# JOHN D. ROBERTS
**(1918-2016)**

**INTERVIEWED BY**
**RACHEL PRUD'HOMME**

**February-May, 1985**

Photograph by Chris Tschoegl. Courtesy Caltech's *Engineering & Science*

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## Subject area

Chemistry, nuclear magnetic resonance spectroscopy

## Abstract

Interview in seven sessions, February–May 1985, with John D. Roberts, Institute Professor of Chemistry (now emeritus) in the Division of Chemistry and Chemical Engineering.

Family background, early education, Los Angeles; Caltech open houses in early 1930s. Studies chemistry, UCLA (BA 1941). Graduate work Penn State University with F. Whitmore; return to UCLA, war-related research; theoretical organic chemistry with S. Winstein (PhD 1944). 1945, NRC Fellowship, Harvard; R. B. Woodward. Assistant professorship MIT; recollections of A. Cope, A. A. Morton; L. Pauling's theory of molecular resonance; molecular orbital theory of R. S. Mulliken. Research on carbonium ions, carbon cations with R. Mazur; dispute with S. Winstein. Consultant at DuPont, starting 1950.

Guggenheim, Caltech, 1952; joins chemistry faculty 1953. H. Lucas, L. Pauling, other colleagues. Guggenheim to England. J. H. Sturdivant, V. Schomaker, D. Semenow, G. Whitesides. Election (1956) to NAS; heads

chemistry section; NAS response to W. Shockley and R. Lewontin affairs. NSF chemistry advisory panel (1957-1962); Mohole Seismological Drilling Project; faculty salaries.

Writes *Nuclear Magnetic Resonance* (1959), *Basic Principles of Organic Chemistry* (1964); W. A. Benjamin; collaboration with M. B. Caserio on *Basic Principles of Organic Chemistry;* writes *Modern Organic Chemistry*.

NMR at Caltech; construction of NMR spectrometer lab; carbon-13 experiments; work of F. Wiegert, K. Kanamori; E. Swift, division chairman; H. McConnell; JDR as division chairman (1963-1968); faculty changes, role of H. Gray; construction of Noyes Laboratories; G. Hammond, acting chairman. Recollections of L. DuBridge and R. Bacher. L. Pauling and anti-nuclear movement. J. Baldeschwieler's chairmanship (1973-1978); presidencies of R. Christy (1977-1978) and M. L. Goldberger (1978-1987).

JDR as provost (1980-1983). Caltech administration 1970s and 1980s; R. Vogt as provost. L. E. Hood, biotechnology at Caltech. Computer scientists I. Sutherland, C. Mead. S. Wolfram, Symbolic Manipulation Program. JDR chairs Athenaeum Board; R. Ireland's role in upgrading Athenaeum. JDR's honors and awards.

## Administrative information

### Access

The interview is unrestricted.

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ORAL HISTORY PROJECT

# INTERVIEW WITH JOHN D. ROBERTS

BY RACHEL PRUD'HOMME

PASADENA, CALIFORNIA

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**Interview with John D. Roberts** **by Rachel Prud'homme**

**Pasadena, California**

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| Session 1 | February 22, 1985 |
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**Begin Tape 1, Side 1**

PRUD'HOMME: Where were you born?

ROBERTS: I was born in Los Angeles where the freeways cross. Harbor Freeway and Hollywood Freeway cover my birthplace. [Laughter]

PRUD'HOMME: What was your family like? What were your parents like?

ROBERTS: Well, my parents were both from Peoria, Illinois, although they didn't know each other there. My grandfather, my mother's father, was an immigrant from Germany. I think his family was reasonably well-to-do. He was the oldest son, and they wanted him to be a priest, but he wanted to be a doctor, so he left home. I think they came from Danzig; he was really of Polish extraction. Then he came to Peoria, and I gather was a very successful doctor there, but died of pneumonia quite young. His wife and three children came out here and settled. My father's family, also from Peoria, had been in this country going back to revolutionary days. His father was a real estate developer in that area. And they came out about the same time—drove out, as a matter of fact. It was written up in the San Francisco paper.

I don't know exactly where my mother and father met, but they had a lot in common at that point. My father, when I was born, was a dairy farmer in Puente. He enjoyed farming; my mother did not. Not long after I was born, he went into manufacturing furnaces, and was really quite successful at that until the Depression, which was devastating for him.

PRUD'HOMME : Because nobody would buy furnaces at that point?

ROBERTS: Well, construction dropped. And he had the idea that you paid your bills, and did not declare bankruptcy. He paid off all his people and kept them on as long as he could, and finally the business just collapsed. We lost the home and moved to a much smaller place.

My grandfather owned a ranch in Hemet, and my father used to work out there and come home on most weekends. In the late thirties, he went back to his furnace work. He had a lot of patrons, people who had furnaces that needed to be fixed up or modified or whatever. So he just ran the business out of his garage, and he was successful at it. So in the first twelve years or so of my life, I really had relatively comfortable circumstances—not wealthy by any means, but upper middle-class kind of living, in west-central Los Angeles. I went to local public schools.

PRUD'HOMME: Did you go to public high school there?

ROBERTS: Yes. Los Angeles High School. And that was really quite good. The only cloud on the sky in that period was my mother was chronically ill off and on. She had operations, heart problems, just about everything you can think of, kidney problems. So that was another difficulty during the Depression period. On the other hand, up to that time, things were really going pretty well.

PRUD'HOMME: Were there any other scientists in your family? How were you encouraged in this?

ROBERTS: No, not really. My brother has an active mind, but he is a builder, a true craftsman in wood, widely respected in his field, with a number of wealthy patrons. My mother was very strongly influenced by her father, and I think she would have liked it if I'd been a doctor. And now I have three children who are doctors, so it skipped over a generation but it's still there.

PRUD'HOMME: Did you have any teachers at school who were particularly influential?

ROBERTS: Well, the real turning point was in the sixth grade, when several things happened. I was about ten or eleven when I got scarlet fever, which affected my hearing very seriously. Well, I had a lot of problems, and I think I got much more introspective than I would have been otherwise. There were no electronic hearing aids at that time, and so I took lip-reading classes, which were required in school for people with hearing problems. It wasn't what I wanted to do, but it was a real help. So I had some problems with school work at the time. And I was sent off to the desert for a while to sort of get back to equilibrium. My grandfather at that time—this was just before the Depression—owned a lot of Twentynine Palms, including an inn. So I spent part of a winter out there; and it was a very good experience. Anyway, I remember reading a book in the sixth grade—biographies of some scientists, people like Pasteur and Edison—which I was very impressed by. That led, not much later on, to Paul de Kruif's *Microbe Hunters*, and so on. Those books really turned me on for science. I was very interested in chemistry pretty early, but there wasn't much focus. In junior high school, a really marvelous teacher, named Bess Reed Peacock, taught general science. She was a friend of one of my aunts, who was a school teacher, and somehow we hit it off really well. There was a stockroom in back, and she'd have me help set up demonstrations and stuff like that. That was a very valuable experience, because she trusted me with the stuff, and I could go and look in the drawers and play with things and find out what they were like. I did not have a specially distinguished career in grade schools—I'd say B's. I had no special facility in math. I did have some musical training; I spent a lot of time on the violin, but that wasn't an in thing in those days, although I kept on for about five years.

Then, when I got to high school, in those days, you could take high-school physics. And something like the same thing happened there. There was an older gentleman who taught the physics class. I got along extremely well with him, and he also let me set up his demonstrations and run his laboratory course, and stuff like that. I remember we had an old, crude X-ray tube, and we'd crank that up and look at the bones in our hands. When I think of all the radiation exposure that we had in those days! In the meantime, I did a lot of chemical experimentation at home. In high school we had sort of a storeroom, and were able to go and work with electrostatic generators and all kinds of things. So I used to set up a lot of stuff at home. I set up a nice big Tesla coil, which caused a lot of radio interference problems in the neighborhood. [Laughter]

PRUD'HOMME: So you had real freedom at school, and at home, to experiment, to mess around?

ROBERTS: Yes. As a matter of fact, my mother gave me a cupboard right in the kitchen, where I could store chemicals and carry on experiments. And you know, I didn't do everything that the chem lab manuals said was OK. I used to make fuming nitric acid and such things. I had a cousin, a very unusual person, who in his own way had a big effect on my life and my children's lives, too. He was not a scientist but was interested in all those things. He was a little more on the daredevil side. He and I used to do experiments at his house. On one occasion, we had to be taken off to the doctor. [Laughter] We had an explosion. But I took my own chances, with some of this high-voltage stuff. And you know, I used to come over to Caltech each year.

PRUD'HOMME: Why did you come to Caltech? What was the impetus or reason for that?

ROBERTS: I can't remember the exact years, but I would say probably around 1931 or 1932. This was a great period in American history, when Einstein was here. Einstein was very popular then. I don't think many people realize how much there was in the popular press about Einstein and relativity. I got interested, and everybody did, to find out about the fourth dimension. I remember some guy claimed he could take a loop like this and fasten it on to the table, and he'd do like this, and he could tie knots in it. He claimed that he demonstrated that it went through the fourth dimension. Of course, I didn't believe any of that, but at the time I didn't know enough to be absolutely sure about it. And at the same time, Caltech was putting on open houses.

PRUD'HOMME: Can you describe them for me?

ROBERTS: Oh, yes, I sure can. I was really impressed by that. Whenever Caltech had an open house, I would get my aunt, the school teacher, to drive over and visit her friends in Pasadena, and just let me off and let me stay during the day. The Gates Lab was here then; the Crellin Lab wasn't here.

PRUD'HOMME: Were the open houses for prospective students, or just for anybody who was interested?

ROBERTS: The whole community! Anybody who wanted to come. And people really came in droves. And they had Culbertson Hall, and Gates, and the High-Voltage Lab, which was very special, and Bridge, and I guess some engineering building—I don't remember whether it was Thomas or not. And of course there was astronomy. Fritz Zwicky was talking then about supernovae and so on; I actually saw him one day—I got to know him quite well later on. And Earnest Watson, of course, was doing his liquid air stuff. There were many active areas. For example, I remember there was a big fuss in the papers about a teardrop-shaped streamlined automobile that they had and also about [Irving P.] Krick's weather predictions. The Kellogg Radiation Lab, I remember especially, too, was just getting going.

PRUD'HOMME: You had two Nobel scientists here, too. There was [Carl] Anderson and there was [Robert A.] Millikan. Or didn't that make as much difference?

ROBERTS: Well, physics was a little beyond my comprehension at that point; I was only about thirteen or fourteen. I remember very specially the aromatic smell of the Gates Laboratory. They used to run chemistry demonstrations in the big lab, and those were absolutely fascinating. It was a marvelous thing for a young person to be exposed to that, but it was a tremendous burden on the Caltech staff and students; I never really quite figured out exactly how they got into it or why they gave it up. But it was a great experience, because they really did very advanced things. The chemistry guys put on demonstrations of stuff that you'd seldom see people doing in research, except in isolated kinds of ways. Of course, the Athenaeum had just been built, and that was off limits. My aunt claims that I said, "Well, they won't let me in there now, but some day they'll invite me in." [Laughter]

Anyway, the High-Voltage Lab was a fabulous attraction. What is now the Sloan building then had no windows in it. It was deep down inside—just a great big basement-like room with no upper floors. And they had these big girders up at the top. The floor of this room was filled with all kinds of electrical equipment—enormous transformers, and condensers, and so on, big swooping insulators. It looked like Frankenstein's laboratory. Great transformers topped with big mushroom rings, you know, they used to shoot sparks off of. They used to take you in there in groups. Very dark, they'd shut the door; and you could just barely see some dim lights. Then they'd charge up these things; and they'd start shooting off sparks, or they'd have a "horn gap" where a pair of wires would be close together at the top of the transformer and far apart at the top of the room. They would start an arc at the bottom and it would grow in length and rise to the ceiling. That was really impressive to watch as the arcs got up to the ceiling, and then crack, and disappear. They'd make this crackling noise as they'd go up. And then they'd charge up the condensers and shoot off big sparks, and blow up some blocks of wood, and stuff like that. That was really impressive! So I was very much influenced by that. I have a picture in my mind of meeting Ernest Swift on the upper floor of Gates at that time, because they were running the kind of experiment I know he did with electrochemical stuff. And I remember this older gentleman, and I asked him some questions about what they were doing; and he explained it to me. I'm sure I saw Howard Lucas at that time, too. Those were very nice years.

Now, at the same time, I was studying on my own. I used to get books out of the library. Looking back on it, I could never understand why I used to like to read about chemistry, because that was regarded as the pits by most people. But I learned a lot of chemistry and was specially impressed by [Edwin E.] Slosson's book, *Creative Chemistry*. My brother, who was older, was taking chemistry, and I used to help him with his homework problems. By the time I took chemistry in high school, I was really quite well prepared.

PRUD'HOMME: You must have known almost everything that was taught in the original course.

ROBERTS: I pretty much knew what was going on, yes. They had a science, or chemistry, club, and the president of it, a man named [David] Pressman, who later on got his PhD here and wrote a laboratory book with Howard Lucas. And I remember a lot of the experiments and demonstrations that he used to use—electrochemical kinds of things—when they had these club meetings.

It's funny, though, that in chemistry, I didn't really have the same rapport with the teachers—possibly because I took it later or possibly because I felt above them, I don't know. The physics guy I got along with extremely well. And in chemistry, I did very, very well in the course; but somehow, I wasn't invited to be a lab assistant or to help out. Maybe they just had a different way of doing things.

PRUD'HOMME: Did that make you think that you might want to go into physics, or astronomy, or something else?

ROBERTS: As a matter of fact, astronomy was my first love. And I think that was partly because of the Einstein craze at that time, and what Zwicky was doing, the expanding universe and all that kind of stuff. I read a lot by [Sir Arthur] Eddington and James Jeans, people like that, which I found very impressive. I enjoyed that immensely. I tried to build a workable telescope one time, unsuccessfully. I spent a lot of time actually doing other things—sports, girls. I didn't really get into scientific things as deeply as many young people do, except for my chemistry, electrical experiments and some rather serious butterfly collecting.

PRUD'HOMME: You weren't isolated within a particular field, one way or the other.

ROBERTS: No. Although I really wanted to be an astronomer. But on the other hand, there was no astronomy taught in high school. I didn't have access to a telescope. I remember going up to the Griffith Observatory when it was first opened. And there was also a man named [William Andrews] Clark, who lived on Adams Boulevard and had a private observatory. He had a big walled estate; and he had a refractor up there that was really quite a sophisticated machine. He was an amateur, of course. And we went and looked through that a couple of times. But chemistry seemed to come so much easier. Physics, I recognized pretty early that I probably wasn't going to make much headway with, because I was not really that good at math. My progress in all of my math courses depended largely on who taught them. I always got along best with the most unpopular, difficult teachers.

PRUD'HOMME: You wanted a challenge?

ROBERTS: I think these teachers somehow expected more discipline, and I was responding to what they were expecting of me. A couple of them were very, very good. In particular, there was a woman named Bridge in high school. She was a little short woman, about this high. She expected a lot; and she really got me to deliver. I loved math when I had it from her. When I got to UCLA, I had a mix of good and bad teachers, from my point of view, but once I got to more advanced math, I had all the bad ones. I decided, nonetheless, I was going to try some really advanced math, pretty tough stuff. And they had a great guy teaching it, but boy, he left me in the dust so fast that I realized I wasn't going to be a physicist. So chemistry, as I said, came very, very easily, and that worked out well.

PRUD'HOMME: Why did you pick UCLA?

ROBERTS: Because of my interest in Caltech—I used to talk about it a lot—my mother wanted me to come here. She wrote Millikan a letter. He said, "Send him over. Glad to talk to him." Well, I was so worried about my math, for one thing; and the fact that the tuition was $200 a year. And no women. I just couldn't bring myself to come over here. [Laughter] You know, these smart guys were going to say, "Gee whiz, you don't belong in a place like this." Which I think probably was true at that time. I don't really think I was ready for it; the standards would be too high.

PRUD'HOMME: Probably one's own instincts are correct in that kind of circumstance.

ROBERTS: I don't know. But anyway, UCLA was handy; it was inexpensive. Still, I had to work a lot to earn money that I needed even to go there. UCLA turned out to be really a fabulous choice for me. Anyway, towards the end of high school, 1935-36, the Depression was still on; things were looking a little better, but by then, I was into the automobile age, so that made a big difference. I wanted to have a car, and I had to work.

Oh, I did all kinds of things; mowed lawns, did housework, and delivered papers a lot. And then I worked for Van de Kamp's bakeries finally as a salesman in their stores. It was pretty hard for me as a salesman; people mumbled, and my hearing wasn't awfully good. I could see that $200 a year was going to be tough. The fees at $27 a year or a semester at UCLA were high, but not anything like as high as Caltech. Actually borrowed a fair amount of money from my aunts to make the grade then. Going to UCLA also offered the possibility of my being able to work, at least for a while.

PRUD'HOMME: Can you describe UCLA and the chemistry department?

ROBERTS: Yes. UCLA in that period was ideal for me.

PRUD'HOMME: What made it ideal?

ROBERTS: Well, UCLA had moved to that campus in 1930 or so. At first, they were very much under the thumb of Berkeley. And Berkeley was not happy about seeing UCLA's influence expand. And the one thing that they wanted to preserve for themselves was graduate work. Yet, at the same time, there wasn't much in the way of universities in Southern California and a lot of need for a great one. I commuted; it was twelve miles or so each way. UCLA had a mix of faculty; some who were quite old and not very good, but a few very young people who were extremely good, and had sort of been hired on the promise that well, things are going to change and we're going to have a graduate school. And some of them were to become real world leaders in research. UCLA had some other great teachers, too. So it was very fine from that point of view.

When I went out there, I wasn't absolutely sure what I wanted to do, and so I decided I would try out chemistry. Now, during this period, I was working every night from about seven to about midnight in the bakery, and all day Saturday. When I went in to sign up for freshman chemistry, the only section that was open at that point had a laboratory section on Saturday morning. And I remember going around to the department's chairman telling him, "Well, look, I've got to work; I've got to get into another section. I can't afford it." He said, "Sorry, I can't change you." I said, "Well, I'm a chemistry major." He said, "Oh, if I could get rid of twenty chemistry majors, I'd be much happier. It's Saturday or nothing." [Laughter]

He was a pretty tough old guy. William Conger Morgan. He taught freshman chemistry. He was a terrible teacher! He was sort of an orator. He told the dirtiest jokes in class I've ever heard anybody tell in a large class. He was an antifeminist; he was conservative beyond belief. And in those days, lots of people took freshman chemistry. At the start of the year the room would be overflowing, and he'd say to the people sitting on the window sills, "I'll have you out of the trenches by Christmas." And he meant it and he did. But I was well prepared for that course, and the first semester I really sailed through even though I worked every evening. I got so I could sleep anytime, anywhere and often in class.

PRUD'HOMME: Your energy level must have been extraordinary to do all of those things that you did.

ROBERTS: [Laughter] Well, when I look back on it, I sometimes wonder how I survived. Because they had eight o'clock classes. And I'd be out there at eight o'clock, and get home at twelve-thirty or one o’clock at night.

