the opportunity to take a brief trip to Europe, where we stayed in Freiburg, and I had the chance to give some presentations in German, which was a struggle but fun as well. Then we came back to the Aberdeen Proving Ground, to do my military service. I was first assigned to troop duty at Aberdeen—I was a second lieutenant. But it turns out that there was a major at Aberdeen who discovered that I had a PhD. And he said, "Well, you know, Lieutenant, most people think that in the army you're either assigned to post A or to post B. But there's a third possibility, which is in process." He

managed to put me in a kind of suspension which was in process, in which time he got me assigned to the research laboratory at Aberdeen. This was, after all, the Aberdeen Proving Ground. You may remember that that's the place where some of the first digital computers were both built and used to calculate the trajectories of artillery and missiles. So I was assigned to a project that involved measuring the permeability of tanks—army tanks—to neutrons. It was a time when the military was concerned about tactical nuclear warfare. And this project involved using a Van de Graaff generator—an accelerator—and pointing the business end of the accelerator at a tank and measuring how many neutrons went through. This was not a particularly taxing project from the standpoint of theory, so I had time to do lots of other things—to learn some of the things I had skipped when I was going through Berkeley. Aberdeen had been quite an active and leading institution during the Second World War, and it turned out that there was still a person there who had taught scientific Russian. So I took the opportunity to learn some Russian at Aberdeen. I also found out that there were a number of other kinds of interesting people in the same situation. There was a young lawyer who had been assigned to this post, and he discovered that there was a patent office at Aberdeen and they seemed to be just sitting there, not doing much. He said, "Well, if there's a patent office, we should write some patents."

COHEN: Is that OK in the army, to do research and write patents on it?

BALDESCHWIELER: Well, we didn't know. [Laughter] So we set about doing this.

COHEN: "Where is it written?" you could have asked again.

BALDESCHWIELER: Yes. So we wrote up some patents. Some of the things had to do with controlling this Van de Graaff generator they were using, but other things as well. We wrote these up and submitted them to the patent office—and of course once they got a submission, they had to do something about it. So this ancient machinery began to creak into motion. Sure enough, it began to spew out patent applications. I had been promoted to a first lieutenant by that time, I should say. The gracious major who had gotten me the position at the research lab

was there to congratulate me and pin on my silver bar. But then I received notice that if I wanted to, I could be discharged from active duty. I accepted that, because I had…

COHEN: How long were you there?

BALDESCHWIELER: I did six months of active duty.

COHEN: Well, that's not a lot of time, considering all that you did there. So you left with patents under your belt?

BALDESCHWIELER: Well, yes. Although at the time I left none of those had actually been issued, it turns out that later they were, which is an interesting part of the story. But I then had the chance to go to Harvard. I had visited there and given a seminar on my infrared spectroscopy and been offered a position at Harvard. So coming out of the army [1959], I actually took a salary cut. [Laughter]

COHEN: I think that's often the case with a first professorial job.

BALDESCHWIELER: That's right. Someone explained to me that the mark of success for an academic person is the first time he places a student at a salary less than his own, [laughter] which means that you place them in a first-class academic job.

So my wife and I packed our U-Haul trailer and pulled it with our little Volkswagen up to Cambridge, where I set out on an academic career of teaching and research at Harvard. I think people today have no idea of how frugally the academic institutions operated. I believe I had an account of $150 to support my research, which was not much. So, one had to do with baling wire and resourcefulness. I found out all sorts of interesting things in those early days at Harvard. One of the things was that there were certain kinds of clamps to put together the racks on which you built the experimental equipment, and those seemed to be in short supply. I found a way to buy some of them with my $150, and I used them as bartering material with the other…

COHEN: [Laughter] Your business acumen coming out.

BALDESCHWIELER: Well, one had to survive. I rode my bicycle downtown to Central Square in Cambridge and found a way to get these, and also some of the ground-glass stoppers and so forth that you needed to make the equipment. There was a ready secondary market for such things in those early days at Harvard. So we were able to get some research started. Since *Sputnik* went up [1957], the climate for the support of research had changed dramatically. I began to do research on nuclear magnetic resonance—in particular, nuclear magnetic double resonance.

COHEN: That was very new at that time.

BALDESCHWIELER: It was very new. We had some new ideas, but I had also to learn a good deal. In the process, it became clear—at least, to the younger faculty—that one could apply for and get research grants on a scale that was unheard of before. So I sent in an application to buy an NMR machine, which was a very large matter. I can remember the disbelief with which the senior faculty looked at me—almost as if I was violating a certain code of ethics to apply for so much money.

COHEN: Did you ask the NSF for this money?

BALDESCHWIELER: Yes. I think this probably went to the National Science Foundation, or to the National Institutes of Health. I don't remember which. But the grant was approved and I was told that I had the funds. So I went to the Harvard stockroom and asked, "How do you get an NMR machine?" And they said, "Well, for anything you buy here, you fill out this piece of paper in duplicate." [Laughter] So I did that, and soon enough the NMR machine appeared. It was an utter revolution for the time. But I found that by being aggressive in these matters, you could indeed attract some support.

COHEN: You asked, you got.

BALDESCHWIELER: Yes. So that's how things began at Harvard. I can remember one key event there. I had been given a small office. I was busy interacting with graduate students, who of course were essentially my age. I was still quite young to be a faculty member, and indistinguishable from the graduate students and postdocs. But one day E. Bright Wilson came

into my office. Now, E. Bright Wilson, I thought, was at least twelve feet tall. [Laughter] He had worked with [Linus] Pauling and written a key book—text—on quantum mechanics [*Introduction to Quantum Mechanics, with Applications to Chemistry* (1935)]. But also he was the key innovator in the analysis of infrared spectroscopy. He came into my office to ask me a question about quantum mechanics. I was dumbfounded. I didn't know the answer.

COHEN: Dumbfounded that he would ask you?

BALDESCHWIELER: [Laughter] That such a person would think that I could possibly have something useful to say on the subject. I think this was the beginning of the revelation that in fact…

COHEN: You knew something.

BALDESCHWIELER: [Laughter] Yes—or that the others didn't know that much. Then—well, you can believe that I went ahead and figured this out very fast after he left my office. In fact, I asked some of the graduate students.

COHEN: Yes—well, you wouldn't have formally studied quantum mechanics in those days, would you?

BALDESCHWIELER: Yes, but I went through it so fast at Berkeley that I never really understood it. In those early years—maybe the first five years—you're learning.

COHEN: Everything.

BALDESCHWIELER: Everything, and very fast. And it's fun. There's a period of enormous growth, and I was certainly in the midst of that. So Harvard was a lot of fun and very successful. In the course of this, I received a letter from the commanding officer at the Aberdeen Proving Ground that congratulated me on having received a patent. [Laughter] It provided me with a ticket to fly to Aberdeen to receive a commendation for this matter and the cash award, which was $25.

COHEN: Oh, so this was a coup for the army.

BALDESCHWIELER: Yes. I received a check for $18.75—$25 minus the withholding tax. [Laughter]

COHEN: What was the patent on?

BALDESCHWIELER: Well, in the end there were probably four or five of these. But this particular one was on a device for controlling the beam of the Van de Graaff generator—something very simple. It was a feedback scheme that you'd think would be so trivial that you wonder how anyone would ever award a patent on such [a thing]. So Aberdeen asked me to come back as a consultant. This was not a particularly glamorous or distinguished place.

COHEN: Now, you were finished with the army? You weren't a reserve officer?

BALDESCHWIELER: Oh, I was a reservist. I had eight years of reserve duty.

COHEN: OK. So you still had to do your reserve bit.

BALDESCHWIELER: Yes. I had been promoted to the rank of captain, and I spent some of my summers doing reserve training. One of those summers was at an army camp up along the St. Lawrence River—Camp Drum. It was a very desolate place, and there wasn't a lot for me to do. But I had some troops and I had to dig out a foxhole in the ground and do various other kinds of things. But I fortunately brought along with me some journals, and I had plenty of quiet time to read the *Physical Review* and try to understand the physics of density matrices. That's where I began to really dig into the underlying substance of nuclear magnetic resonance, which is a very fascinating business. Most chemists who learn spectroscopy learn the spectroscopy of single atoms and molecules. But NMR is done with a coherent radio frequency excitation. This means that all of the nuclei are excited in a coherent way and their responses to this excitation are also coherent. This type of coherent response was not normally considered in other areas of spectroscopy, but it turns out that with the development of lasers, coherent responses are central. And so in this sense NMR very much led the physics of molecular spectroscopy. But the person

who had understood this all in advance was Felix Bloch at Stanford. His papers, which were published between 1946 and 1951, were utterly impenetrable to me.

COHEN: So that's what you had with you that summer?

BALDESCHWIELER: That's what I took with me. [Laughter] During my military service in the summer, I had a quiet chance to read those papers again and again and again, until I finally began to grasp the essence of those ideas.

And then, while at Harvard…. Harvard had also been a place where a good deal of early work on magnetic resonance was done—not only by Purcell in physics but also by Herb [Herbert S.] Gutowsky in chemistry. He had subsequently moved to the University of Illinois, where he was a distinguished leader in the field. One of his colleagues, whose name was Charlie [Charles P.] Slichter, came to Harvard to give a series of lectures on, would you believe, the density matrix. So I had a chance to begin to learn some of these subjects in detail. I worked on nuclear magnetic double resonance. I was using two frequencies to excite a system simultaneously. It went very, very nicely. We published a good deal and our work received a good deal of attention. But I had also begun to do some consulting work with Aberdeen, so I had a little bit of outside activity in addition to the…

COHEN: I have the impression that you liked that.

BALDESCHWIELER: Yes, I enjoyed that. You know, sometimes the issues were of modest intellectual content but of significant impact. So we had a good deal of fun on both sides of that. [There were] lots of wonderful students at Harvard.

COHEN: Now, this was all pre-Vietnam days, wasn't it?

BALDESCHWIELER: Yes.

COHEN: So you wouldn't have been bothered by students who minded that you were doing army work.

BALDESCHWIELER: No—but that did come later, in fact. My days at Harvard were 1959 through 1965. Harvard had a reputation for promoting virtually no one from a junior position. When I came to—I guess it was—year five in this process, I decided it was time to strike out. So I spoke to various people about my potential interest in moving, and there was a lot of interest at other academic institutions in inviting me to leave Harvard. That included Stanford, which was of course particularly interesting to me because Varian Associates, the producer of the magnetic resonance machines, was in Palo Alto. And Harden McConnell, who was a brilliant, dynamic person in this area, had moved from Caltech to Stanford. So the deal was done and I was invited to go to Stanford.

While I was at Harvard, our double resonance work was exciting and, I think, important. But in the field of chemistry there was a good deal more interest at that time in molecular beams—that is, isolating chemical reactions to assess single pairs of interacting molecules and atoms. And Harvard had hired Dudley Herschbach, who had done his PhD work at Stanford and then was a leading young faculty member at Berkeley. I looked at the molecular beam work and thought, "Well, you know, the NMR studies are finally going to become mature and I should think of something else to do." Of course, that particular judgment was completely wrong. [Laughter] The field of NMR went on to really enormous areas of growth. But I thought it was perhaps time to change my field of research.

I had a chance to give a talk at UCLA—in that period of time when I was on the circuit, looking at other places. I met with Dan Kivelson, who is still a faculty member there, and he showed me some work he had done, looking at the resonances you see when you make a plasma, which is a highly excited mixture of electrons and ions, from an arc. When that's in a magnetic field, the electrons and the ions—but the electrons in particular—move in a circular, or cyclotron, pathway at a specific frequency. And he was able to detect those electrons with an electron resonance spectrometer. He showed me that you could not only see the resonance of the electrons but you could also see the line width, which was affected when the electrons collided with neutral molecules. From the line width of the electron resonance, you could calculate the cross-section for the collision of the electrons with neutral molecules. I thought that was fascinating. On the way home, it occurred to me that one should be able to do a similar experiment with molecules rather than with electrons. And so I set to work figuring that out. I remember I went to George Kistiakowsky—the *great* Kistiakowsky. You may remember him.

COHEN: Well, I know of his reputation.

BALDESCHWIELER: Kisty was a person who had worked with the atomic energy program during the war. His field was kinetics and so forth.

COHEN: He was at Harvard, wasn't he?

BALDESCHWIELER: Yes. And he also was the president's science advisor during the Eisenhower administration. But he was still very busy with the COSPUP [Committee on Science and Public Policy] and PSAC [President's Science Advisory Committee]. So I assisted with the physical chemistry teaching. He was supposed to teach the course, but he was often gone, so I taught much of the course, which was fine, except for the lecture on glass blowing, which he always did—that was a wonderful show. I took the idea of doing cyclotron resonance to Kistiakowsky, and he said, "John, it will never work." [Laughter] That's just what I needed, of course.

COHEN: To spur you on. [Laughter]

BALDESCHWIELER: That's right. [Laughter] I had a wonderful student with me at Harvard whose name was Jack [Jesse L.] Beauchamp, who came from Caltech as you may know. He was a *Wunderkind*, and I had the fantastic good luck that he elected to work with me. So I put him to work thinking about this, and I also set to work thinking about how to build this new instrument. The offer from Stanford came through, and so as part of that I negotiated sufficient funding to build the instrument.

COHEN: Now, did you go as a full professor to Stanford?

BALDESCHWIELER: I went as an associate professor. Again, very conservative days. I had noticed a reference to an obscure journal in which the work of Kivelson at UCLA on electron cyclotron resonance had been published. I noticed a question in the discussion period which was asked by a person whose name was Harmon Brown. Now, Harmon Brown had been a graduate student with me at Berkeley—he was one or two years older—and he had gone to work at Varian. I thought, "If Harmon Brown is asking a question about electron cyclotron resonance, I

wonder if Varian has an interest in the cyclotron resonance field." So I followed that up. Although Varian was slow to admit this, it turns out that indeed they did have an interest in cyclotron resonance. Because the high-resolution magnet for this technique would come from Varian, and since I had money from Stanford, Varian agreed that they would build the first prototype. They would get the benefit of learning how to do this at somebody else's expense, and I would get the benefit of the substantial instrumentation expertise there. The person who led that project was Peter Llewellyn. So, from the Varian side they developed the instrument. Jack Beauchamp worked on the theory. And I moved the whole group to Stanford in 1965.

COHEN: So you took up residence in California again—permanent residence in 1965.

BALDESCHWIELER: Back to California in '65. The work at Stanford went beautifully. We had good support. The cyclotron instrument was completed. I also brought the NMR machine, so I had two research programs going.

COHEN: You must have needed a lot of room.

BALDESCHWIELER: Yes. It turned out to be a large group, and I had students of spectacular quality. You know, sometimes you look at a group of students and say, "They can't all be this good!" It was just a wonderful time.

COHEN: The stuff must have been so exciting that it attracted them.

BALDESCHWIELER: It was exciting, it was clean. It was this kind of science at its best. Beauchamp had done work with the Boltzmann equation. We predicted that in effect you could look at multiple ions with different resonant frequencies and predict the ion molecule chemistry in the gas phase. It turned out to be very interesting stuff. The chemistry was unique, the resolution was high. You could look at all different kinds of reactions under conditions, for example, where there's no solvent. It turns out that much of traditional chemistry in solution is driven by the solvent. If you can look at the reactions in the absence of the solvent, then you can deduce what the effect of the solvent is. This, again, attracted a lot of interest. I received a Sloan Foundation fellowship as part of that high visibility, and also the Pure Chemistry Award

[1966] from the American Chemical Society. This was for a young person—under the age of thirty-five. So we were having fun. Then I got an invitation from Bill McMillan, on the UCLA faculty, who was a distinguished chemist and had also been deeply involved in highly classified military matters—in nuclear, tactical, and electronic issues. This was an invitation to attend a defense science seminar. Bill had noticed that the people who were involved in advising the government on technical matters relating to defense were all getting older.

COHEN: They were still left over from the Los Alamos days.

BALDESCHWIELER: They were left over from two sources—either Los Alamos or the MIT Radiation Lab. It turns out that Bill had convinced the forerunners of ARPA [Advanced Research Projects Agency] that they should spend some money introducing a new wave of young academic people to defense-related matters, and that that would be of substantial long-term benefit to the country and to national defense. I was a natural for this, and I was delighted to do it. I spent—oh, it must have been four weeks in a series of meetings with high-level uniformed military.

COHEN: Did you go to Washington to do this?

BALDESCHWIELER: Well, it was primarily held at UCLA, so it wasn't far to go. But then we did a lot of field trips, including visits to various ballistic-missile development sites and an aircraft carrier. A rowdy crew of young people, I might say. [Laughter] Immediately after this, I received an invitation from the Army Scientific Advisory Panel to become a member. Again, this made good sense, because I was one of the very few young academics who had credibility in the academic sense but also knew something about small-unit tactics and small arms. I had been an ordnance officer, so I knew and also enjoyed the technology of tanks and artillery and such things. So I was swept into the Army Scientific Advisory Panel and had a chance to do many interesting and hopefully useful things in that context.

The work progressed at Stanford. I had some children, by the way, [laughter] as all of this was going on. My first son, Eric, was born in 1964 in Cambridge. Karen came in 1966. David came in '74.

COHEN: You lived in Palo Alto?

BALDESCHWIELER: Yes. Well, actually in Portola Valley. We had a little house in the hills.

COHEN: Do you know what a little house in Portola Valley costs now? [Laughter]

BALDESCHWIELER: Oh, I know. [Laughter] And to think what I bought it for.

Anyhow, the Army Scientific Advisory Panel began to occupy time. I put real effort into this. It was interesting and fun for me. It was a good panel, and I worked on a variety of interesting problems. Of course, by this time our involvement in Vietnam had grown. For many of the military, of course, it was an opportunity to try their skills. I got swept into this in an interesting way. There was a research group at General Electric.

COHEN: In Schenectady?

BALDESCHWIELER: Yes. They claimed that they had developed a method for detecting people, and the idea was to use this in Vietnam. It was very hard to find the Vietcong, particularly under layers of jungle canopy or under conditions of darkness. GE had a device that they claimed could literally detect people by odor—a people sensor.

COHEN: Where would this be? On an airplane?

BALDESCHWIELER: Well, it could be carried by a soldier in a backpack unit, or it was helicopter-mounted. It was based on a technology that was a derivative of a scheme used to find German submarines during the Second World War.

COHEN: That was a sonar thing, wasn't it?

BALDESCHWIELER: Well, later in the war, when [the Germans] were under great pressure, they developed what's called a snorkel, a breathing apparatus, so they could run submerged under diesel power, with only a small vent on the surface. An engine spews out particulates. So during the war, we used the equivalent of a cloud chamber to detect particulates. The way it

works is that a particle acts as a condensation nucleus and creates a large droplet or a track that can be easily seen with primitive optics. So the antisubmarine forces found submarines by flying a search track with this particle detector. In fact they were able to find submarines this way.

Begin Tape 2, Side 2

BALDESCHWIELER: But GE had decided that if they used the same cloud chamber but put in front of it a wick that had hydrochloric acid in it—every first-year chemistry student knows that if you have a bottle of ammonia and hydrochloric acid you get a cloud of particulates. They reasoned that since people must give off ammonia, if you had a wick with hydrochloric acid in it, people giving off ammonia would create particulates, which could be counted, because a cloud chamber is very sensitive; this would be a way you could find people.

COHEN: But at a distance?

BALDESCHWIELER: [Laughter] Well, your expectations, it turns out, are exactly right. But this had built up a huge head of publicity within the military. The Army Scientific Advisory Panel asked me to go see if this was real. Because, you know, there were also people with divining rods and other such things. Everybody believes in his own particular version of this. It was very hard to do a serious test. GE had some chambers which were clean and isolated. You could detect a person in these chambers, but just from a small range. You could tell smokers from nonsmokers, by the way, which is interesting. But I never thought it made a lot of sense. It was already deployed in Vietnam. There was a version that was carried on your back, and the snooper end of this was mounted on the front of a bayonet. The soldier would walk down the pathway with this gizmo in a backpack with a little pump going—*putt, putt, putt, putt*.

COHEN: This was a little cloud chamber he was carrying around.

BALDESCHWIELER: That's exactly right. Then there was a version that was mounted on a helicopter. So I went to Southeast Asia during the war to look at this, and of course to look at some of the other technology being employed. It was a very interesting time.

COHEN: You were not caught up in the politics?

BALDESCHWIELER: Well, I was very loyal to the US government, and I remain very loyal. My attitude at the time was that the war itself didn't make a lot of sense to me, but the young servicemen who were exposed were my age and my colleagues, and I felt…

COHEN: You had to do what you could.

BALDESCHWIELER: Yes. I would do the best I could to give them tools that would help save their lives. Basically, I have a very patriotic view of this and thought that most of the protests were really very ill-conceived. But at Stanford, as at other places, there were a lot of very robust protests.

COHEN: Actually, not at Caltech.

BALDESCHWIELER: No, not at Caltech. But at Stanford I stood guard at the labs at night, because people were throwing rocks. Anything technical was considered suspect. At the outset of this trip to Vietnam, there was an army car that was supposed to pick me up, and that wasn't too good. The driver of the car practiced three times, on three separate days, coming to my house and making the run from my house to Travis Air Force Base so he would be sure to be on time. He had to do this quite precisely. Fortunately, no neighbors reported this obvious olive-drab car. On the appointed day, he was there at *exactly* the second—this was not plus or minus a few seconds. I got into the car and arrived at Travis Air Force Base at *precisely* the right moment. And then it turns out that my airplane to go to Southeast Asia had lost its windshield in the Philippines. There were so many people going that every seat was assigned. You were assigned to a particular seat. If that seat didn't come, you didn't go. So I sat three days at Travis Air Force Base, waiting for the seat to arrive. I had been very careful not to mention to my colleagues, of course, where I was going and what I was doing. It was a remarkable tour. I was there about a month in Southeast Asia and got to see all kinds of things that you would not possibly believe.

COHEN: You went right up into the front lines, where the fighting was?

BALDESCHWIELER: Well, the fighting was everywhere, and I was shot at a number of times. I recall wondering, "What are they shooting at me for?" I hadn't done anything—I was just trying to get the technology straight. [Laughter] But we flew with the helicopter gun ships. I think the army personnel were called the snooper battalion. They had a nice patch, with an animal that had a curious long nose. There would be helicopters flying at treetop level, and then two more gun ships behind. The chemical officer would watch the machine. It had a needle in it. The needle would bounce back and forth, and he would be watching this. And we'd fly a search pattern.

COHEN: Of course, you didn't know who they were—whether they were soldiers or children or whatever.

BALDESCHWIELER: No. He'd say, "Mark!" and they'd either put down a flare for an air strike or mark the coordinates to bring in artillery. From time to time, they'd hit something. That is…

COHEN: You'd find somebody.

BALDESCHWIELER: Yes. But it turned out that the movement of the needle was completely random.

COHEN: Could you tell that?

BALDESCHWIELER: I couldn't tell for sure at that time. But I thought about this. The human observer, you know, is remarkable. They'd fly a similar search pattern every day. What I suspect is that the human observer was reacting to some other set of clues. You know, this is very hard to test with a proper set of controls. What I recommended was that they go back and do a proper controlled experiment.

COHEN: Fly without it and fly with it?

BALDESCHWIELER: There are all kinds of hypotheses about this. So we set up in the Everglades a month or two later. We had a tank of ammonia with a meter, so we knew exactly how much ammonia was coming out of the tank, and we measured the meteorology.

COHEN: You came back here and did this in Florida.

BALDESCHWIELER: Yes. Your instincts were exactly right: The distance and concentration profile simply didn't work. You know, the developers had also done experiments in Panama. A soldier with the input on the end of his rifle would walk down a trail, and there would be an ambush set up at a certain point, which would consist of some GIs sitting off to the side of the trail. The soldier with the instrument would walk down the trail, and as soon as he thought he saw a signal on this system, he would drop a three-by-five card on the trail. And then he'd go back and do it again. At the end of the day they looked at the location of all the three-by-five cards and made a plot. It was a nice Gaussian curve, right around the point of the ambush. That was the evidence they used to say, "This thing works." Well, of course, when I looked at the data carefully, it turns out that, yes, on the first run they walked the full distance [down the trail]. But as the day got warm, they'd walk shorter and shorter distances, [laughter] so this experiment was simply a measure of how far they cared to walk.

COHEN: So what happened? Did they just put the kibosh on the whole thing?

BALDESCHWIELER: Well, you know, sometimes—and this is very important in science—sometimes things work for reasons you could never anticipate. What happened was that the business with the ammonia was completely fallacious. But under conditions where the background of particulates in the air is very low, as it is in places in Southeast Asia, the device was very effective. The VC troops were much more sophisticated than many people thought. They had large units, particularly the North Vietnamese troops that came down, the NVA. For example, they cooked with little…

COHEN: Gas stoves of some kind?

BALDESCHWIELER: No, little hibachis. And they would have some electrical generating equipment, and the brushes from the generators put out particulates. In fact any substantial human activity increases the particulate level. Indeed, for reasons that were not considered in the beginning, you could find concentrations of human activity this way. But in any complicated development project of this kind, it's hard to do proper controls. A web of misinformation is built up. I find this an enormous amount of fun—you know, sorting out such problems. Human bias plays an enormous role in experimental observation. Every time I do an experiment, I bear in mind that I already have a view as to how it should come out. Sometimes the outcome of an experiment is obvious but different from your expectations, and you can't see it. I have lots of examples from my research. Anyhow, this was fun. We had lots of interesting experiences during that one-month tour in Southeast Asia, in addition to the people-finder. It was just before the Tet offensive. The conditions were appalling.

COHEN: For the local people or for the army troops?

BALDESCHWIELER: For the local people—the army troops were fine. I mean, they were well equipped. But in comparison with the local citizens—in the Mekong Delta, for example, you'd see a family living essentially on a mound of refuse, surrounded by muddy water. You could tell that they weren't very happy with the presence of the US forces, which I was one of. The Tet offensive followed very shortly after we left, fortunately.

COHEN: That was the beginning of the end.

BALDESCHWIELER: I can remember thinking, "What a complete mismatch this is. What in the world are we doing here?" I was given a briefing by a young lieutenant colonel in a nicely starched uniform who talked about the hamlet pacification program. You'll remember that [Secretary of Defense Robert S.] McNamara at that time had stressed the quantitative evaluation of everything. So there was a hamlet pacification index. This was calculated by having a team go in and interview the village father and count the number of foxholes around and the other defensive measures. All kinds of things. You'd check this off, and it went into a computer. The

colonel showed me a plot of the hamlet pacification index as a function of time. It was a straight line. It started in the mid-sixties and went on up through the time that I was there.

COHEN: So they were not pacifying?

BALDESCHWIELER: Well, I looked at the curve and I asked, "When were US forces introduced into Southeast Asia in large numbers?" I then said that what the curve showed was that the introduction of US forces into Vietnam made no difference at all. And the room was utterly silent. Do you remember when the presidential candidate from Michigan—George Romney—went to Vietnam? He came back and said he had been brainwashed, and that ultimately led to his withdrawing from the Republican presidential primaries and his loss of credibility. Well, I'm sure I heard the same briefings, the same stuff. It really didn't make much sense. I can tell you many, many such stories. For example, we landed in the middle of the Mekong River on a boat that was the headquarters for the riverine forces, which were small armored boats that went up into the estuaries of the Mekong. I said, "Well, here you are in the middle of the river, but it's not that far from the banks, is it?"—I was a real pain in those days—"Why can't a Vietcong simply come up with a recoilless rifle and put some rounds into this boat?" And the commander said, "Oh, all the surrounding areas have been pacified." As we took off from the boat, a stream of tracer bullets followed us. [Laughter] But fortunately their aim wasn't so good.

Anyhow, there were all kinds of other interesting technologies at work there. You may remember the McNamara Line. These were sensors that were used at Khe Sanh but also along the Ho Chi Minh Trail. I was very interested and deeply involved in all of those technologies—and, once again, I was one of the few bona-fide academics who had both some experience and some understanding of basic small-unit tactics. So I was asked to join a panel of the President's Scientific Advisory Committee. Johnson's science advisor was Don [Donald F.] Hornig, who was a chemist. I was asked to serve on a sub-panel—the ground warfare panel of PSAC. The President's Science Advisory Committee had a series of sub-panels—ground warfare, strategic matters, naval warfare, and a variety of other things. So I got involved both through the Army Scientific Advisory Panel and through this operation for the White House.

COHEN: It sounds like this took a good bit of your time.

BALDESCHWIELER: Yes. I was back to Washington, I would say, every week, and of course at the expense of academic research, a trade-off I was prepared to make. I kept a very low profile with colleagues at Stanford. I think it was only the chairman, Bill [William S.] Johnson, who knew that I had been in Southeast Asia at all. Some of our colleagues at Stanford were utterly irrational on this subject, and non-critical. I won't name any names here, but some distinguished people you know very well simply were prepared to be dishonest in order to further their view on the war and its…

COHEN: Dishonest in what respect?

BALDESCHWIELER: Well, dishonest with respect to the facts. They distorted what was really happening. I had the chance when I was there to visit something called the Iron Triangle, which was an area of Vietnam that was quite close to Saigon. This was the place where the Viet Minh in the French days had operated from. It was extensively tunneled—there were underground hospitals, storage facilities, training areas. It was in a jungle area, but it was an enormous network from which forces could strike and then retreat. The army ultimately had a straightforward solution for this, which was to defoliate it and then to plow it. I was in the area where they were operating plows called Rome plows—*huge* construction vehicles. They'd work five of those in parallel and simply plow the area, taking down trees and everything—flattening it. That finally solved that problem. But that also was…

COHEN: They killed the landscape.

BALDESCHWIELER: Yes. Well, you had to do that, if your objective was to gain control, because there was no other way to do it. But that was my first exposure to defoliation problems.

COHEN: Were they using Agent Orange then?

BALDESCHWIELER: Yes. When I went back to Washington, working on PSAC, our immediate concern was with how you deal with the kind of military problems the Vietnam War presented. For example, methods for dealing with artillery attacks across the demilitarized zone, and a lot of interesting technology, which I hope somebody remembers, because for sure it's going to be

needed again. But when I was on the ground warfare panel, I put in a good deal of effort, and I was finally put on PSAC itself—I became a member of the regular organization. And it was an interesting assignment, because, as predicted, most of the people involved had backgrounds in either nuclear physics or were from the MIT Radiation Lab—those were the radar folks. I was the token chemist in this. Nixon replaced Johnson, and Lee DuBridge [Caltech's president 1946-1968] replaced Hornig as head of PSAC and became the science advisor. I was on PSAC at that time. There was an incident involving the defoliation matter that was a profound embarrassment to the administration. There were claims of birth defects arising from Agent Orange and all of the other usual catastrophic scenarios were creating headlines, propagated by the forces that had interest in embarrassing the administration in any way possible. What came to light in the early days of DuBridge's watch was that there had been some tests done—I think they may have been done by Litton.

COHEN: The company?

BALDESCHWIELER: Yes. They had a diagnostic lab, and they were given a contract to take some samples of chemicals used as defoliants and test them on laboratory animals. In high concentrations in some tests the chemicals turned out to be mutagenic. For some reason, this report was never released, until it was discovered much later by a reporter—and then, of course, the administration was accused of having suppressed this vital information.

COHEN: That's what it would look like.

BALDESCHWIELER: This is of course what it looked like. What the facts were is much harder to tell. So I was the point person to try to figure out who had done what to whom in this particular case. From the standpoint of the White House, I think there was no awareness at all of the existence of the report.

COHEN: So had they just not sent the report in?

BALDESCHWIELER: It's the usual issue: How much of what is done reaches the level of science advisor? He had never seen it, that I know for sure. Whether somebody else in the White House

organization had seen it and suppressed it—who knows? But for whatever reason, it was never released. But it was one of these issues that was highly visible. So I went to work to try to figure out what had really happened. This is a very interesting scientific story. There were at least seven different vendors who produced the Agent Orange material. It was basically an agricultural product. And one doesn't think of such products as pure chemicals; if the active ingredient is ninety percent of what it's claimed to be, that's good. And then, the basic defoliant was formulated with diesel oil, solvents, and all kinds of other things. So whatever is being sprayed around is a very complicated mixture. First of all, I tried to understand what exactly had been tested. Were pure materials tested? Whose materials were they? And so on and so forth. As I dug into it, I found that the most responsible vendors were Dow and Monsanto. They had discovered that, in the chemical synthesis of 2,4,5-T, which was the active ingredient in Agent Orange, depending on the conditions, trace amounts of an impurity called dioxin were produced—tetrachlorodibenzo-p-dioxin—now very famous. Anywhere from two to sixty parts per million were produced.

COHEN: So the people who looked at it didn't understand this?

BALDESCHWIELER: Probably the people who did the testing did not understand this. It was very hard to find out what it was they had tested and where they had obtained the test samples. The story really is the story of the impurity and who had allowed it to get into their final product. Most of the vendors were very unsophisticated and did not have the technology to detect the presence of the dioxin.

COHEN: Well, it had been used as a herbicide commercially?

BALDESCHWIELER: Yes, it was a commercial herbicide, but it had never been used on the scale that it was used in Southeast Asia. Well, when people were being shot, we knew…

COHEN: You had to do something.

BALDESCHWIELER: Yes. The troops were being ambushed from the sides of trails, and so the foliage was cleared for 200 or 300 yards on each side of the trail.

COHEN: Wow! Bad story, bad story!

BALDESCHWIELER: Yes. So which risk do you care to take? In retrospect you can be analytical, but at the time we were taking casualties. Years later I would be deposed in the litigation on Agent Orange, because I was one of the people who had been involved from the standpoint of the government. The producers were being sued by a variety of people who claimed they had developed, from their exposure, cancer and other maladies.

Also, I wanted to mention that land mines were a particular problem in Southeast Asia—all kinds.

COHEN: And they remain so.

BALDESCHWIELER: Yes. In particular, antipersonnel mines. Many US casualties were the result of maiming and wounding by such land mines. So I had some experience and exposure to land-mine warfare, and in later years I found myself, again, involved. This and chemical warfare and biological warfare are, from the military standpoint, very messy issues and typically not of great interest to the ranking military officers, because working on those problems is not a way to get promotion and visibility in the military. Such messy problems are not the equivalent of a command of a major mechanized unit or a squadron of aircraft. I always felt that it was useful to have some knowledge and exposure in these areas, since we would certainly confront such problems in the future.

COHEN: So, when you were on PSAC you were going back and forth from Stanford?

BALDESCHWIELER: Yes.

COHEN: You could still head your research teams?

BALDESCHWIELER: I had research going, but this was a time of a lot of activity. I was on PSAC because I was very interested in…

COHEN: And also you felt you should do it.

BALDESCHWIELER: Yes. I had, as I say, a certain Boy Scout [sense of duty].

COHEN: How often were you in Washington?

BALDESCHWIELER: Oh, at least once a week. It was very, very rigorous.

COHEN: It was hard on the family, too, I'd guess.

BALDESCHWIELER: Yes, it was. A price was paid.

COHEN: How long did that go on?

BALDESCHWIELER: Well, it went on until the end of the sixties. Let's see. Nixon was elected in '68. So halfway through the Nixon administration, I was invited to spend full time in Washington. That's when I joined the Office of Science and Technology. DuBridge had become a victim of [presidential advisors Bob] Haldeman and [John] Erlichman, who felt that the scientific community was much too loose and not loyal. DuBridge left [in 1970], just before the mid-term of the Nixon administration. And Ed [Edward E.] David from Bell Labs was appointed the science advisor. [The science advisor] had three roles: as science advisor, as head of the statutory Office of Science and Technology, and as chairman of PSAC. I was invited to be the deputy director of the Office of Science and Technology, and I remained vice-chairman of PSAC and went to spend full time in Washington.

COHEN: You took a leave from Stanford?

BALDESCHWIELER: I took a leave from Stanford. I left my poor students at that point. [Laughter] And I packed my wife and two kids and dogs and cats into the car [laughter] and set off to Washington.

COHEN: You had never lived in Washington before.

BALDESCHWIELER: No, but I had been going there so frequently that I knew my way around at least certain parts.

COHEN: Maybe this is a good place to stop, and we can start with Washington next time.

BALDESCHWIELER: OK. [Tape is turned off]

JOHN D. BALDESCHWIELER

SESSION 3

January 24, 2001

Begin Tape 3, Side 1

COHEN: You had some thoughts about what you would like to add.

BALDESCHWIELER: Yes. I remember that I mentioned last time, or perhaps the previous time, that Professor Paul Flory had been very helpful to me when I was at Cornell. He had given me some insight into molecular structure, which really determined my subsequent career. It turns out that when I left Harvard to go to Stanford, by that time Paul Flory had gone through two steps of his own. He went from Cornell to the Mellon Institute, where he was director, and then he left the Mellon Institute and went to Stanford.

COHEN: So there was a confluence.

BALDESCHWIELER: Indeed, one of the very attractive features for me in moving to Stanford was that Paul Flory was there.

COHEN: Did you have some contact with him in between those years?

BALDESCHWIELER: No. But we were both delighted at the opportunity to work on the same faculty. So that's an important point. And he will turn up again, in a significant way.

Then I also wanted to mention that when I was at Stanford—I guess on the scientific side I had mentioned that I had one very good idea in my last years at Harvard, and that was the ion cyclotron resonance spectroscopy, which we then implemented at Stanford. And during my years at Stanford, one of the attractive features was that Harden McConnell was there. He had come from Caltech and was on the Stanford faculty, as was Lubert Stryer. And both of them were interested in scientific problems in which they labeled biological macromolecules. McConnell did it with stable free radicals, which he studied by electron spin resonance. And Lu Stryer was interested in labeling with molecules that fluoresce. In both cases, you could study

molecular rotation, or tumbling, by looking at the depolarization of fluorescence, or at the line shape of the ESR signals. At a seminar that Stryer gave, I can remember that my eyes glazed over as the seminar went on. But during his talk I wondered—and it was just a passing thought—but I wondered whether one could use the emission of gamma rays from radioactive nuclei as a method for studying molecular rotation. It was just a passing figment of an idea. But fortunately, after the seminar, in the next few days, I followed up on the idea to see if this was crazy or not. The notion was that if you attached a radioisotope to a large biological molecule, would the motion of the large molecule affect in any way the emission of gamma rays from the radioisotope? That really was a harebrained idea. But in the process of seeing if there was anything to it, I came across a very interesting series of studies that involved the perturbed angular correlation of gamma-ray emission. The shorthand for this is PAC, for perturbed angular correlation. What some earlier work had shown is that if a cascade of gamma rays is emitted from a radioisotope, there is a correlation in the angular distribution of the emission of the radiation. Perhaps it's too complicated to tell the full story here, but the idea was that the molecular rotation could influence this angular correlation. So in my last few years at Stanford, I started a project in which we looked at a radioisotope of indium that was bound to a macromolecule. We showed that you could see variations in the angular correlation of gamma rays with changes in the molecular rotation or the molecular flexibility. That is, for a helix coil transition in a macromolecule it was possible to see the changes in the angular correlation of the gamma rays. This involved using a technique whereby the emission of the gamma rays was measured with two scintillation counters and the correlation between the two gamma rays was measured as a function of the angle between the direction of [their emission].

It was the presence of McConnell and Stryer that got me interested in the physical measurements on the biological materials. It had occurred to me that gamma rays are highly penetrating, so that you could label the biological molecule and inject it into an animal, for example. Since the gamma rays come out, you could measure not only where the molecule goes but how rapidly it tumbles; it would be telling us something about its dynamics *in vivo* while the animal went about his business. So that was a piece of science that I started in my later years at Stanford, which was interrupted by the move to Washington.

COHEN: Did you actually work with animals at this time?

BALDESCHWIELER: No. First, we did *in vitro* experiments. But I picked those experiments up when I returned to Caltech. And I think it's important to mention that it started [at Stanford], and it started because of the presence of two colleagues who were doing something related. Of course, I stress continually the benefit of being at good places, with good colleagues doing interesting things and good students to work with.

Then I spoke a bit about my experiences in Southeast Asia, and I wanted to mention one other incident. Well, there were so many, but one was particularly interesting. I had noted that there were a variety of different, very clever ways of dealing with the problems of a war in the jungle, where it's very difficult to see and find the enemy. And the issue of sensors was a paramount interest of mine. There were a variety of interesting sensors—seismic sensors and motion sensors of all kinds. But one of the most interesting was a little plastic cosmetic container, with a couple of Mexican bedbugs inside.

COHEN: How did you get Mexican bedbugs in Southeast Asia?

BALDESCHWIELER: They were procured here. They were put inside a little plastic cosmetic holder with holes in it, so air could get in. It was a really simple thing. It was mounted on top of a microphone. If you had a bivouac in a jungle site, or you made an encampment, these things were put out around the perimeter of the encampment. These were very hungry—the fiercest-looking bedbugs that you could imagine. I mean, they're huge.

COHEN: When you say huge, you're saying almost an inch?

BALDESCHWIELER: About the size of a five-cent piece. You would not want to encounter one of these. [Laughter] Anyhow, they were very hungry. If any human being came within the range of these insects' ability to sense a human being by odor, they would get very excited and flap their wings against the box, and you could pick this up with the microphone.

COHEN: Who thought of that?

BALDESCHWIELER: I don't know who thought of that. There was something called the Limited War Lab, located in Aberdeen. They had all kinds of ingenious things of this sort. You know,

when survival is at stake, people think of these things. It's part of the give and take of tactical warfare. It was really clever. It gave you early warning. If you had several of these along the trail, you could determine in what direction a force was moving and how fast. So it was very useful. The reason I tell you about this is that, at the time of the Vietnam War, you may remember that there were accusations that the US had been involved in wholesale use of chemical and biological warfare in Southeast Asia. In the United Nations, the Russians accused the United States of biological warfare, which we denied. We asked the Russians to show the evidence, and they cited this bedbug detector as biological warfare. The Pentagon was absolutely incensed and demanded that these things be taken out of the theater.

COHEN: You mean they didn't know this was going on?

BALDESCHWIELER: At the higher levels in the Pentagon, they had no idea it was going on, and they felt completely embarrassed. And of course we lost our bedbug sensors. [Laughter] But it was amusing that such a harmless thing became a pawn.

COHEN: So that was the biological warfare—Mexican bedbugs.

BALDESCHWIELER: That was the biological warfare.

COHEN: From how far away could they sense a human being?

BALDESCHWIELER: It obviously depended on the direction and motion of the wind and so forth.

COHEN: And it didn't have to be a human being. It could have been a dog.

BALDESCHWIELER: Yes, anything that was a meal. But they seemed to be particularly sensitive to people.

COHEN: OK. Good story! [Laughter]

BALDESCHWIELER: So it was in 1970 that we packed up the family and the kids and set off for Washington. I was not exactly a neophyte at the White House, since I had worked on the President's Science Advisory Committee, PSAC, for quite a few years. It was the end of the Johnson years that I began at PSAC, and then I was a member in the beginning of the Nixon years. And remember that Nixon promised to get us out of Vietnam, and Kissinger came in as the national security advisor. He headed the National Security Council, the NSC, in Washington. In 1970, I came in as the deputy director of the Office of Science and Technology, which is a statutory office. At that time it had about, I would say, forty people.

COHEN: Did you have to be confirmed by Congress?

BALDESCHWIELER: Oh, yes. I got marched before a Senate committee. This was all new to me, but I was well prepared by the staff. They indicated the kinds of questions the senators would ask. It was a distinguished committee, by the way. I'm trying to recall who was on it. [Jacob] Javits [R-NY] was among the inquisitioners. [Laughter] But generally it seemed to go smoothly, and I was confirmed.

COHEN: The opponents of the war wouldn't have come out on this committee, would they?

BALDESCHWIELER: No. Although they clearly could have. Vietnam was an issue that pervaded every aspect of Washington and politics—not only in Washington but nationally. I was set up in the old Executive Office Building with a big office, overlooking the Rose Garden. It was the old War Department from Civil War times on—the massive building just west of the White House.

COHEN: Where in Washington did you live?

BALDESCHWIELER: We lived in Cleveland Park. We rented a house. This was an area that became popular during the Kennedy administration. Many of his staffers had large families occupying those large old houses.

COHEN: That's a nice section of Washington.

BALDESCHWIELER: It's a nice area. And I could take various forms of public transportation. I rode my bicycle sometimes to work, and I could walk. It was very pleasant. I had nice digs—a big desk with an American flag. [Laughter] But step by step it began to become clear that I didn't have a very clear idea of what I should do and how to go about it.

COHEN: And no laboratory.

BALDESCHWIELER: No laboratory bench. [Laughter] I inherited a staff of people who were, by and large, quite competent. Many were detailees that came from the different agencies, including agencies such as NIH [National Institutes of Health], the Department of Defense, and NSF. The office itself didn't have a very large budget, so we had to do much of our work by influence, as do all the people who operate out of the Executive Office. And I began to discover that…. Well, I had gone to Washington in a very idealistic frame of mind—a Boy Scout frame of mind. I began to learn, step by step, that life there was very different. It resembled more of a king's court, in the sense of large numbers of people competing for time with the president, exposure to the president, and that was the currency of [the place]. If you were perceived to have access, then those things you wanted to do got more attention. And I came in late, essentially two years after the Nixon administration had begun. Lee DuBridge was the science advisor for the first two years of the Nixon administration. He resigned in 1970 and Ed David, from Bell Labs, became the science advisor. I came in as David's deputy. And although I had worked with DuBridge on PSAC, David was our new science advisor when I arrived, and I was his deputy.