**Begin Tape 1, Side 2**

ROBERTS: Well, we had old William Conger Morgan. He was chairman of the department. We got off to a bad start. [Laughter] And then a very good thing happened. He gave the lectures; and then they had recitation sections once or twice a week before the laboratory. My laboratory assistant was named Jerome Rubin, who later changed his name to Jerome Vinograd and still later was professor here at Caltech. Vinograd was very good; he expected a lot. I remember him lecturing hot summer afternoons; he always wore a coat, and he used to perspire a lot, but he was good. That made the lab quite enjoyable. It made up for Morgan quite a bit. In the second semester, an older man named [Francis E.] Blacet taught the course; he’s been professor and dean and all kinds of things at UCLA. I was still doing great in the lectures, but the laboratory then took a different turn. We had qualitative analysis, in which they give you unknowns, and I was supremely overconfident. I dashed through that using a book by A. A. Noyes. Anyway, [laughter] I didn’t do too well in in the lab, and I got a B in chemistry for that part of the year. But physics was very, very bad for me; I didn’t do well either in the physics laboratory.

PRUD’HOMME: Was it the math?

ROBERTS: I really didn’t understand a lot of it. The lab wasn’t easy. The experiments never seemed to work. And I was so busy with working and so on that I was having a pretty hard time putting in the time to really learn the physics. I did pretty much the stuff that I was interested in. Humanities were terrible. In philosophy, I had the chancellor, a man named Ernest Carroll Moore. I had always wanted to learn about philosophy and had done some advance reading on it. But Moore was interested in education in Greece in the time of Plato. And that really turned me off of philosophy, I can tell you. [Laughter]

PRUD’HOMME: It took you five years to get your bachelor’s degree. Was that because of your work?

ROBERTS: Well, it was a number of special circumstances. In the second year, when we had quantitative analysis, I remember I had the flu the first week or so. I was overconfident again. We were doing volumetric studies; you titrate an acid with a base and carefully measure with a buret how much of the basic solution is required. I was struggling to try to catch up, and the professor came around. He was looking over my shoulder, and he looked at the numbers in my lab book and said, "You know, I could do better with a graduated cylinder, just pouring it out." [Laughter] Well, he sat down and analyzed what I was doing wrong. And he found out pretty quickly some of the faults in my technique. He was an outstanding teacher—a man named William Crowell, who actually worked for Don Yost, one of the greatest and most original of the Caltech chemists. I got better as I went along. Around near the end of the year, I was doing pretty well in the lab, and I told the professor, "You know, I can do this special experiment much better if I could find a way to keep oxygen out of it. Do you have something that we can use to remove the oxygen completely from the nitrogen atmosphere in the flask?" Using nitrogen out of the tank was not satisfactory; you had to take the oxygen out of the nitrogen; it wasn't that pure. And Crowell said, "Well, you do that with a spiral bubbler with chromous sulfate solution in it." This is a glass tube with spirals coming up the inside. I said, "Do you have one?" He said, "No, but you could go and make one. There's a glassblowing shop downstairs." Well, that got me into glassblowing. I worked like a fiend on that bubbler for the next two weeks, and by God, I learned to make spirals before I could join two pieces of tube together. So I came up with a spiral bubbler almost the last day of the class. It wasn't a very pretty one, but it worked. There wasn't really time to finish the experiment, but the professor came around and he asked if I'd like to work during the summer. I said, "Gosh, I'd really like that." He said, "I'll pay you $10 a month." And so he set me up in my first research job. It wasn't related at all to what I'd done in the lab—he had an idea that he wanted checked out. I remember for the first time in my life that I realized, or he told me, "You can't trust even sodium chloride out of the reagent bottles from the stockroom. That's got to be quite pure and you've got to recrystallize it." So then I had to find out how to crystallize things. And I started to really use the library for the first time for research information.

I remember very clearly my crystallizing the sodium chloride and getting long needles. Sodium chloride is supposed to be in nice little cubes, and I thought, my God, what awful stuff; those long needles are impurities. So that sent me around to some other members of the faculty, and they showed me how the long needles were really sodium chloride that was crystallizing in that form. It was still isotropic. But that was good experience. And I did a lot of electrometric titrations during that summer to try to measure chloride ion in the presence of bromide. I can't say that I made great progress, but I was able to use my glassblowing, and I really got into research. Then Crowell said, "I need an assistant during the lab course. How would you like to do that?" During part of the summer, I had helped some of the assistants make up unknowns and stuff like that. So that gave me the opportunity, and I got out of the night work at Van de Kamps and just worked for them on Saturdays after that.

PRUD'HOMME: That was quite an honor to be asked to be an assistant.

ROBERTS: Yes. What made UCLA so special, for me, was that they didn't have a graduate school. Berkeley wouldn't let them give PhDs. They only gave master's degrees; Jerry Vinograd was a master's candidate, but they needed assistants badly.

PRUD'HOMME: So you got incredible experience, then.

ROBERTS: That's right. And then that next year I was to start organic chemistry. There was an old gentleman out there named G. Ross Robertson, one of the best teachers that I've ever run across. I was ready to start the organic lab course, and he came around and said he'd seen me around in the summer, and could I make some stuff that they wanted to use in the course? I didn't know any organic chemistry at all at that point, but I managed to make the stuff. It was ethyl iodide, which is now regarded as a relatively potent carcinogen, or at least a health hazard. I made a lot of it for him.

And then I got started as Crowell's assistant. Now, Crowell was a man who appeared quiet and reserved. He was a good teacher, but you had the feeling he was sort of detached. Well, when I started teaching his course, we'd sit together on the window sill in the lab, and he would say, "Well, there's Jones over there"—this was after three or four days—"You know, I think he's going to be all right. Now there's Smith; he's got some problems." He really knew the people, he knew them well. But he didn't act like it, and most of the students didn't know.

Organic chemistry was a very mixed experience, because Morgan taught the lecture course, and Robertson taught the lab. Robertson and I got along just fabulously from day one, but Morgan and I did not. William Young, who had been one of Howard Lucas's PhD students, had persuaded Morgan to use Howard's organic text book. But Morgan just skipped the first couple of chapters. He said, "It's not going to do you any good. I don't understand it, you won't understand it. Let's go on with some other stuff." Well, Howard's book was really revolutionary at that time, and it's too bad that we didn't have somebody who could teach it. But the laboratory course was a delight. Robertson wrote the text used in it himself; he was a very practical man. He gave wonderful lab lectures; I got a lot out of that. And then I worked on a research problem for Crowell on osmium, which turned out to be in conjunction with Don Yost. And that was great. I really had a wonderful time in that research project.

PRUD'HOMME: What fascinates me is that you were doing essentially graduate students' work. You were doing research, and, as I understand, publishing?

ROBERTS: That's because the UCLA profs at that time didn't have many graduate students; they took in just about anybody who was interested. And I found that research was the thing I could really do. I wasn't very good in classes, but I could get equipment together and usually made things work. Also, I was a teaching assistant in analytical chemistry and sometimes in organic. Robertson was a special person who helped me out in almost everything that I was interested in doing. And later I was teaching assistant in freshman chemistry. One of the people in freshman chemistry at that time was Jerome Hines, later a Metropolitan Opera star.

PRUD'HOMME: Did he sing his way—?

ROBERTS: Yes, he did. He'd sing in the lab. It was really funny. And George Pimentel, who's a professor at Berkeley now, and I think he's the head of the chemistry part of the Lawrence Livermore Lab, was an undergraduate at that time.

PRUD'HOMME: And you did a publication with Dr. Crowell in 1940?

ROBERTS: Yes. That was while I was an undergraduate. We had three more papers that came out of other undergraduate research. And Caltech was somewhat involved in part of this: About 1939, one of [Linus] Pauling's people came over, a man named Charles Coryell, who later became famous in the atom-bomb stuff—his group discovered the element promethium. Charles was a big, sloppy guy, very friendly, a delightful person.

PRUD'HOMME: How sloppy? What do you mean?

ROBERTS: Well, he tended to talk so fast and he was so full of ideas that nothing ever really came out very clearly for me. He was personally a little bit on the sloppy side. His clothes were rumpled and his tie was always pulled around. But he was inspiring. And during this period, there was another young man in the same class with me, a man named William G. McMillan, who is now a professor at UCLA, and had an interesting career of his own. We were to go in very different directions, and were very different people, but we got along very well together—we both worked in the same lab doing undergraduate research. I was really becoming very interested in organic chemistry even though I was still working for Crowell. But I began to feel after two years of that, I ought to be trying something else. I went around to Robertson and asked about research work, but he said, "You want to work with Bill Young. I can't give you the kind of problem you really ought to have." So I went down to talk to [William Gould] Young, whom I hardly knew, although I'd taken a course from him. Young started talking to me about a problem that he was interested in; he wanted me to pursue that. And it was really scary for me, because it was in an area that I knew almost nothing about. I was going to start in the fall, but in the meantime, Saul Winstein arrived fresh from Harvard before going to be on the staff at Illinois Institute of Technology. Saul was a really great scientist. He got a master's degree with Young at UCLA, and his PhD at Caltech with Lucas. Saul kept on extending the new areas he had started on with Lucas, and he'd been off at Harvard as a postdoc with Paul Bartlett, who was a great physical organic chemist. When Saul came by, he told Bill Young that we just had to finish work on a particular problem. So they picked on me to do it, and it sounded attractive. So I went to work on that. And I really did a master's thesis in a year there.

McMillan and I were doing our course work at the same time, and he was doing research for Young, too. Coryell, who was teaching the P-chem [physical chemistry] lab said, "Well, you guys can do the experiments that everybody else does, or you can do research projects." Well, we did the first experiment, the same that everybody else did, and the rest of what we did were all research projects. And Coryell was fabulous. So we moved out of the P-chem lab and into Young's research lab, which we had to ourselves, and did our P-chem experiments night and day. We had an interesting relationship, because McMillan was a very fine mathematician—I guess mostly a theoretical chemist now—and he was good at electronics and good in the lab, too. I used to gather the equipment and set up the apparatus, and then he would decide what we were going to do. [Laughter] And we did some great things. I still treasure my three notebooks that we filled from that time. [Looking for notebooks] You'll notice something in there that represented a change in my life—I decided somewhere along the line to change my handwriting.

PRUD'HOMME: You're wonderfully neat.

ROBERTS: Well, these notebooks were for show.

PRUD'HOMME: Here you're much more Anglicized.

ROBERTS: Yes, that represents a change. After this first experiment, we went off on our own, and we got two publications out of that. Coryell kept prompting us. I was amazed, you know; I never realized what we could do in thermochemistry—you know, they'd talk about calorimeters and everything that goes with them. Well, Coryell had us using an ordinary thermos bottle as a calorimeter; it was perfectly legitimate for the kind of experiment he had. He said he wanted to know what the heat of formation of sodium dithionite was, and he had a way of working on that. Also, McMillan and I collected a lot of data on a three-component, boiling-point vapor-composition system. We were hoping to find a ternary maximum boiling mixture. There was one part of the composition diagram we couldn't investigate, because we couldn't get one component pure enough. And ever since then, whenever Bill and I get together, we say, "Gee, we should have been able to do that; we might have found something that nobody ever guessed was going to be there."

Charles Coryell was responsible for my first real professional contact with Caltech. During our last days at UCLA, Charles asked Bill and me if we'd like to go over with him and visit Caltech. We said, "Sure!" And it was the first time since the old open-house days that I had been here. Charles was a very good friend of Edwin Buchman, and I was very impressed with Edwin. He had an unusual position here. He had been involved with the first commercial synthesis of vitamin B1 developed at Columbia and had an income from the patent royalties. He had come to Pauling and said he would like to do research and would need no salary, so Pauling got him appointed as a research associate. Edwin had a relaxed attitude about science, which was refreshing. He told us about trying to synthesize cyclobutadiene and he didn't seem to be in a great hurry: "We'll get it someday." Saul Winstein told me that Edwin was the only person he knew who could write a paper, put it in his desk drawer for a year, and then see if he still liked it.

We also met Verner Schomaker, who showed us his electron-diffraction machine, and we had a visit with James Bonner, who was then working on the active principle of what caused avocados to ripen—big jars of avocados with gas passing through them. It was a great visit, and I was impressed with Caltech, both chemically and physically so different from UCLA.

PRUD'HOMME: Your early instincts that UCLA was the place for you must have been accurate, because you had all kinds of opportunities, and the good teachers gave you such responsiblity and such academic freedom that you could do almost anything.

ROBERTS: When I look back on it, I can't see how anybody could have had it better, because I had such a good relationship with the faculty, too. When I was a senior, doing this P-chem and at the same time working on this research problem, around the end of the year Young said, "That's pretty good stuff. We ought to give a paper on that at the ACS meeting in Atlantic City." So he asked me to write up a long abstract. That's the first time in my life when I really had to write something. And when I went to UCLA, one of the things I did not want to take was English. [Laughter] So suddenly it was thrust on me that I was going to have to write something different from lab reports. Here's where Robertson really took over. I'd had some experience with him on reports for research projects that I'd worked on as part of his course. He'd sit at his desk and take an hour or two hours to explain to me what a split infinitive was, and when you hyphenate words. I didn't know any of that. He was a good artist, too, and he spent a lot of time teaching me various tricks of draftsmanship.

PRUD'HOMME: Your technical drawings in these notebooks are absolutely wonderful.

ROBERTS: Well, he spent time showing me how to do it, particularly for publication. I used to take drafts of my papers to him and he was great about finding ways to fix places where the meaning wasn't clear. This was in the spring of 1941, and I was having to think about graduate school or getting a job. The market for people in Los Angeles wasn't very good; and because I had done a lot of research, they kept urging me to go to graduate school. But my grades really weren't very good. I applied to Wisconsin and Penn State, and I was turned down at Wisconsin but admitted to Penn State. A great guy there named Frank Whitmore was a big influence in organic chemistry in that period, and I had some correspondence with him ahead of time. Bill McMillan had been accepted at Columbia, and so, in September of 1941, we went East together. I was going to give this paper at the ACS meeting, and Blacet went with us on the same train. It was my first out-of-California experience. We had a wonderful time going across the country on a slow train; we didn't take the Super Chief or anything like that. We went to New York, and then to Atlantic City. The paper went quite well, and I met Whitmore there; I was surprised he seemed to remember who I was. I don't know how many professors now would recognize a student by name when they had a fairly large group of applicants to deal with. At Penn State, it wasn't easy being away from home; I'd never done that before.

PRUD'HOMME: Did you work directly under Whitmore?

ROBERTS: Well, that's what I went there for. He gave a marvelous paper in Atlantic City when I was there; I remember it to this day—the results, and everything. I did some research on that problem myself later on. I went to talk to Whitmore in his small office. It was jam-packed with chemicals. He kept virtually everything that he used in that room. He died really quite young—I think at fifty-five or fifty-six. He seemed old at the time; but I'm sure that the chemicals had a lot to do with it, some of the things he kept in there.

PRUD'HOMME: The exposure to all of this stuff.

ROBERTS: Yes. It was a smelly room. And he had chemicals on his desk, and papers and books sort of like this. And he'd pull out a slide, he'd write on it, use a pad. And he would take and put in a piece of carbon paper; he gave you the original and he kept the carbon. Every interview. You were going to do this, and so on. I was very interested in his work, but he told me to go out and talk to everybody else, and then come back and talk to him. Well, I went and talked, and they had some outstanding people. One was J. H. Simons, who did pioneering work on fluorocarbons and wanted me to study the diffusion of protons into gases—the kind of thing that became great when ion cyclotron resonance was invented by John Baldeschwieler later on. The

Penn State faculty were interested in me because I'd done a lot of research. But I really wanted to work for Whitmore. So he said, "Well, I have this project here. All the people who work for me have to help index my previous students' work. I want to know all the compounds"—he was setting up a card file on all the compounds he'd made—"Here, I want you to take this thesis and you can go and work up the card file for that." The son of a gun gave me a thesis, about that [2 inches] thick, and it was a thesis testing the usefulness of ozonization for alkene structure determinations where he'd got all kinds of products out. Well, I did it. [Laughter] Then I went back to him and said, "OK, I've done that. Now can I work for you?"

PRUD'HOMME: Was he testing you?

ROBERTS: Oh, I'm sure he was. He said, "You go in the lab, and if you can find a problem that I'm interested in, why, I'll let you do work there." And so I went there and got a bench. His lab was a brand-new lab. Across the one side of it was a natural-product lab where a man named Russell Marker worked—he was the first really great American steroid chemist. He was using Mexican yams and so on to get the steroids out. He had an enormous influence, although he himself was a very strange guy. I never got along with him very well. His students were boiling up vats of these yams that they'd get down in Mexico and extracting steroids out of them with boiling alcohol. He and Whitmore agreed that there wouldn't be any chairs in that lab, or any stools.

PRUD'HOMME: So you couldn't sit down.

ROBERTS: [Laughter] Well, you sat on the floor, or on a table. You couldn't sit on the work benches, because they were oiled stone and oil came off on your pants. But the lab was really well designed for working. There were some really great people in that lab. Whitmore at that time was just getting interested in silicon chemistry. He had two graduate students just starting in silicon chemistry; one of them, Leo Sommer, is now a very well-known guy in the field. Leo worked at the desk opposite me and became a good friend. So I got very interested in silicon chemistry as a result.

They were also making hydrocarbons there for the American Petroleum Institute; the lab got money wherever we could get it. Penn State was very good at fractionating equipment, distillation equipment. And I had the experience in glassblowing that those guys did not have. So I used to make things like glass helices for people, and learned how they put them into fractionating columns. I had a lot of absolutely fantastic experiences. And then a chemical problem came along. There's a very unusual alkene called 1,1-dineopentylethylene, a very unusual structure. And Whitmore had a whole gallon bottle full of this stuff. It's the kind of thing you'd never expect to see. He had a glass-lined reactor down in the basement, a large vessel, in which they used to oxidize commercial diisobutylene and triisobutylene. And from these reactions you'd get out what would be relatively a minor percentage product, but because of the scale you got them in very large amounts. Some of these chemicals were fantastic chemicals. And dineopentylethylene was really an attractive thing, and so I said, "Well, I'm going to find out what happens when you add bromine to it." Whitmore told me that it doesn't behave like an ordinary alkene. Anyway, I started to work on that. It didn't go especially well; but Whitmore taught me the importance of interviewing people and making it very clear to them what you want, and giving them a copy of what you wanted. More important, he demanded monthly reports. And you didn't tell Whitmore that you weren't going to write a monthly report. He was very authoritarian about that, but he went over them with you and told you what he liked and what he didn't like, and he expected them to be typed. I don't think I ever saw him in the lab. When you went to an interview, he was actually in a different building—maybe he came once to the lab where we worked to bring a visitor or something, but he never came to the lab and talked to you. A very close relationship developed between the more senior people in the lab. They were there to teach you, and I had some skills that I could give them, too.

**Begin Tape 2, Side 1**

ROBERTS: Well, winter was coming. [Laughter]

PRUD'HOMME: And you were a boy from Southern California.

ROBERTS: [Laughter] That's right. And I'd never been in winter before. It snowed once when I was in junior school. Anyway, Penn State was tough for me in some ways. I was assigned to be a lab assistant in the home economics course for chemists, which wasn't very inspiring. Furthermore, it was stated in the catalogue in those years that State College had "none of the distractions of metropolitan life." But the students made up for that in many ways. We used to go to a restaurant called The Corner. And there was the man named John G. Aston who taught graduate physical chemists. I remember particularly he was very friendly, although he was a notoriously absent-minded guy. He was alleged to have driven to New York, and then forgot his car and came home on the train and subsequently went back to the train station to go back to New York and bought a *round-trip* ticket—quite a number of things like that. Well, anyway, he used to join us sometimes at The Corner. However, mostly I was eating in the faculty club. I lived farther down the street in a room in a private home. And outside the lab I was pretty lonely. I really loved to listen to music, and I used to go down in the basement and listen to a rickety old record player they had at the faculty club. One of the professors invited me up to his room to hear Dvorak's Fifth Symphony played on the first real hi-fi that I'd ever been exposed to. God, that was quite an experience. So, as much as I could, I used to listen to what classical music there was on the radio; and I was very interested that Eugene List, who had been a friend of mine in grammar school, turned out to be a well-known pianist—partly because of playing for Truman, Stalin, and Churchill at Potsdam. Gene was quite a prodigy and made his debut with the Los Angeles Philharmonic at thirteen. I remember very vividly sitting down to the radio to listen to the New York Philharmonic with Eugene List as soloist. And just before the program started at five minutes to one o'clock, or two o'clock—I've forgotten which—they announced Pearl Harbor. I guess I did hear some of Eugene List that afternoon; it was interspersed with bulletins and we were all very nervous. It seems to me like I'd been at Penn State for four or five years, but this was December 7th—not even anywhere near a semester. And yet I was in research; gosh, everything was just humming along. Then this was injected on top of it. Whitmore didn't really know what the new graduate students were going to do. And I felt pretty much up in the air. So right after Christmas, I went up to the National Organic Chemistry Symposium in Ann Arbor to talk with Bill Young from UCLA, who was going to attend. I wanted to find out what he thought about the future. Gosh, it was a great meeting; I don't think I've missed one since.

PRUD'HOMME: It must have been exciting to be there, especially after feeling so isolated at Penn State.

ROBERTS: Yes. And all the big guns in organic chemistry at that time were giving talks. But of course, the war was on; and nobody knew what was going to happen or even what was happening. Young said he didn't know exactly what I should do, but there was a war research project coming on at UCLA, and if I didn't feel very comfortable at Penn State and I might be drafted—it turned out later on I was 4-F anyway, I could come back in February, and there would be a job for me. So I left after those few months, the first of February, and went back. And yet it seemed like I'd spent a long time at Penn State. I made a lot of good friends; and Whitmore was very generous about the whole thing; and he was a good friend for some years after that, until he died.

So I got back to UCLA, and started on the war project.

PRUD'HOMME: What was the project?

ROBERTS: There were several projects starting up. The chemistry building at UCLA had a big sub-basement, and we had labs down there with one or two hoods per lab—not much ventilation. We had a room with plywood doors so we could leave the main door open but you couldn't see inside.

They wanted us to work on the extraction of oxygen from the air. They could see this was going to be a big problem in the field. And they also wanted to put it on bombers so that they wouldn't be carrying highly compressed gas cylinders that exploded when they were hit by machine-gun bullets and stuff like that. This was hardly expected to be a terribly secret thing, because the main chemistry had been discovered by a Japanese working in a German lab. Melvin Calvin, the photosynthesis man, was very important in this project. They were carrying on an effort at Berkeley. I guess we were primarily making compounds for them, the inorganic chemists and all that, this kind of cobalt complex compound is very common now, but not so much then. The principal compound we studied was very interesting, a light-brown substance that absorbed oxygen and turned black; if you let it stand in the air, then you could heat it, and oxygen would come off. Then it could be cooled back down and the cycle repeated. And you didn't have to get it very hot to get the oxygen off; you could just heat it in the steam bath. So you could oxygenate and deoxygenate it. And the question was: Can you make something that will work faster—it wasn't terribly fast—and then how can you change the oxygen pressure over the stuff? And more than anything, how can you improve the chemical stability, because every time oxygen comes on and off, some irreversible reaction takes place. At that time, we didn't know about what is called singlet oxygen, which is much more reactive than ordinary oxygen. Some other people there were working on photochemical processes, which people later found involved singlet oxygen; if particular hydrocarbons were irradiated in the presence of light, they'd add oxygen. So some were working on that, and we were working on making compounds for Calvin's group that might prove to have the right kind of properties. We were operating wholly in the dark. There was a very bright young guy in charge of our group named [T. A.] Geissman, who was very good in synthesis and had worked for Roger Adams at Illinois as a postdoctoral fellow. Four of us worked down there in the relatively small lab with not much ventilation, and we had to make some pretty horrendous compounds. One of us was a graduate student, a romantic Russian named Tulagin. Another, Irving Webb, had just gotten his bachelor's degree. Then they got a PhD from Caltech whose name was Maury [Maurice J.] Schlatter, who'd worked with Edwin Buchman on a compound called cyclopropene. Cyclopropene was a very, very interesting compound. It had first been made in the Soviet Union, but nobody really believed the work was correct. Maury Schlatter showed that the work was correct; he also had a lot of synthetic experience. So he came over and was sort of the straw boss over the other three of us. Maury had been well trained by Edwin Buchman and I really learned advanced organic laboratory technique from him. I already knew something about distillation, but I never really had done much crystallization, or worked with small amounts of valuable compounds. And Maury had the techniques, the centrifuges, everything. It was a seventh heaven for me, because Maury was also a good glassblower, and between us, we did a lot that I really profited from. In other respects, it was a crazy mix of people.

I don't think we accomplished anything really great for the war effort. We did get involved in one strange project, which Young brought down to us. He said, "I've got a secret project I want you guys to work on for a couple weeks." We were not to tell anybody. Later on, they brought in a bunch of cartons. In them were oblong black things, about three inches long, with little grooves in them, made of plastic. In another box were things that looked like little pencils, about two-and-a-half inches long and about pencil diameter. And then a box of little clips. We had to take the pencils, glue them in the grooves in the little black things, and attach a piece of string and put a clip on the end. [Laughter] Of course, our minds were working around, and we'd look at these things, trying to figure out what they were. You could look in the end of the little pencil-like thing and see a little blue color in there, the color of a strike-anywhere Ohio match head. And somewhere else you could see there was a hole with a spring inside. Well, we did the job. In the process, one of these black things broke, and it had an awfully strong gasoline smell. Well, it turned out that the filler was napalm—jellied gasoline—and these were incendiary bombs that we were assembling.

PRUD'HOMME: In the middle of your chemistry lab.

ROBERTS: Yes! They wanted somebody to do them. And the guy who came and got this was the biggest surprise of all. He was Professor Louis Fieser, from Harvard, who was one of the great chemists of that age. There's a little pile of books by Fieser & Fieser up there on my shelves; he and his wife were chemists. This was my first experience really getting close to what might be called a mainline guy. When he gave us a seminar, he came in an overcoat with a great fur collar, carrying a gold-headed cane. He used to carry a little sterling silver box—he was a heavy smoker, who died later from lung cancer—and he'd stuff his cigarette butts in there.

Anyway, it was only much later that we learned what this was all about. Some mad genius had gotten to Roosevelt with a plan to burn down the Japanese cities using these little incendiary bombs; he was going to fasten them onto bats. They were going to get bats down in Carlsbad Caverns and cause them to hibernate by cooling them down, then pack them in boxes and fasten the clips onto them. They were going to parachute the boxes from a bomber, and as they came down, the box would open, and the bats would get warm, come out of hibernation and go and scurry under the houses and everything. Quite a plan. Well, the fuse on these things was something I never would have let anybody take up in an airplane; the fuse involved a spring mechanism with a needle on it. The blue thing we saw was indeed a match head. They were going to inject copper sulfate solution into the "pencils" that would corrode an iron wire that was holding the spring back, and that spring was going to drive the needle into the match head and cause ignition. Well, imagine going over Tokyo loaded up with a bunch of those things, worried whether the fire was going to start in the plane before you let the payload go. And what if you had to turn around and go back? That never got very far, but in the process of demonstrating this, Young and some of his people burned down an abandoned airport. [Laughter]

As the oxygenation compounds projects wore on, they began to look rather promising.

Geissman was sent off to Philadelphia to a laboratory where they were actually going to do the engineering, with the parent compound. Although Berkeley people had found some better compounds, they were not easy to make.

Then I moved into a different, less happy phase, in which I analyzed the samples. They'd send me bottles and bottles of the stuff, and I was supposed to find out how much oxygen there was. I'd had all this analytical experience. They were sending the samples before Geissman went, and he moved me up to his lab. He was very, very smart, but a tough guy to work with and we didn't get along too well. I'd open the windows in the morning and he'd close them. His idea was that there is always enough oxygen in a room for people to survive. After he went off to Philadelphia, he got very heavily contaminated with this stuff in an explosion in one of the labs, and he almost died. Further, he had serious health problems for the rest of his life.

PRUD'HOMME: You were married in 1942.

ROBERTS: Yes. My wife and I went to Los Angeles High School at the same time, but I didn't know her until we finally became acquainted in senior year and became very friendly. We graduated in February 1936. When I started UCLA, she went to Berkeley, and she was there for about a year and a half. Then she had to help support her family, so she went into the insurance business. We decided finally to get married when I came back from the East, in July of 1942. We lived out in Westwood, and she used to ride the bus to work downtown. But we had a lovely apartment, paying all of $50 a month, right on Hilgard, just below UCLA. I made $800 a year, I think. But between us, we made enough. The earlier famous "attack" on Los Angeles, with all the firing of anti-aircraft guns [laughter] had caused concern about the possibility of raids on the city and they recruited me as a gas warden, which meant that I had a gas mask and a helmet, and that's all—no real instructions about what I was supposed do. I was supposed to go up in the blackout to UCLA whenever there was an air-raid warning and wait for something to happen.

If I had to work in the lab at night, sometimes my wife would come to the lab and fix dinner and so on. It worked out very, very well. We almost went broke; we were married for about two months, and I guess our parents couldn't figure out why we came around so often for dinner. We were down to our last dollar; but we turned the corner.

**JOHN D. ROBERTS**

**SESSION 2**

**February 28, 1985**

**Begin Tape 2, Side 2**

ROBERTS: I think what made this period a fruitful one, in addition to all I was learning about lab technique from Dr. Schlatter, was that I was also getting a quite marvelous education in theoretical organic chemistry from Professor Saul Winstein. Now, Winstein, as I told you before, got a master's degree at UCLA, where he worked with Young. Later he came over here to Caltech and got a PhD with Howard Lucas, and then went to Harvard. He came back to UCLA just as I graduated, starting as an instructor. And, of course, I had worked on his project that wound up as a paper at the ACS meeting. But during the war, Winstein was principal investigator on a project, synthesizing anti-malarial drugs, but he was more interested in physical organic chemistry. He didn't have anybody to talk to, really. Young was very busy with the war projects and also was chairman of the department. So Winstein used to come down, when he had read something interesting and wanted to talk about it, and bounce it off of me. And it was a great experience. He was a very unusual scientist.

PRUD'HOMME: Unusual in what sense?

ROBERTS: He was a very deep thinker, very meticulous, and he had a sort of bulldog way of wanting to know the truth, very outspoken. At his seminars, he really wanted to know everything before the speaker went on. I learned from him not to let people snow you, and then later try to catch up. Winstein always wanted to get to the roots of a subject when somebody was telling him something, and he always wanted to be sure that he kept himself up to speed all the time. He was an excellent chemist, and I think he probably would have won a Nobel Prize if he'd lived.

When they finally dropped the war project, I started to do graduate work. I had a super set-up, because I was allowed to do my graduate work where I'd done my war research. For the graduate research I was to do, we needed a special fractionating column to separate the materials we had, which gave me my first real experience in building apparatus. My father built the iron frame for it, and I did the wiring and most of the glassblowing. And then we purchased a column commercially; it cost all of $200, and believe me, that was a big sum in those days; that didn't come easily, it was a good fraction of the department's research budget for the year.

PRUD'HOMME: What kind of column was it that you bought?

ROBERTS: It was a fractional distillation column. It is shown in this photograph in my thesis and for that year was quite a fancy piece of apparatus; it had a lot of features that no other columns had then. It was very automatic. I set it up so that it would run unattended, and it had automatic things for determining the throughput of the column and the takeoff rate. Anyway, the column was really a great help to me, and it took all summer to get it together and get it working. My thesis problem turned out to be a really golden problem. It involved rather simple compounds. Young had done a lot of work in the past, and everybody thought they knew what would happen when I started work on the reactions I was doing. There was some evidence from S. J. Cristol, a MS at UCLA earlier under Young, that I would just be bolstering existing theory, but then, as it turned out, everything came out differently. They'd been getting mixtures of products out of the reagent I was studying before, and they decided they pretty much knew that the reagent was a mixture and that it would always give mixtures of products. And then when we started getting single products out of almost everything, it was very exciting, and very hard to reconcile with what was already known. But I had so much experience from that war project and had learned so many techniques, and also had the lab all set up, that I was able to do all my thesis work in about eleven months.

PRUD'HOMME: Yes, because you got your PhD in '44.

ROBERTS: Right. My wife had to leave for work very early, so I got up early in the morning. I'd be at the lab at seven-thirty, before the other guys got there. And by nine o'clock, by the time they came in, I'd have three or four reactions going at once. I couldn't really be interrupted. So for that period and for those reactions, I think we made some rather startling discoveries. I got about seven or eight papers out of that work finally.

PRUD'HOMME: That's pretty extraordinary, just for a graduate student.

ROBERTS: Well, I had a student here, now a very fine chemist at DuPont, who wrote fifteen papers out of two years and nine months of work. But he worked here in a very exceptional period.

A wonderful part of my PhD period was that Winstein and I spent a lot of time talking about my thesis work and current physical organic research. One problem I had was that I hadn't taken any graduate courses because I was working on war projects. The dean called me over after I had applied for a PhD exam date, and said, "Hey, you can't get a PhD without courses." And I said, "Well, look, I've spent time; I'm almost done; it's too late." So he said, "Well, isn't there anything that we can put down for you?" [Laughter] I said, "No, I don't think so." I don't know what they finally did. They were very concerned about appearances, because they had just started their PhD program. Actually, the discussions with Saul were the equivalent of many courses.

So I got done—I graduated in the fall of 1944. Then they put me into teaching a navy course as an instructor; the navy was still around taking courses, and I taught analytical chemistry.

PRUD'HOMME: Did you like teaching?

ROBERTS: Oh, yes. It was a real struggle for me, though. I needed a hearing aid; and you know, in those days a hearing aid was about the size of a present-day transistor radio. I didn't like it much, but it worked, and that helped a lot. Then, toward the end of the war, we began to get some visitors, and I enjoyed meeting some people I'd heard about, particularly Paul Bartlett, who was a professor at Harvard.

PRUD'HOMME: Can you describe him for me?

ROBERTS: Bartlett had been one of [James B.] Conant's students at Harvard. And Bartlett set a new style in this country for physical organic chemists. Physical organic chemistry was going big in Britain, particularly with the mechanistic work of [Sir Christopher] Ingold and [E. D.] Hughes. But they were working on compounds that you could get off the shelf. They never made anything special; they did not utilize the special characteristic of organic chemistry, which allows you to tailor-make molecules to prove particular kinds of concepts. Bartlett started doing that in the late thirties, and he really made a major breakthrough with a particular class of compounds, which I and many, many other people exploited, called bridgehead compounds, by which you can make a molecule and you can locate a group in a particular place, and you have a very known sort of spatial arrangement; the molecule's rigid, not floppy, and so on. Bartlett proved that if you took that group off, the reactivity was very low because the molecule was constructed so that it couldn't flatten out. The bonds were so arranged to hold it pretty rigidly; it took too much energy to get it into the nearly planar configuration, which was expected to be most stable when you took that group off. Bartlett's work was the start of a new style and fitted wonderfully with what he always liked to say he really wanted to see, factors of a million; that he didn't care about factors of 10 percent or so, which many scientists were satisfied to work with. Bartlett was really just getting going then, and he was quite a figure. I remember I talked to him about the possibility of coming to work with him, and I guess he talked to Young about it. I decided I would apply for the National Research Council Fellowship, which at that time was a pretty special fellowship. I don't know just where the money came from, but there weren't many of them. A lot of people were coming back from the war, but they weren't quite oriented yet, and so I was in a favorable position by finishing my PhD when I did.

At the same time, while I was doing this instructorship at UCLA—well, I don't know whether Young really liked it or not, because I was supervising some of his people, but I started my own research program. I'd been reading a very special, almost encyclopedic, book full of odd facts by Whitmore, with whom I'd worked at Penn State. And his statements about cyclopropane derivatives struck my fancy; they seemed to relate to what I'd done earlier with Winstein and Young, and work that I'd done for my PhD thesis. So I decided I'd work on cyclopropane compounds, and particularly on cyclopropyl chloride, which had been made by the Soviets a long time ago, but almost nothing was known about it. I built a really fancy Rube Goldberg device to make that stuff. So I was helping some of Young's students, and writing on my thesis, and the cyclopropane work went really very well. I decided that I would put that in my National Research Council Fellowship proposal, because it was a competitive fellowship. Then, Bartlett, who was on the committee, told me later that the committee didn't think the proposed work was likely to be very interesting. [Laughter] But they were persuaded that I had done a lot of research and a lot of papers and everything by then. So I probably would be a good risk. And I got the fellowship.

Another opportunity I passed up was with the US Rubber Company—that was in the spring of 1945, when I was still at UCLA, and before I got the National Research Council Fellowship. There were, in fact, a couple of industrial interviews that I was interested in. One was at DuPont, which was the big chemical company in the United States at that time. The job was in organic chemistry and I thought the DuPont interview went well. However, a couple of days later, Young came down and said, "You know, you didn't make a very good impression on the interviewer. He just didn't think that you had the right attitude." And I said, "Well, I asked him about how DuPont operates. Do you work on a strict nine-to-five schedule, or can you work at night? What's it like on weekends?" He said, "Well, I guess that wasn't the right thing to say."

PRUD'HOMME: You didn't have the appropriate corporate attitude.

ROBERTS: That's right, I did not. But the US Rubber Company was different. At that period, they had started a basic research program and had hired three really good people, F. R. Mayo, C. Walling, and M. Mathison, to set up a group on polymerization. They were really doing superb work. Young was a consultant to them, and they were really interested in me. So I did make a trip back east to interview US Rubber that spring. I think that at the same time I went to the ACS meeting, because I remember talking to Bartlett about the offer they made. That's when he told me that I was likely to get the National Research Council fellowship. The US Rubber Company was really impressive; they had an old ratty lab, but the spirit was great. They offered me a very good job, but fortunately, I got the fellowship, and I went to Harvard. My wife and I left for Cambridge in September of '45.

PRUD'HOMME: Except for the Penn State period, you had never worked away from Los Angeles, actually. The move to Cambridge must have been quite a difficult move.

ROBERTS: Well, no, I think we were ready at that point. It was a great idea, and this was the perfect time to go to Harvard—just like when I was an undergraduate, it was the perfect time to go to UCLA. At that time, however, with so many people coming back to Harvard, housing was unbelievably bad. We stayed in the Brattle Inn, which was right at Harvard Square, for a few days. Harvard had a system of giving out a list of available housing every afternoon at five o'clock. Everybody would grab the list, and go tearing off to see these places, and if you didn't have a car, when you got there, there'd be a line of people going in and coming out. There'd be a toaster in the closet for a kitchen and so on. My wife finally went out and rang doorbells, and found a place that was just awful, but it was convenient. Then she finally got on the waiting list for Harvard housing.

We'd had all our earthly possessions in a great big steamer trunk; and the steamer trunk got lost. [Laughter] We moved in to Harvard around Christmas time or a little after. I remember it was really getting cold. And finally she found the steamer trunk herself in a Boston freight yard.

PRUD'HOMME: She sounds like a persistent lady.