It was an interesting process, trying to sort out how this strange organization really worked. There were all kinds of interesting things. One of the first things that happened to me was that I got a written directive that said, "The President has requested that you set up a panel to study" such-and-such an issue, and it was signed by Henry Kissinger. I was pretty sure the President had never seen it, and I suspected that Kissinger hadn't either. So I went to one of the old-timers and asked, "What do I do with this?" And he said, "Well, the first thing you have to do is figure out where it came from. Certainly neither the President nor Kissinger has seen this."

COHEN: You could tell by the request that it wasn't something they would have asked for?

BALDESCHWIELER: Yes, and it turns out it wasn't. This had come from some fifth-level person in one of the executive offices. I learned that this was part of a mechanism of building up support for various people's ideas and initiatives.

COHEN: Could they really sign someone else's name?

BALDESCHWIELER: Well, it was the usual Xerox copy of something. And there was a signature machine, of course. We had some funny stories about the signature machine. [Laughter] So it gradually began to dawn on me that this is a very complex and…

COHEN: *Alice in Wonderland.*

BALDESCHWIELER: Yes. Intrigue of all kinds. And the currency here was, in part, public exposure, and embarrassment, and all of the tools that you read about. [Laughter] You may remember [Daniel Patrick] Moynihan and the "benign neglect"—that happened during my tenure. Moynihan was on the Domestic Council and he was brought in to help Nixon develop effective policies for dealing with urban issues. He was an incredibly articulate, thoughtful person, and he spoke his mind, as a brilliant and respected academic would be expected to do. And you remember that the "benign neglect" statement was somehow leaked to the press?

COHEN: Sure.

BALDESCHWIELER: That was clearly something that came from the forces of Erlichman and Haldeman. Do you remember them?

COHEN: Oh, yes. They are two very familiar names, of course.

BALDESCHWIELER: This was the stuff of everyday action.

COHEN: And you wondered what you were doing there?

BALDESCHWIELER: I certainly was about as far out of place as you can imagine. [Laughter] I found some things that I could usefully do. It's interesting—everybody's busy protecting themselves and trying to move forward issues of various kinds. What I found is that when any scientific issue that affected policy was brought up, typically there was a very small window—maybe a week or so—in which the issue was publicly visible and in which the President was in a position to make a decision of one sort or another. And then, whatever it is, it's gone. Then it's replaced by something else. But each of these issues is typically very complex. During that period, chemical and biological weapons disposal came up, and there were all kinds of things—mining the harbors in Haiphong. Issue after issue, a lot of defense issues but also a lot of domestic energy issues.

COHEN: Did all those pass through your office?

BALDESCHWIELER: Well, of course there was a certain kind of competition for people to get their particular views on these matters implemented in policy. From the technical sense or a scientific standpoint, these were all very complicated. I certainly didn't have the wisdom or the background to pontificate on any of these. So what I tried to do was anticipate what the issues might be—try to guess what would be a publicly visible issue in three or six months, and then put together the usual set of panels and study groups to get some thoughtful answers to these things. Some of those, of course, we never used. But for some we were fortunate enough to have the work done.

COHEN: So in some sense you needed a crystal ball.

BALDESCHWIELER: Yes, and I think that's what the job really was. [Laughter] And when you had that work done, you could then insert it into the argument and get a decision. And very frequently things would go in a sensible way. This is what happened with the chemical warfare and biological warfare matters of that time. For example, the issue of toxins hit the press. Nixon had made a decision that the US would ban biological warfare, based in part on our work. It was clear that if someone attacked you with a biological pathogen, the last thing in the world you wanted to do was respond with a biological pathogen. In fact, it might take you weeks or months

to figure out what the agent was, or even that you had been attacked, and who did it. And by that time, to retaliate in the same way made no sense. And for us to make first use of BW also made no sense. So, why have it at all? In fact, it was an extremely successful decision, because you may remember that the army had research facilities at Fort Dietrich, Maryland, and production facilities at Pine Bluff, Arkansas, for manufacturing substantial quantities of biological agents—not nice stuff to have around, for a variety of reasons. [Laughter] Those stocks were destroyed, the program was terminated, and the facilities were eventually turned over to the National Cancer Institute. And so we, I think, added a critical and coherent argument in favor of banning biological weapons.

There were lots of other cases, of course, where we missed. [Laughter] Each day was a new set of firefights. There was a meeting at 7:30 every morning in the Roosevelt Room of the White House, and the various characters in the cast you've read about were sitting around the table. I periodically represented science at that table.

COHEN: So this was the whole circus—I mean, not just the scientists. You were the science person.

BALDESCHWIELER: Yes, or David. He did most of it, but I did it in part. After being at that meeting for a number of times, I—you know, it was a bit like when you come into the movie and sit down. At first you can't really understand what the characters are saying. It's kind of a blur, and then it becomes clear. I began to sort out who the various people were around the table. The President, of course, was not there, but it became clear to me that there was a group of people who had come into the room together, and so they obviously had met earlier. So it seemed that many things had already been decided by the time these meetings happened. [Laughter] Those people were the Domestic Council.

COHEN: That was Erlichman?

BALDESCHWIELER: Erlichman headed the Domestic Council, and Haldeman was the chief of staff of the White House. This was a group of people that had been with the President in the campaign.

COHEN: So it was *really* political.

BALDESCHWIELER: Absolutely. They were very close-knit, and they had a very different mind-set than you'd normally expect. Their job was to protect the President on political matters. They took care of the relationships with the Congress. The National Security Council, on the other hand—Kissinger's staff—was absolutely professional. First-class—and he was a professional. They were fun to deal with.

COHEN: It must have been like going to the theater every morning at 7:30. [Laughter]

BALDESCHWIELER: Yes. Of course, I had been working in a laboratory, so the various political issues and which committees and forces in Congress were important and so forth I had no clue about.

COHEN: Did you have any feeling as to whether these people understood science?

BALDESCHWIELER: Well, nobody cared to understand science. That was not important at all. The question was whether there'd be any highly positive or negative impacts from a policy or an action that had a technical component. What I learned is that virtually everything had a technical component. So I tried to insert good sense when I got a chance. But these were very fast-moving targets. [Laughter] There's one fun example that I remember, and that was the SST, the supersonic transport. The way this came to be an issue was that there was a large constituency—the aerospace industry—that wanted to build an SST and they very much wanted to have a government subsidy to do it. Now, it seems that Nixon was having trouble, as you might expect, getting his defense appropriation passed, because there was a lot of opposition to the Vietnam War. A key congressman was Scoop [Henry M.] Jackson from the state of Washington. I think he chaired the Senate defense appropriations subcommittee—at least he was clearly a major force.

COHEN: A Democrat?

BALDESCHWIELER: Yes, and Nixon needed his support to get his appropriation. The SST was being built at Boeing in Seattle. So there was an agreement that Jackson would support the budget and the administration would subsidize the SST.

COHEN: So it was all deals, deals, deals.

BALDESCHWIELER: Yes. Well, that deal went on. You may remember that there was a huge protest over the SST, from the standpoint of its environmental impact. These planes flew at high altitude, and there was a constituency that was concerned about oxides and nitrogen, which would deplete the ozone.

COHEN: Did they talk about the ozone layer even in those days? It was already on the table?

BALDESCHWIELER: Oh, yes—it may well be one of the earliest debates. In any case, we had done a study in OST on the SST, and what we had learned was that if you looked overall at commercial air travel, the major issue was not the flight time between cities. That is, the total time for travel between any two places was more or less the same, and dominated not by the flight time but by other matters, such as the time that it takes you to get to the airport, handling and management of baggage, check-in time at the airport, and, in a major way, the air traffic control and delays in scheduling takeoffs and landing. Thus the faster flight time of the SST was almost irrelevant. The recommendation of our group was that if you're going to take a federal role, it should be focused on what the real problems were—for example, air traffic control. This report existed, but it was in the White House and was not public. But at the height of the uproar over the SST, one of our PSAC members leaked the fact that this report existed. And this created a storm.

COHEN: Now, was this leaked from your office?

BALDESCHWIELER: Well, it was leaked by one of the members of PSAC. He was not literally employed in our office. I had no control over this, except that there was an understanding that people who worked on such matters would have access to inside information, in exchange for

not taking a public position on one side or another, which would destroy our credibility. The effect of leaking the report was to put the whole science office under enormous pressure.

COHEN: I had this feeling in the back of my mind that Nixon didn't like any of the science.

BALDESCHWIELER: Well, Nixon, one on one, was really tremendous to deal with. He was very smart and understood the issue immediately.

I forgot to say, on the biological warfare thing, that we had recommended that biological warfare simply be rejected and no longer be a part of US policy. Chemical warfare was different: There, retaliation in kind was our recommendation. But there was a class of problems in between, and these were the toxins. And this was a wonderful academic debate: Is a toxin biological or is it chemical? If it's biological, it would have been banned under the administration policy. If it was a chemical, then it would be in a category of things that would be stockpiled for retaliation in kind—not for first use, but retaliation in kind. We had a wonderful time with this.

COHEN: And Nixon understood immediately what you were talking about?

BALDESCHWIELER: Yes. The chemists would say, "Well, even if it's a protein, that's simply a chemical. It's a sequence of amino acids. It's not alive. It's not a pathogen." And the other side would say, "Well, this is…."

COHEN: It reminds me of the game "Animal, Vegetable, or Mineral?"

BALDESCHWIELER: Yes, that's right. Toxins were typically produced by living cells. We took this viewpoint, and—*boom!*—Nixon decided that toxins are biological and therefore they are banned, which was absolutely the right answer. And Nixon had no…

COHEN: No problem understanding this.

BALDESCHWIELER: Yes. He caught on quickly. But with the SST, there were several fallouts. One, of course, was that the OST was discredited—I think not fairly, but it certainly was.

COHEN: Because of the leak?

BALDESCHWIELER: Yes. Outside forces learned of the presence of this report, so they sued to get the report. They knew that the report recommended that we not build the SST. The administration bitterly fought for building it, although they were on the wrong side of the issue. But the deal had been made with Jackson, so a political agreement was driving this [decision]. People from the outside were upset.

COHEN: When you say "people from the outside," are you talking about ordinary citizens, groups?

BALDESCHWIELER: Various well-organized environmental forces. But you have all the pieces of a soap opera here, because there were industry advocates for building the SST—and the arguments were cogent. You may not eventually want to build a commercial SST, but you always learn by government support of high-risk aerospace adventures.

COHEN: You want more money for research.

BALDESCHWIELER: Yes—more successful American commercial presence in aircraft. And so there were some cogent arguments for it. The environmental forces simply didn't want it for a variety of reasons. The environmental argument was, I think, a convenient base for them. The uproar over access to the OST report was the beginning of the Freedom of Information Act, which Congress passed after this and which said that outside parties should generally have access to government documents. I was named in the suit to get the material. Fortunately, I didn't have to defend myself on that. [Laughter] But it was interesting. A very distinguished kineticist had gone through an argument about how the exhaust from the SST would create ozone depletion, which would ultimately cause an increase in cancer incidence. In his argument he had used more than thirty coupled differential equations. This was very, very complicated. Well, this was one thing I could deal with. So I got some help from some people I trusted to look at this. The argument was utterly fallacious, so the outside forces were not completely

honest either. As so often in these cases, it's a mix of posturing and misinformation based on arguments that defend a position that people hold for very different reasons.

COHEN: It sounds like it was a really stressful post for you.

BALDESCHWIELER: Well, I don't remember stress so much as it's being very interesting and confusing. I didn't feel that there was any personal downside for me, as long as I didn't expose myself in ways that I…

COHEN: That you'd get emotionally involved in?

BALDESCHWIELER: Yes. I was careful not to do that. In the end, you may remember what happened. What happened on the SST was that finally Congress settled the matter by denying the budget. In retrospect, somehow the right answer came out of this whole turmoil. But for me it was a very important lesson in how these issues get resolved. In the end, the right answer was attained. The environmental issues may or may not have been important. But what was really critical were the economic issues—those matters that had been identified by the study in the first place. [Laughter] Do you remember when Russia and the English-French consortium both went ahead to build SSTs? The Russians built several T-141s. One crashed at the Paris Air Show, and that was the end of that. The remaining plane was used on a mail run from Moscow to Tbilisi. And the French-English consortium built about twenty aircraft.

COHEN: The SST was an economic failure. So how long were you there, doing this kind of thing?

BALDESCHWIELER: For two years. And it's curious. I think I would conclude that when an issue has high enough visibility and importance, somehow our political system gets pretty close to the right answer, but not by a pathway that any of us could see in advance. Then, of course, you worry, as do I, that there are many other issues below that threshold, where the outcome often doesn't make much sense for the public interest.

COHEN: Did you get into much social life in Washington?

BALDESCHWIELER: Yes, the social life was also interesting. We were invited to countless embassy receptions and diplomatic affairs. We were also able to attend major government—not several a month, but there would be a dozen such opportunities within a year. [Tape ends]

Begin Tape 3, Side 2

COHEN: So you met some interesting people.

BALDESCHWIELER: Yes. We went not to every one of these, but we went from time to time. That was ample, because you could overdo it. One had to be very careful what one said at such receptions…. Most of the people I knew drank nothing, because…

COHEN: They had to keep track of what they were saying.

BALDESCHWIELER: Yes. There would be a sea of people, including reporters, who were very interested in what was happening in the administration.

COHEN: Ah, so they would try and find out information from you.

BALDESCHWIELER: Yes. So these were occasions on which one had to be very careful.

One of the most interesting things that happened to me in the White House had to do with the international side of science. One of the things I did early—I was asked in one of these communications—was to outline some basic ideas for interacting with Russian and Chinese scientific programs. In both cases, there were interesting technical people and programs that we could interact with. There was US technology that obviously they would be interested in. We were invited to stand on the White House lawn and wave goodbye to Nixon as he flew off to China. [Laughter] We had worked closely with the National Security Council and prepared this material for the NSC staff, and it was part of what Kissinger took with him when he went to China. At the time we did the work, we didn't really know why we were doing it. It was just something we were asked to do and respond to. There was a program with Russia as well—détente. This was part of Kissinger's grand vision of modern power politics. There was greater animosity between China and Russia than with the US. China and Russia were not a great

Communist monolith. The Vietnam War was a huge distraction from what the real geopolitical forces were. Although Kissinger was an arrogant, difficult person to be with, I have enormous respect for him.

COHEN: Coming from academia, though, you'd be more used to people like that. [Laughter]

BALDESCHWIELER: Yes. He understood his business very well and was able to make it work. I had several Kissinger experiences. The National Security Council was his staff. The National Security Council is composed of four or five cabinet members—the secretary of defense, the attorney general, and one or two other people—but they rarely met. The work was done by the NSC staff, through a series of what were called National Security Decision Memoranda, which the staff would prepare for the President. We called it, "Choose Option B" [laughter]—typically there would be three options. Option A was "Do everything." Option C was "Do nothing." And Option B was somewhere in between, which of course, obviously, if you set this up right, the President would choose. And so the staff, in this way…

COHEN: Would make one option extreme and one not good enough, so that you had to settle on the middle way.

BALDESCHWIELER: Right. I stood in for the science advisor at one meeting of the NSC. What Kissinger did was to create a whole series of different subcommittees of the National Security Council. They were essentially the same set of people, minus one. So he could get the decision he wanted out of a subcommittee, because he knew what position each person would take. [Laughter]

COHEN: It sounds Machiavellian. [Laughter]

BALDESCHWIELER: Oh, it was, yes. I remember I was in the Situation Room, which is the bunker under the west wing of the White House. There was a table about this high, with all of these characters sitting around it. I remember [Attorney General] John Mitchell's face in particular. And sitting around the wall would be a variety of military officers…

COHEN: In a second circle around the wall, behind the table?

BALDESCHWIELER: Yes. Some of the information from these sessions had been leaked to the press, and that was a major asset for the opposition. Kissinger was very paranoid about such matters. He suspected that one of the young military officers was…

COHEN: One of the people sitting in the outer circle?

BALDESCHWIELER: Yes—that one of them had done that. It somehow was the beginning of that whole sequence of paranoia, where the administration began looking with suspicion on…

COHEN: Is that when they started making those famous tapes?

BALDESCHWIELER: Well, the famous tapes, of course, were going on all the time.

COHEN: But nobody knew about it. Did you know about it?

BALDESCHWIELER: No. I'm presumably on them. [Laughter] Oh, it was an extraordinary period of time. I focused on the budget. Both China and Russia were important for me, because after I left the administration, I became involved in both of those programs and they have been an important part of my career ever since. You know, I enjoyed languages and international matters, and this was a chance really to become involved.

COHEN: Now, how did your work day go? I mean, it started at 7:30.

BALDESCHWIELER: Yes.

COHEN: Up to evening receptions. It sounds like a really…

BALDESCHWIELER: Oh, it's a brutal pace and a very, very demanding job. But I don't recall it's being particularly stressful. I mean, it was just…

COHEN: You just worked all day.

BALDESCHWIELER: You just worked all the time, yes. And you had to turn things out so fast that very often it was not perfect. [Laughter] In fact, it was almost never perfect.

COHEN: Did you appoint some of your own people, even though you came to a ready-made staff?

BALDESCHWIELER: Yes. There was some turnover. These were, of course, temporary jobs for all of them. You do these things either early in your career or late in your career. In the middle, you can't do it, because you have kids to get through high school and college. For me, it was early in my career. We just had really a wonderful time.

COHEN: You had taken a leave from Stanford. Did you realize your stint in Washington would only be two years?

BALDESCHWIELER: No.

COHEN: How did that go?

BALDESCHWIELER: What happened is that I was interested in the budget. Those were quantitative things that I could do and I could do well and carefully. I enjoyed the planning aspect. So I became responsible for coordinating the entire science budget, which was big.

COHEN: That's a lot of responsibility.

BALDESCHWIELER: Well, but everybody has his hand in it. The agencies prepare their pieces. The Office of Management and Budget, OMB, goes through it. I sat through the director's reviews on all these matters and tried to rescue things I thought needed to be saved from the ax. [Laughter] In fact, the director's reviews were a very brutal drill, at which the OMB staff, with a few people from agencies such as ours, would be present. They would go through the entire budget of each department of government. At that point, all the propositions had come from

each of the departments, and they had tried to make their case. The director of the budget—at that time [1970-1972] Caspar Weinberger—had to make it all fit. He didn't like trains, I remember, so he zeroed the entire railroad research budget at one of these meetings. [Laughter] And then the Department of Transportation gets one shot; they can come back and try to fix one or two things. He liked airplanes. And he thought that transportation should be all short-haul airplane. There was a character who sat in the corner at those meetings and he literally wore a green eyeshade. He had four or five cigars in his pocket, which he would open one by one. He didn't smoke them, but he chewed them and spit the chewings into a Styrofoam cup.

COHEN: Who was this?

BALDESCHWIELER: I asked, "Who is this?" It turned out he was the guy who took the minutes—who wrote down what the director of the budget decided. He was responsible for the round-off error. I've often thought, what a powerful position in Washington, because when you're dealing with billions, the round-off error is quite a few million. [Laughter]

COHEN: Now, did you have to argue for your science budget then?

BALDESCHWIELER: Yes, occasionally. Not often directly in the budget review, because you couldn't appear to be partial to one program or agency. But we could be helpful in getting the process of appeal focused.

COHEN: How was Weinberger on science, by the way?

BALDESCHWIELER: Weinberger was generally quite supportive of science. He understood it—except for trains. [Laughter] There may have been other things that I don't remember. But I had a wonderful experience: I learned that the week between Christmas and New Year's was very valuable, because it turns out that I was in my office that week and I got a note that said, "We have an additional $100 million. Can you use it in the science budget?" [Laughter] Can you imagine?

COHEN: And nobody else was around.

BALDESCHWIELER: Nobody else was around, so we put that to good use very quickly. So there are certain opportunities. Within the Office of Management and Budget, there are staff people just below the level of political appointees who are critical in this process. They keep a very low profile, but when you find out who they are, you can very often manage to keep things out of the review. The director's review can be very dangerous.

COHEN: So you had to learn to play this game.

BALDESCHWIELER: Yes, absolutely. It's much better to take a little less than you'd hoped for and not have it be discussed at the review, because you could get an almost random response and you might have the whole thing zeroed.

COHEN: Now, when you would have these reviews, say, for science, would the other people understand what they were reviewing? Or did they only look at the bottom line and see how much money it cost?

BALDESCHWIELER: Well, I must say that the OMB staff in my day was excellent—very bright, sort of Harvard-MBA-type people who were very quick studies, and very smart—and arrogant, which is part of it. In this kind of a forum you really can't argue the technical issues in detail. One has to make kind of a quick one-liner and hope that that has an impact on the bottom line.

But I wanted to say that I was doing the budget, toward the end of 1972, and I was having trouble getting anybody to return my phone calls or pay attention to what I was doing. Of course, this was reelection time, and it was Watergate time.

COHEN: They had other things on their minds.

BALDESCHWIELER: The whole depth and extent of the Watergate matter was not yet clear. There was a long period, remember, in which nothing surfaced.

COHEN: That's right.

BALDESCHWIELER: A few people who were the literal perpetrators of the break-in were apprehended, but it was very low-key. And at that time…

COHEN: Nobody's answering your calls.

BALDESCHWIELER: Yes. So I thought, "I'm not very good at this, and I've done the best I can." Well, I had the good fortune to know some people at the National Institutes of Health. And the NIH had a six-month fellowship in the National Cancer Institute, and I had been harboring an interest in biological matters. So I effectively resigned, on December 31, 1972.

COHEN: So in a sense, you were a victim of Watergate.

BALDESCHWIELER: By good luck, and perhaps some useful instinct, I was gone on the 31st of December. And I was very quickly installed at the National Cancer Institute, injecting mice with carcinogens.

COHEN: That was quite a change!

BALDESCHWIELER: I worked in a laboratory.

COHEN: So did someone take over your old job? What happened? [Tape ends]

Begin Tape 4, Side 1

BALDESCHWIELER: It was like coming out of *Alice in Wonderland*.

COHEN: Did you make this decision overnight? I mean, how did that work?

BALDESCHWIELER: Relatively quickly, I must say.

COHEN: You didn't worry about them getting someone to take your place, or anything like that?

BALDESCHWIELER: Well, the office was coming under increasing pressure, as a fall out from the SST matter. You know, there were a variety of changes going on in other parts of the administration. Various cabinet members resigned, which is typical at the end of an administration. But a special counsel was being appointed to look into the Watergate matters. So it was, as I say, some combination of instinct and luck. I decided that it was time for me to go. At least I wasn't being very effective at those things I thought I was able to do.