ROBERTS: She's very persistent, and she's very organized. She really knows what she's doing. Anyway, she got all that worked out. And then she went to work in an insurance company down in Boston for one of the general partners of a big firm. And that was a good move for her.

Well, Harvard at that time was just getting over the war. Bartlett had been working on the DDT, nitrogen mustards, insect repellants, and several other projects. Many people were just leaving, finishing up their work. I got a lot of equipment from them, and Bartlett assigned me to a lab right across the hail from his office. And so it was easy to get set up. The graduate students were an unusual group, a lot of the big names now in organic chemistry. Of course, many of the older group were just getting their PhDs then, or they were graduate students, or postdoc fellows.

PRUD'HOMME: Because you had this great flood of people coming back from the war.

ROBERTS: Yes, or from war projects, or whatever. It was a very academically oriented group and hopeful, because many academic jobs were becoming available at that time. The youngest member of the Harvard faculty in organic chemistry was Robert B. Woodward, who later [1965] got a Nobel Prize in chemistry. Now Woodward was a true genius in all kinds of ways. Of all the people I've seen in chemistry, I would say that Woodward, Pauling, and maybe one or two others were in the genius category. And Woodward was certainly a genius; he even acted the part, a really interesting person.

PRUD'HOMME: How did he act the part?

ROBERTS: Well, he liked being an oracle. He liked being consulted. He liked to pontificate. He always wore blue—blue coats, blue ties; at this time he used to wear blue sport coats. Later in life, he always wore a dark-blue suit and a light-blue tie and a white shirt. You never saw him in anything else. He even had his office painted with a ceiling like a dark-blue sky. He had a blue car, and his students went and painted his parking space blue. Woodward had been a child prodigy and went to MIT very young. I remember a plaque in the chemistry building at MIT that said he'd won a prize as the best freshman in chemistry in 1936 or so. He was a year older than I was. And then Woodward promptly flunked out of MIT. Woodward didn't like to take advice from people. He learned a lot, but most of it he learned by reading. He started working, doing research for somebody in food technology, and they were very impressed with him. The MIT people in chemistry heard about him, and they knew he'd been this prizewinner as a freshman chemist. So they worked out some kind of an arrangement for him to take all the courses by examination. He didn't do very well, but he passed, and they gave him a BS degree. Then he did a PhD there, basically a PhD on his own. It wasn't an earth-shaking thing, but it was a remarkable thing for somebody to do in that kind of situation.

PRUD'HOMME: Sounds rather like you.

ROBERTS: Well, he was much more of a loner in the way that he functioned. I lived on guys like Young and Winstein and so on, and I got a lot of help from them. Woodward did almost everything by himself.

Just before we came to Harvard, he and a man named [William von Eggers] Doering made the first synthesis of quinine. It was written up in *Life*, several pages, showing them working. So Woodward and Doering were already celebrities, but Doering had gone off to Columbia before I came. Woodward was assistant professor, and some time in 1945 or '46, Woodward was promoted to tenure; that's the last person promoted to tenure from below at Harvard in organic chemistry. It's been forty years now. They have never found anyone that lived up to Woodward. [Laughter]

PRUD'HOMME: That's extraordinary!

ROBERTS: It is extraordinary. Harvard has always have gone out and hired the established people; they took one of our people last year. Harvard has a real presence in organic chemistry; they've been recognized as being probably the top in research. We've been the top in graduate teaching for quite a while. But Harvard was extraordinary. And, at that time, it was way out ahead of everybody else; organic chemistry wasn't much here in 1945, except for Lucas and [Carl] Niemann.

Anyway, the young postdocs used to come and cluster and listen to Woodward explain and pontificate. And sometimes he was right, sometimes he was wrong; but he was always interesting. He had an extraordinary way of lecturing, which many have tried to copy and never quite succeeded. He drew every organic structure out on the board; he never abbreviated. And he'd do it very precisely and very slowly. You just couldn't forget, because you saw that done so many times in the course of a single lecture. As part of a course, he gave a remarkable series of lectures on penicillin. I think he only gave courses for a very few years and then he never taught again.

PRUD'HOMME: Sounds like he was a superb teacher.

ROBERTS: Well, he was. But he was not eager to be involved in a regular course schedule. Harvard gave him a research professorship finally. Everybody wanted to hire him away. The Swiss tried particularly hard to get him. He always said, "Well, I will listen to any offer." But he never really listened; it was sort of an ego thing.

Woodward had had a very brief and bad period at Illinois after he got his PhD and before he came to Harvard. He loved to play poker, and the guys at Illinois got the feeling that he wasn't paying his debts. They didn't quite accuse him of cheating, but they certainly accused him of being a welsher. Most of the Illinois crowd really put him down; they didn't like the way that he was so good and so arrogant. When Harvard got him, he had really left a bad taste at Illinois that caused some problems for him later on. As he succeeded at Harvard, the Illinois crew got more and more alienated. I was sort of in the middle; I was a good friend of Woodward's, and a good friend of Roger Adams's, the chairman at Illinois, and many of the Illinois guys, and so I'd hear about it from other sides. It is important that Adams himself admired Woodward's chemical talents, but there were others who could say nothing good about him.

Woodward didn't talk about it much. He stuck mostly to chemistry. The thing I think I got especially from Woodward was to look for unconventional solutions. Woodward would take any problem; he loved games and things that kept his mind at work, and he'd try to find unconventional solutions. And that I learned a lot from. That really fit in very well later on.

PRUD'HOMME: Not to get locked into a predictable…

ROBERTS: That's right. Always look for an unconventional outcome. I extended that myself, to trying to work on things I didn't understand. Instead of trying to prove that something was true, I would try to find something that I didn't understand and work on that, because if you think about something you don't understand enough, and you don't see any answers at all, that I've found has always been the best thing for me to work on. Anyway, that philosophy was developed at Harvard, that idea for choosing research problems. So I worked across from Bartlett's office on my cyclopropane compounds. Bartlett hadn't yet gotten back into regular consulting for industry and the government. I used to have lunch with him almost every day. He would pontificate in his own way, but a much different kind of way. Bartlett was much more modest, a more human person than Woodward.

It was an extraordinary period. We used to go and play football in a courtyard. Sometimes Louis Fieser would come by and drop his gold-headed cane and take off his fur coat and play football with us. Bartlett went skiing with us and things like that. Woodward was going with a very attractive young woman in that period whom we all liked and he later married. I remember we went skiing with them locally. Nobody ever believed that Woodward would be willing to do any physical exercise. He was sort of like Alexander Woollcott or whoever it was; you know, that business about, "Well, I would lie down and wait until the feeling went away." However on this one occasion, he we did go skiing to a small local area. Woodward was a heavy smoker, and we went up this rope tow, up a rather steep hill. I'd had very little experience then in skiing, but I rolled and slithered down and came back up a few times. And there was Woodward at the top of the hill, surrounded by cigarette butts, trying to make up his mind to go down. Well, his girlfriend was a good skiier, but she had gone all the way down, then fell and cut her knee on a piece of log that was in the snow. So I had to go up and tell him. I said, "You know, Bob, you have to get down there and get her to the doctor. She's going to have to have some stitches or something in her knee." [Laughter] Well, he put both skis together, started down, fell, rolled over a couple of times, got himself back up, and then schussed all the way to the bottom. And as far as I know, he was never on skis again; but it was a great performance.

PRUD'HOMME: Who were some of your other colleagues and friends there?

ROBERTS: Well, there was a man named Elliot Alexander, who later became a professor at Illinois. He and I were in the same lab together. He was a very brash, very bright young guy, very opinionated. When we were in the lab at the same time, we seemed to always be arguing about chemistry. For quite a few years, he was probably one of my chief scientific competitors. Just as he was getting going well at Illinois, he and his wife were killed in a private plane crash. Elliot was determined to conquer all. I guess weather was something he thought he could lick.

Gardner Swain, who later became a professor at MIT, was somewhat in the same category. Then, Swain was regarded as a real fair-haired boy, a student of Bartlett's, and doing very exciting work. He did some of the first work on determining the lifetimes of free radicals in solution. Swain had a facility for doing very simple experiments and extracting a lot from them in a very, very simple straightforward way. George Hammond was a graduate student with Bartlett; he came here as professor and was division chairman for several years. Also, Harold Kwart, later at Delaware, and a marvelous experimenter, was in the next lab. And William Sager worked with Kwart and later went to the University of Illinois at Chicago Circle. And then there was David Curtin, who was a very good friend of mine. We worked together on some important experiments at Harvard; he went to Columbia and then as professor at the University of Illinois. And Bill Dauben, who is now at Berkeley. Don Cram is now at UCLA, a very well known and imaginative chemist; he's just been appointed to be the first Winstein Professor, and I will give a speech at his inaugural dinner about Winstein.

I used to go to Bartlett's course at Harvard, the first true physical organic course I'd ever had. Bartlett would be so very careful working out the mathematical equations on the board, he always appeared to be deriving them on the spot. Then he'd get stuck and stand there and look at the board, determined he was going to get through it. It was part of his teaching, to give somebody that feeling. In the same way, Robertson at UCLA would give students the feeling they were one step ahead of him, but that was a good feeling to have.

PRUD'HOMME: Think of what you do for your students, to prove to them that this is a cognitive process that we are doing. And they can do it, too.

ROBERTS: That's right. And I was really impressed by that. Anyway, Bartlett's course was a very fine course and very helpful. Woodward gave the course I mentioned earlier at the same time.

And then I began to worry about jobs, because the fellowship was only for a year. I sort of wanted to get back to California, but there was no opportunity at UCLA or at Berkeley or Stanford—although Stanford was not really a top place at the time. Berkeley was where I had my heart set on, and Young put in some support for me at Berkeley. Before I had come to Harvard, I had met with their Dean Wendell Latimer, who was a well-known inorganic chemist, and I talked to him about a job. Actually, I told him that I had been very impressed by the work of a man at Berkeley named Rubin, who was one of the first people to use carbon-11 as a tracer and had started to work on photosynthesis. During the war, Rubin had been tragically killed in a laboratory accident. I told Latimer I'd like to work with carbon-14, which was just coming on, and try to carry on Rubin's work on photosynthesis. I said I knew I don't have any experience in it. And he sort of muttered something about, well, maybe Calvin was going to work on that. Calvin had been the one who was involved in the war project I worked on earlier. I was still hopeful that something would come out of that, because I felt that I had something to offer Berkeley in continuing Rubin's work, which Calvin actually did later with great distinction and a Nobel Prize [1961]. But nothing seemed to be coming up, and I was beginning to get worried. So I was worried about getting a job and, quite to my surprise, MIT entered the picture.

**Begin Tape 3, Side 1**

ROBERTS: Just about the time that I went to Harvard, Arthur C. Cope, whom I had met when I was at UCLA, had been hired to go to MIT as the head of the chemistry department. Cope was regarded as one of the great corners in chemistry then. He had been at Columbia, still in his thirties but very highly regarded. He had done some excellent research and had had an important influence in the war. Now, MIT and Harvard, during this period in organic chemistry, were worlds apart. MIT was badly in-grown. They had excellent undergraduates but the graduate program wasn't very good. Roger Adams, who was at that time sort of the pope of organic chemistry, got to be a very good friend of Karl Compton, who was president of MIT. I heard that Adams told Compton that their department was a slop and they should hire Arthur Cope; Adams had worked very closely with him during the war. And Cope was doing wonders at Columbia. He got the American Chemical Society Award in Pure Chemistry, and I remember going to the lecture he gave—very impressive.

Cope was a good friend of Bartlett's, but he came into a terrible situation at MIT. They had a good group in physical chemistry, a little bit in inorganic chemistry. And one thing Cope did was to hire an inorganic chemist—my old friend and physical chemistry lab teacher, Charles Coryell at UCLA. Coryell was flushed with successes from his research in promethium and all that stuff at Oak Ridge, and doing exciting things in inorganic chemistry. Cope was faced with a department in which virtually all the organic staff were MIT PhDs and tenured. The organic staff resented his presence enormously, particularly a man named Avery A. Morton. Now, Avery A. Morton was a peculiar guy—very handsome fellow, very excitable, though almost shrill. Morton had done some very imaginative stuff, but he also seemed to have electrical plus and minus mixed up. And so, anything that mainstream organic chemists thought happened because some electrical effect was plus, Morton would talk about as though the effect was due to electrical negative. But Morton was riding high. He had just discovered a magnificent process for polymerizing butadiene, which never became practical, but it was an extraordinary process. It was the forerunner of many very important polymerization processes that have been discovered since. Nobody knew how it worked; it gave a very unusual polybutadiene product, probably very crystalline. Morton was pursuing this with his own theories, and nobody understood what he was talking about, because he always seemed to have plus and minus mixed up. And Morton really wanted to be chairman of the department, or at least the head of the organic group. MIT's department had been a terribly feudal system, and it wasn't working in organic, because they kept hiring their own PhDs. So Cope came in as chairman, and many of the organic guys already there hated him and tried to throw up every obstacle they could in his path.

Matters were complex for me with respect to Morton, because I actually gave a seminar on some of Morton's work when I was a postdoc at UCLA. In some of the work I had done on the war project, I became very interested in a reaction called metalation. Morton had published on this reaction, and while Morton just seemed backward in the interpretation of his results, his results were indeed interesting. Knowing of my interest, Winstein, at some point, had asked me to give the particular seminar. Earlier, in ’42, he had asked me to give a seminar on some of Frank Westheimer’s work. Both of those seminars turned out to tie into the work I was going to do later on. Anyway, when I got to Harvard, there Morton was, right next door. I heard one of his students give a lecture, and I thought, Gee, these guys are onto something interesting, and I think I ought to do some experiments and find out what’s really happening in this reaction. So my friend Dave Curtin, who was a postdoc with Fieser, and I began to do some metalation experiments at Harvard. These were done along with the cyclopropane work with which, basically, I was trying to set up a program that I could move graduate students into at some later time—and it was really going great. The metalation excursion was an offshoot, in an *entirely* different direction. So we did these experiments and we wrote up a paper, and we got more or less what we expected, and it looked like Morton was his usual 180 degrees out of phase.

The editor of the *Journal of the American Chemical Society* was at Harvard, a man named Arthur Lamb, a very courtly, courteous, and kind person. Well, he sent the paper of course to Morton for review, and Morton went right through the ceiling. Here were these upstart postdoctoral fellows challenging him. He wrote referee reports that were absolutely blazing: These guys are challenging the established art in the field and so on. Lamb was trying to mediate, and of course this was really a minor paper; it was about two pages of journal. But Morton was absolutely infuriated. And a week later, he’d write Lamb a letter; and he’d follow that with another letter before he got an answer to the previous, and so on.

PRUD’HOMME: It’s a great way to get your name known around the university.

ROBERTS: Well, actually, this was more or less a private fight. But one day Arthur Cope showed up in my lab. He’d been talking with Bartlett, and he asked me how I would like a job at MIT. Well, I was thinking about going back to California. They had all the local ACS meetings at MIT, and it actually looked like a pretty stodgy place. And then there was Morton. Well, I said, “You know, I’ve got this problem with Morton.” He said, “Oh, I’m not going to worry about that. I’d like to have you come down and talk about it.” Then Charles Coryell came up and he was very anxious that I do it. He had some belief in me from what we’d done before together at

UCLA. Then I talked to Young, and he said the Berkeley job wasn't going to come about. He said, "You really should take the MIT job. I have unbelievable faith in Arthur Cope. If you do well there, you'll be able to get a job somewhere else if you don't like it. But you've got to recognize that Cope is really making an opportunity for you." So, I said, OK, I'd take the job. When Morton found out about that, he was livid, and that really soured a lot of things.

Cope brought in two other people in organic chemistry at the same time. One of them was John Sheehan, who had worked at Merck on penicillin during the war. Sheehan was a first-rate chemist, really Nobel Prize category and, in fact, he did things that Woodward was not able to do, but his image wasn't as impressive. He looked a bit like an Irish politician; he just never looked quite smart enough, but he was a very, very clever, very disarming guy. Gardner Swain also came in at MIT at that time. We were going to be the nucleus of the new wave—Swain and I were both physical organic chemists, and Sheehan was to be the synthetic chemist. And our presence was not welcomed by the older guys. Some of the physical chemists were very, very nice, and there were younger people coming in all around. Well, Cope was really everything you could want, a very unusual man.

PRUD'HOMME: What an incredibly difficult job he had, to change that department around.

ROBERTS: Yes, but he was the head. He could do things without really consulting the rest of the faculty.

PRUD'HOMME: Why wasn't he called the chairman?

ROBERTS: Because he was the head. He could make decisions without consulting with the department; he could hire people without consulting. That had been one of the problems in the past with MIT. A man named [Frederick G.] Keyes had been the head, and Keyes bitterly resented Cope's coming. He was a physical chemist, very much like Morton. He built himself a lovely chairman's office in the Eastman building at MIT. And when Cope came in, by God, Keyes wasn't going to move out and let him have that office. So Cope's office was up on the third floor.

Cope was a very soft-spoken person, seemed almost effete until you got to know him, then you found he was really intense inside. If we were in a faculty meeting, Cope would slump down more and more in his chair and his voice would get softer and softer. But, if you knew him, you knew he was mad. The students used to call him "the iron fist in the velvet glove."

MIT had really done very well in chemistry under Cope's direction. They were in the process of building new laboratories. I was way off in the sticks to begin with, in an office and a room that had a great big window, and every afternoon the sun came in there unmercifully; it was old [Building 2 at MIT], it was dirty, and there was a small hood in the corner. God, it was awful. But it was the first job where I was teaching elementary organic—although at the start I was sort of a teaching assistant for Ernest Huntress, who was well known for his work in qualitative organic analysis. His lectures were amazing. He had a big room with a flat floor and a blackboard halfway around. Huntress used to go in the hour before and fill the blackboard completely. Then he'd go in with a stick, and tap, tap, tap, tap, and cover the material on the board. By the time fifty minutes were over, he was all the way around at the other end of the board. And the students were absolutely devastated. So I would have quiz sections and help grade exams. Huntress was a great guy for efficiency in grading exams; we all sat around a table and passed papers around in a factory-style grading system. With Huntress, I got along reasonably well. Morton, of course, wasn't on speaking terms at all. Swain and I were close, and we ran seminars together. So I got a research program started. I didn't have any graduate students at the start of 1946, and, in fact, there weren't many graduate students coming at this period. But that was Cope's campaign from day one, to get good graduate students in there. Furthermore, I didn't have any financial support, but MIT had a Laboratory for Nuclear Science and Engineering, which had evolved from the old Radiation Lab. Jerrold Zacharias was the director of this lab, and they had lots of money. Because I had started getting into work on radioactive tracers, I had an excuse for getting some funds.

Well, here I was an instructor, and I'd been there, I think, about six months, when Cope came around and said, "I've got some money for you from the Laboratory of Nuclear Science and Engineering"—indeed, $44,000 in 1946 dollars! Well, that was a sum of money I hadn't even dreamed about. So then I tried to buy equipment that I could use for the radioactive tracer work. There really wasn't much available; we actually had to make some—counting equipment and that kind of stuff. I bought everything I could buy but only spent about $25,000. We really got beautiful new labs, all stainless steel, and I moved upstairs and started building equipment for students, doing glassblowing and stuff like that, and starting my work on dipole moments. I was able to get a postdoc in, though—a woman from Cornell. Then I had Don Cram, who was just finishing up at Harvard before going to UCLA, in for a summer as a postdoc and he messed my lab up a bit when a reaction got away from him. I was working in the lab myself a lot, and I finally got a graduate student who did some nice work, and things began to roll. We had good luck; the nuclear science and engineering people kept supporting the work. They never quite understood what I was doing, but they were willing to give support. Cope really built the place up. Sheehan turned out to be a real star, and Swain was doing extraordinarily well, though he was getting into some scientific fights with the English people, Ingold and Hughes. Bartlett had a small seminar at Harvard we used to go to, which was very, very lively, and Swain and I used to contribute a lot to that.

We were living in Harvard housing, which was just up the street from the Harvard chemistry department. So I was around Harvard a lot, and I was acting with Swain to bring the Harvard and MIT people together and provide a link. I think that was quite important, a good thing for all of us.

And so then, with money and the new labs, research really began to take off. Back at Penn State in ’41 I had heard about some research that Henry Gilman, a very famous figure at Iowa State and still active in chemistry, had done on a strange reaction treating chlorobenzene with amide salts. That stuck in my mind, and it turned out that the combination of that reaction, which Gilman later published, and the work I had done in Morton’s area, came together and helped us to understand what was going on in that reaction. I had several excellent undergraduate students working on senior theses, which then were required at MIT and usually represented almost a master’s degree. One of my seniors was a man named [E. M.] Kosower, who later was a professor at Stony Brook. I started him out to try to see if we could understand this reaction that Gilman had discovered. A man named [J. F.] Bunnett had published an interpretation of this kind of reaction, which he called “cine substitution,” because the group winds up on the benzene ring, at a location where you would not expect it to be. And so Kosower did a lot to make this kind of stuff look really interesting. And at some point along the line, I put another undergraduate and a graduate student, named Howard Simmons, now vice president of DuPont, to work on this problem, which, as it developed later, was a benzyne mechanism. That was really a big breakthrough in those kinds of reaction. Some corresponding work was going on in Germany, but we got there well ahead of them. Later I gave a talk about that, and Gilman was really complimentary. We became quite good friends. Gilman's reputation among chemists was of a hard-driving, unfriendly type, but I've never found him so. But a lot of interesting things happened. I think the most amusing is the one which led to my interest in molecular orbital theory.

PRUD'HOMME: I just want to ask you one thing. You were promoted in '47 to assistant professor. So in three years and eleven months, you wrote a PhD thesis, you moved across the country, you worked at Harvard, did extraordinary original research work, began to teach, and then in '47 you were already an assistant professor at MIT. That's some going for a very, very short period of time.

ROBERTS: Well, but you've got to remember that not so many people were going to postdocs during that period; a lot of people were coming from abroad to be postdocs and so on. Yes, I got off to a fast start, but I had a year and half or so, two years during the war, in which I really was *trained* to do research. I'd been doing it all along, and that was the only thing I really did well.

Anyway, this was a period of great ferment in the theory of chemistry, the theory of molecular structure. As I told you, when I had William Conger Morgan teaching freshman chemistry, the first chapter of Lucas's book had stuff like resonance in it. And Morgan said he didn't understand Pauling's theory of resonance in molecules. And there were others, as we talked and listened, who didn't understand what Pauling was talking about, particularly with benzene. Pauling would say, "Well, you've got two resonance forms of benzene, and they're nearly the same." And he said, "If they're nearly the same, they're both important, the molecules are a composite of the two forms, and will not be like either form." Nobody could understand the reality of the separate structures, and Pauling's book wasn't much help on this. There were all kinds of fights going on in the literature about what was resonance. The Soviets felt that somehow this violated Marxist principles, and they were very exercised about it.

In the meantime, a man at the University of Chicago named Robert S. Mulliken, whose father, incidentally, had been an organic chemistry professor at MIT, was developing what was called molecular orbital theory. A German named [Erich] Hückel had started it off in a very simple form, and Mulliken was developing it into a very important technique for calculation of molecular properties. Mulliken had a hard time making an impression; he was sort of quiet, rather blank-looking, and could never seem to deliver a straight answer to a question. And here was Pauling, the real genius, publishing papers that people could understand—almost, not quite. Pauling was talking about structures that they could visualize; Mulliken wasn't. So Mulliken's work was just overshadowed by Pauling. Well, then a book came out by an Englishman named Charles Coulson, called *Valence*. Charles Coulson was a very, very smart guy—I think he was really a mathematician who got interested in chemistry, and he had a very bright guy named [H. Christopher] Longuet-Higgins working with him. They started publishing papers that made Mulliken's stuff somewhat more accessible. Coulson's book talks about molecular orbital theory, but he never really tells you what's going on. In seminars at MIT, we kept trying to figure out just what those guys were talking about. It's so funny, because now it seems so simple, and almost everybody knows it now. Well, then a book came along by a man named Michael Dewar, an Englishman at Oxford. He's now at the University of Texas. Dewar came up like a meteor, a comet almost, on the chemical world. Michael Dewar was a sloppy, very bright young Englishman. And he wrote a book, something like *Molecular Orbital Theory for Organic Chemistry*.

PRUD'HOMME: *The Electronic Theory of Organic Chemistry*.

ROBERTS: Right. And I looked it over, and thought, Gee, that really looks like great stuff. I was teaching the first course in physical organic chemistry. It was the start of the second year of the MIT sequence of organic chemistry, a required course at MIT for entering graduate students and for the seniors—Chemistry 5.43. I had been getting more and more interested in this molecular orbital stuff. And I knew that I wasn't going to sit down and learn it unless I really had to. And so I told the class the first day, that now I am going to teach the course without mentioning resonance at all. I'm going to give the whole course from the standpoint of molecular orbital theory. Cope and Swain and all these guys were a little aghast when I announced that day what I had done, but they were basically sympathetic, because everybody was in a sense tired of resonance; they didn't quite know what to make of it. This was almost my Waterloo, because as I began to make up my lecture notes, I realized that Dewar had left out some things that were absolutely vital to explaining what was going on. I could see that if somebody asked me certain questions, I was just going to melt into the woodwork because I wasn't going to be able to explain it. And I did not see any obvious answer.

Nothing I read in Coulson's book addressed this problem, and nobody around seemed to be able to help me. I began looking at a lot of books on structural theory, and nobody seemed to say anything about this particular kind of problem. But it was a central problem in organic chemistry. How do you use molecular orbital theory to actually predict where the charge is in a molecule, or how do you actually predict in a qualitative way that some particular arrangement is more stable than some other arrangement? Well, this was not trivial.

My old buddy William G. McMillan, who'd been my lab partner at UCLA in physical chemistry, had come on the scene at Harvard as visiting professor. Now, McMillan and I were different in so many ways. One way was obvious from the books on our respective shelves. McMillan's would be solid black at the top of the pages [when the book was closed], then white starting at the point beyond which he had not penetrated. Mine tended to be black only on narrow strips usually in the middle! Anyway, I called McMillan up and asked him if he understood all this stuff. "Oh, yes, it's simple stuff." Then I said, "Well, look, I've got a problem. How do you explain this?" He said, "I don't know offhand. But I'll tell you how you can find out. There's a book by Eyring, Walter & Kimball, which is a famous book on quantum chemistry. It's a book that has everything that you need to know in it. Sit down and work your way through it, and you'll be all set." I said I had looked at that, and I said, "As you know, I don't know much mathematics; I don't understand anything about Hamiltonians. And I don't think I'm going to get there." He said, "Well, don't worry. Just go and work on the book."

**Begin Tape 3, Side 2**

ROBERTS: [Continuing] Well, I got through about three pages of Eyring, Walter & Kimball, and I looked ahead and found that what I wanted was more or less in chapter nineteen. I knew I wasn't going to make it before I used up the material I had backed up for these Ch 5.43 lectures. So I called McMillan up again and pleaded. He didn't want to get involved, but he said, "OK, if you come over here, I'll look at it with you." So I went over there, And I said, "OK, here I've got an allyl cation with just three carbon atoms. I want to figure out how the charge gets on those end carbon atoms, and I guess the molecular orbital theory tells you, but how does it do it?" So he started writing down equations. I said, "What are you doing there?" and he said, "Oh, I'm using group theory." Well, group theory was something that I'd heard about but never been exposed to; I had no idea what it was about. So he was using Eyring, Walter & Kimball, looking things up in the back, character tables and so on, and he finally came out with an equation that didn't look terribly complicated. And I said, "I don't understand how you got that equation, but I do want to know." He had to sort of relearn this stuff as he used it, but at least he knew what he was doing. Then I watched him do it, and I said, "Gee, that doesn't look very hard." And he said, "No, it isn't."

So I finally began to get the idea, and I began to see how I could get somewhere with this lecture. Up to then, I had no idea about degenerate orbitals or how you figure which ones were degenerate and which weren't. But now I was able to read some of Coulson's original papers—actually very well written—and then I could make the calculations, finally I was able to steam into class and really explain what was going on.

I couldn't get Swain interested in it, but I had a postdoc on a fellowship, a very good man named [Andrew] Streitwieser, now a professor at Berkeley, who got excited about it. He and I sat down and started to do research. We didn't know much about what we were doing, but Streitwieser was an energetic guy and we did a lot. And we published one very nice paper, I thought; anyway, it was pretty widely quoted. So that was our foray into molecular orbital theory.

Then I began to give lectures about it; I could show the organic chemists what was involved, and then I finally wrote a book about it, which was a nice little book—it's gone through thirteen printings. It certainly wasn't widely admired among the physical chemists, and a lot of it wasn't really right. But it was pretty good. Students loved it, and I'm thinking of revising it now that the computations can be done so easily by computer. So that got me started in a lot of new directions and I got some reputation for doing that job, and helping to bring organic chemists more generally into the molecular orbital age. I didn't make any basic contribution except as sort of a teaching function.

The other programs went very well. One of the better programs involved carbonium ions. One of the best graduate students I've ever had was a man named Robert Mazur, who later earned fame as the discoverer of NutraSweet. Mazur was deeply interested in mathematics, and I still don't understand why he started doing some work related to cyclopropyl compounds, which by now was back to where it was fairly closely related to things I'd done before with Young and Winstein, way back. The work we did now was based on research by some Russians who'd been working in this field for some years before but did not explain. Mazur's work, when published, turned out to be one of the most cited papers ever, according to the documentation institute. Mazur did a marvelous job. He only had relatively primitive tools to work with, but he was so careful. Much that he's done has been attacked from time to time, and everything has held up in the final analysis. That was one of our first really big breakthroughs. We discovered some chemistry in there which was really unusual, and this led to the work that's still going on, what is called the nonclassical-ion controversy.

When I was a graduate student at UCLA, Winstein used to come and talk to me about some work going on at Northwestern and some work done in England by people on what we call carbonium-ion rearrangement reactions. Then, when I was working in Bartlett's lab, as part of my cyclopropane work I was studying some other compounds to compare with cyclopropyl derivatives. And at one stage, Bartlett suggested that a compound related to one he had been working with would be a good one to put in the series. He said, "I don't know what's going to happen, but it would be an interesting compound to have." So I studied it, and it turned out to be very unusual. Because I was aware that it tied in with the stuff that Winstein had been doing at UCLA, I used the interpretation that the Englishman had published and sent a copy of the paper to Winstein. He was quite incensed, because he felt that I had stolen this stuff from him—it was in the open literature, but it was true that I got the idea from him. Bartlett had, too, because Bartlett said, "This is something that Saul ought to be interested in. You ought to send him the paper." When Saul objected, I said, "OK, I'll leave the data in, but I'll take out the interpretation because I know you are going to publish that."

Then I went into working on these unusual carbonium ions. It was pretty clear that norbornyl compounds of the type Bartlett got me into and the English work was related to were really going to be the key in this area. A postdoc named Hine and I began work on carbonium rearrangements; we devised a test of rearrangement of norbornyl compounds, using radioactive tracers, which fit in well with other research that I was doing at the time. We found some extraordinary things in there. Hine carried on the work, and when I wrote a paper up, Winstein found out about it and was teed off again, because he'd been working on those same compounds, too, although in a very different way. He said he should have the privilege of publishing first. So he published his stuff, and later I published mine, and it turned out that the two papers were very complementary; the world needed both of these tests of things. So I began to work on a whole bunch of compounds in the carbonium-ion area, and every time I kept tripping over Winstein, because Winstein felt that the nonclassical-ion problem was his problem. But on the other hand, we had gotten into it initially through Mazur's work on the cyclopropylcarbinyl compounds, in which Saul had no interest initially. We finally managed to settle all that pretty well. We remained good friends, and we taught each other a lot, and later on we joined forces against a common detractor. But there were some difficulties for a while. Certainly this happens all the time.

Woodward had been very helpful in the nonclassical-ion thing. He had encouraged me to look very hard for unconventional solutions to that problem. And that had led to the idea of the so-called pyramidal structure for the cyclopropylcarbinyl cation, which later turned out to be wrong, but it was very exciting at the time. Later on, I showed theoretically why it wasn't right in that form. Anyway, that research program on carbocations really went along like a house afire.

In a graduate seminar, I had examined a theory developed by [F. H.] Westheimer and [J. G.] Kirkwood for a particular kind of compounds on how what happens at one end of the molecule influences what happens at the other end. For quite a while at MIT, I wanted to enlarge on that in a different way, using bridgehead compounds of something like the kind Bartlett worked on ten years before. I couldn't get graduate students interested, but finally a young man named Walter Moreland came along and said, "This is my kind of problem. I'll work on that." This work was to be a tremendous influence in the long run, because we were making use of a different kind of rigid way of holding groups apart in fixed relation to one another, and seeing what the effect was. The results we got didn't fit very well with the Westheimer calculations—the theory was relatively crude in those days, anyway, but that created quite a splash.

I'd been at MIT seven years, and there was the possibility of a sabbatical coming up. During the summers, when my wife and I came out to California, I used to come over to Caltech, particularly to talk to a man strongly affiliated with Linus Pauling named Verner Schomaker. Pauling wasn't really accessible to me, but Verner was. Verner was an enormous influence on Caltech—he's back at Caltech now; retired from the University of Washington. Verner was one of Pauling's PhD students who worked on electron diffraction, studying the shapes and sizes of various kinds of molecules, organic and inorganic. I used to love to talk to him about that work; Verner is a man who likes to talk and clarify things, and in some ways for me was sort of an ideal consultant. He liked to try to understand what people didn't understand, and tried to clarify it, if he could. If Verner said he didn't understand something, that usually meant that nobody understood, really. So we used to talk a lot, particularly about the theory of molecules and molecular orbital theory. Verner had a memory that was just absolutely crystal clear at all stages. When you came back after a year, he'd sort of pick up in the middle of the last sentence of your earlier conversation and go on from there. So I was learning a lot from him.

At the same time, I started, about 1950, to do some work as a consultant at DuPont. Cope really wanted to do everything for me. He got this research support and everything, and then he knew that one of the more obscure labs at DuPont was looking for a consultant—the Explosives Department, in fact. I was in competition for that job with Elliot Alexander, who had been my lab mate at Harvard, and was then at Illinois. Roger Adams was pushing Alexander, and Cope was pushing me, and I got the job, which was the first time I'd gotten ahead of Alexander on anything.

In connection with my DuPont consulting, I sometimes used molecular orbital theory. I actually gave some lecture courses on the subject to DuPont people. I met there a young man named [Rudolph] Pariser, now a laboratory director at DuPont, who had worked with a theoretical chemist named [R. G.] Parr at Johns Hopkins and was doing the next level of molecular theory. In the kind of theory I had been doing, you put electrons into a framework of orbitals and assumed that the electrons don't really interact with each other by repulsion. And so the theory lets them bunch sometimes, in defiance of electronic repulsion, rather than being evenly distributed. In resonance theory, that doesn't happen in quite the same way, and as a result it usually gives different results in the actual calculations. Pariser and Parr were trying to find a way of doing these calculations much more simply than doing the full schmazz of advanced quantum mechanics. When I'd go and visit DuPont, we would talk about that, and Pariser got me pretty excited about it. I wanted to take off some time and really learn that; I could tell it was going to be tough. So that looked like an ideal thing to do on a sabbatical, for which I had time coming up. And there was no better place to do that than at Caltech, with Verner Schomaker around. Of course, I knew at the same time that Howard Lucas was going to retire. And Caltech looked like an awfully attractive place to be, particularly because of Schomaker and Buchman. When I came out, I would visit Buchman as well as Schomaker, because Buchman was doing small-ring chemistry of somewhat the same kind as we were doing at MIT; he was particularly interested in the cyclobutadiene problem, which I also was working on. And of course, I had a tenuous connection with Yost. So I got a Guggenheim Fellowship to take a sabbatical and we came out here in the fall of 1952. The plan was that we'd stay through the fall here, and then part of the spring in England.

That was a great period. I worked very, very hard on the molecular orbital thing, although the facilities here for doing the calculations at that time were primitive. I just used a Marchant calculator. The calculations were extraordinarily detailed for such procedures and took a lot of time. Pariser had not made clear how difficult it was, but then he was using a real high-speed electronic computer at Columbia. There wasn't much around Caltech in the way of computers during that era; it was just when computers were starting to be important in chemistry. So I worked and worked, and I had one particular molecule that we were trying to synthesize at MIT that nobody had ever made, and there was a lot of speculation as to whether the molecular orbital theory was really right. I thought if we could take account of electronic repulsions with more advanced calculations, maybe we could find out. There's a lot of numbers that you have to put in here, including Parr and Pariser's semi-empirical constants, and I'd worked and worked and finally worked out all this out. Then I went back and I talked to Pariser about it. And Pariser said, "Well, that's great. You've done a wonderful job. I didn't think you could get through to here. You'd be very interested to know that we now have much better values for the constants to put in." I said no, I was not going to go back and take that damn Marchant calculator and slog my way through the calculations again, with all the new semi-empirical numbers that he had. But by then, I'd learned a lot, and I didn't regret it, but I never did pursue that anymore.

And at that point, Caltech offered me a job. Edwin Buchman had sort of hinted that they were going to. And Ernest Swift came over and talked, the only person who actually interviewed me. I gave some lectures here during the summer, one on the benzyne stuff, one on the nonclassical ions; and Pauling got very interested in nonclassical ions. At that time, nobody really believed the benzyne story. Anyway, I had to make a decision about coming to Caltech. And believe me, this was tough, because Cope really put the pressure on. But it was a terrible situation for me at MIT. Morton hated my guts. The older people were not all that happy with me either. Further, I didn't like the physical plant at MIT—big, enormous buildings; cold. The labs were nice, and the students were good. But the faculty here and the facilities and this building were just so different. Gates was still a lab at that time; and my office was going to be in Crellin; it was Howard's old office. Caltech was an unbelievable contrast to MIT, and it was close to home. My wife, however, was very happy in Cambridge. We were so close to the Harvard people; we used to go to all the parties and everything; and there was much more social life there than there was here. But we had two children at that point. And of course Cope was really tough to deal with; he was really unhappy. Cope never really forgave me for leaving. He was friendly but never quite the same.

PRUD'HOMME: Of course, he'd given you so much.

ROBERTS: That's right. Cope and Sheehan and I were really very close friends. We used to spend a lot of time together. We used to go skiing together, and all kinds of things. And they wanted us to write a book together. But there were other problems at MIT.