COHEN: Didn't Edward David go at some point? I have a feeling that there was no science advisor for a period of time.

BALDESCHWIELER: That's right. We went together. But we did not coordinate this.

COHEN: He had the same sense that you did.

BALDESCHWIELER: Yes. He was far more visible and exposed than I was, being deputy. The anger coming from the Erlichman and Haldeman forces was very deep. They thought the technical people were the enemy. It was not a supportive kind of environment. As I say, I was shielded from much of this by David, who took it head-on every day. [Laughter] But we resigned, effectively, together. And all the detailees went back to their places of origin.

COHEN: So who did the science budget then?

BALDESCHWIELER: Well, it seems to me that it went to Guy [H. Guyford] Stever, who was at that point the director of the NSF. The Domestic Council decided there were a lot of parts of the administration that weren't needed. [Laughter] And so the OST went down. But I was well installed at the Cancer Institute with my mice.

COHEN: With your new field. So this was really a new field for you.

BALDESCHWIELER: Absolutely. I had never done animal experiments or anything like that. So I learned how to handle mice and I got bitten a few times. [Laughter] It was fun, in part because

nobody in the lab had any idea what it was I had been doing. I just kind of dropped out of the sky, as far as they were concerned.

COHEN: Well, you had been getting more and more interested in biology.

BALDESCHWIELER: Yes. It was a time of very tight budgets, because we were fighting a war. And you may remember that the Cambodian incursion happened. And then the bombing of Haiphong and Hanoi, and the mining of the harbor. These were all issues in which we were in the middle. In fact, I briefed Nixon on the mining matter. He seemed already to understand it completely. So I often wondered whether…

COHEN: He knew about it beforehand?

BALDESCHWIELER: He was either very smart—which I'm sure of—or, of course, there was somebody else who advised him so that he could appear informed when he spoke with us. He was amazing to watch. When groups come to see the President at the White House, it's every fifteen minutes, and it's like a circus, with each of the rooms on the ground floor of the White House occupied, you know, by the boiler makers, the 4-H club, the scientists, the bridge builders, whoever. Nixon could stand up, no matter what the group was, with no notes, nothing, and make an eloquent, compelling presentation and listen to their response. I saw him do this on a number of occasions. I often thought he might have been a marvelous actor, that he had all the talents it takes to be on the stage—like Reagan, of course.

COHEN: Oh, I don't think anybody doubted that he was very smart.

BALDESCHWIELER: Yes, and he could learn in a flash.

COHEN: Like Clinton.

BALDESCHWIELER: Yes—we have a system where that's what it takes to get elected.

But I was gone. I was at the NIH with my mice. Every morning we would rush downstairs to get the newspaper to see who among our friends was on the hot seat. [Laughter]

Those people who had not been returning my phone calls were the plumbers, who were involved in all the various matters associated with Watergate. I was pleased to be out. I felt I was very, very fortunate.

COHEN: What a terrible time our country went through then.

BALDESCHWIELER: Yes, it was terrible. I remember for some reason I was taking a cab in downtown Washington, at a time when cabs were scarce. There were people sharing cabs. My cab stopped at a light, and both doors opened, one on each side, and in jumped two people. One was from the *Wall Street Journal* and one was from the *New York Times*. They both had their initial transcripts of the Nixon tapes. They didn't know who I was. I was just in the middle, while these two guys were exchanging comments: "Did you see this?" "Did you see that?"

COHEN: Did you know the transcripts were out then?

BALDESCHWIELER: Of course I'd heard that the transcripts were coming out, but what was in them I could not believe—the profanity, the simply dumb, insensitive things that were said. The president has always been a quandary for me—how on one hand he could be a person so skillful and adept and, on the other hand, there was something clearly missing. Someone told me once that anyone who has the perseverance to get elected to that office often has a few dimensions that are incomplete.

COHEN: So you were at the NIH for six months, and you really enjoyed that.

BALDESCHWIELER: Yes.

COHEN: Sort of like coming back into academia?

BALDESCHWIELER: At the time, I participated in other people's experiments. I learned how to dissect rabbits.

COHEN: You learned your biology.

BALDESCHWIELER: Well, not serious biology, but at least in a framework where I learned how to run gels and analyze proteins. I had a chance to read some books. It was quiet.

COHEN: Considering what you were coming from, this was a rest cure. [Laughter]

BALDESCHWIELER: Yes. That was when I got a call from Jack [John D.] Roberts [then acting chairman of Caltech's Division of Chemistry and Chemical Engineering—ed.].

COHEN: I was going to ask, "How did you come to Caltech then?"

BALDESCHWIELER: Well, I was planning to go back to Stanford. I had an interest in science policy and running things. I was still well energized and activated, with a national or global viewpoint on policy matters.

COHEN: And Stanford has that approach.

BALDESCHWIELER: Yes. I was planning to go back to Stanford, not sure that I would literally do research, thinking that maybe this is the time when one becomes a dean. I had thoughts of academic administration, although I certainly enjoyed science as well. It was during that time that I got a call from Jack Roberts. Chemistry at Caltech had gone through some turmoil. I guess academic departments that are good do go through turmoil.

COHEN: Well, there were strong personalities and they had their own ideas.

BALDESCHWIELER: That's right. That's part of the process.

COHEN: That was the Pauling controversy and all that?

BALDESCHWIELER: Post-Pauling. But there were other characters. There was the distinguished photochemist—George Hammond. Hammond had been chairman [of the chemistry division] for a period of time [1968-1972]. Things had not gone well in that period. Jack Roberts had taken over on an interim basis and was very anxious to get off the divisional chairmanship hot seat.

He called. I didn't know much about Caltech. I had been there to give a seminar. But when he explained the divisional chairmanship to me, that sounded like just the right thing. It sounded to me as if I would have a chance to administer something. And this was, in a sense, the best of both worlds. And far better than being a dean in a larger academic institution, where any type of direct contact at all with the technical content of a…

COHEN: So did you come out and visit a little bit?

BALDESCHWIELER: Yes, I came out and visited. I must say, I was very much attracted by the nature of the job itself. And I had a view that a lot could be done. The chemistry division at Caltech had been very well funded historically, through gifts, and the faculty was not used to competing vigorously for outside grants. The sources of support were diminishing, and we needed a much more aggressive approach. I both enjoyed that and thought I could—

COHEN: Now, did you have any business interest during all this time? I mean, you had gotten involved in stock early on.

BALDESCHWIELER: Yes. I did a little start-up in my years at Harvard, which was called Research Systems, Inc. That had always been part of my thinking. I thought that one could work in collaboration with industry and bring in some financial support. I very much enjoyed the notion of building, conceiving of programs and designing programs, raising money—literally making things happen. I felt I could do that at Caltech, whereas I would be very hampered in doing that as a departmental chairman, for example, at Stanford.

COHEN: Is that because it would be a much bigger job, many more people that you'd be managing? Or was it just a different climate there?

BALDESCHWIELER: Well, it's a different organizational set-up. Here I would have direct access to the provost and the president, whereas at an institution the size of Stanford, that would not be the case. And here I had control of the budget. Harold Brown [Caltech's president 1969-1977] was here, and I had known and worked with Harold. He's an extraordinary person.

COHEN: Now, how did you know him?

BALDESCHWIELER: Well, he had been involved in defense matters for…

COHEN: Oh. So this was from your two years in Washington.

BALDESCHWIELER: Well, he was here then, of course. But he had been Secretary of the Air Force; and prior to that he was what was called director of defense research and engineering—DDR&E. And remember, he had come from Livermore [director of the Radiation Laboratory, 1952-1961]. He had worked on the nuclear weapons systems. DDR&E at the age of thirty-four. [Laughter] It was a spectacular, highly visible rise through government public service on the defense side.

COHEN: So you had gotten to know him in your committee work, before you moved to Washington.

BALDESCHWIELER: Yes. I wouldn't say well, but enough to be acquainted and have enormous respect. He was wonderful. To see him at a briefing, for example, with some general with a whole stack of viewgraphs and so on and so forth. Harold of course understood and remembered everything. He would say, "By the way, General So-and-so, in viewgraph number twenty-five you said thus-and-so." Of course, the impact was usually devastating. And Harold responded to you instantly—almost too fast. When you went to see him, you got a decision. You may not have wanted a decision, but you got one. [Laughter]

COHEN: So you not only had Jack Roberts asking you to come, you really had Harold Brown asking you to come also.

BALDESCHWIELER: Yes, he was part of it. I decided to come, but I finished out my stay at the NIH. I remember Norman Davidson came to visit. It was wonderful. He saw that I was reading a basic biochemistry book. [Laughter] And he was very encouraging about that.

COHEN: Now, did you know that you would be able to set up some research of your own, even though you would be division chair?

BALDESCHWIELER: Yes.

COHEN: That was understood.

BALDESCHWIELER: That's right. But I came with my eye primarily on running the division.

COHEN: It was 1973 that you came to Caltech?

BALDESCHWIELER: Yes. And maybe that's a good time to stop.

COHEN: A good time to stop. [Tape is turned off]

JOHN D. BALDESCHWIELER

SESSION 4

January 30, 2001

Begin Tape 5, Side 1

BALDESCHWIELER: A few things occurred to me. I have not said anything about consulting, and since some of these were interesting assignments, I thought I would go back a step or two. Most of the outside activities that I discussed, both in my earlier days at Harvard and at Stanford, were with the federal government in defense-related agencies. I described our development of the ion cyclotron resonance spectroscopy while I was at Stanford. Very early in that process, I got a call from a young researcher at Monsanto who said he was interested in this area of research and would like to come out and see me. This young man's name was Jay Henis. He appeared shortly after that call and looked at our work on cyclotron resonance spectroscopy. He was very excited about it and decided that that technology should be brought to Monsanto, and he convinced Monsanto of that. So they negotiated with Varian Associates to buy its first commercially sold instrument.

Then Jay asked me to come to Monsanto, initially to help with the instrument—training people and getting it up and running. The relationship with Monsanto broadened, and I became a consultant to their whole instrumentation and spectroscopic group, and later to other parts of the company. It was a very interesting exposure to yet another side of technology development. The relationship with Monsanto continued until the time I left Stanford to go full time to Washington. At that time, I had to divest myself of all such relationships—although during the years in the White House I attempted to hire Jay Henis as a member of the staff.

COHEN: You had a high opinion of him.

BALDESCHWIELER: Yes, he was a very, very bright and energetic and creative young person. So during my years in the White House, Monsanto continued with its work. I didn't reestablish that relationship until a number of years later.

COHEN: Now, was that the first consulting you did?

BALDESCHWIELER: I had done some other consulting, but it was mainly minor. This was the first major agreement I had with a large chemical or pharmaceutical concern. I mention it here because it turns out that, as often happens in life, such relationships reappear many times. This is the case with both Monsanto and Henis.

And then I also thought of one other situation from the time of the Executive Office that's worth mentioning, since it's relevant to today's experiences. When I arrived in the Executive Office, after a number of weeks of finding my way around I discovered that the person who was the administrator of bureaucratic matters within the office wielded extraordinary power. He was a large, very aggressive, blunt individual who had been a colonel in George Patton's Third Army in the Second World War, and he was used to crossing the countryside at high speed in tanks. As often happens when military retire, they find another job in government; they call it double-dipping. He turned up as the administrator in this office, and he was a particularly difficult person to deal with. He, for example, controlled the allocation of parking spaces and the hiring of secretaries. He was in charge of the clearances that allowed people to come into the building during the day.

COHEN: And you had nothing to say about this?

BALDESCHWIELER: That's exactly what I discovered. He also controlled the security clearances for the people that we appointed to panels. He wielded enormous power. For example, I would find that when I had a list of people I wanted to bring in as consultants, he decided himself—personally—that there were some people on the list he liked and some he didn't like. Those he didn't like—somehow their papers got lost, or they never made it through the process. So it was that kind of person who made those decisions. You could fight with him, but of course that was a full-time job, and if you battled with him on every appointment and every matter, then your people wouldn't be let into the building or you'd lose your parking space. [Laughter] It was a very difficult situation. I raise it here because I think of all those new presidential appointees going into those offices; the people several levels below the threshold of presidential appointments are already there and exercise this kind of control. So you may think that the new people are going to do something, but in fact it may well be that the steering wheel isn't connected to the rudder, if you know what I mean.

COHEN: How did you cope with this man?

BALDESCHWIELER: Well, I simply compromised. There were some things I simply couldn't get done if I wanted to do other things. He won a fair portion of those battles. He wielded his power with blunt effectiveness.

COHEN: He was a civil service employee, then.

BALDESCHWIELER: Yes, and he was very tough to deal with. I won't mention his name—to protect the guilty. [Laughter] It turned out that it was often very difficult to make any progress at all. For example, when we appointed people to our panels, they also had to be cleared by a different White House functionary, on the Domestic Council. He made what he thought was a very reasonable compromise. He said, "Just give me half Republicans." Well, if you're trying to compose a scientific panel of distinguished people, it turns out that that's a very tough criterion to meet.

COHEN: Most of the people don't want to be asked.

BALDESCHWIELER: Yes, they don't want to be asked. But some of our appointees would be rejected simply on the basis of this kind of filter. When the Office of Science and Technology came apart at the end of the Nixon administration, I thought, "Well, at least one benefit of this is that my nemesis won't have his job." He vanished from view for several months. But he turned up later, in the Ford administration, and his job was running the presidential signature machine. Most of the documents that come out of the White House with the President's signature the President has never seen or signed—the machine does that. So this person was in a position of even greater power [laughter], because from time to time, I assume, if he didn't…

COHEN: If he didn't like the document, he didn't sign it.

BALDESCHWIELER: That's right.

COHEN: You're sure he was capable of doing this?

BALDESCHWIELER: Oh, absolutely capable of doing this. However, it turns out that from time to time, he did also save us from various disasters because of his ability to control matters. In one case, the forces of Erlichman and Haldeman had decided that the Office of Science and Technology couldn't be trusted and certainly wasn't loyal to the President. So they set up a competing office with the same function, and they found a person to lead what was essentially the very same function but under the control of the Domestic Council. That was just an absurd situation. In this case, my colonel used his very considerable talents to crush that.

COHEN: He gave them no parking places? [Laughter]

BALDESCHWIELER: How he did it I did not inquire. But he certainly led the counterattack and quashed that. Of course, the whole thing was eliminated just a few months later. At the same time, you may remember, the same group was so paranoid about what was going on in all the departments of government that they sent people from the Domestic Council staff to act as assistant secretaries in each of the government departments.

COHEN: Spies.

BALDESCHWIELER: Exactly. These were people who were to report on what the secretaries…

COHEN: Were you aware of this at the time?

BALDESCHWIELER: Yes, by that time it was quite clear. I can remember telling my wife, "Someday I'll write a book, and nobody will believe it." [Laughter]

The other time he saved us was after the President's negotiations with China. The issue came up of the proper vehicle for carrying out the scientific exchange programs with the People's Republic of China. We thought it important that it not be an agency of the federal government. So we had, after discussions and consultations, created something called the CSCPRC—the Committee on Scholarly Communications with the People's Republic of China. And it was a tripartite effort of the National Academy of Sciences, the Social Sciences Research Council, and the American Council of Learned Societies. Our view of this exchange was that the Chinese were very anxious to get access to US technology. We were much less anxious to

get access to Chinese technology, although there were some interesting things. But what we wanted, and what our social scientists wanted, was access to cultural matters—to the libraries, to the scholarly resources. And these of course the Chinese were extremely reluctant to give people access to. So that was the basis for the deal. If they wanted to learn our technology, they had to give our scholars access to their scholarly resources. It worked out very well. But in the process of getting this done, the deal had to be approved by the National Security Council. So it went to Kissinger in the usual way—in the form of a decision memorandum. NSDM it was called—National Security Decision Memorandum. And there were three boxes: yes, no, discuss. To our shock, this thing came back with a check in the no box, after all this work and consultation. We couldn't believe it, and we tried to get back to Kissinger. He was, of course, unavailable, gone, disappeared—you know, on to the next crisis. So everybody was just flabbergasted. Our pugnacious colonel, fortunately, had served in Patton's army with Alexander Haig, who you may remember was a general who later appeared on the scene temporarily as secretary of state. Haig was the military attaché to Kissinger's office at that time. So our administrator was able to go immediately to Haig, who had access to Kissinger. Kissinger looked at it and said, "Oh, I checked the wrong box." [Laughter] And so this extraordinary program was launched. He just checked the wrong box. He was busy with a whole stack of things.

COHEN: I don't know how you came through this without ulcers, or something critical. [Laughter]

BALDESCHWIELER: Well, I think being very young and naïve probably helps in such cases, because everything is a learning process.

In these discussions, do we also talk about family and so forth—about wife and kids? I haven't said much about that.

COHEN: No. I know you haven't, but I haven't asked you. That's your own decision. You mentioned that you had a wife; you mentioned that you moved children around. And they certainly must have affected what you did.

BALDESCHWIELER: Yes. Well, I should mention that Marcia and I were privileged to have two beautiful children at this time, Eric, who was born in 1964 at the Boston Lying-In during our Harvard stay, and Karen came during our Stanford years.

COHEN: So they were just little ones when you were in Washington.

BALDESCHWIELER: They were little when we went to Washington. They were in PS 89 in the District, along with the kids of all the embassy families. The smartest kid in the class, of course, was the son of a Chinese cook. [Laughter] They had a wonderful time. We had a wonderful time in Washington. Cleveland Park was a nice neighborhood. One doesn't think of Washington that way. But it really was a family neighborhood.

COHEN: Oh, it still is.

BALDESCHWIELER: Yes. We were between the zoo and the cathedral. [Laughter] So that was the shape we were in when we bundled our goods together and came to Caltech. That was because I was very positively influenced by both Jack Roberts and Harold Brown.

COHEN: You must also have seen that you were an academic.

BALDESCHWIELER: Oh, yes.

COHEN: Because you certainly had the opportunity [to do other things]. I bet Monsanto would have taken you in a minute.

BALDESCHWIELER: Well, there were a series of such choice points, and I'll come to those as we go along. History doesn't allow you to explore those alternatives. But I think when the ultimate instinct came into play I enjoyed the basics of teaching and research.

COHEN: So you came to California, which was nothing new. You had already lived in the Bay Area. Where did you live when you moved to Pasadena?

BALDESCHWIELER: We found a beautiful house on Hill Avenue, just south of California.

COHEN: That was close.

BALDESCHWIELER: We had a house to sell in the Bay Area, which had escalated in value to an extent that we could not conceive. At the time we came, of course, Pasadena was still reasonable, and one might even say depressed.

COHEN: Now, did Stanford mind your going off this way?

BALDESCHWIELER: Yes. There was a counteroffensive. But I must say I was very much attracted by the notion of a division chairmanship, which was more than a departmental chairmanship but different from a dean. It gave me access to the Institute Administrative Council. I had a very strong interest in academic administration, and I thought this would probably be my career pathway, so it seemed to make good sense. I had tried in my career to stay at premier institutions.

COHEN: Did you pick up your consulting with Monsanto again?

BALDESCHWIELER: Not immediately. I maintained personal contact with Jay Henis. I had the opportunity to begin a consulting relationship with Merck. That was time-consuming but very interesting.

COHEN: Tell me how you found Caltech when you first arrived. I mean, you were anticipating some problems.

BALDESCHWIELER: Yes. Well, there was some disarray in the division. George Hammond had been chair for a while, and you may remember that George had gone through a period where he was nominated to become director of the NSF. And then he made some unfortunate statement. At least the *Pasadena Star-News* picked up something he said about the Nixon administration. These were still perilous times from the standpoint of the ongoing conflict in Vietnam. He had said something that didn't sit very well with the administration, so he was dropped. I think that

was part of the turmoil. Then Hammond left, in 1972, and went to UC Santa Cruz—and then eventually to what at that time was Allied Chemical, which later became Allied Signal. Jack Roberts had taken over as interim chairman and was clearly very anxious to unload that. So I came in in 1973. The situation, as I've mentioned, was that Caltech had been a very well-financed institution and most of the older faculty were accustomed to getting substantial support from the division and the institute. They were not particularly aggressive in writing grants and proposals. I think one of the reasons that Harold Brown was supportive in bringing me in was that I had an interest in financial management and he, at that time, was trying to gain control of Caltech's budgets.

COHEN: Was it just the chemistry division that was supported like this?

BALDESCHWIELER: No. But I think in some ways chemistry was probably the most subsidized. Physics had historically had more sources of support. There was the legacy of [Linus] Pauling, of course, which was still here. He had a large group of people doing research in his areas of interest, and when he left [1964], many of those people remained.

COHEN: Well, that was one of the problems, I think, that Pauling left behind.

BALDESCHWIELER: Yes, of course. This is a standard academic issue. When you have a person of such great distinction who builds a department around his own research interests, you don't have the stability and the diversity of a whole department of colleagues. So there was clearly turmoil around that. I came in and saw lots of things that could be changed and fixed. I worked really very hard at that.

COHEN: Who was the provost at this time?

BALDESCHWIELER: It was Bob [Robert F.] Christy. I really took this chairmanship on with tremendous energy and enthusiasm. For example, I changed the way that all of our committees were structured. I tried to get virtually everybody to be chairman of some committee, so that everybody had a stake in supporting the department. The committees' functions, of course, played together. We had all kinds of things, from undergraduate study to graduate study and

committees involved in recruiting. I called that the staffing committee. And it was substantially different from the older structure. What I tried to do was engage everybody in part in the management of the division. And that took off very nicely. I paid a great deal of attention to the financial matters. I discovered that we had actually substantial endowment funds.

COHEN: Do you mean just in chemistry?

BALDESCHWIELER: Yes. Biology had much more. And many of those initial gifts had been half biology and half chemistry, but the history is interesting to follow. [Laughter] Many of those chemistry discretionary funds had been taken over and merged into the general budget. So we had less discretion than biology for historical reasons, but we still had some. Whoever was the provost in earlier times managed to get control of those funds on the chemistry side, but not so successfully on the biology side.

COHEN: Pauling [division chairman from 1937 to 1958] was traveling all the time.

BALDESCHWIELER: I presume he was not paying attention. But I worked this in very great detail. And probably brought a good deal of know-how based on my recent experience. [Laughter] I found, for example, that there were discretionary funds, and I used them to negotiate with the federal agencies. I was trying to get the faculty into the process of applying for grants, and this worked most successfully with the younger people. I would argue that, say, we have this young person who has made a quality proposal, and if you [a federal agency] are prepared to finance the personnel costs, I'll cover the equipment with our discretionary funds.

COHEN: I see. So you actually took it upon yourself to help people with their grants and bargaining with the agencies. Is that common?

BALDESCHWIELER: No, I think not. I did a variety of things of this sort, to build the financial base of the division. And I loved doing it, I must say.

COHEN: And it must have made you very popular. [Laughter]

BALDESCHWIELER: Well, in part, but not universally, because I also brought considerable pressure to bear on those who were not actively doing research. And there I think I probably overdid it and created a lot of resentment where I really didn't need to. That indicated a lack of experience in dealing with a university organization. Harold Brown provided tremendous support for me in these matters.

COHEN: Well, he probably saw things as you did.

BALDESCHWIELER: He clearly saw things as I did. But there were certainly rumblings of discontent, which I tended to ignore, because I was, by my measure, being very successful at what I was trying to do. And there were lots of fun things. You may remember that we had an energy crisis in the early seventies, and I was able to create an energy initiative. I broadened this to bring in people from other departments. But the combination of chemical engineering and chemistry was a very powerful one in this matter. We could lead with chemical engineering and get support for the basic sciences through this kind of rubric. I must say, I was very pragmatic about such matters. My driving interest was to build the basic financial strength of the division. I had a vision of ours being the top chemistry department in the country.

COHEN: As we still are.

BALDESCHWIELER: We really did that. There was, as I say, not universal joy with this kind of pressure that I brought to it. [Laughter]

COHEN: How did that manifest itself?

BALDESCHWIELER: Well, we had some faculty defections.

COHEN: They just went somewhere else.

BALDESCHWIELER: But this was not altogether unintentional on my part. I tended to view any departure as an opportunity to bring in some young people. And I was biased very strongly in my management toward the young people—to whom I provided very considerable mentoring,

particularly in the business of getting their research financed. I must say, my two star mentorees were Ahmed Zewail and Peter Dervan. [Laughter] So I feel very pleased with where I put the effort and the emphasis, but there were a lot of people who didn't like it. As I say, we had a residue of people who had become accustomed to having their work supported and not having to argue the public value of what they were doing. I always felt that one could have it both ways—that is, engage in research that had supportable public benefits but which also had as much fundamental content as you would wish. I think in almost every research project—at least, in our area—you can find both those things.

COHEN: Now, how did you deal with space? That's always the big challenge.

BALDESCHWIELER: Oh, it was very, very difficult. What I finally tried to do was create a policy where, if one of the subgroups in chemistry wanted to bring in an appointment, they had to…

COHEN: Provide the space?

BALDESCHWIELER: Yes. They had to make trade-offs among their own existing laboratory space in order to bring this person in. As you'd find in any management role, what people want is to accrue the benefits to their area and have the cost fall somewhere else. So I tried to get the cost and the benefit together.

COHEN: Now, did this extend to postdocs and students also? They had to provide the space for them?

BALDESCHWIELER: Well, in chemistry it's laboratory space that counts—hoods and benches and so forth. The students and the postdocs occupy that space. The faculty was very cavalier about such matters. I think it's useful for everyone to be a chairman at some point in their career, so that they'll understand. [Laughter] But the faculty would be very cavalier and argue vehemently that we should do this and do that—oh, and somebody else can figure out the space and the cost.

COHEN: Now, where were you, in the seventies? Would you have lost Gates [Gates Laboratory of Chemistry] yet? When was that?

BALDESCHWIELER: Yes. Gates was ’71, so Gates was gone. But Noyes [Arthur Amos Noyes Laboratory of Chemical Physics] was built already. It was relatively new [1967]. And in fact the first major step I took when I came to Caltech was to create the initiative to build the Braun building [Braun Laboratories]. Most people don’t remember that. But I came from the Cancer Institute, and I knew that in the NIH there were funds for construction. So one of the first things I did was to get a grant written with, I must say, the extensive help of Norman Davidson.

COHEN: So the money for the Braun building came from the NIH?

BALDESCHWIELER: The start-up funds. Then we succeeded in finding a major donor to carry the major cost. But that was a time when you could still get federal money for buildings. I had been in the Cancer Institute, so I thought cancer was the horse to pull this. So I made such a presentation to the trustees, with Norman’s help. And during my tenure in Washington there was the war on cancer. That was another one of these huge imbroglios.

COHEN: That Nixon was going to cure cancer?

BALDESCHWIELER: Yes. That was on my watch. And it was clearly a run on the Treasury by certain parts of the biomedical community that felt that under the label of cancer you could get a whole lot more money. And they created a separate cancer institute, which is almost a super-institute, within the NIH. So the director of that institute for a number of years had power essentially comparable to that of the head of the whole NIH. It worked, of course. A lot more money flowed into cancer, and a lot of important fundamental biomedical work was done under that rubric. You know Ray Owen? In this case, Ray really was a hero.

COHEN: In what way?

BALDESCHWIELER: Well, he chaired the advisory committee for the new institute for a period. And that advisory committee had enormous power. That was one of these initiatives, like the Human Genome Project. Such things happened in high-energy physics as well, of course. A major objective was getting a kind of momentum behind it, and a lot of funding. And from that funding there was some fallout, which helps others.

COHEN: So who did you put in the Braun building? Are chemists in there?

BALDESCHWIELER: It's half chemistry and half biology. But those were standard techniques for raising money.

COHEN: So that was a good part of your job.

BALDESCHWIELER: I viewed that as essentially being my job.

COHEN: Now, were you doing any research yourself? Did you have a lab going all this time?

BALDESCHWIELER: Yes. I had a lab. I brought with me a group of people from Stanford. I brought a group of students and postdocs that were working with me there. We had some laboratory space in Noyes, although that was not the top priority for me. I really made a major commitment to the management side of the job.

COHEN: But some research still was going on.

BALDESCHWIELER: Yes. And it turns out that that was very important in the end. So I view that period as successful. I really enjoyed the planning and fund-raising and financing.

COHEN: Well, that's your old love of putting things together.

BALDESCHWIELER: Yes. In a way, perhaps that's right. It was putting things together.

COHEN: How did you apportion your time? You must have been quite picky about that to do all these things.

BALDESCHWIELER: I was very busy. As I say, I started consulting with Merck. And in the middle of my tenure as chairman—you see, at Merck I worked directly with the vice-president for science and technology, whose name was Lew Sarett. Merck had acquired a number of companies that were not directly in the pharmaceutical business, at a time when Congress was

bringing great pressure against pharmaceutical profitability and drug pricing—the same as today. The situation seemed so threatening that they had acquired a whole series of companies and other businesses. Sarett was the person who synthesized cortisone; he was a very distinguished synthetic chemist. He had under his wing the technology and science in all of these far-flung subsidiaries. And so the two of us formed a team, and we were terrific together. For example, one of their subsidiaries was the Calgon Company.

COHEN: That softens water.

BALDESCHWIELER: Yes. And at that time they also produced activated charcoal for filtering things. Well, he knew nothing about that, but that was great fun for me.

COHEN: So that was a different company altogether doing that?

BALDESCHWIELER: Yes. They owned Kelco, which is in San Diego. It extracts polysaccharides from kelp. The stuff that makes salad dressing viscous—you know, when you get your salad dressing on the airplane, if you look carefully at the label you'll see that it contains xanthan gum, which is a…

COHEN: Which is made from kelp.

BALDESCHWIELER: A similar polysaccharide called algenate is extracted from kelp. A major process industry was initially built around kelp. They'd harvest the kelp and extract the algenates. They had huge process equipment and research supporting that. Xanthan gum actually came from a fermentation process. So they not only processed kelp but they found that they could make similar materials from fermenting cells from microorganisms. And then another company was Baltimore Air Coil. Baltimore Air Coil made cooling towers—the kind of tower that you see when you drive down the Pasadena Freeway, with the steam coming out the top.

COHEN: These were all companies that Merck had bought.

BALDESCHWIELER: Yes. So Lew didn't understand about any of those parts of the business, but with my engineering background, I did.

COHEN: How much time did you spend at this? I'm just wondering how you could have found time to do all this.

BALDESCHWIELER: Well, I was very busy. And I not only did this, but I also was directly involved in the US-Soviet joint scientific program after leaving Washington. The administration had agreed with the Soviets to do a whole lot of stuff, but of course there was nobody there on our side to do it. [Laughter] I was part of the process of deciding what the major areas of cooperation would be.

COHEN: Now, this would have been in the late seventies?

BALDESCHWIELER: The middle seventies—'73, '74, '75. I led the program on chemical catalysis. It was a major international effort. And I really enjoyed this, because there I had a chance to resurrect my Russian-language skills, and I became passable in at least colloquial Russian.

COHEN: How much time did you spend with this? You had to go to Russia?

BALDESCHWIELER: I had to go there. We had a group of a dozen US scientists in various subareas of catalysis, with comparable Russian counterparts.

COHEN: And you put that together yourself.

BALDESCHWIELER: I put that together. And then we had to travel to everybody's institution. And we had to raise the money.

COHEN: That took a lot of time.

BALDESCHWIELER: Yes. And we exchanged students.

COHEN: Did you see a lot of ballet?

BALDESCHWIELER: Yes, for fifty kopeks. [Laughter] We had a wonderful range of experiences—and there are stories, which we will someday have a chance to at least tell our kids.

COHEN: Can you pick one or two of them that would be sort of indicative of the flavor of what was going on? [Tape is turned off]

Begin Tape 5, Side 2

BALDESCHWIELER: I had close personal relationships with a lot of the Soviet students. I was in Moscow on one of these trips, and two of them came to me and said, "Would you like to go to Tashkent?" Tashkent is in Uzbekistan. And I said, "Sure." They said, "Well, we want to get some melons, and so we're going there this weekend." This kind of centrally planned economy is utterly bizarre, because, for example, flights on Aeroflot were essentially free. If there was space, you got on. And as foreigners—you know, some big, burly Russian officials would get on and literally throw people off if a foreigner needed a seat. That made us immensely popular with the traveling public. [Laughter] But these two students had a string bag.

COHEN: And they picked melons.

BALDESCHWIELER: No, they were going to buy melons. Uzbekistan is in Central Asia. So this was like taking a trip from Los Angeles to Miami on a whim. There was space, we got on, we flew. Then there were these wonderful Central Asian cities, with mosques and bazaars and minarets. We also went to Samarkand and Bukhara, all in this weekend. My Russian was good enough to manage to talk to people. So this was like going from Los Angeles to Miami to Boston, and back to Los Angeles. It was particular fun because the farther you got from Moscow, the more congenial and interactive the people became—I could talk freely about the bureaucrats in Moscow.

COHEN: You had other dialects?

BALDESCHWIELER: No, no. Everyone could speak Russian. Oh, I shouldn't get started on Russian stories. [Laughter]

COHEN: [Laughter] OK. So how much of your time was expended on this?

BALDESCHWIELER: Well, there were blocks of a week or two here and there. I was really extraordinarily busy.

COHEN: But you were having a very good time.

BALDESCHWIELER: Yes. But of course, some things were lost. Obviously one's marriage pays a significant price, because Marcia was busy raising two small children by herself. And pretty soon, another one came along, in 1974—David. And she had career interests as well and had been moved from place to place according to my career profile. She had built a practice in psychological counseling, and that got disrupted with each of the moves. So there was some price indeed to all of this.

But the situation at Caltech went on. I was, as I say, having my fun. I thought, at least by my measure, it was a very successful run at the chairmanship.

COHEN: How long did you hold that position?

BALDESCHWIELER: Until 1978.

COHEN: So you got the department into a new mode of operation that was successful.

BALDESCHWIELER: Yes, at least I thought it was successful. We took some losses. Some people left who felt, I think, that they had not been served well by my priorities, and some others left because they had significant outside offers. However, we tended to hire quite well. As I say, this was the time we brought in Peter Dervan and Ahmed [Zewail]. I think both of those, in retrospect, were extraordinarily successful appointments.

Let's see, now. Ford served until 1976, didn't he? That's right. And Carter was elected in '76. And this, in a certain sense, was a critical turning point in my career, because Harold Brown left Caltech and went back to Washington as secretary of defense.

COHEN: Christy was an interim president.

BALDESCHWIELER: Christy was an interim president, and he had a very different background and basis for running the institute.

COHEN: He was probably much more traditional.

BALDESCHWIELER: Yes, absolutely much more traditional. I saw myself as running the department according to the priorities that Harold Brown had set. I was very comfortable with that. This was by no means a consensus of the division itself. And I think when Harold Brown left…

COHEN: You lost your major support.

BALDESCHWIELER: Yes, that's right. The rationale for what I was doing was much more difficult to defend. I also learned that over a period of time in these jobs you…

COHEN: You lose your power?

BALDESCHWIELER: Well, you create a trail of people who are—what's the word I'm searching for?

COHEN: Malcontent?

BALDESCHWIELER: No, just a kind of resentment, because…

COHEN: They haven't gotten what they want.

BALDESCHWIELER: They haven't gotten at least *all* they want. In the process of making decisions, there's always some resentment that builds. In other kinds of organizations, the hierarchical structure supports the people who make those decisions. But in the university context…. [Laughter] I must say, I was rather insensitive to that. And at the end of my five-year period there was simply not the base of support that would carry me on for another five years.

COHEN: And you sensed this yourself?

BALDESCHWIELER: Yes. Well, people were quite forthright on such matters. [Laughter] I was set back a bit, because…

COHEN: You weren't used to it.

BALDESCHWIELER: Yes. I think it became clear to me that I did not have the complete set of skills to be an administrator in a university environment. Probably, for that matter, not in an industrial environment either. I really enjoyed the planning and program development and putting it together, but the personnel matters I found very difficult to deal with—in part because I judged them to be not worth the expenditure of time and effort. That was a significantly bad judgment. I think one needs an array of skills, some of which I simply didn't have.

COHEN: But you accomplished a great deal.

BALDESCHWIELER: I think I was very successful.

COHEN: It's always good to leave at that point, perhaps.

BALDESCHWIELER: Yes. I left the division in very sound fiscal and academic shape.

COHEN: So Christy was there as acting president, but then of course Murph [Marvin L. Goldberger, Caltech president 1978-1987] came.

BALDESCHWIELER: Yes. By that time I had finished my tour.

COHEN: Who was the next chair?

BALDESCHWIELER: Harry Gray was the next chair. And he brought, as you might judge, almost an inverse set of skills. Harry's strengths were people. And certainly his strengths were my weaknesses. So in some sense there's a certain cyclic…

COHEN: Well, that's good.

BALDESCHWIELER: Yes, that's right. However, I was in some disarray about what to do next. Several things had happened. Merck had offered me a position, basically as the successor to Lew Sarett, as vice-president of science and technology.

COHEN: Where's their home office?

BALDESCHWIELER: It's in Rahway, New Jersey—or it was at that time. And I had grown up in New Jersey. It was fine. They transported you around in big limousines and paid obscene salaries and so on. But I decided against that—again, for reasons I think are more instinct than rational. I thought that my heart was really closer to research. I don't think I have the kinds of organizational political skills that would be required to survive in that environment. So I think, in retrospect, that was probably a wise choice. The person who took that job did it extraordinarily well. It was Roy Vagelos, who also came out of academia.

COHEN: So you stayed here and were just a professor now.

BALDESCHWIELER: Well, various other things happened. I took a six-month sabbatical. I spent time at the synchrotron radiation laboratory at Stanford and at Bell Labs in Murray Hill, New Jersey. I had a wonderful time in both places.

COHEN: With the idea to get back into your research mode?

BALDESCHWIELER: Yes. I had set my sights on doing something biological. But even after working in the Cancer Institute and gaining some skills and insight, I didn't really have a significant research idea. My students were still moving along, but I had not given them a great amount of attention. In the Bay Area, I was also recruited by Xerox. George Pake, who was head of the Palo Alto Research Center [PARC] of Xerox, recruited me to become vice-president of research at Xerox. Their headquarters is in Stamford, Connecticut. It was a very, very well-appointed research laboratory. It was a beautiful place. What we take for granted in our modern office equipment—computers, bit-map displays, mice and so forth—that was all just happening. It was happening there at Xerox. They led. They didn't get the commercial benefit finally, but they were the leaders in the development of the software for displays, for icons, for…

COHEN: Yes, I remember. But they didn't go with it, or something like that.

BALDESCHWIELER: It was a very curious organization. They had developed xerography. The principal laboratory for the copying business was in Webster, close to Rochester, New York. The huge success of xerography was part of their problem later on, because they expected all their initiatives to work as successfully as that, and of course it's very hard to develop a commercial product. They were in the lead with George Pake, and the laboratory in Palo Alto was just a fantastic place. But there I suspected that they would have great difficulty in bringing these products to commercial success, and I think I was quite right.

COHEN: You were looking at all these opportunities in the six months you were on sabbatical.

BALDESCHWIELER: Well, they came along from time to time. I didn't actually take the initiative on them, but they were based on a network of people I'd had the opportunity to work with at various times. But I decided to stay with Caltech. And other interesting things were going on. The China program began, and I was able to take a major initiative in that. I actually traveled with Glenn Seaborg on the first chemistry delegation to China.

COHEN: What year was that?

BALDESCHWIELER: 1978. It was just at the time of the transition from the Gang of Four. Again, I was in very familiar territory from the standpoint of setting up international programs and doing things that I liked to do. But again, there was a heavy dose of travel and commitment away from the campus. When I returned from the sabbatical, I began to look carefully at what I was doing. You may remember the angular-correlation experiments. We had left our cyclotron resonance work behind at Stanford, because Jack Beauchamp was here. He was one of the reasons I came to Caltech. He was my former student from Harvard-Stanford days. And he was doing a beautiful world-class job in this field. So I didn't need to do that, but I had brought the angular-correlation experiment and equipment with me. We were working quietly on the physics of this experiment. I had a postdoc from MIT—a wild, creative person whose name was Ronald Gamble. And he came and started working in this area. Gamble introduced the idea to me of liposomes. Liposomes are little vesicles made of phospholipids, the same material from which a human cell membrane is constructed. These were just coming into scientific view. So he taught us how to make these. On one of my visits to Merck, I spoke with one of their top scientists, T. Y. Shen. Shen knew about liposomes and said it would be wonderful if we could do something jointly. It turns out that T. Y. Shen had synthesized a series of sugars, which are the molecules that are used for signaling on cell surfaces. These were sugars bound to cholesterol, which would incorporate in the membrane, so the sugar could be displayed on the outside surface of the liposome. I had the idea—you know, one of those things that came to me on an airplane—that we could use our angular-correlation technique to put a radioisotope inside the liposome. The isotope would tumble with a frequency that we could measure, even when it was in an animal, by measuring the angular correlation. If the liposome broke, then the radioisotope would bind to large molecules—large proteins or glycolipids—in the circulation and its tumbling frequency would change. So we would have an assay for the integrity of these tiny structures, which are about one one-hundredth the size of a red blood cell. Our primitive idea was that we could find molecules that would target the liposomes to the specific tissues in the body. It was absolutely naïve. [Laughter] It was an utterly fanciful set of ideas—but we set out on this course. I had been fortunate to maintain research grants, although I must say that it was getting tougher and tougher, and I had less patience to write the detailed research proposals that were required and do battle with all of the review panels and funding agencies. But we did it and worked for a number of years in this area.

Well, a series of interesting things happened. The phospholipid vesicle work began. And one of the first things we found was that when we injected the liposomes into mice and did our angular-correlation experiment, we observed that the liposomes simply disintegrated. So most of the previous literature simply was not valid, based on injecting liposomes into animals; without an assay for the integrity, it was not possible to know what these structures were doing. We found that the liposomes disintegrated, and we found out why they disintegrated. We were able to change the formulation and produce very robust and stable structures. Then one day one of the students said, "Well, now it's time to cure cancer." [Laughter] And I said, "Right. Why not?" So we set out to do this. I made contact with a group at the City of Hope [in Duarte,] which had the right kinds of cells—they had lines of tumor cells in mice, for example. One of the first things we did was to expose the liposomes with Merck's polysaccharides on the surface to tumor cells and compare that to liposomes without the surface markers. And lo and behold, the ones with the surface markers bound enormously more effectively to the cancer cells than the ones without the markers. So there was great joy in our research group. Then some wise old hand—maybe it was Norman Davidson—said, "Well, you know, all kinds of things happen in *in vitro* cell cultures, but we don't really have a test for what kinds of things are effective without an *in vivo* experiment." So we tried an animal model. We implanted tumors in mice and injected them with our favorite formulation. Of course, guess what.

COHEN: It worked?

BALDESCHWIELER: No. [Laughter] Everything went directly to the liver and the spleen, almost completely. In fact, we did the control, which was just the indium with no liposome at all, and that worked far better than our liposomes. [Laughter] And not having a lot of sophistication in biological work, we simply set about empirically, changing things to see what it was that changed the biodistribution of these liposomes. We had confidence in the formulation—that it was robust and we could measure the integrity. So we tried all different kinds of proteins on the surfaces, and all kinds of other things. We were, it turns out, unable to see what those experiments were really telling us. Finally it was inescapable. The formulations that worked best had nothing at all on the surface. We were so convinced that you needed a cell surface receptor to which to bind that this didn't make any sense at all to us. We then tried varying the

size. It's not only the size but the distribution in size that matters. We refined the techniques to make liposomes that were of various sizes with a reasonably well-controlled size distribution and found that there was an optimum—we were measuring, in this case, the relative distribution between the liver and the tumor. We were able to optimize to the extent that we had almost ten times the dose going to the tumor as to the liver—ten times the dose per unit of mass of tissue. We thought, "Aha! This is potentially important and potentially of commercial value." This was in the late seventies or maybe 1980. So I thought, "Well, this is the time to build a company"—although it was not very popular at that time, and it was definitely not popular at all at Caltech.

COHEN: Was this technique to just identify the cancer? Or was it to shrink the cancer? What was the purpose of this?

BALDESCHWIELER: In the first case, we had a radioisotope inside the liposome, and that would light up in a tumor.

COHEN: So that was identification.

BALDESCHWIELER: If you took a gamma-ray image, you could literally see the tumor. So we thought of this as a diagnostic tool. If you can get the liposome to the tumor, then we thought we could load this with a chemotherapeutic agent and have the benefit of targeting. The limiting feature in chemotherapy is often the side effects, which are terribly toxic. Very often there's liver toxicity or kidney toxicity. If you can focus the drug more on the tumor and less on the sensitive tissues—this is basically a drug-delivery biodistribution argument.

COHEN: So you had the idea that you could commercialize this.