I had had some battles. One was about the library. When I came, they had a wonderful library in the Eastman building, divided among chemistry, physics, and mathematics. Then somebody had offered to build a monument, called the Hayden Library, along Memorial Drive. And I had been delegated as a chemistry representative to a committee to decide how to use the space. It was a very searing experience, because it turned out that John Slater, a famous physicist at MIT, wanted the Eastman library rooms for his research. And there was a push to get the library out of Eastman so he could get his group in. It was the first time I'd really been exposed to that kind of a power play. And they were building this monumental library, which wasn't really a place to do serious studies. What a contrast to the Harvard chemistry library, a great chemistry library; it was compact, easy to work in. The Hayden Library had this great enormous room with chemistry stacks in a small part of it, cold and inconvenient, not a place you wanted to go and browse. I didn't like that at all. And I got in a lot of trouble by being outspoken about it. But they just steamrollered those of us who wanted to stay in the Eastman Building.

PRUD'HOMME: They pushed it right through.

ROBERTS: Yes, they pushed it through. The administration had to show they were using the Hayden Library; and Slater wanted the Eastman space. But it was not a problem unique to MIT. A lot of people don't care much about their libraries. I've had that problem here too. Libraries tend to give way to research space.

Another problem about MIT was the faculty club; it was far away from our building and then rather poorly patronized. Because MIT is big, when people went to the faculty club for lunch, they almost always went to eat with their departmental colleagues whom they may not have seen for quite a while. Here, the Athenaeum was a very dominant part of what attracted me. The atmosphere was so collegial; it was really such a gracious place.

**JOHN D. ROBERTS**

**SESSION 3**

**March 7, 1985**

**Begin Tape 4, Side 1**

ROBERTS: I've thought of a couple of things that might be interesting that I didn't tell you about my MIT days. Of course, the [Sen. Joseph R.] McCarthy era was on then. I came from a Republican family, and being under the influence of the *Los Angeles Times* and so on, I think I was fairly conservative throughout the first New Deal years, not knowing any better. I became more liberal later on as I became exposed to different things. The McCarthy era was really an eye opener, I think, for all of us, because they were taking dead aim at many of our heroes—[J. R.] Oppenheimer and people like that. It was disturbing to see scientists taking sides in this thing, with fellows apparently exploiting it for their own purposes. At MIT, there was a very large bloc, because there was a mathematician there by the name of Dirk Struik, who was also a historian of mathematics—in any case, a highly regarded guy. He was indicted by the Commonwealth of Massachusetts for treason—a professor in good standing at MIT! Believe me, that stirred a lot of people up. He was supposed to have been a member of a communist cell and so on, and the institute had to decide what they were going to do; they finally wound up suspending him with pay. This involved some pretty emotional faculty meetings. The Commonwealth never did try Struik—I think he's still alive. Anyway, he was under a cloud for the rest of his life.

PRUD'HOMME: The implication is that as scientists you don't have any privilege of immunity.

ROBERTS: Well, I think we were polarized by being involved. Today, people function so that there's a much greater diversity of scientists' opinions along self-interest lines. For quite a long time, everybody seemed to be more or less on the same side. But now many people are going for things more on the basis of their self-interest, I think, than on what might be regarded as useful for science as a whole.

To return to where we were, there were some people at MIT with whom I was very good friends who, to my surprise, were attacked by McCarthy as members of communist cells and that kind of thing. One of my very good friends who was a fine physical chemist—a very mild guy; I spent a lot of time talking with him, he was very helpful to me in some theoretical matters—was so identified, and there was a question of what would happen to him. The chairman of the math department was another one who was in that same unit—quite unsettling. Actually, nothing happened to them that I know of, no formal institute action or anything like that. And of course Pauling was very heavily involved. I was not here when things were at their worst—so I understand; some of the members of the board of trustees wanted to give him the old heave-ho. And Jim Page and Bob Bacher and some others exerted a lot of influence to keep the board from doing it, but some of the board members resigned, as a result. That was sort of an awakening to a lot of problems with regard to politics and so on. I guess following that period, I became much more liberal. Anyway, all of that was an important influence at the time.

PRUD'HOMME: Can you give some background on the chemistry department at Caltech when you came here, in terms of its reputation, and the relationship between chemistry and physics at Caltech?

ROBERTS: Yes. Well, it was interesting. There were mixed feelings about it. Caltech was obviously very well known in physics. In chemistry, it was very well known in those areas in which Pauling was strong, particularly as regards the applications of chemistry which were coming to biology. I had read Pauling's papers about immunology. But, as I said before, there was a long period in which the resonance business was causing all sorts of problems in people's minds, because they weren't quite clear how it operated and what to say about it in papers. That was controversial, and I think Pauling's excursions into biochemistry were sometimes regarded by people in that field as being intrusions and saying what everybody already knew, presumably—but in fact I don't think that was true. I think he had a tremendous effect. And certainly he had the effect of popularizing the field and catalyzing and getting other people in. Yet the Caltech department was very narrow. There were three organic chemists.

Howard Lucas, who was retiring, had done amazingly well. I never understood quite how Howard got here. He worked down in Puerto Rico, after getting a master's degree at Ohio State in sugar chemistry, and his first paper was on the milk of Puerto Rican cows. [Arthur Amos] Noyes heard about him somehow, and the story was that Howard was supposed to be somebody who would teach organic chemistry, which I gather Noyes didn't think much of, except that he knew it ought to be taught, and Howard would not be bothersome by creating a big organic research program. Howard never did create a big research program, but he created a very good one. It was first rate; Howard was well recognized as being one of the best of the crop of people who were interested in applying physical chemistry to organic chemistry. And his book [*Organic Chemistry* (New York: American Book Co., 1935] was very influential.

PRUD'HOMME: What was Howard Lucas like?

ROBERTS: Tall, thin, very polite, very courtly, soft spoken, genial. I had heard that he had been married but got divorced very young; he was a confirmed bachelor since then. He was very modest. Like Ross Robertson at UCLA, he just was not out pushing himself hard. His organic chemistry lectures were very well received; he was said to cover the lecture desk with a sea of bottles and do all kinds of demonstrations. I personally think these demonstrations must have greatly shortened his life, because of the fumes, which he shouldn't have let himself be exposed to in that kind of situation. The students used to say he'd come in, he'd have all his bottles ready, and he'd say, "Well, what do we have here?" and then start to lecture. He made the students work hard. He got organic chemistry to be so many hours of work that finally it had to be cut down some. For a while, it was a required course for almost everybody. I think even the physicists took it in the early days.

Howard was a great creature of habit. He sat in the same chair every day at the Athenaeum, and the first thing he would do was spoon the ice out of his water glass and put it in an ashtray. There was always a group around him, and different people came. A very kindly man—he didn't seem to have a lot of outside interests, and he wasn't in terribly good health when I came as Guggenheim Fellow. He was, however, chairman of the faculty in his last year before retirement.

Then there were some older people—[Stuart J.] Bates particularly, who taught physical chemistry. I didn't get to know him very well. Don Yost had done some very unusual things. I never understood what motivated him. He used to write things for the *Journal of the American Chemical Society*—book reviews and things like that. And these things were absolutely outrageously funny—the posture he took, the language he used, and so on. He'd evolved a peculiar style; it seemed to have some Spanish element. Don was a prodigious smoker; he seemed to have just a succession of cigarettes going through him. He was not in good health at the time I came. But Don had seen the possibilities in many techniques very early, and he had moved into them. But, before they became really popular, he moved out and would go to something else. He didn't stay with anything long enough to become a leader in any field. He just did whatever he was interested in, when he saw a problem he wanted to solve. And I don't know what Yost's earlier relations with Pauling were like. I'd heard that Pauling had given Yost trouble, because Yost had sold some equipment and used the money to buy something else. It was clear that he and Pauling were not on good terms. In chemistry faculty meetings, Yost almost always voted against whatever was proposed. Yet he was a very interesting person to talk to, and by the time I got here he had trained several extraordinarily good students. And if you asked people about Don Yost and NMR, hardly any person in the whole country would say that Don Yost had ever done anything with it. He just got in and got out. And he'd done really pioneering NMR with wartime surplus stuff; he was a great guy for just assembling things and doing experiments with minimal equipment. By the time I got here, he was rather embittered by Pauling or whatever, and he had started to work on some mathematical problem, which, I understand, was something like solving fifth-order algebraic equations rigorously rather than by approximation. In this he was sort of wasted; in some ways, I think he got himself into that, although I don't think that Pauling helped.

There were mixed feelings about Pauling among the staff, and very mixed feelings outside. Some people were mad at Pauling because of the communist issue—more conservative people among the scientists. I know that when I talked to Bill Young at UCLA about coming to Caltech, he was very negative about it; he said. "Pauling will run over you." He knew some people who had, in principle, been run over—like Yost, I guess—and felt that I would likely have the same fate.

Then, among the other chemistry faculty, there was Carl Niemann, who had the office next to mine, who was from the University of Wisconsin. Carl was really a biochemist, but he'd been well trained in organic chemistry. He was doing very basic enzyme work at a time when it wasn't very popular. The kind of thing that Carl did was not much appreciated until very much later on. He was trying to do fundamental work, and he wanted to work on an enzyme that he could have some control over—that he'd really be able to study well, and that was available, and so on. He was making organic compounds that would fit with that, so his students were well trained in synthesis and in studying enzymes.

Laszlo Zechmeister was a refugee from Hungary. The first I had heard of him was through the work that he and Pauling had published on the properties of coloring matter from various vegetable materials—carotene, lycopene, things like that. Zechmeister was a master of what then was called chromatography, that had been invented by a Russian, [Mikhail Semyonovich] Tsvet—a particular kind of chromatography. They would put a solid material in a column and they'd put solutions through it, and there would be selective adsorption of substances from the solutions. Zechmeister would have these great big funnel-like columns, and he would remove and lay out the column material carefully and then go through with a knife and carve out what he wanted of the adsorbed substances. He was a very fine technician and a very kindly man. He was primarily working at isolating things, and he was very good at it; his students were well trained and some turned out very, very well. He was really of a different culture altogether from the stream of modern organic chemistry at that time.

Then there was Richard Badger in physical chemistry. Badger was in one way like Yost, in that he was a prodigious smoker. He was very soft-spoken, a very intellectual person. He was a skillful artist, painting and making things out of silver. He had been here for a long time; I think he'd been a graduate student here. He taught the physical chemistry laboratory, and he did a wonderful job. His idea was that you don't bring in black boxes and use them as black boxes; that each student must find out for himself what's inside the box. Thus, instead of using a plotter, you read each data point separately, and then you later plot it yourself. He was very strong for fundamentals.

Then there was Edwin Buchman, who was very instrumental in helping to get me here. As I mentioned earlier, I first met him in 1941. Edwin had worked—I guess at Columbia—on the synthesis of vitamin B1. He was one of the patent holders, he gathered considerable income from that, and I think he came from a reasonably well-to-do family. Edwin came out here at some point, just walked in the door, and said to Pauling, "Well, look, I want to do research, don't need money…" And Pauling, being an opportunist, took Edwin on. And Edwin started doing research, some on vitamin B1 and others on the small ring compounds. And he turned out some very good people, including Maury Schlatter. Edwin tended to be very indirect, and you had to read him to find out what he thought about things. He married Max Delbrück's girlfriend; and she's still alive—she's a Russian woman; they had a couple of talented children. Edwin was quite influential in the department. He was not paid; he didn't have tenure; he had an appointment, what was called a research associate then. Edwin would befriend all kinds of people who were bright and had some unusual characteristics. He managed to listen to them and acted as their sponsor, or godfather, and tried to smooth them over rough spots, and so on. He had a long, long list of good friends, especially Charles Coryell, who after his wife died and Edwin died, married Mrs. Buchman.

There was Robert Corey in structural chemistry. Corey was a very quiet man. I never got to know him very well. Walt Schroeder was already here and working on proteins. Pauling had gathered around him a group of people who were doing things he was interested in. It was characteristic of him that once he set somebody up, unless he actively participated in the work himself, his name would not appear on the publication. He didn't try to claim credit for what Schomaker did in electron diffraction. Corey he worked with very closely; and almost all of Corey's papers have Pauling's name on them. But others, like Dan Campbell, who was here in immunochemistry, had been brought in because of Pauling's interest in that. Campbell was set up independently with a professorship. Some appointments like that had been made that I think annoyed some of the staff. I have heard rumors that Corey's appointment was not really run through the usual formal procedures; and I don't know about Campbell. But Pauling had decided that he wanted these people promoted to professorships.

PRUD'HOMME: It sounds as if Pauling was more interested in research than in teaching.

ROBERTS: No, I don't think that was true. You don't write a freshman book without having some interest in teaching, believe me. No, Pauling was very interested in teaching, and he expected it was going to be done well. He expected the students to learn, and he also hoped they'd learn some research. And like many of the rest of us, he was not strong on required courses, not strong for special examinations, but preferred to have students pick up what they needed to carry on in research. He wasn't strong for specialization; he didn't want our chemistry group divided into specific groups: organic chemists, inorganic chemists, and so on, and to have special examinations in each of those areas. He appreciated that you couldn't know everything, but he wanted people to understand that you can learn anything you have the will to learn.

Pauling really overshadowed Caltech chemistry, among other reasons because so many of his people—[Richard E.] Marsh, Hughes, Schomaker, Corey, Campbell, and a lot of postdocs—were in the Pauling orbit, many of them independent, but basically doing things that Linus was interested in. But most of these things I was also interested in, so the department was a great place for me to come into.

When I came, I was very well pleased. Pauling had made arrangements for postdoctoral support for a few years, one postdoctoral fellow, equipment, and so on. I was able to get rid of some of the old stuff I'd had at MIT and get more modern, better equipment. And then there was the question of Dorothy Semenow.

PRUD'HOMME: You brought people with you from your MIT group.

ROBERTS: At MIT at the start of the academic year of 1952-1953, I had nineteen people working for me. It was way too many, and I was sort of depressed with the quality of some of them. Fortunately, there were four students, all, in my view, very, very good, who said they would like to come to Caltech in the fall of 1953. The others were quite uncertain; and a few were close to finishing at MIT. But Dorothy Semenow did represent a bit of a problem, because it was not going to be possible to get her in under the Caltech rules for admission of students. When I asked Pauling about it, he said the faculty at large had voted in recent years against letting in women, and the trustees had ratified that. But he said they had never really considered a specific case, and he would like her to put in an application. So that's what we did, and believe me, I was impressed that they were able to get that one through by June. Any academic institution that's going to make that kind of a fundamental change—and can do it starting in February and have it done by June—is quite astonishing. I thought Pauling was very good about that. It was something he believed in, and he was willing to spend the time to make it work. At first, they set up some special rules that it required consideration of only outstanding women and so on, but they soon modified that.

PRUD'HOMME: I think it was women graduate students will be admitted only under exceptional circumstances, something like that, giving lots of outs. What was your impression of Lee DuBridge?

ROBERTS: At that stage I had no real perception of him. I had not met him, although I had met Millikan. I think Millikan died just before I came in 1953. I had met him during the fall of my sabbatical, before we went to England.

That was a great period, in England, really much more of a triumphal tour than I had expected. I got to meet Sir Robert Robinson, who was a famous organic chemist at Oxford. I'd heard that he was a nasty old man and so on, but with me he wasn't that way at all. He was so different from many people: he didn't want to tell you, he wanted to listen. He had a standard thing he liked to get out of his system about how he felt about some of his English colleagues, how they'd stolen his ideas and so on. And after ten or fifteen minutes of that, he'd say, "Well, tell me what you're doing." And somehow I always seemed to have some unusual kind of chemistry that he was interested in. I was just into the benzyne mechanism that had not been really published very much then. We were going to be working on it here in a big way, but we'd done the preliminary work at MIT. The lectures I gave on that in England really aroused people to loud argument. I remember Christopher Ingold, who was a physical organic chemist at the University of London; he was sitting in the front row and he stepped on his seat and stepped up on top of the lecture desk, and got behind it to lead the discussion. It was really fun. I saw Robinson, I'd say, maybe half a dozen times after that; we always just got along great. Christopher Ingold had a terrible reputation, too, because he had written some very nasty papers about people whose work he didn't like. The first time he ever came over to this country—before I left MIT, about 1950—we were amazed to find this courtly English gentlemen in our midst. When you talked to him, he was so affable. But when he started to write, wow!

So then came moving out here. Howard had been in this office. He was determined that he was going to get out of the way, and he went to Hawaii for a year, as a visiting professor or something.

PRUD'HOMME: What a gracious thing to do.

ROBERTS: Really gracious, yes. That was Howard's style; he was like that. He really felt that he shouldn't be in the way.

PRUD'HOMME: You took over his course in organic chemistry?

ROBERTS: Yes. And I taught a more advanced course, too, at the same time, based on some things I'd worked on. My sabbatical time here was spent on that. I remember Dorothy Semenow telling me at one stage, "You know, I've never really been in a lecture before when it was so clear that the professor didn't know what the hell he was talking about, [laughter] just learning as you go along." And that was sort of like the experience I'd had at MIT in molecular orbital theory. Anyway, it was sort of fun.

PRUD'HOMME: Tell me about the difference in the two communities—MIT and Caltech. Did you have any moment at which you said to yourself, "Oh my God, why did I do this? Why did I move here?"

ROBERTS: No, no, I've never really felt that way at all.

**Begin Tape 4, Side 2**

ROBERTS: After I'd been here a short time, I said to myself, "Gee, I've got thirty-five more years of this place. That sounds great!" And that's the way I've always felt since. I had been offered jobs earlier at Florida State, and then Columbia. But this was the place I wanted to be. The only other place I ever talked to seriously was Princeton, and that was because a good friend of mine, Don Hornig, was chairman of the department there and absolutely insisted that I come, even though I told him there wasn't much of a chance I was going to do it.

PRUD'HOMME: Can you compare the students at MIT and Caltech?

ROBERTS: Well, I think the students are pretty much the same. MIT had a larger undergraduate body, but I think they had about the same percentage, although more numerically, of the near-genius or the genius type. When I was there, Woodward had been there in the late thirties; and there was a man named E. J. Corey, who was here the other day as the Beckman Lecturer; and then there was Ed Kosower, who had worked with me, who did some of the early benzyne work. Kosower was a marvelous student, but far out. So overall, the undergraduates seemed pretty comparable.

One thing I had a terrible time with when I first came here was the honor system. MIT was so anti-honor system, you couldn't believe it. They used to give the organic chemistry exams in big drafting rooms where the tables were far apart, and they had people patrolling, walking up and down, and students really tried to cheat. You'd catch them at it all the time; it was terrible. They would go over the papers with a fine-tooth comb, looking for cheating and so on. When I came here, I couldn't believe that there was an honor system. You gave them the exam and you were supposed to leave the room. Once I got used to it, believe me, I decided that was great; I liked it. And it's a different feeling.

I've been a little disappointed that the Caltech undergraduates and the faculty don't seem, to me, to have as much contact with each other as I would like. Of course, that's partly because I've never really been on any popular undergraduate orbit. I never taught freshman chemistry, never was heavily involved with undergraduates, except in the courses that I gave, first to juniors and then to sophomores. But the students are very good. I remember one of the first classes I taught, it may have been organic chemistry, and Howard Berg, who was a professor here and is now at Harvard, was an undergraduate then. Howard was extraordinary. I used to try to keep him from getting 100. And I would pretty much sink the ship for the rest of the class. He was just as good as any of those people at MIT, but as I said numerically there weren't quite so many of them here. You don't really expect very many people of that caliber in any group.

PRUD'HOMME: And you didn't feel that you had to walk on eggs because of Pauling?