BALDESCHWIELER: Yes—that there ought to be something of therapeutic value here. My postdoc, Gamble, had left Caltech and was at JPL [Jet Propulsion Laboratory]. I talked with him, and he thought this would be good fun, a good thing to do. My secretary, whose name was Nancy Kossewitz at the time—later to become Nancy Kossewitz-Templar—said that she would join in this. So I embarked on a crusade to raise some money to create a company. And I must say, I had no concept of what this would take, or of what a complex undertaking this would be. I

gave lots of talks. I remember I went back to Monsanto to see if they would support this, and the director of the relevant division fell asleep during my presentation and snored. [Laughter] You know, you read the stories of the founders—[Chester] Carlson, in particular, trying to get Xerox started. It was the same thing—many, many, many discouraging incidents. But finally we found some support, from a person whose name was Teddy Walkowicz. And it turns out Walkowicz had a Caltech background. He had worked with [Theodore] Von Kármán here during the war and with H. S. Tsien. After the war, Walkowicz was in various air-force-related businesses, until he was attracted by the Rockefellers to start their first venture capital fund. He was running a venture fund at that point which was divorced from the Rockefellers, called New Technology Ventures. But Walkowicz came to me because he knew I was part of the China program, and he wanted to reestablish contact with Tsien. So that was the key. We got to know each other, and he became interested in what we were doing. He said, "How much money do you need?" And I said, "Oh, I think probably forty thousand dollars"—I had thought, well, you know, we could have a couple of graduate students and postdocs. [Laughter] He said, "We'll invest a million and take half the company for the equity." I was flabbergasted. I had never heard of such a thing. But this was our opportunity, so we agreed.

COHEN: Now, as a professor at Caltech you couldn't be president.

BALDESCHWIELER: No, I wasn't. Gamble was president. This was not a very popular thing to do at Caltech in those days. In fact, it was very much frowned upon, because it was viewed as a distraction from fundamental research. I always looked at these things somewhat differently. I thought that if you want real public benefit from your science, you have to take some initiative in these directions. Understand, at that time we had no idea how our system worked. But we had the empirical evidence that…. We had a few pictures of mice with…

COHEN: Maybe this is a good place to stop. We've been talking an hour and a half.

BALDESCHWIELER: Oh, wow! Look at the time.

COHEN: We can start with Teddy Walkowicz.

BALDESCHWIELER: And the mice. OK. [Tape is turned off]

JOHN D. BALDESCHWIELER

SESSION 5

February 7, 2001

Begin Tape 6, Side 1

BALDESCHWIELER: Another thing I should mention is that I was elected to the National Academy of Sciences in 1970.

COHEN: So you were very young.

BALDESCHWIELER: That was at age thirty-six.

COHEN: That's almost a record, I think.

BALDESCHWIELER: It's close. At the time I'm not sure I recognized the import, because I was so busy with all the things I was doing. [Laughter]

COHEN: They used to say the average age there is dead. [Laughter]

BALDESCHWIELER: Yes. [Laughter] But of course on reflection it was really a very substantial and much appreciated recognition.

I've mentioned Monsanto. It was one of my very early consulting jobs, which I had to of course terminate when I went to the White House in 1970. I reconnected with Monsanto about ten years later, when they were in search of a new vice-president for science and technology. They were on the verge of entering into the field of biotechnology. These were still the very early days. But they had the vision that biotechnology could be used to improve plants. A person by the name of Howard Schneiderman, who had been a biologist and academician at Irvine, took the position and led Monsanto into the area of recombinant modification of plants. It was the basis for their development of a variety of recombinant species, particularly those that would be resistant to herbicides, so that you could spray a field and kill the weeds without killing

the valuable plants. In retrospect, as I thought about that, he and Monsanto turned very strongly in the direction of biotechnology—I would have brought to them a more balanced effort.

COHEN: Now, they offered you that job, I gather.

BALDESCHWIELER: No, in the end, they didn't. But that was the occasion on which I reconnected with Monsanto. And my friend Jay Henis, in the meantime, had developed a whole technology of hollow fiber membranes for gas phase separations—my connection with Henis will occur again. So I once again became involved in Monsanto affairs. But in retrospect I would have done it differently. I'm not sure that the outcome would have been better, but certainly different. I don't think I really appreciated the power of biotechnology at that time.

COHEN: They're certainly having plenty of trouble from that.

BALDESCHWIELER: Yes. I think the trouble reflects the fact that they didn't consider the public policy aspects of what they were doing in sufficient depth. Anyhow, it was a very interesting early seed stage of lots of things that are front and center today. I think we were just starting on Vestar. I described our work with phospholipid vesicles.

COHEN: Right. And you thought that perhaps there was something there, and you went out to get capital.

BALDESCHWIELER: Well, the name of this new entity was Vestar—for "vesicle targeting." A very clever name, conceived by Ron Gamble, the postdoc who had brought to my attention the phospholipid vesicles in the first place. Again, Gamble will be a character who threads in and out of my career. [Laughter] After lots of trying and lots of disappointments, we finally found a venture fund called New Technology Ventures that agreed to put up a million dollars for half of the company. At that time we thought this was just extraordinary. [Laughter] This was just shortly after the formation of Genentech [1976], so these were very early days to begin such an enterprise. The principal general partner of New Technology Ventures was Ted Walkowicz. He was a tough, demanding investor, but also a very talented one, with a Caltech history.

COHEN: Where did you set up this business?

BALDESCHWIELER: We set up this business on the corner of Walnut and Mentor, in a building which is still there, on the northwest corner of that intersection. This was a building that had a small pharmacy on the first floor and several doctors' offices. In time, we took over the whole building and modified it and put in rather extensive facilities. But first we had just one room and a card table and four chairs.

COHEN: Now, at that time was there already a policy about businesses at Caltech?

BALDESCHWIELER: Yes. It was clear that you could not take a management position in a start-up. On the other hand, there was no restriction against owning shares or being a member of the board or a consultant. So that was the way that I managed this. Gamble became the president of the company, and my long-time assistant, Nancy Kossewitz, became the administrative assistant and the person who got things organized at the new company. We had hired some postdocs from my group and got ourselves under way.

COHEN: You hired postdocs, who then became full-time employees?

BALDESCHWIELER: Yes. We had money and we could get going. There was not a lot of support for such activities at Caltech, so we simply kept a very low profile and were extremely careful that there was no use of Caltech facilities or equipment.

COHEN: Was that a first? Or were there already some other companies that had been formed?

BALDESCHWIELER: Oh, over the course of Caltech's history there had been a number of companies that had spun out of the institute. But they had not been either a major source of income for Caltech or a major source of employment.

COHEN: Was this going on already at Stanford, which has a reputation for doing this?

BALDESCHWIELER: Yes, Stanford and MIT were considerably ahead. They had well-organized technology-transfer offices and patent and licensing groups.

COHEN: So the institutes themselves encouraged this. Did they have the same rule that a professor could not be…

BALDESCHWIELER: Typically, yes. And so the whole thing got under way. It was a very, very exciting time. [It was] literally a garage-style operation. I had a consulting agreement with a company called Science Applications, Inc. In the course of that, I met Dick Callahan. Callahan was interested in moving from Science Applications, so we brought him in as a business development person for Vestar. It soon became apparent that Callahan had far more talent for managing and leading the company than did Ron Gamble, who was a creative inventor type but less meticulous about getting business matters done. [Laughter] So my first challenge as chairman of this board was to make a transformation in which Callahan became president and Gamble became chief scientist without losing either or both of them, [laughter] since they both were very talented.

COHEN: That took some finesse.

BALDESCHWIELER: Yes. Fortunately it worked. So we were under way. We had developed a collaboration with the City of Hope. They were most gracious in allowing us to take images of mice on their equipment. We worked closely with Dr. Cary Presant, who was the head of oncology at the City of Hope, and one of his colleagues, Dick Proffitt. Dick Proffitt, very shortly after that, joined Vestar. So we began to build really a very high-quality team. We had patents on the angular correlation method, which was developed at Caltech, but the other key patents were on work that was done at the company and were company property. In the early 1980s, Harold Brown, who had just completed his tour with the Carter administration as secretary of defense, was back visiting the campus, and I had a chance to talk with him. You may recall in the closing days of the campaign that Reagan had successfully attacked Carter as being soft on national security and defense matters. Harold had disclosed to the press the existence of the Stealth technology, which had been a very closely held secret. He was, I think, considerably

criticized for this, and also clearly had been asked to make the revelation to support the Carter campaign; he was clearly feeling unhappy about that. But at the time we had a chance to talk about what each of us was considering doing next. He had already established an affiliation with a large New York venture-capital and investment-banking firm called Warburg Pincus. Warburg is a very well-known German merchant banking family, and after the war some of their assets became available in this country. Warburg reflected the European origin of the merchant banking activity, and Pincus was a real person, who had transformed this old firm into a modern venture-capital investment firm.

COHEN: Harold Brown had taken a position with these people?

BALDESCHWIELER: Yes, and he asked if I would become a member of their technical advisory board. That was very exciting, because he was very exciting. He had brought a number of other people onto the technical board—people I had known at various times in my career. One of the first things that happened was that John Vogelstein, who was the head of the venture part of Warburg, came out to see me and indicated an interest in investing in Vestar. We were just at the point of doing our second round of financing. We raised, in the second round, in excess of $6 million.

COHEN: From Warburg Pincus?

BALDESCHWIELER: Well, there were six investors—the original group and five new ones, which included Warburg. Warburg had by far the deepest pockets, and over time they developed a very large equity position in Vestar.

COHEN: And was Vestar doing good business by this time?

BALDESCHWIELER: No, Vestar was still doing research.

COHEN: Still developing the product.

BALDESCHWIELER: Yes. And we had in mind that the product should be a diagnostic, because with our gamma-ray images we could see where the liposomes had gone. They were targeting tumors so the tumors could be easily imaged, and we thought this was a pretty clear opportunity for a diagnostic product. Throughout this time, as I mentioned before, we didn't really understand how this worked. I don't know if I mentioned this, but our first idea was that there would be receptors on the tumors that would respond to specific oligosaccharides on the surfaces of our liposomes. And step by step we discovered that the best and most effective targeting was achieved with nothing at all on the surface of the liposome. In fact, the effective targeting was related to the size and the size distribution of the liposomes, and also to how stable the membranes were. The clue that solved the targeting problem came from [J.] Harold Wayland, the professor in engineering who died recently. He had for years been doing studies on the flow of blood in the vascular system, and he had a very effective scheme for looking at fluorescently labeled cells in the vascularity going through thin sections of tissue from live animals. We learned from him that from time to time the vascularity is imperfect—that is, there are gaps in the vascular system. In rapidly growing tissues, the body builds capillaries so rapidly that the architecture is imperfect and there are gaps. These gaps are large enough to let something the size of a liposome escape. So targeting tumors requires that more of the liposomes exit the circulation at the site of the tumor than in other places, such as the liver. In the liver the capillaries are also fenestrated, so we found in the literature information on the size of the fenestrations. If we had liposomes of exactly the right size, they would be less likely to escape into the liver and much more likely to exit the circulation at a tumor. In looking back, I can see that we really had a very naïve understanding of physiology. If the vascular system is intact, as it is in most parts of the body, then there's no way that our liposomes could ever possibly have found a tumor. There were other groups that were using monoclonal antibodies to target tumors; however, they had never done proper controls, because virtually any monoclonal antibody would target a tumor for the same reason our liposomes did—simply on the basis of the imperfections in the vascularity of a rapidly growing tumor. This was the most important scientific discovery of my career. And at that point we felt we could not disclose or publish this. So this remained proprietary, within Vestar. We couldn't file a patent on the mechanism itself. You have to file on the utility. We did file on tumor targeting. However, we never did what would be required to

make a full publication of this, because that would have required very extensive controls and experimental work which was off our main track.

COHEN: You may have given up a Nobel Prize. [Laughter]

BALDESCHWIELER: Well, I sometimes wonder. But it's one of those trade-offs you make. At the time that was the best information that…

COHEN: Now, you were doing this strictly at Vestar? This wasn't going on in your lab at Caltech?

BALDESCHWIELER: That's right. This was all work that was going on at Vestar. We published one paper in *Science* which showed the targeting [Richard T. Proffitt, et al., "Liposomal Blockade of the Reticuloendothelial System—Improved Tumor Imaging with Small Unilamellar Vesicles," *Science* (1983) 220, 502-05], but we never disclosed the mechanism by which it happened. It turns out that the fact that the mechanism had little to do with the surface chemistry of the tumor and everything to do with the vascularity was a very important asset, because it meant that we had a very general mechanism for targeting tumors of all kinds, independent of their tissue of origin. And the types of tumors we targeted best were the ones that were most rapidly growing, which was, of course, metastatic disease, which is exactly what you would want to do. So this seemed like altogether a very powerful discovery. And clearly it was.

COHEN: So it was really very exciting there.

BALDESCHWIELER: It was a very exciting time. We kept this closely held. And in some ways it added to the drama, to the excitement, that we certainly knew some things. There was competition at the time. Two other liposome companies started up at roughly the same time we did. One was called The Liposome Company—TLC—based in Princeton, New Jersey. And one based in Menlo Park, California, called Liposome Technology, Inc. The one in Princeton was started by a venture investor and the one in Menlo Park was based on work from the laboratory of Demetrios Papahadjopolous, who was one of the very early pioneers in this field.

COHEN: Did you know how far along they were? Was there sort of an industrial buzz or something?

BALDESCHWIELER: Yes. We knew the people, we knew their work. And so we had a reasonable idea. We felt we were ahead. In fact history would confirm that that was really so. I remember going to meetings and hearing people be very demeaning of liposome work, because for so many years it had not been productive. But we knew things that they didn't know.

So that was the beginning of Vestar. In collaboration with the City of Hope, in particular Cary Presant, we were able to move quickly into some early-stage human trials. Oh, and I forgot to say that as the work progressed it became clear that Callahan was not a long-term CEO for this operation, and we were under considerable pressure from our venture investors, particularly Warburg Pincus, to bring in a CEO with valid pharmaceutical company experience. So we made several attempts to hire people. One of the people I was very anxious to hire but whom Callahan helped defeat was a person named Henri Termeer. Henri was a young man in the Baxter [Travenol] organization, who was looking for an entrepreneurial home. The job he took turned out to be CEO for a start-up company called Genzyme, which is in the Boston area. Under his leadership, the company became fabulously successful. So we again missed a chance for greatness. But we did finally hire Roger Crossley. Roger came from Lederle [Laboratories]. He's an MD—British—and had been the person who brought Tagamet through its clinical trials and into launch. It became a billion-dollar product. Roger was a booming, talkative fellow and quite knowledgeable about the clinical development of drugs. He became our CEO. He came out here and took charge of things immediately. One of his early requests was that he should have a special parking place and should travel back and forth from the airport in a big limousine. And of course our early-stage entrepreneurs looked with utter disbelief at this. He caught on quickly that that was not good PR. [Laughter] He soon got very much into the swing of development. He was able to help us get our first IND filed.

COHEN: IND?

BALDESCHWIELER: Investigational New Drug application. That took thousands and thousands of pages of documentation and used up all the money that had been invested to get this done.

Finally we were able to get our liposomes into humans. And some of the first images were of cancer patients. Some of the most dramatic were of patients with advanced prostate cancer, which metastasizes to the bone. Literally we could see everywhere these metastases formed in the body. It was clear that this was working very well.

COHEN: Now, these patients were all at City of Hope?

BALDESCHWIELER: City of Hope was our primary site. And then we worked later at Norris-USC [USC/Norris Comprehensive Cancer Center]. Then as the trials expanded, they of course went nationwide and finally worldwide.

In bringing along a small company, you often come to a point that I call "You bet your company." And this happened to us in the mid-1980s. Roger had correctly noted that on the order of only one out of ten of those products that enters the human clinical trials finally succeeds. Taking a therapeutic through clinical trials would cost as much as the equivalent risk of taking our diagnostic through clinical trials. On the other hand, the profitability of a therapeutic would be far greater, because the diagnostic field was highly competitive and the margins were much smaller. Our work on the diagnostic had shown the efficacy of the targeting, but in the end it was unlikely that there would be a profit of sufficient magnitude to build a company. We argued that we should have several products running in parallel, because they could be defeated by factors you simply can't anticipate. On the other hand, we had a finite amount of money, so there was a climactic meeting of the board in which we decided to abandon the diagnostic product, the gamma-ray imaging product, and put all our resources into several possible therapeutic products, none of which, of course, we had developed. That was a critical turning point.

COHEN: These things are not for the weak-hearted.

BALDESCHWIELER: They are not for the weak-hearted. [Laughter] I think that a certain level of naïveté is probably helpful, because it was far more demanding than I could possibly have imagined. To get a product into humans means that you have to meet standards of what are called good laboratory and good manufacturing practices that are just extraordinary—unusually

expensive, unusually time-consuming. Obviously, you don't want to have any fatalities in your clinical trials. [Laughter] You'd also lose your company, and maybe more. So it's a very risky undertaking in many ways.

In the process, I had given a public lecture. Afterward a very dynamic young lady, Jill Adler, came up to talk with me. She was a junior faculty member in biology at Cal State Polytechnic in Pomona. She had been working with antifungal drugs, and in particular with what was the most powerful but also the most toxic of these drugs, amphotericin-B. And after the lecture she said, "Well, why didn't you simply encapsulate amphotericin-B in your liposomes?" That would eliminate the toxicity, which was primarily to the kidneys. Our vesicles didn't pass through the kidneys and would be effective in delivering an antifungal drug. So we arranged for her to spend six months in my lab at Caltech, learning how to make liposomes and how to incorporate amphotericin into the liposomes. She brought with her some of her students from Cal Poly, including one who was a high school biology teacher on leave. That was a very tough lady. [Laughter] She had all my Caltech students very quickly lined up.

It turned out that the drug could be incorporated into liposomes, and in animal models it worked the way Jill Adler had hypothesized. So we added this to the mix of things going on at Vestar. In the end, after an additional number of years of research and clinical trials, it was the amphotericin liposomes that emerged as the first commercial product.

COHEN: Did she ever go to work for you at Vestar then?

BALDESCHWIELER: Well, she was a consultant. She spent her summers and then several full years at a time working at Vestar. She and Dick Proffitt worked very well together, and they were the ones who put together the final effective formulation. Amphotericin was an obvious product at that time, and it was obvious to the other two competing companies, and they both tried to make such products as well. In the end, neither of the other two were successful.

COHEN: You had those good high school biology teachers. [Laughter]

BALDESCHWIELER: [Laughter] It was a combination of skill, good luck, and a mix of good people. I think we brought a certain physical sophistication to this research that the other groups didn't have.

COHEN: Now, is this considered OK? I mean, I was just wondering about this. You brought her to your lab here at Caltech to learn the procedure.

BALDESCHWIELER: Yes.

COHEN: And then the procedure goes to the company.

BALDESCHWIELER: Yes. But there was no intellectual property involved at that point. The effective formulation was developed within the company.

COHEN: I see. So it's just the idea of learning techniques that she didn't know.

BALDESCHWIELER: Yes. And it has always seemed to me that this is exactly the role for our kind of institution. That is, to train the people. It's these people who go out and do useful things. The benefits will come back to Caltech in due time, in the form of support for the institute. So I think this was the institution at its best.

COHEN: You know, in some sense this was one of Millikan's early ideas for Caltech—that they should be a light to lead surrounding industries.

BALDESCHWIELER: Yes. So with lots of ups and downs and blind alleys and so forth, which are far too numerous to even recall, the amphotericin liposomes finally went successfully through the preclinical trials—animal experiments, tox, pathology. And Crossley filed the IND. He argued that it's actually faster to start drug trials in the US, because the FDA [Food and Drug Administration] says that after you file the Investigational New Drug application, you can begin human trials.

COHEN: Ah, so you have already gone through the animal parts when you file.

BALDESCHWIELER: Yes. You can begin within thirty days, unless the FDA says no. The big problem is that applications are often stalled for years in the FDA. But if you feel confident enough in what you are doing, you can just go right ahead. On the other hand, to finish the New Drug Application for approval—that works far faster in Europe. So we chose to begin in the US but then move the human trials abroad to Europe, to the UK, to the Netherlands, to Sweden. So this required filing in all of those countries. And so, step by step, we…

COHEN: Became international.

BALDESCHWIELER: Yes. And so the company became primarily a European company. In Europe you can also sell drugs on what's called a named-patient basis. Even before approval, if a physician has a specific patient who is in critical condition, he can get the drug before approval. In the US that was impossible. We were operating on a shoestring, and these named-patient sales enabled us to generate some revenues even before the approvals were in hand. The drug was a huge success in the clinic. Systemic fungal infections are typically the cause of death in patients who have been over treated with chemotherapeutics for cancer, for example, or in AIDS patients, where the immune system is ineffective, or in organ-transplant patients. We had patients who, after receiving treatment, got up and left the hospital. So in terms of personal reward and satisfaction, in terms of lives saved, this was a hugely gratifying outcome.

COHEN: What is the state of this business right now?

BALDESCHWIELER: Well, there have been all of the usual machinations. As soon as we successfully generated revenues, litigation was brought against us by the competitors. That was a long, agonizing, unusually expensive set of trials. We prevailed. We grew the products. In what must have been 1993 or 1994 our major investor, Warburg Pincus, had been in the field now for well over ten years and they were anxious to realize their investment. The company was public, by the way. Crossley also helped us take the company public. And so Warburg envisioned a merger of Vestar with another company in their portfolio called NeXagen, which was based in Boulder, Colorado. The combination became NeXstar. The concept was that we had a delivery system, the liposomes, and NeXagen was developing molecules that were the

result of combinatorial synthesis. The combination of Vestar and NeXagen lasted for a few years. However, the NeXagen technology didn't work out. The original Vestar products were basically supplying the cash. Finally it was clear that that was not a successful arrangement, and NeXstar was acquired by a Bay Area company, Gilead Sciences. Now our technology provides the principal revenues for Gilead. [Laughter]

COHEN: Are you on its board?

BALDESCHWIELER: No. I chaired the Vestar board, and then was a member of the board of the merged company, NeXstar. With the Gilead acquisition, I was finished with my role. I'm now an interested shareholder. [Laughter]

COHEN: So all of that was really very exciting.

BALDESCHWIELER: Yes, it was really a remarkable time. In the process, of course, I had the opportunity to work with a substantial number of very bright Caltech students. They were a lively inventive bunch. They went to work for the company, but they also envisioned starting things themselves, and during that period we started a significant number of other undertakings. The most interesting was called Combion, and that was the result of Ron Gamble and myself again. Gamble had dropped out of Vestar after the product began to be a significant source of revenue. He was the ultimate inventor. He set up his own shop in Pasadena—it still exists—called Gamble Associates. There was a magic between Gamble and myself—always a very stimulating direction and very inventive.

COHEN: A good chemist.

BALDESCHWIELER: He was a very good chemist. We conceived the idea of using a basic ink-jet printer to produce large combinatorial arrays of, for example, DNA. A company called Affymax had published work on producing DNA and protein chips using a photolithographic fabrication method. We read their paper and thought, "Well, we can do this much better." In the early days of Vestar, we had experimented with ink-jet printing technology. We tried making inks with liposomes carrying a pigment. It was effective. We called this Vestink and we tried to sell it to

Xerox and Hewlett Packard, and made some progress but never succeeded. But we had ink-jet printers in mind. Then it occurred to us that you could use an ink-jet printer to do chemistry. With four different ink-jet heads, you could have one for each of the DNA bases, A, T, G, and C, and then you could synthesize DNA on the scale of an ink jet droplet. We could print out thousands—tens of thousands—of features.

COHEN: Amino acids?

BALDESCHWIELER: Well, either amino acids or DNA bases, so that you could have 1,000 or 10,000 different sequences all on a small chip. [Tape ends]

Begin Tape 6, Side 2

BALDESCHWIELER: We decided that that could be done. It was the same experience of going far and wide trying to find the right people to finance this. We finally got financing from a Bay Area venture capital firm. You know, a new idea is very hard for people to accept. You find the occasional courageous investor. I had a postdoc join me—Tom Theriault. He's from Maine, and the name is of French-Canadian origin. Tom had done his PhD with [Harden] McConnell at Stanford. I asked if he would be interested in carrying this forward. So he did the bulk of the electronic and mechanical developments required—substantial electromechanical invention, which he and Gamble were superb at. So Combion got under way. This actually worked, and the technology was acquired in the late 1990s by Incyte Genomics. That was the basis for their entry into the biochip business. Incyte is also an interesting company, which had its roots in Monsanto. So there are many competing options for those who want to invest in such matters. [Laughter]

COHEN: Is this an attitude, then, of postdocs that come here? Are they looking for something like this?

BALDESCHWIELER: Well, historically, no, of course. Historically Caltech would place more than half of its graduates into academic positions through the route of postdoctoral fellowships. Typically a postdoctoral position was the entree into an academic position at a top research

university. But the climate began to change in the early ’90s, and academic jobs were harder to find. Those opportunities were replaced by the entrepreneurial start-ups, either in new start-ups or in companies the students started themselves. I think that that set of career paths replaced the jobs that were lost from the downsizing of Bell Labs and IBM and the reduction in positions at national labs. These were more than replaced by the opportunities on the start-up side.

COHEN: Of course, there’s much more risk.

BALDESCHWIELER: Yes. Caltech was obligated to respond to these changes and has done so.

COHEN: But it’s taken them a while.

BALDESCHWIELER: Yes, and that has become one of my principal missions—to encourage those changes at the institute.

COHEN: Do you want to talk about that? Or do we have more companies to talk about?

BALDESCHWIELER: Well, there were some more companies. [Laughter] Vestar and Combion were the most successful of those. I of course was very active with Warburg Pincus.

COHEN: You remained a consultant for them?

BALDESCHWIELER: Yes, on their technical advisory board. And I worked as a consultant to a number of other companies in that period.

COHEN: But you still continued your teaching and your labs here?

BALDESCHWIELER: Teaching and research, yes. The attitudes at Caltech toward entrepreneurship have been changing. There is greater comfort with people being involved in helping to start outside ventures.

COHEN: When would you say this occurred? When Everhart [Thomas E. Everhart, Caltech president 1987-1997] came?

BALDESCHWIELER: Everhart certainly was supportive. He helped, for example, to transform the original Caltech patent office into a combination of a patent office and an office of technology transfer. We searched for a person to head that operation. In the old days, the patent office simply filed patents.

COHEN: If you had a patent, you'd go to them and they would do it.

BALDESCHWIELER: Yes. But they took no responsibility for licensing the patent or helping the development. Or going to find somebody to license it or helping someone start a company to make use of it. The Office of Technology Transfer envisioned a person who is much more entrepreneurial and…

COHEN: Now, how did this happen? Did you propose this to Everhart, or did he consider this to be one of his missions?

BALDESCHWIELER: There were several go-arounds on this, and I was certainly part of the force in helping to get this started, but not exclusively. There was a patent and licensing committee which felt that Caltech simply wasn't getting the benefit of intellectual property that was developed here. And also there was a very important legal change, which was the introduction of the Bayh-Dole Act. This act was passed in Congress without a lot of people understanding what it was. [Laughter] It gave title to intellectual property—property that was developed with government grants—to the university or institution where the work was done. Historically anything supported by the Atomic Energy Commission or the NIH or the NSF, for example. Formerly, such intellectual property was the government's, and since it was funded by the government everyone thought it was logical that the government should own it. The only difficulty was that then it wasn't developed, since it was in the public domain. So people didn't patent things; it was all in the public domain. And something in the public domain…

COHEN: Doesn't give any profit.

BALDESCHWIELER: You can't develop, particularly pharmaceuticals, where, in the end you're going to invest $50 million or $100 million. You simply don't undertake that, unless you have some proprietary position—some protection to exploit the benefits of the investment. This seemed backwards to many constituencies, who viewed the Bayh-Dole Act as a huge giveaway of government-developed property; on the other hand, from the standpoint of the public interest, it was exactly the right choice, and the effect has been dramatic.

COHEN: So this was a big thing.

BALDESCHWIELER: This was a huge thing. It passed without a huge brouhaha, and it's a key factor in what we see going on right now. What is developed in the Caltech research lab with government support is Caltech's intellectual property. Caltech is free to license it in any responsible way it sees fit. The government typically gets the use of the technology royalty free. Very often the inventors—our aspiring entrepreneurs—develop something in their lab and then want to create a company. They must first license the technology back from Caltech. Caltech is a major beneficiary, but the technology is typically exclusively licensed to the start-up.

COHEN: Now, could Caltech say that they don't want this person to do this, therefore they're not going to give it up?

BALDESCHWIELER: Yes. The director of the Office of Technology Transfer has fiduciary responsibility for licensing this property, for the benefit of the institute and the public. The director would be obligated to license it to the licensee who would provide the greatest benefit to Caltech—for example, if IBM came along and offered $10 million. Everhart strongly supported creating such an office, and we went through probably 500 résumés and chose Larry [Lawrence] Gilbert as director, who turned out to be an absolutely ideal person in this role. Larry helps people promote the technology they've invented. He caused to happen a significant number of changes in policy. Perhaps the most important was that, in lieu of up-front licensing fees, Caltech would take an equity position in the start-up venture. And so when the entrepreneur goes to license his own technology back from Caltech, there are typically two steps. First, he executes an option to execute an exclusive license. This option costs a nominal amount of

money and gives the entrepreneur a six-month or one-year period to execute an exclusive license. During that period, he has to raise money. Caltech then will agree to execute the license when he's raised a certain sufficient amount of money. Caltech takes a small equity position, and also royalties. But, as was our experience with Vestar, the product you initially invent is unlikely to be the final product. Thus, if the royalties are the only benefit, the institute may not get anything. But by having a share in the ownership of the company, Caltech has benefits from all of the successful products.

COHEN: That was really a huge change for Caltech. And not everybody's happy with this?

BALDESCHWIELER: This was initially viewed as a potential source of conflict. But I think it's worked smoothly. Now there's reasonable comfort with this approach. I should describe one other interesting thing along this pathway, and that's that the family of J. Stanley Johnson provided a substantial gift to Caltech. Johnson was a graduate of Caltech in 1934 in mechanical engineering. His family made a major gift to Caltech for the support of a chair in the teaching of business economics. Horace Gilbert had taught such a course at Caltech for many years. Johnson, along with many of his fellow students of that era, felt that this was the most useful course they had taken at Caltech, and they wanted to see it continued. Horace Gilbert had of course retired. The Division of Humanities and Social Sciences didn't want to make use of the chair. They thought it would be a distraction to the teaching of economic theory. So the engineering division said, "Well, we'll make use of this." They put together the usual kind of committee to decide what to do with such a chair. I was a member of that committee. After sitting through several committee meetings on what to do with this gift, it occurred to me that I could teach such a course, having been through substantial trial in these matters. [Laughter] And so I volunteered to teach a course in entrepreneurship. The committee supported that, as did the curriculum committee. So this led to the creation of E102 in the engineering curriculum. And for the last four years I've been teaching this course.

COHEN: I understand they turn people away.

BALDESCHWIELER: Well, yes. It's turned out to be a very popular course. What I do is go through the things that happen to you when you propose to create a new venture. A lot of students—graduate, undergraduate, and postdocs—have had a great deal of interest. They learn about many practical things, such as stock options.

COHEN: Are you teaching that now? You're emeritus as professor of chemistry, but you're still continuing as J. Stanley Johnson Professor.

BALDESCHWIELER: Yes, I continue with that. And the gift from Stan [R. Stanton] Avery to establish Avery House was also a major factor in the direction of supporting entrepreneurship, and that was clearly Avery's vision.

COHEN: To have different kinds of people living together and to sponsor visiting scholars.

BALDESCHWIELER: Yes, and a place where entrepreneurship is fostered.

COHEN: Is that working?

BALDESCHWIELER: Yes. And Nick [Gaylord E.] Nichols [director of Industrial Relations Center] has been hugely successful with his series of programs, such as the Caltech-MIT enterprise of forums and other entrepreneurial workshops.

COHEN: So then David Baltimore [Caltech president 1997-] coming—I mean, I would imagine he favors all these activities.

BALDESCHWIELER: Baltimore is very supportive. Last year I think Larry Gilbert said that there were twenty-seven companies that came out of Caltech.

COHEN: Just for the year?

BALDESCHWIELER: Just for the year 2000.

COHEN: We're competing with Stanford, at least. [Laughter]

BALDESCHWIELER: Yes, that's right. [Laughter] And we're, what, one-fifth their size?

COHEN: Well, that's a tremendous change in the university-as-ivory-tower concept.

BALDESCHWIELER: Yes. Obviously one has to keep in sight the risks as well as the benefits, and keep all this in some reasonable balance and perspective.

COHEN: So you've come up to today. How about winning the [2000 National Medal of Science]? Do you want to talk about that?

BALDESCHWIELER: Well, this was a complete surprise. I had served on the selection committee for this after my tour of Washington in the seventies, and I had been on hand for a number of the presentation ceremonies. But I hadn't thought about it since. So this came as a complete surprise, I must say.

COHEN: Although I suppose you were not as struck as some people, because you had been in the White House.

BALDESCHWIELER: Yes. But it was a wonderful occasion. It was beautifully done. We had the chance to bring both our families. I should mention that there was a divorce and remarriage along the way, in 1988. I think that the price of my workaholic schedule was ultimately paid in a marriage that wasn't successful. After that I was remarried to a lovely lady whose name is Marlene Konnar. We had known each other at Stanford twenty years earlier. She had gone to Stanford because early in her career she had worked with Paul Flory. When Paul was at the Mellon Institute, she had worked as his scientific grants administrator. He was attracted to Stanford, was on the Stanford faculty, and she moved to Palo Alto with her then husband, who was employed at the Ames Research Labs. Flory heard that she was in the Bay Area and hired her at Stanford.

COHEN: You didn't know her then?

BALDESCHWIELER: I knew her, but only casually. We connected many years later, when I was a consultant at Ford. In my mix of industrial jobs, that was another very, very interesting one, I must say.

COHEN: The one at Ford. You haven't mentioned Ford before. [Laughter]

BALDESCHWIELER: Yes. [Laughter] That was an extraordinarily interesting mission. At Ford I worked with Bill Baker, who is a former director of Bell Labs and vice-president of research for the old AT&T. One of the important outcomes of my Ford work was…. You know, they're putting more and more electronic gadgets under the hoods of automobiles. They can and often do interact with each other, so one needs a systems-architecture view for designing the combined electrical and mechanical parts of an automobile. And it turns out that the Bell Labs connection was critical, because their success has been basically as an architect for the communications system. So we got Ford and AT&T together to work on this.

COHEN: Now, was that when Ford was setting up its research labs?

BALDESCHWIELER: No. Ford had a well-established research lab. That was at the time, again…

COHEN: Because Terry Cole was instrumental in setting up that research lab.

BALDESCHWIELER: Yes, I knew Terry well and he was part of what attracted me to work with Ford. They put together a technical advisory group. At the time, as Terry said, Ford was losing $400 a unit and making it up on volume. We worked with Ford through that whole transition—a great reduction in the number of employees and the introduction of computer-based design. Anyhow, this is complicated, but that brought me to Detroit and Ann Arbor. Marlene, after she ended her marriage, had moved back to Ann Arbor with her sister. We crossed paths again. So, for the Medal of Science, we had a chance to bring our combined families to Washington. We met with the President and had an altogether very good time. I should also say that in this period—I would say the last five years—I also became actively involved in more local economic development matters, initially through what's called the biotechnology corridor.

COHEN: I was going to ask you if you were involved in this corridor.

BALDESCHWIELER: Yes. I was on the committee of the Pasadena City Council to evaluate and recommend the development of that large piece of property for high-tech use. It was an extraordinary experience and, in many ways, far more difficult than working in Washington.

COHEN: Well, there's more people to consider. And there's this proposed extension of the 710 freeway.

BALDESCHWIELER: They're very close. Such a project brings out an incredible array of people who have different positions. It was more difficult than the Washington environment. But that has led to a growing participation in local economic development. The most recent chapter in this part of the story is the formation of something called Pasadena Entretec. This is a not-for-profit corporation that we formed. I found a very talented woman, Stephanie Yanchinski, who is Canadian and had done a similar project in Toronto. What she's doing is creating an organization to encourage…. You see, our companies are coming out of Caltech, but then…

COHEN: Where do they go?

BALDESCHWIELER: Right. The benefit of these ventures staying in Pasadena is enormous—the numbers are startling.

COHEN: So this is the next logical step.

BALDESCHWIELER: Yes. We do what we can to retain these projects in Pasadena, as opposed to having them go to San Diego or the Bay Area. The Entretec organization is housed close to the campus, in one of those little houses on Hill Avenue.

COHEN: How long ago was this formed?

BALDESCHWIELER: Entretec is, I guess, only three or four months old. And then I should mention another thing. As these companies emerge from Caltech, they need funding, and if you

look around, there are not a large number of local venture-capital firms in this area. My vision is that Pasadena can become the Palo Alto of the San Gabriel Valley, [laughter] as it should be, because our institution is a premier drawing card. In my view, it should have happened years ago. And so, with two colleagues, John Glanville and Malcolm Cloyd, we have set up a venture capital fund called The Athenaeum Fund. What we've done is to create a limited partnership. Limited partners are typically high-net-worth individuals in Pasadena who also have an interest in Caltech and in local economic development.

COHEN: Do you go to the Caltech Associates? Are these the people involved?

BALDESCHWIELER: Many Associates are investors in the fund, but not all the investors are Associates. The fund closed in March of last year, so we've been operating just about a year. So we are now in a position to provide funding.

COHEN: I see. Nobody else can participate now? How does this work? What if I had $1 million? Could I come into the fund? No, it's closed.

BALDESCHWIELER: It's now closed. But if we turn out to be good at this—[laughter] which is the purpose of the experiment, in part—then we'll open another fund in, I would guess, another year or so. John Glanville turns out to be quite talented and successful at attracting investment funds. Malcolm Cloyd is a person with a lot of operational experience. And I bring the technical understanding. We've made six investments to date. I should point out that I've retired from Caltech [1999], so this gives me the chance to do more things like this.

COHEN: Yes—although you're still teaching your course.

BALDESCHWIELER: I still teach the course. But I think there could be potential areas of conflict of interest. So as a retiree…

COHEN: You can do what you want. [Laughter]

BALDESCHWIELER: [Laughter] And not do those things I don't wish to do.

COHEN: Well, you're quite amazing. OK. Usually at this point I ask, "How do you feel about having come to Caltech? Was this really good for you?"

BALDESCHWIELER: Oh, in retrospect it was a tremendous move. I could not have possibly anticipated the sequence of events. It turns out that it's given me the chance to do those things I wanted to do in the sense of high-quality basic science but also the opportunity to develop commercial ventures and to play an international role in science. Had I stayed at Stanford, I don't think all of this could have possibly happened.

COHEN: Well, I think Pasadena's a rather special place. I mean, you're very involved with the symphony, I know—or at least your wife is.

BALDESCHWIELER: Yes.

COHEN: And Caltech is small.

BALDESCHWIELER: Yes, and the benefit of that smallness gave me a chance to range over interesting opportunities that would never have occurred in a larger institution.

COHEN: So if you were to say what your largest contribution to all this is, you would have a hard time deciding which one.

BALDESCHWIELER: Well, I like to think that the underlying science is my most important contribution, because that's the foundation on which everything was built. I like to see those creative contributions develop and flower. I like to make things work. I think if you just invent something and walk away from it, that's…. They're like your children, right? [Laughter] You have a real commitment and a continuing interest in the outcome of things.

COHEN: So you still like your attachment here with Caltech? You'll continue with this course?

BALDESCHWIELER: Yes. It's really a platform from which many, many constructive things can be done. And I really enjoy working with the bright people who want to start companies and

who have the necessary level of enthusiasm and talent. That's extraordinary and it's fun. [Tape is turned off]

JOHN D. BALDESCHWIELER

SESSION 6

February 22, 2001

Begin Tape 7, Side 1

BALDESCHWIELER: These will be just a few thoughts that occurred to me after our last session which I hadn't mentioned. One is that I have not talked about family, and I should say a few things here, since it's the great joy of my life. I mentioned my first marriage with Marcia, and we had three children. The oldest is John Eric, the middle is Karen Ann, and the youngest is David Russell. And then I acquired a stepdaughter through my second marriage, to Marlene Konnar. I wanted just to say a few things about what each of these children has done, since it's been really quite extraordinary. Our oldest, Eric, went through Poly and was always quite a space case; he was interested in computer games and mathematics. I recall him coming home once with a report card that had an A, a B, a C, a D, and an F. [Laughter] We said, "This simply could not be an accident. This must be a very bright young man." He went on to Carnegie-Mellon to do a degree in mathematics and computer science. He then worked for several start-up companies in the Bay Area. He decided to take off and take a trip to Europe and learn German, and he spent some time in Germany and then toured in Europe. He benefited from the fact that he has a Swiss passport and was able to find legal work in Switzerland.

COHEN: Now, is this automatic that he has a Swiss passport, because his forebears are Swiss? He was not born in Switzerland.

BALDESCHWIELER: No. Nor was I, but I have Swiss nationality because my father registered me at the time he went to Europe during the Second World War. He made a deal with the Swiss consulate to fix his relationships with Switzerland, so my sister and I were registered. I found this out much later.

COHEN: I see. So if your children are registered Swiss citizens…

BALDESCHWIELER: You're Swiss, yes.

COHEN: To the third generation?

BALDESCHWIELER: Well, as long as you keep it going. I may have said that this is a plot by the Swiss to populate the world with Swiss citizens, but it goes rather slowly, since it's a small country to start with. To renounce your Swiss citizenship some say you actually have to have a psychiatric examination. [Laughter] At any rate, Eric was able to get a position at the ETH, the Swiss Federal Institute of Technology. He did quite well, and met his bride-to-be, Sarah, in the south of France. He then returned to do graduate work at Berkeley. In the course of doing his work, he met the people who were to become the founders of a company called Inktomi. He finished his master's at Berkeley, but instead of carrying on for his PhD, he became a founding software engineer of the Inktomi Corporation. That has turned out to be one of the huge successes. So he married Sarah, and we have a grandson, Thomas James.

COHEN: Did he register him in Switzerland? [Laughter]

BALDESCHWIELER: [Laughter] He hasn't done that yet, but he has until Thomas is twenty-one.

COHEN: And he lives in the Bay Area?

BALDESCHWIELER: He lives in San Francisco.

My daughter, Karen, whom you know, decided that she would do her high school work at the public high school, in large measure because she was interested in music and all of her musical friends were there. She finished there and then went to Harvard and started off as pre-med. She decided after two years that her interests were in English literature, and her record looks like two different people. After graduating from Harvard, she returned, first to teach at Thatcher, in Ojai, and then at the Harvard-Westlake School. She decided, in the process, that she could be more effective in setting up a private tutorial business, which she did in collaboration with two young men from UCLA. She did English and reading and language, one of them did math and physics, and the other did chemistry and biology. They formed a company they called Tungsten, and she worked at that for nearly eight years. She finally decided that she was interested in business and needed to understand a good deal more about what she was doing.

So she applied for and was admitted to Harvard Business School, and she finished that last year and is now the chief financial officer would you believe, of a company called the Kaplan College. Do you remember the Kaplan Corporation? One of their businesses is preparing students for the SATs, preparing those kinds of test materials. She's in mergers and acquisitions. [Laughter] So she's very happily launched on her career, and she'll be involved in acquiring companies with businesses in distance-learning and computer-based teaching.

COHEN: Does she live in New York?

BALDESCHWIELER: Well, she commutes. She wants to live in Boston, and the company's headquartered in New York, so she spends some of the weekdays typically in New York and her weekends in Boston, where she prefers to be.

The youngest, David, had difficulties in high school. He was much more interested in doodling and drawing pictures of dragons and such than doing his Spanish and mathematics homework. [Laughter] It occurred to us that this boy might have some artistic talent. So he went to the College of the Pacific and majored in graphic arts and fine arts. After leaving the College of the Pacific, he went into free-lance artwork in San Francisco. He had all kinds of accounts. He had a pots-and-pans account—somebody has to draw the pictures of pots and pans that you see in newspaper advertising. He designed tomato-can labels and worked for a while for a group of very suspicious-sounding Ukrainians who were selling binoculars and other retail optics. [Laughter] I advised him to be sure he got…

COHEN: Paid in advance? [Laughter]

BALDESCHWIELER: [Laughter] Yes—paid up every week. But then he finally found a job with a company called Macromedia, which is involved in developing computer-graphics programs. He was first the quality-control person, so he had the fun of trying to see if he could make the programs crash by doing various things that artists might like to do. But he eventually became part of the development team for a graphics program called Flash, which is a very important and useful product. So he is now an expert on this particular program. He left the company just

recently and is now working on his own, but doing work for the company. He says he's paid more to do the same work that he did when he was employed. So he's also very enterprising.

My stepdaughter, Devrie, was an undergraduate at the University of Michigan. She did graduate work in physical therapy at the University of Miami, at Coral Gables. And she set up shop in a clinic also in San Francisco. She's married to a fine young man whose name is John Brennan, who is a rising star at the Gap in San Francisco. So that's the family. They're all doing very well.

I was in the Athenaeum the other day and Ned [Edwin S.] Munger said that he thought I was the person who had started more companies than anybody else at Caltech. I had not thought about this, but it may be true. But it occurred to me that I might write down a list. I typically don't put that on my CV, except where I'm on a board or something of that sort. But I went through and I counted ten. These were companies I started myself, or in collaboration with other people but I was involved as a direct founder. And I can say quickly something about each of these.

COHEN: Yes, why don't you? I'm a little confused about what the rules are at the institute that allow you to do this or not to do this.