ROBERTS: In my view, Pauling was a very fine division chairman. There was only one occasion in all the time I was here when I thought that Pauling really pushed something through that I couldn't quite stomach. And that was the appointment of Jurg Waser here, not too long after I came. The problem then—maybe it was the early sixties—was that they'd been having trouble with the teaching of freshman chemistry. Pauling and [Norman] Davidson were both teaching it. Pauling loved to teach the course, but as he became more famous and the Nobel Prize came on, and he was into banning the bomb and so on, he was everywhere. He would sort of come in from a trip and ask somebody, "Well, what am I supposed to talk about today?" And that wasn't going over too well with the students. Davidson wasn't very happy with the situation. And so Pauling decided that we had to have somebody to teach freshman chemistry who would be a good lecturer. Waser had been a student here, and he had moved on to Rice University. Although a wonderful person, I didn't feel he was what we needed to add strength to the department. It always seemed to me that you could find people like Harry Gray, who were great chemists, who loved to teach; these were the ones really needed to do the job. Anyway, I argued very hard against Jurg in the division meeting. And Linus finally just said, "Well, I want it. We need him, and we need him now." And so he won; I voted against it. By that time, I was occasionally moving in step with Yost. But that was the only occasion in which I felt that he had gone too far to get one of his own students back, who wasn't really going to cut the mustard. And that's what happened. Jurg had real ability to lecture and all that, but, in the long run, he didn't really command the respect of the students in a way that was needed to do the job.

Pauling had J. Holmes Sturdivant, who had been one of his students and was now a professor, to help him run the division. Holmes had not done very much research, but he really knew how to do things. Sturdivant was fantastic. I remember hearing Schomaker and other people complain about Sturdivant's autocratic ways and so forth. But generally, he had a facility for being right, and he didn't brook stuff from people with fuzzy ideas. George Hammond, whom we brought here later on, was a brilliant guy who had a lot of great ideas, but with a lot of fuzz. And Sturdivant just drove him right up the wall. Sturdivant could puncture whatever it was that Hammond wanted to do; a good idea, but it just wasn't framed right. Sturdivant was very, very efficient. He knew all the people in the physical plant; he knew how to get things done in a way that was extraordinary. He'd always have projects lined up. And any time any money was available or people were available, Sturdivant was right there. We were extraordinarily well treated. We had money in the budget for equipment that you don't ever get today in any chemistry department budget.

PRUD'HOMME: Did you feel that the chemistry department was ranked below the physics department?

ROBERTS: Oh, I don't see how I could feel that. I think the physicists may have felt that way. But on the other hand, Pauling was really a superb chemist. And the people he had, most of them—not all of them—were very, very good people. He made a lot of people better than I think they would have been otherwise by putting them in situations where they didn't have to do much teaching, or by getting them money. And the students were good. Corey, for example, was a perfectionist, a worrier, certainly a careful person, but I think he needed a Pauling to make him see the big problems. And Corey did them very, very well. But Corey didn't teach.

Schomaker didn't teach for a long time. Schomaker had been one of Pauling's appointments, originally as a research associate, and they made him professor before I came. But he did hardly any formal teaching, even though I thought, "My God, this man must be the greatest teacher on earth." I was learning so much from him. But he was only teaching part of Pauling's "Nature of the Chemical Bond" course. So I kept pushing for Verner to teach elementary physical chemistry. Well, they finally got him in there. Too bad, because it wasn't the right thing. It was terrible because it discouraged Verner. I didn't realize that what the students wanted was some predigested stuff that they could learn and sort of swallow, at least certainly in the beginning. They didn't want Verner's saying, "Look, even I don't understand this." They got worried; they wanted something that would let them off feeling that things would be simple. So Verner finally left. Pauling, I think, somehow had intuition about Verner, that he was just right where he was. But as a professor, he had to become a part of the rest of the ballgame.

Anyway, the students were very, very good.

PRUD'HOMME: And you could bring with you four people from MIT.

ROBERTS: I brought with me four people. This brings us to Dorothy Semenow. Dorothy had been a superb student at Holyoke. She was an excellent perseverer, a very sharp mind; she was a very good experimentalist. She came here terribly under the guns, a really tough situation. She was visible all over, in the newspapers and everything, at the outset. And she had come from a pretty unhappy family; she had problems with her mother and her father and most everybody else. And she had developed one characteristic that was really tough for her progress. She had to have appreciation for everything she did, before she could go on and do more. She was an independent thinker; she had excellent research ideas. But she couldn't go ahead with anything until it had been approved; and everything had to be discussed and worked out. She had started working with me at MIT, and when she started on this problem, she didn't get very far, because I was away on a sabbatical, and she was taking courses. So she moved out here and then really got started in research. Things were going very, very well, and she was doing wonderful work, very critical, and so on. But I didn't quite realize what was happening, and I must say, when I was at MIT, I really didn't have much appreciation of what made people tick, and why some people could get things done and other people couldn't. It was beginning to make me rather uncomfortable that Dorothy would do something and then have to come and explain the whole thing and talk about everything she was going to do. She had a boyfriend who was a graduate student working with Badger. And sometime, probably in the middle of her second year here, he committed suicide—shot himself, in a car up on Mount Wilson. That made things worse, of course, and by now I was much more a member of the national community, running around giving lectures all over the place. Dorothy was beginning to get depressed, although her work was going great. Then she started coming in, and instead of working, she'd come in to talk. I had other things to do, and yet I was very anxious that she would do well. I was on the spot, too, for having got her here and wanting her to work out well. Then one day she threatened to commit suicide. Well, at this point, I had no idea what to do; I had been keeping the whole business to myself. I went to Carl Niemann—he was chairman of the graduate committee—and I said, "Look, we've got a problem, and we have to do something about it." He sent me to John Weir, who was a psychiatrist or a psychologist here, counseling the students, and I went to talk to him about it. And I said, "You know, John, I don't know how to handle things like this." He said, "Well, let's see what we can do." But things kept getting worse and worse, and she was really emotionally upset. And her father came out, and that made it worse. So finally, one day we had to have her institutionalized for a while. And that was a terrible experience. That was awful!

Then came the problem about whether she should come back or not. Finally she came back, and she made a pretty good recovery; it wasn't great. Then she began to realize that things were really much more complicated—that there were really deep-seated problems, going back a long way. She didn't really do a lot more, but she finished her thesis all right. And then she went to UCLA and worked for Bill Young, whom I had worked for before, as a postdoc. And that went pretty well. But this whole business affected her, so finally she went into psychology, and she's become a professional counselor. And I guess she's done pretty well at it.

PRUD'HOMME: What a pity, though; because she started out with such talent in chemistry.

ROBERTS: I wasn't much help, because I didn't really understand what was happening for a long time. Anyway, she dropped out for maybe three or four months, and she got back on her feet. It was a terrible experience; and it certainly made me realize that I was powerless to deal with that kind of thing.

I finally began to realize that instead of trying to mess around with people's personal problems, the best way might be to encourage them and be more of a consultant than a research director. I started doing that, and I must say that after that, people turned out very much better. We had some real world leaders. I guess I was getting more into the Ross Robertson mold, of encouraging people to go out and get ahead of me. Later on, I had some amusing problems with one of the students, George M. Whitesides, who was I think the best student I ever had, and is now a professor at Harvard—before that he was a professor at MIT. George and I were almost competing in the lab from time to time, we thought so much along the same lines. I would be doing an experiment, and he'd come in and say, "Hey, I was doing that last week." [Laughter] But Whitesides and some others like him were phenomenal. It was at that stage when I began to appreciate what these people were doing, and how relatively less I was doing myself, that I stopped putting my name first on their papers.

PRUD'HOMME: It must have baffled some of them, when they were used to being coddled in their research, to suddenly be let go.

ROBERTS: Yes. And they learned and most responded very quickly. I had a lot of people from abroad, who came with their own fellowships and were used to being directed daily in their lab work. Some of them had a very hard time; others just sort of blossomed out; they'd say, "Gee, I've never had to do anything like this before. Now I know I can go back and do something." And I felt pretty good about that. But I had some pretty dismal failures along the line. I remember two of my graduate students—one of them was from Harvard, both were NSF fellows, and touted as the absolute best, but under this system they didn't amount to a hill of beans. One of them later came to life under a postdoctoral supervisor, who brought him up to speed or propped him up or something.

PRUD'HOMME: You had your students write monthly reports.

ROBERTS: Yes I did, and filed them.

PRUD'HOMME: And your reputation was that you never read them. But at the end, they realized that you'd read every single one.

ROBERTS: Oh, well, I don't know that they really thought that. Certainly, for the first few, they got them back corrected. But they may have never known that I read them after that. Later on, I went to a different system. I had one guy here a few years ago who absolutely refused to write the monthly reports. And I was very unhappy with him, because it dragged the whole group down. So I switched over to having meetings in my office, in which I took notes and they stood up to the board and talked about their stuff once a week. And that worked pretty well, too. They sat and talked, and I asked questions. I never really went around the lab, you know, every day, and talked to everybody.

PRUD'HOMME: Whitesides said that graduate studies in the department had a "theological quality." That the "intellectual atmosphere of the organic group was all at once elegant and refined and sophistic." And that Linus Pauling "was the top of the pyramid," but he was never there, but you were always there.

ROBERTS: [Laughter] That's not true, either. Maybe my presence was more often felt than Linus's. Well, Whitesides you have to know to appreciate. He's a great kidder. He has a marvelous reputation; if that guy doesn't get a Nobel Prize, I'm going to be astounded. He's amazing.

PRUD'HOMME: How did a graduate student get assigned to work under you?

ROBERTS: Well, actually it wasn't very complicated. There were a lot of things here that I liked. We didn't have the exam system we had at MIT, where you had two oral exams, and they were pretty tough. Once I remember, they had nineteen faculty members on one, examining one of Morton's PhD students, where a plus was equated to minus, and minus was equated to plus. It was a searing experience; it was embarrassing for everybody. But Cope was determined that he was not going to let someone come up for an exam in that kind of situation again. He really made a show out of it to set an example—right then the velvet glove was pretty thin.

Here, I found exams were much more relaxed; we had an oral exam called the candidacy exam. People didn't seem to feel quite so beset, and they would rise to their capabilities more when they didn't feel trampled down. Now I have heard, and I didn't have this feeling myself, maybe because I wasn't closely enough involved—but when Whitesides and some comparable students were in my group, they told me that some of the students felt intellectually outclassed.

Becoming a graduate student in the group was relatively easy. We had a system in which people were not preordained to work with anybody. We tried to set it up so that the first year the students had no commitment to anybody, but we wanted them to get into research as soon as they could. So we had a deal whereby we would give little talks to the entering graduate students as a group—we still do that—and tell them a little bit about the fields we're working in. And then the students would come around individually and talk to you and find out what specifically you'd like to have them do.

After my experience with nineteen students at MIT, I didn't feel I really wanted to have that many people at once again. I had this big lab here, on the end of the third floor of Crellin, that would hold about eight people. I had another little room across the hall, and it could take two, in a real squeeze. I used to have two or three postdocs. So I used space as a convenient limitation on personnel. Instead of saying I had to have more space for more students, I always felt that what I had was just about the right amount and I let that be my guide as to the number of students. Whitesides and Bruce Kover once made some point of it; they came around, and I told them that I didn't have any space, and there was just no way they were going to get in. And I don't know why they persisted, but I figured that anybody who really wasn't sure what he wanted to do would say, "OK, I'm not really wanted; I'll go and do something else." So when each came back and said, "Look, I'm really going to be mad. I came here to work with you," then I was willing and said, "OK, let's see what we can do." And we doubled people up on benches. We usually put two new people at a bench until a space became available, and we'd come to equilibrium again.

PRUD'HOMME: They had to be determined, in other words.

ROBERTS: Well, they had to be determined if there wasn't enough space. I was happy to get students. I always wanted to have the plate full of *willing* people, who wanted to do things I was interested in. But I didn't, at the same time, want to have indigestion. It seemed to me that many students didn't really know what they wanted to do, and they would be happier finding somebody who really wanted them rather than, say, pushing their way into some uncomfortable situation. They had to have a certain amount of willingness to put up with the crowded space. I don't think I tended to make it look very attractive, but we had a good group most of the time.

PRUD'HOMME: Did you ask your students to make a lot of their own equipment, as you had done? Or do you feel that that's important?

ROBERTS: Yes, I felt it was important, and as soon as I got here, I did what I had done at MIT; I set up a glassblowing bench, and encouraged people to learn how to use it. But they didn't really do it much. Somehow, equipment was more available, and I had a lot of stuff around that I had made myself. I still have a lot of cabinets full around the hall: Here are really nice fractionating columns, for instance. In the last twenty years, the style of research has been affected by many more things being available. You can buy many more pure chemicals, large quantities of pure acetone or whatever, you can buy these relatively cheaply. The stuff we bought earlier was commercial grade, often unsatisfactory. And then as techniques developed, we began to use less material. We developed things that we could use on droplets instead of with hundreds of cc's. So the style has changed, so that we're getting by using much less stuff. When we wanted to find out what a structure of a compound was when I was doing my PhD thesis, I would make ten or fifteen grams of stuff in a run, and then do some chemistry on it, make some derivatives and cut the molecules up and try to find out what kind of pieces they were made of. Now we can usually do that stuff in ten minutes, by just running it in a NMR spectrum. We have to worry about the structures still, in some cases, but most of the time, structure determination is done by physical methods. Very, very powerful, and in a much, much shorter time.

**Begin Tape 5, Side 1**

PRUD'HOMME: Could you talk about your writing, and then eventually go into your relationship with W. A. Benjamin? And more on your extra Caltech world—NSF, the American Chemical Society, NAS.

ROBERTS: Well, I don't know quite where to start.

PRUD'HOMME: OK, how did you come to write the *Basic Principles of Organic Chemistry* with Marjorie ?

ROBERTS: Well, that really gets back to W. A. Benjamin. In the good old days, in the sixties and before, book publishers used to come around looking for both texts and advanced book manuscripts. And so at some time or other, this bright, handsome young guy from McGraw-Hill came. It was W. A. Benjamin. His uncle was president, and later chairman of the board, and Bill was working for McGraw-Hill as chemistry editor. He was a very attractive guy. He was really quite a winner all over. He loved high living, he was a gourmet, a great talker, and really quite a cut above all the people I'd become acquainted with in the publishing business. Before that, most of the publishing of organic chemistry was done by John Wiley. But McGraw-Hill had had a few very successful chemistry books. One was G. N. Lewis's famous book on thermodynamics, and another was [Louis P.] Hammett's book on physical organic chemistry, which was tremendously influential. They were trying to get back into the field.

I talked to Benjamin, and he invited me to be on an editorial board that he was setting up for the international chemistry series. So we would have meetings occasionally in New York and talk about possibilities to get people to do things and so on. At about this time, I had been making a big splash in the nuclear magnetic resonance [NMR] field through the work I had done, and being willing to talk about it in a way that organic chemists could more or less understand. I gave forty lectures all around the country—NMR for the common man. And they were pretty good lectures; I still have the original slides—I drew and photographed them myself.

When we came to Caltech, Edwin Buchman moved out of his house for six weeks so my wife and children and I would have a place to live—a beautiful home in Altadena. He had a cabin up in Big Bear, and we were able to go up there for some years during the summers. I drew these NMR slides at Big Bear; my wife was a little upset that almost all the time I was up there, I was drawing those pictures. They were in color, and they were fantastic. I had this all well in my mind, and I decided to write a book about it. And then Benjamin came along, and he was definitely interested in getting that book for McGraw-Hill, although he also had under contract what turned out to be a marvelous book on NMR by Pople, Bernstein & Schneider. We lived then up in northwest Altadena; I had an old car that I had bought from Harvey Itano, the man who worked with Pauling on sickle-cell anemia, and I had a dictaphone; I'd dictate this book driving home, just working from the lectures, which I could run through in my mind. It wasn't a terribly long book. Of course, they wanted to redraw the illustrations completely, but in the process, we decided we were going to turn out the first four-color advanced book in chemistry. I don't know whether you've ever seen it; it's quite a tour de force, or so I thought then. [*Nuclear Magnetic Resonance*, McGraw-Hill, 1959]

PRUD'HOMME: [Looking at book] I see why you spent so much time drawing.

ROBERTS: Well, those were based on what I had, one way or another.

PRUD'HOMME: They're beautiful.

ROBERTS: They are! And it got pretty fair reviews, as a matter of fact. People were guarded, because, you know, it was written by somebody who was a nonspecialist and an organic chemist. But with color it was really a breakthrough.

PRUD'HOMME: The use of color is extraordinary, and the visual presentation.

ROBERTS: Yes, well the original slide of this figure looks quite a bit like that. Anyway, the book was a winner; it was cheap, by modern standards; it cost $7, which was a high price at the time. But I got really involved. I went and actually watched them print the first part of it. Here's one of the illustrations that you may have seen, made from the original drawing. I always had a love of illustration, which I picked up from Ross Robertson.

Anyway, that book came out.

PRUD'HOMME: That was in 1959?

ROBERTS: Yes. I was at Harvard as a visiting professor for six months. I remember going to the printers and watching them print the thing, and then coming back and finding Hardin McConnell's name was misspelled in the acknowledgments. I couldn't believe I could misspell his name and not find it in the proofreading. It turned out they'd dropped some of the type and reset it without letting me see the result. This particular volume is the second printing with the error corrected.

Anyway, that was fun, and I enjoyed writing it. I'd written a lot of papers, but I'd never written a book before. A publisher had approached us—Cope, Sheehan, and myself—to write a basic organic book when I was at MIT. We had great plans—I actually wrote a chapter. And those guys never wrote anything. I was pretty disappointed. And yet, I was so busy, and I didn't have any urgent desire to write books at that time. Guys like Woodward felt that people who wrote books were pretty low scum who couldn't write research papers. Even Bartlett, many, many years later, still had that feeling when we tried to get him to write a book. He finally did, but only because it was a very unusual book.

Anyway, I had preserved the first chapter I wrote for the Cope-Sheehan-Roberts effort and used it as a basis of the course that I started to give here. And Edwin kept urging me to get some notes together for the students. Howard Lucas's book was no longer the pacesetter that it had been, because people had picked up a lot of those ideas, but it was a good solid book. But in his second revision, Howard simply put in too much stuff; a lot of it was interesting, but trivia. Anyway, I taught my own course, and I had made some outlines for a book, but had never gone very far.

In the meantime, I remember going to an ACS meeting in Cleveland. Bill Benjamin had invited me to have breakfast with him; Bill always did things right—eggs benedict or something, in his hotel room. And Bill said, "Well, I've got an idea. I want to go out and start a book company. And I'd like to have you come in with me, right from the start." His idea was that he and I and a lawyer friend of his, and Konrad Bloch, who was a Nobel Prize winner at Harvard, and Don Hornig, who was the chairman of the department at Princeton, would be the guys to put the thing together. The five of us would be the board of directors. And he and I and the lawyer would be the promoters. Well, this was an entirely new idea to me. I had never thought of being in a company, except for consulting for DuPont and so on.

Bill was married to a really fine woman at that time, who was eager to do it, too. They were all set to start in their living room in Manhattan, and we had to raise some money. I thought it would be kind of fun to do. Bill knew that I was thinking about writing an organic text, and he wanted to get in on the ground floor on that.

I was intrigued and I talked to Edith about it. I'd have to invest some money myself to buy promoter's stock, which wasn't very much. So we set out to get this company going. Bill's a brilliant person—he's full of ideas, and a good salesperson. Some people he turns off because he's too smooth. But he knows the book business—even then, he'd had a lot of experience, even though he was quite young. What we didn't know about was starting the company. I was out hustling people to buy stock, probably illegally. We got incorporated in New York, and I helped him—I remember very vividly—working with the California Commission on Corporations, so we could sell stock in California. That was a traumatic experience, but it was interesting.