BALDESCHWIELER: Oh, it's quite clear. There's no problem starting a company and being a founder and owning shares and serving as a consultant or serving on the board of directors, for example. What's not allowed is for a faculty member to take a direct management role in the company. So, for example, I could not be a vice-president of technology or a working manager of any kind and still abide by Caltech's guidelines.

COHEN: I see. But the board of directors would set policy.

BALDESCHWIELER: Yes, but I'm not working for the company every day. A board meets four times a year typically. So that's not a conflict with teaching.

COHEN: OK. Why don't you say something about each of these companies and what led you to do them?

BALDESCHWIELER: Well, each of these is a complicated story, but I'll give you the gist of it. I counted ten. I may have missed some. The first was called Research Systems, Inc. We started that in Cambridge when I was still an assistant professor at Harvard. I did this in collaboration with the person who ran the electronics shop, Richard Volpicelli. We manufactured and sold an attachment for an NMR spectrometer which would enable the spectrometer to carry out a double-resonance experiment. We also got several contracts from NASA to build microwave-based instrumentation to study the contaminants in space capsules. But the company finally expired, since we all had other things to do. [Laughter] It was a fun location, in a garret in an old building just off Harvard Square.

Number two was the start-up called Vestar. And that came out of research based here at Caltech. And part of the actual application of our liposomes in cancer diagnosis and therapy was based on support from the City of Hope. Vestar merged with NeXagen to form NeXstar, which was acquired by Gilead, so our technology and products are still being sold.

The third was called CNS, for Cambridge Neuroscience. And Cambridge Neuroscience came out of my work at Warburg Pincus, the New York-based venture capital firm. They asked at that time what was going to be the next thing that would be as important as recombinant DNA and from which commercial development would evolve. It occurred to me and another colleague on the Warburg board—Howard Goodman—that it was probably some form of neurochemistry or neurophysiology. So we formed a company to work in this area, with Warburg financing it. This was a lot of fun, because the first thing we did was to create a scientific advisory board, and we put on that board all the distinguished neuroscientists we could attract. We sorted out what the best commercial opportunities might be. Then we established the company and hired the CEO.

COHEN: So your contribution was not only the idea for the work, but actually the knowledge of the people that could do it. So you were of great value to Warburg Pincus.

BALDESCHWIELER: Yes. The company became rather large. They hired an extensive team of synthetic chemists and electrophysiologists to study the effects of various small molecules. They found a very active class of compounds and in fact took several of these into clinical trials. Unfortunately, the compound that they chose finally to take through full clinical trials failed—or

at least it did not succeed immediately in its clinical trials, highlighting the risk of this kind of venture. The cost of taking a compound through the clinic is so expensive that you can do only one at a time. And in this case the company ran out of money and Warburg elected not to continue financing it.

The next company is called ExoGene. That was based on technology that came out of Caltech, from the labs of Jay Bailey. He had a clever idea about how to introduce into cells in fermentation the ability to more efficiently metabolize oxygen so that one could run fermenters at higher concentrations themselves and therefore increase substantially the efficiency—and therefore reduce the cost—of producing the fermentation products based on cell culture. That company was done in collaboration with a colleague named Neil Diver, who was a local businessman. Both of us had sons in the usual array of Indian Guides, Boy Scouts, Polytechnic School, and so forth, and he was a very experienced businessperson. So together we did a number of things. He brought the business talent and management talent—he was a very tough and efficient manager—and I tended to the scientific side.

COHEN: And where did you get the money for that?

BALDESCHWIELER: He had to raise money.

COHEN: He did that.

BALDESCHWIELER: Well, we had to do it together. He set it up, but we would pitch these things together. And of course Jay Bailey, who was on the chemical engineering faculty here, was part of that.

COHEN: Now, he left didn't he?

BALDESCHWIELER: Jay Bailey left and is now at ETH—the Swiss Federal Institute of Technology. Now, that company ultimately expired, because the potential customers for the improved cells were people who were using fermentation for various purposes. Typically we could improve the normal strains considerably, but the experienced manufacturers had developed the strains very sensibly themselves, based on classical selection. We could not add a lot of

value to the real commercial strains—which of course you don't get access to when you're doing the early experiments.

COHEN: I see. They would have been patented and it would be part of the secret of the company.

BALDESCHWIELER: That's right. So that one expired. You can see there's a pattern here. [Laughter]

COHEN: I was just going to say that they seem to have a birth, life, and death, so they go the cycle.

BALDESCHWIELER: Yes, that's right, and I think that's more or less consistent with the statistics for such things, which is that the number of successes is typically a small fraction of the number of start-ups.

The next one was called QuanScan, and that was based on research in my lab on scanning-tunneling microscopy. One of my postdocs, Paul West, was a part of the founding group. We built scanning-tunneling microscopes and sold some. We had some Warburg money in this, but not a lot. The instrument business is also very difficult. So Neil Diver, our peripatetic businessman, helped us get this started, but in the end the money ran out, so the technology of the company was sold to a different venture group. And a company was re-created out of essentially the same people. It was called Topometrics, and they moved to the Bay Area. This has been going for the last ten years.

COHEN: So that's been a successful one so far.

BALDESCHWIELER: So this is successful; it's been acquired.

COHEN: That seems to be the name of the game. Someone has to acquire you.

BALDESCHWIELER: Yes. In the instrument business, having only one set of instruments is very precarious, because things change. So one needs to have a large enough array of products to survive the inevitable demise of one or another in the marketplace.

So that gives us five start-ups. The sixth was called Cryopharm, one of the most fun projects of all. It was based on research in my lab by Ray [Raymond P.] Goodrich. Ray, when he was taking his oral exam for the PhD, one of his propositions had to do with the freeze-drying of liposomes—our little synthetic cell-like structures. I believe it was at that exam that it occurred to me, and I'm not sure whether it occurred to others, that his technology for lyophilization—that is, the freeze-drying—might work for living cells.

COHEN: You mean without destroying them?

BALDESCHWIELER: Yes. So that you could freeze-dry, for example, a red blood cell and store it as a dry powder. Then, at a time of your choosing, you can add water, shake, and have the cells come back intact and biologically functional. Ray Goodrich was a superb student—you know, one of those people you see at Caltech and you can't believe how good they are. He was such a person. He was a synthetic organic chemist by training, from Ohio State. So we formed Cryopharm, for "cryo" and "pharmaceutical," and set out to produce freeze-dried red blood cells. We got contract support from the military, because they have to set aside huge amounts of human blood for actions such as the Gulf War. The blood has a shelf life of thirty days and then it's lost. The American Red Cross has problems, because the place where you need blood for transfusions is typically not geographically the place where you can…

COHEN: Store it in the cold?

BALDESCHWIELER: Or where the donors are. There's a mismatch between where you need it and where you can find donors. There's also a mismatch in time. You need it at times when accident rates are high—for example, during the holidays—and that's not the time to get donors. So we envisioned tremendous efficiencies that could be possible by having at least part of the inventory of blood storable for long periods of time. We succeeded at this largely because of

Ray's persistence and skill. Again, Neil Diver was the businessman in charge of making this work.

COHEN: You did that right here in Pasadena?

BALDESCHWIELER: We did this in Pasadena, over on Nina Street. Diver remodeled a set of buildings on Nina which were originally partly a woodshop, and there were some other things there. So we built a clean room and advanced laboratory facilities there, step by step. It has been essentially an incubator for many, many subsequent companies.

COHEN: I see Cryopharm ended in '95. So you sold out to somebody else?

BALDESCHWIELER: Well, no—there's a very interesting outcome here. Warburg, which was providing the funding, decided that the blood business was pretty mundane. Ray had also found a method for virally inactivating blood: that was a small molecule you could add to the blood supply and then irradiate with ultraviolet light and remove all the viral and bacterial pathogens. And this was at a time when the blood supply was under considerable suspicion. Today one still must choose donors with great care and test the blood exhaustively. This was one of those decisions where the board and the founders of the company differed from the objectives of the investor. From the standpoint of Warburg, the blood business looked like it would be an ordinary business, whereas the upside in the viral inactivation appeared to be very large. For a venture investor, a mundane business is not nearly as interesting as one where there's a huge upside. They're prepared to take a larger risk, because their investments are spread over many companies. If you have a large portfolio, then you can take higher risks. A few big successes carry all the investments that are either mundane or that fail. So they forced us to divest the red blood cell business and go with the viral business. The viral business met difficulties in the clinical trial area. At that point, although the science was working well, Warburg decided to abandon further investment.

COHEN: And the other part of the business? Did you sell it off to somebody?

BALDESCHWIELER: That was licensed to a company called Kobe, which is a subsidiary of a Swedish firm, Gambro. Gambro sells the devices for red blood cell purification, blood-handling equipment, and equipment for kidney dialysis. So that one was lost to our competition in this area, whose chemistry was far less efficient. They were able to get access to our patents. Our patents were sold to Baxter Healthcare Corporation. Baxter licensed this to our competition. The competition developed the technology and has become a very large and successful enterprise.

But Goodrich didn't give up. He took a job with Kobe in Denver. He now has a much better chemistry, which is extremely promising.

Combion I think I mentioned. This was based on creating combinatorial arrays of oligonucleotides and proteins using ink-jet technology.

GeneSoft is yet another [start-up]. That is alive and well. It's based on technology that came out of Peter Dervan's group. Peter had discovered a class of small molecules that actually fold into a hairpin and fit into the narrow groove of DNA and bind with sequence specificity, so that the right sequence on this small hairpin-shaped molecule will bind with a specific DNA sequence but not any other. This enables the displacement of protein factors that determine transcription. He could design a small molecule to displace a protein, or a class of proteins, that would bind to DNA; and in that way the transcription or the expression of a gene could be disrupted. These are the keys to the kingdom. Dervan's technology allows you literally to turn on and off the expression of DNA with specificity. This was Peter's idea—in fact his work for twenty years—but he came to me to ask for advice in commercialization, so together we created GeneSoft.

COHEN: Now, does the research continue there?

BALDESCHWIELER: Yes. Patents were appropriately filed. We found financial support from an investor called MPM Bioventures, Boston-based, which is a subsidiary of BB Bioventures, which is based on European money from Switzerland. But part of the benefit of going with this particular investor was that they brought us into contact with a CEO candidate whose name was David Singer. He's the son of Maxine Singer. He's a very talented young man who had finished a tour as president of Affymetrix. And he has been superb. GeneSoft has raised a lot of money

and has beautifully equipped facilities in the Bay Area and is working on the basic Dervan hypothesis of treating diseases such as cancer by…

COHEN: Now, you sit on these boards, of course.

BALDESCHWIELER: I'm on the board.

COHEN: And I suppose Peter Dervan is on the board also.

BALDESCHWIELER: Yes, as well as some very distinguished members of the pharmaceutical and financial industries. So that's a very high-class operation, and it's working well.

COHEN: When you say "working well," does that mean that they actually have a product they are selling? Are they making any money yet?

BALDESCHWIELER: No, not yet, and that's typical in this kind of biotech venture. There is recognizable progress toward taking molecules into the clinic and hence into product launch. Dervan's work suggests a very rational approach to designing effective therapeutics. It's very broad, because the genetic control of many disease states suggests that he should have a very important class of molecules. So that represents a very rational approach to drug design. Among the company hires were people from the pharmaceutical industry, who suggested that they also try, in the spirit of screening molecules, a wide variety of different combinations of pieces of the Dervan chemistry just to see if any of them was active. [Laughter] That also has worked. So that company has discovered some very interesting antibiotics. In a period when resistance to antibiotics is a major factor, this class of molecules is important. These molecules attack the genetic machinery of the pathogen. So in pharmaceutical development you can have both approaches—a rational approach and the empirical screening of large numbers of potentially active molecules. This has become an important element of combinatorial chemistry—where you produce millions of different molecular structures, test them all simultaneously, and from that find the molecules that are active. Historically, of course, many pharmaceutical products have been found empirically. That still works. So the company has both parts to it. That's GeneSoft.

I mentioned the Athenaeum Fund, didn't I? This is a small seed investment fund. Three of us are the general partners.

COHEN: Glanville, you—and who was the other person?

BALDESCHWIELER: Malcolm Cloyd.

COHEN: Is he a local businessperson or is he a science person?

BALDESCHWIELER: No. He's an operating person from the West Side of Los Angeles. He's a Caltech Associate. Our Caltech roots are very much in evidence here.

The last of these is called Pasadena Entretec. And that's a not-for-profit company, the first of that kind that I've…

COHEN: Making it attractive for the companies to stay here.

BALDESCHWIELER: Yes. It's an organization that is basically designed to encourage the companies that we start to stay here.

COHEN: And what is staying here dependent on? Is it dependent on being able to hire people?

BALDESCHWIELER: It's an interesting number of factors, in my view—although this isn't the result of rigorous analysis. It depends on the ability to hire qualified technical people, very often those with computer-programming software skills, and there we do have a pool of talent that's very effective. But also, even more critically, [it depends on] management talent, and there we're lacking. This is the reason that GeneSoft, which I would have loved to have kept here, went to the Bay Area—because David Singer was there. He wanted to stay there, and his joining the firm was contingent on the firm's being there—no contest. So what we're trying to do is create a large enough pool of enterprises here in Pasadena that we achieve what I call a critical mass, which is to say a sufficient number. Because as these enterprises start and wane, if there's a large enough number, employees have confidence that if the firm they're with expires they can

be very easily employed in another one, which is likely to be in a growth phase—particularly the managers. These companies are extraordinarily fragile in their early stages.

COHEN: Is there a specific geographic place in Pasadena where this is happening? Is it that Fair Oaks corridor?

BALDESCHWIELER: Well, that's something we've been trying to make happen. It's had its difficulties. Finally it has won approval from the Pasadena City Council. And we're waiting on developers prepared to build on the land that's available. The largest landholders in this area are the city itself and the Huntington Hospital Trust. Start-ups happen spontaneously wherever the conditions are right, and Pasadena has lots of interesting cubbyholes where things can be started.

So those are ten. I don't know if that's a record at Caltech or not.

COHEN: It sounds like a lot to me.

BALDESCHWIELER: Let's see if there are other things here I should talk about. I've served on a large number of boards, some of which I've mentioned. The scientific advisory boards include Monsanto, Xerox, Warburg Pincus, Ford. More recently I've worked with B. F. Goodrich.

COHEN: Is that an Ohio company?

BALDESCHWIELER: It's in Ohio, yes—Brecksville. My colleague, Jay Henis, involved me with the Gallo winery…. [Tape ends]

Begin Tape 7, Side 2

BALDESCHWIELER: Henis is a person who's come in and out of my career a number of times. And he was asked to serve as the vice-president for research at Gallo wineries. Neither he nor I knew anything about Gallo—nor does anybody else, because it's a…

COHEN: Well, Gallo used to be sort of synonymous with cheap wine.

BALDESCHWIELER: Cheap wine, of course. But it turns out it's a *huge* operation. It's completely family-owned—it's a privately owned company. The first generation is Ernest and Julio, and three from the second generation run the company, and a larger number of sons and daughters are involved. But it's fully integrated. That is, they make glass…

COHEN: They mold their own bottles and things?

BALDESCHWIELER: They mold the glass into bottles. They print the labels. They make the closures. They do everything peripheral. There has not been a lot of technology involved. When you visit, you see a huge number of things that are the result of essentially historical accidents and that you would do very differently. Among these, it turns out, is that Gallo throws away several billion pounds a year of waste product—stems, seeds, skins. And that's full of interesting chemistry. Something present at a part per million is still a huge amount of material. One will find all kinds of interesting natural products in such waste. And Henis has set off on such a quest.

COHEN: And the Gallos are interested in this?

BALDESCHWIELER: Yes. And so some things have happened there.

COHEN: And where is that set-up? Is that up in Napa Valley?

BALDESCHWIELER: It's in Modesto. The Gallos have a number of wine varieties that are very, very high-end. In fact, they own significant property in Napa Valley. Very often these are not clearly labeled as [Gallo wines].

COHEN: You get to go up and taste. [Laughter]

BALDESCHWIELER: Yes, of course.

COHEN: So that's a relatively new venture.

BALDESCHWIELER: Relatively new for me, yes.

COHEN: You're on the scientific board?

BALDESCHWIELER: Scientific advisory group of Gallo.

COHEN: Well, I would guess it's not too hard to get people to look at wineries. [Laughter]

BALDESCHWIELER: From the standpoint of the chemistry, it's extraordinarily complex. There are all kinds of interesting things. For example, the smoky taste in chardonnay—clearly there are small molecules involved. One should be able to extract those components in much more efficient ways.

COHEN: I see. So what the company is looking for is to take ordinary stuff and get the good stuff out of it.

BALDESCHWIELER: Sure. That's one strategy.

COHEN: I mean, you look at things in France…. You know, this half acre is producing the great stuff. The sun hits it in a certain way.

BALDESCHWIELER: There are constraints, of course, and if you simply add things to wine, that's adulteration. But you can blend.

COHEN: It depends what country you're in as to what adulteration is. [Laughter]

BALDESCHWIELER: Yes, that's right. Anyhow, this was fun. And another Henis enterprise was called Orex [Pharmaceuticals], through which he got access to Russian technology for the oral delivery of insulin. And so he put together his usual crew of suspects, which includes me and some other colleagues, particularly David Kipnis, who's at the medical school at Washington University in St. Louis—a very distinguished medical researcher. Orex was based on Russian experiments that involved an oral delivery system for insulin. Oral insulin is potentially a huge

matter. So we tried to reproduce the Russian work but never actually succeeded. Henis was the one who organized that.

COHEN: You mean they had something effective and going and you were not able to reproduce what they did?

BALDESCHWIELER: Yes.

COHEN: Didn't that make you suspicious about what they had?

BALDESCHWIELER: Absolutely. [Laughter] Rigor is critical. But eventually a Russian financial group purchased the assets, so everyone emerged intact from that.

COHEN: Did they really effectively treat their diabetic people with oral insulin?

BALDESCHWIELER: Well, they had done a series of trials, both animal and human. But the trials didn't have the kinds of rigorous controls that we would demand here. Under those constraints, it didn't appear to follow through.

COHEN: Well, nobody has ever boasted about medical care in Russia.

BALDESCHWIELER: No. One starts with a great suspicion. Other boards I've been on—Drug Royalty is an interesting Canadian company. It's in the business of buying royalty streams on pharmaceutical products. For example, if Caltech is receiving royalties on a license, they would offer Caltech what's called the present value of the royalty stream. So if we wanted to build a building, they would give us cash now, in exchange for the future royalties. I'm on the business board there. And there's a small company called Epic, in the Boston area—Epic Therapeutics. There is a wonderful, wonderful caper which was called TechLink, which was based in Boise, Idaho. I got involved on their board of directors through one of our trustees, Steve [Stephen R.] Onderdonk. This was a company whose purpose was to allow aircraft to bleed off the water portion of the waste in airplane toilets. It had to decontaminate and sanitize water so that they

could drop it overboard. And this has, of course, enormous benefits from the standpoint of weight, efficiency, and operating cost.

COHEN: And that's a functioning company?

BALDESCHWIELER: No.

COHEN: It's just an idea?

BALDESCHWIELER: No. This was tried but in the end it did not succeed. And the wonderful end to this story was that there were two regulators, the FDA and the FAA [Federal Aviation Administration]. The FDA finally demanded that we prove that this device would never fail the purification—never, ever fail—and that we would have to get approval from each of the fifty states we flew over, individually, as well as each Indian reservation. [Laughter]

COHEN: I guess they weren't too anxious for that technology.

BALDESCHWIELER: They did not want this to succeed. I have always been amused at how this subject would come up for discussion at the tribal council on an Indian reservation. [Laughter] Anyhow, I cannot get on a commercial aircraft without thinking about TechLink and looking at how the toilet works.

COHEN: Gallo sounds like more fun. [Laughter]

BALDESCHWIELER: We had another wonderful one which was called Land Ray, which was a scheme involving ground-penetrating radar which was being optimized to detect gold in gold mines. I don't know why I get involved in such things, but I do. I do enjoy it. Almost always the technology involves the issue of careful measurement and careful controls. They found gold this way, but the question is, would you have found it anyhow if you simply dug? [Laughter] I think I talked some about our people-detection in Southeast Asia; the dynamics are just the same. You have advocates of such schemes, and it's very easy for the experimentalist to bias an observation in the direction of the hoped-for outcome.

And then there are some not-for-profit enterprises that I do. One is the Huntington Medical Research Institute. I'm on the board.

COHEN: A lot of Caltech people are involved in that.

BALDESCHWIELER: I think it's a very well-run enterprise. And the Institute for Human Virology, which is run by Bob Gallo, who is the person who discovered the AIDS virus.

COHEN: Oh, sure. I'm still on Gallo wine.

BALDESCHWIELER: Yes. Well, there is a connection. Because one of the by-products from the waste of the wine production is a compound called resveritrol, which improves HIV therapy. We call it the Gallo Gallo. [Laughter] We're still under negotiation—that is, for Gallo winery to provide…

COHEN: To provide this waste material—well, to them it's waste material.

BALDESCHWIELER: It's potentially very valuable therapeutic material for the Institute of Human Virology.

COHEN: Wasn't Robert Gallo here a few years ago with the idea of setting up a company?

BALDESCHWIELER: Yes, setting up a research institute. And he did, but in Baltimore—that's the Institute of Human Virology.

COHEN: And is that going on successfully?

BALDESCHWIELER: Yes. They're affiliated with the University of Maryland, the Baltimore campus.

I was in the CIA advisory group. But I don't talk about that very much. [Laughter] But it was interesting. And the Brazil program—did I mention that?

COHEN: The Brazil program?

BALDESCHWIELER: When I was at Stanford, Carl Djerassi, who was the developer of the birth control pill, was involved in setting up a number of international capers. And one of these was a program to bring advanced teaching in chemistry to Brazil. Caltech and Stanford worked jointly with two institutions in Brazil.

COHEN: Who was involved with that at Caltech?

BALDESCHWIELER: Well, George Hammond was an original. Harry Gray was part of this.

COHEN: OK. Of course Aron Kuppermann was from Brazil.

BALDESCHWIELER: Yes. He speaks fluent Portuguese, so he was key. From Stanford, it was Djerassi, Henry Taube, Bill Johnson, and myself.

COHEN: Does that mean you spent time in Brazil setting up this program?

BALDESCHWIELER: Yes. What we did was to hire young postdocs from our groups to go to Brazil as what we called Academy of Sciences Assistant Professors Abroad. They did what an assistant professor does here—that is, they set up a research group and brought in funding, learned Portuguese. Many of them married Brazilians. We provided the support. We would travel back and forth and carry spare parts and equipment and bring things back from there.

COHEN: And now Brazil is not paying any attention to your patenting. [Laughter]

BALDESCHWIELER: Of course not. We spoke about China and the Soviet programs. So that's it. Those are all the things I wrote down. [Tape is turned off]