Bill had a tendency to fly off the handle, and for a while, I had him send copies of all his important letters to me, and Edith and I would go over them and rewrite them before he'd send them out. I spent a lot of time on that.

We got some amazing people. We used stock options as bait for many of them. [Richard] Feynman was involved. Some of his manuscripts were just collections of research papers and so on, but when you get Feynman to do it, that's not bad. We had an amazing library of people, in physics and chemistry. And then a man named David Pines, who is at the University of Illinois, got involved. Pines was very good with the physics people, and Bill and I worked on the chemists. Bill was very anxious to get something out, get something published, and at that time, I was interested in an aspect of NMR called spin-spin splitting. He decided that he wanted to publish a book on that, irrespective of what it was like. He did publish first a collection of meeting abstracts—he was desperate to get something out—and that was the first book. But this one was the first commissioned manuscript. [*Spin—Spin Splitting in High Resolution NMR Spectra*, 1961] This is the way it came out; that's what it looked like in those days. And it was pretty funny. This was the shortest book with the longest title—the title on the inside is not the same as the title on the cover [laughter], but not much different. This book I wrote in two weeks. I was giving some lectures at Fort Lewis College in Durango, Colorado, where the NSF had a summer program. They had Bartlett, Hammond, and me, and Stan Cristol, really a high-level bunch of people for the collection of liberal arts people that came. We were giving lectures in the morning, and then we had the afternoons off, and then we'd have an evening session. Everybody else went on hikes while I sat there every afternoon and wrote that damn book. And it was the most wonderful experience; there was this expansive window, looking over the mountains and the thunder clouds gathering every afternoon. So I cranked that out in jig time. I had a lot of work to do on the figures and so on afterward. But that book was interesting to me because it's an expansion of about ten pages in Pople, Bernstein & Schneider's book on NMR.

The one thing that we did do in our NMR books that was really new was to introduce the spectral problems. In other words, we gave people a spectrum and had them work out the structure that they corresponded to. And these really caught on. But as far as I know, these were the first books that ever had these kinds of problems in it. *Spin-Spin Splitting* also has some pretty fancy artwork. The artwork itself was all done by professionals. But Bill and I worked very closely to get this finished. Bill was very proud of how fast he could get the books out. This was done in a few months, which is very unusual for books.

Then it came down to doing the big book [*Basic Principles of Organic Chemistry*, 1964]. In the meantime, Dr. Caserio had arrived on the scene. She's English; she had been educated up to the bachelor's level in England, and then had gotten a PhD at Bryn Mawr in physical organic chemistry. She wrote me a letter, inquiring about a postdoctoral fellowship. That was one of the most unusual letters that I ever got from anybody who wanted a postdoctoral fellowship, asking if I had a job. She had some specific ideas and she actually knew what we had been working on. It was a very articulate, well-written letter. And I was immediately impressed. She came and worked for seven or eight years altogether. She married one of my postdocs, Caserio—her name was Becket when she came. Her husband worked in industry down toward Anaheim. He was a very good postdoc from UCLA, a good man in the lab, doing interesting stuff. So she stayed as a postdoc much longer than anybody had with me before, primarily because she wasn't very mobile about where she could go. And I had need for somebody like that. She was a very fine teacher, a good lecturer, and when I went on trips she would give lectures for me and cover courses and so on. And at some point, we decided we would write this book. That started an interesting period of collaboration. I was in a situation in which at least I had some veto power over what happened. I obviously had the lead role in this undertaking, but she's a very talented person, and she could really carry her end of the work. We would operate by sort of deciding the general plan we were going to follow. She would write a chapter and I would write a chapter; and then we would exchange chapters and work along in that kind of way, and it worked out very, very well. She carried a full load of research here, but she worked hard. She had the first of her children while she was here. Sturdivant was afraid she was going to have in the lab; she was working up to about the day before the boy came. Anyway, she did very well. Then she finally got a professorship at Irvine, and moved down there. We continued to work, almost all the time by mail. We seemed to be able to function by just exchanging chapters, and we'd get together and discuss matters maybe once every four or five months.

It took three years to write the first book; I had a lot of very amusing experiences with Benjamin in the process—being on the board of directors, trying to help him run the company, and at the same time having a conflict of interest that, look, I want this book to be just right. Well, he did too, but he knew he had to worry about the money part of it. And I wanted to have it be as fancy as possible. Before this book, I had been giving a course in molecular orbital calculations, and he wanted to get out a book on that. That was a very different kind of book. I don't know whether I worked on these books at the same time or not. Anyway, the molecular orbital calculations came along, and it was very different from one standpoint in that this was a book that *I* did.

PRUD'HOMME: You did the whole thing, I understand.

ROBERTS: I did the whole thing, right down to the dust jacket. All Bill did was to put the publisher's stuff on the back of the title page.

PRUD'HOMME: But by this time, you had taught yourself a tremendous amount about production, photography, the whole bit.

ROBERTS: Yes. I drew all the figures. This was one of those summers I spent at home in the den, drawing, drawing, redrawing everything. I'm not very happy with the way some of the drawings came out, but I kept trying. And it really came off; the book went through thirteen printings. So I'm now thinking about trying to revise it.

**JOHN D. ROBERTS**

**SESSION 4**

**March 21, 1985**

**Begin Tape 6, Side 1**

PRUD'HOMME: Last time we were talking about what you have written. You talked a bit about your relationship with William Benjamin, and how you'd encouraged him to leave McGraw-Hill and start on his own.

ROBERTS: Well, yes, but all I could say is that I encouraged him, in the sense that I thought it would be a good thing for him to do. And I think, on the whole, it was a marvelous experience for me, although I found being on the board of directors somewhat disturbing as a responsibility. My attorney finally advised me not to do that, so I got off. Bill didn't mind particularly, and I still acted as a consultant. This was also the period in which I was becoming division chairman [1963], and that made it much more difficult to keep involved.

I still marvel at the enormous number of books, and the enthusiasm that he generated. But he simply could not delegate authority, and he always had some new idea that he wanted to have done. In the final analysis, that was the problem with the company that we could not overcome. All the books I did with him were fun to do, although the large organic book was very difficult, because it took so long. Even the second edition took three years. He was disappointed because the first book was so big that he thought it would not go well on the marketplace, except for places like Caltech. Actually it did pretty well. So in order to make him happier we arranged some other things.

PRUD'HOMME: This is the *Basic Principles of Organic Chemistry* book?

ROBERTS: Yes. And we had negotiated a pretty tough royalty rate—15 percent of the list price, rather than the publisher's net. In addition, by getting the manuscript in by a given time, we got another 1 percent—that's a pretty heavy load. So I took one copy of *Basic Principles*, riding back and forth across the country in airplanes, and I cut more or less on the average of every third line or so to produce another book, called *Modern Organic Chemistry*. Of course, you had to have written the book to know what you could cut out without getting embarrassing results. Later on, we made arrangements for another book for which a man named Ross Stewart, who was at Vancouver, came in as a collaborator and principal reviser. I liked Ross Stewart as a coauthor. It was supposed to be a short book, but Ross really crammed in a lot of stuff. It was a rather dense book, but it was a lot shorter. One of the best things about this book was that my attorney suggested we give our children the copyright and that was great because it got our kids through Stanford.

About the time this book was finished, Bill sold the company. When the company was started, people on Wall Street looked on publishing as *the* growth industry. But they found that it didn't grow quite the way it was expected to. We had gone into a second financing, and the stock was going to sell for quite a bit. Everybody was hoping that there'd be wild enthusiasm. Well, just about the time the thing hit the market, business fell off, and the market fell off. The underwriters were pretty annoyed, and it was quite an unsatisfactory experience. So there was this real squeeze on money. But the company had this marvelous backlist. Bill had published many useful things, and a lot of original books. Then Addison-Wesley entered into the fray, and they finally gave Benjamin sort of a management buy-out contract. But by then the company had gone through some real transitions from three or four of us sitting in the living room planning the whole thing, to an office over a bowling alley on North Broadway, with the balls clickety-clacking down the alley, to some quarters on Park Avenue. I think probably fifty or sixty people were working for it about the time it got sold and Bill moved off to Paris.

PRUD'HOMME: When you're writing something, do you take off a chunk of time, or do you write continuously along with your other work? Or does it vary from book to book?

ROBERTS: Well, if I'm writing a book, I have a very set idea in my mind of what I'm going to put in the chapter. I believe that the brain has a lot of coprocessors in it. It seems to me that one segment is whirling away there and sorting out facts. When I make up my mind I'm going to write a particular chapter on a particular thing, it sort of gets into background in my mind and gets rolled around. And then suddenly, I come out with a first paragraph. When I sit down, I always say to myself, as long as I've got that first paragraph, I know I can go. And yet, I'm not really conscious of all the stuff that's been rolling around there until that paragraph comes. And then I can write pretty fast.

The normal way in which I wrote almost all my large projects would be to try to spend mornings at home, two or three a week. I usually write to the accompaniment of classical music, and write on yellow pads, longhand, until around noon. Then I come down and have lunch with the people here and then do things in the afternoon. When Dr. Caserio and I were exchanging chapters, we'd send out chapters for review. We found certain people in colleges, particularly a man named George Hall at Mount Holyoke, who was really a demon at reading everything and nitpicking and making suggestions.

PRUD'HOMME: He was your ultimate test, then.

ROBERTS: Yes. For the first edition, we actually did an almost complete syllabus in advance. We did things like this [showing bound original copy]. The formulas in the first edition were taken right out of the syllabus. And we used the syllabus here at Caltech. That was a monster job. There are two parts to most organic books: One part covers the basic groundwork, and the other has chapters on special topics that some people use and some don't. I enjoyed writing those specialty chapters; for some, particularly the more industrial parts, I could draw on my experiences with DuPont.

Most people don't realize that writing a book is only about half the work. You deal with editors who go through and edit it, and then with artists and designers, and all that goes with that. Then just as you think you are finished because you've run through the galley proofs and the page proofs, the editor calls and says the index has to be out right away. That can be a very big and boring job, but I developed some special techniques for doing indexes. I always did the index myself, because I think it has to be somebody who knows the book from start to finish. For the latest edition, I wrote a very elaborate computer program for getting the index out. It really was great.

PRUD'HOMME: It's a very logical thing to use a computer program for.

ROBERTS: The hard part about it for chemistry is that the names don't alphabetize easily, because they may be prefixed by a number, or have a number in the middle, or a Greek letter. I wrote an elaborate program to take care of that and I've used it for several indexes since then.

That’s not easy programming, but it worked quite successfully.

The last edition did not turn out to be so hot in the marketplace. I honestly believe it’s a great book. I’m really proud of it, and yet on the bookstands it’s not been a great success, although I know professors who say, “Gee, it’s great to prepare lectures out of.” When I write a book, I have to have the missionary spirit. I have to feel I’m going to do something that somebody hasn’t done before, cover things they haven’t covered before, or write about them in a different way than ever before. I wrote about resonance in that book in a way that Verner Schomaker and I had decided was really the best way to write about it. And I’ve never been able to get a peep out of my colleagues—I don’t mean here, I mean generally—about that. I don’t know whether they don’t appreciate it or whether they never read it or they think it’s what they knew all along or what. The thing that most people say sabotaged the book—and Marjorie was apprehensive about it—was that I insisted on using systematic nomenclature. Now, as organic chemistry expands—I was reading the other day that six million new compounds have been made since 1965, or something like that—molecules have to be named to be intelligible. Chemists called compounds acetone, acetic acid, alcohol, without there being any real systematization. They’re sort of nicknames. And as they got to know what the things were, they devised naming systems that are quite good. And so you have a system in which everybody talks about ethylene and propylene, but then they don’t say butylene anymore. They used to say butylene when I was young; but now they say butene, pentene, and so on. So you’ve got a problem; if you want to really make this really fit together for students. You want to say ethene, propene, butene, and so on. And then when you get down to the very simple compounds, people talk about formic acid with one carbon atom, they talk about acetic acid with two carbon atoms, and they talk about propionic acid, which fits with propane, because they each have three carbons; and then you talk about butyric acid which fits with butane. Well, formic and acetic in representing one and two carbon compounds don’t fit with methane and ethane. So from a student’s standpoint, you can use names like methanoic and ethanoic, propanoic, and so on. And as you get very high up in the acid series, there are names which have been devised like names for Pullman cars. They’re just crazy; there’s nothing systematic about it.

So I decided here was a chance to follow the example of *Chemical Abstracts,* which was dipping its toes into the water, trying to be systematic and trying to get rid of a lot of the old names, and really get people moving in what I hoped would be the right direction. [Laughter]

Well, it's going to be another 100 years, I'm afraid.

Well, there's a lot of chauvinism about nomenclature among chemists. They like to stick to their pet nomenclature systems just like they would want to stick to their personal names. I remember when I was working with Bill Young, and submitting manuscripts to the journals, and getting the referee reports. Young worked for years on two varieties of compounds which were butenyl derivatives and had been known from the early days as crotyl and methylvinylcarbinyl compounds. Strangely, although crotyl and methylvinylcarbinyl don't sound very closely related, they're actually isomeric, have the same number of carbons and hydrogens. Bill Young had been using these names for a long time. So when I sent a paper in I used these good old time-honored names. Finally, one referee came back hard and said, "It's high time you guys came into the modern world and give up those names." So I talked to Bartlett about it. And Bartlett said, "Gee, it's terrible what you guys are doing, using those old names. They are uninformative." And so I went and changed the names on the manuscript and sent it back in. And Young was a bit infuriated, naturally; "I've used those names for twenty years. What are you doing? Who said you could do that?" And I said, "Well, it seemed reasonable." And now those old names are no longer in use. Instead butenyl names are well-accepted. But it takes time.

The other thing that killed that book, I guess, more than anything, was that I decided to add a lot of material that was too challenging. I kept trying to find out from my colleagues what they wanted in a book, and they'd always say, "Well, gee, you really ought to put a little bit more on the field that I'm interested in." And so I finally decided I would give them a cafeteria, and let them pick and choose as they go down the line. What we got out of that was just a lot of flak that, well, the book is too heavy, it's too big and so on. It's never been a big success, but it's a nice, even if demanding, book. It's still selling a little.

PRUD'HOMME: Maybe it'll pick up; you don't know.

ROBERTS: We're actually thinking now about doing it over again. It's been eight or ten years, and we're thinking about having somebody else come in, like Ross Stewart, and revise it. Still Bill Benjamin is not around to help egg us on to get started. It was nice what he did just before he sold the company, when he arranged with the help of my wife to have a mammoth surprise party for me at the Athenaeum, celebrating my thirty years of research. Several people came from the East for the bash and they published a very nice Festschrift volume which reprinted all of my papers up to 1970.

PRUD'HOMME: I want to talk about your association with the National Science Foundation. You started out in 1957 as a member of the advisory panel for chemistry.

ROBERTS: That was a very interesting experience. When I came here to Caltech, I had transferred some support I had at MIT from the Office of Naval Research. The ONR asked very, very little of you. About this time, though, the guy who was running the ONR chemistry program, a man named Lewis Butz, had gotten the idea that the navy really ought to be doing in-house work rather than relying on all these flaky university professors to do things that the navy was interested in. Butz almost ruined their program, as a result of that. So they lost interest in my work at some fairly early stage, but everybody was hot about the possibilities which could be foreseen from the formation of the National Science Foundation. DuBridge was very much a leading figure in that.

The man running the chemistry program, Walter Kirner, had been associated with many people during the war who were working on war projects, particularly with Arthur Cope, and they were just getting into the business of peer review. I remember that Kirner sent me a proposal to look at. He said he had six reviews on it; and the grades ran from one to six. He wanted me to analyze it. So I pointed out that it was a pioneering sort of proposal by a person who was not terribly thoughtful, who was intuitive in a sense, and who just went into the lab and sort of looked for things. The reviewers who thought it was good were probably the same kind of people, and the people who believed in doing particular kinds of very planned research were the people that didn't like it.

A month or two later Kirner invited me to come to Washington and be on this panel. It seems to me it was about two weeks after the first *Sputnik* [October 1957], and there was a lot of excitement. People were really beginning to feel like the country ought to be doing something big in science. So I came on to the panel at that time. And Kirner was quite different from a lot of bureaucrats, and I don't mean that in a bad sense; you have to have dedicated bureaucrats. But Kirner ran the program really with the help of the advisory committee. He did not make decisions indiscriminately. There were several of us in the organic area. And we worked very,

very hard. Gosh, I remember when I stopped working for the NSF, and I decided I had to do something about all these proposals—I think I had eight feet of proposals that we ran through the shredder.

Allan Waterman was still head of the NSF at the time, and he came and talked to us. The early meetings were in the old Cosmos Club—the Dolley Madison house—across Lafayette Park from the White House It was really very, very exciting, the exposure to so many different kinds of things. Then, the foundation moved to the old Indian Affairs building; they were growing very fast, and kept reorganizing. I was in there longer than the normal period—I think five years on the chemistry panel, and you're only supposed to be on three. I was chairman for 1959-1960; and they sort of kept revising the thing and extending my term. Kirner was really a wonderful person. I got to know all the staff, too, very well.

Then, of course, they kept changing directors and kept expanding. A lot of the original people came in from ONR. I was elected to the National Academy in 1956, and I was interested in that. And then changes were taking place in the foundation. Kirner was an old-fashioned type of gentleman—kind to everybody. He didn't go about overselling things. Some of the other people were much more the pushy sales type, and for a while, proposal pressure was the big deal. If people were proposing to spend $10 million on a project, and in some other discipline they only have proposals to spend $5 million, they would ratio the budgets two to one between those disciplines. In this period, the mathematicians were proposing foundation support for everything—all kinds of assistantships, everything. The mathematicians were in fact getting more money than chemistry. The chemists on the panel began to do a lot of agitating because they felt that the chemical laboratory kind of work needed more dollar support than purely theoretical work. Finally, I made a presentation to the chemistry section of the National Academy about what was happening. [Glenn T.] Seaborg and a few other people were there, and I think I had a good effect.

Then, things began to change fairly rapidly. Some of the entrepreneurial types in the foundation were still generating proposal pressure, but the budgeting process became more sensible. I honestly don't know what is the right formula for dividing things up. I don't believe in proposal pressure. It would be nice to have a sense on how to make decisions based on scientific quality rather than proposal pressure, which is really just politics—and there was a lot of politics. Kirner and I got along so well; I was on his committee and later chairman, also for a while, of the mathematical and physical sciences divisional committee. That was a very interesting experience, because at this point I was dealing with people who were in many kinds of fields.

Harry Hess was one of the great geologists pushing the Mohole thing—this idea of drilling through the Earth's crust, through to a discontinuity that the seismologists discovered below the surface rock, where the crust would be thinnest at the bottom of the ocean. They proposed building a mammoth drilling rig and drilling through to get samples. This was going to be an absolutely major undertaking. It seems to have gotten under way in a sort of an off-hand manner by a bunch of guys over a few beers.

**Begin Tape 6, Side 2**

ROBERTS: The Mohole was very much in everybody's consciousness, and then, for the first time that I was really aware of, politics in a big, pork-barrel way, got mixed up in it. The chairman of the congressional subcommittee in charge of NSF appropriations was a man named Albert Thomas, who was very well known and influential. Albert Thomas was from Texas, and he was determined that the Mohole was going to use Texas contractors. He got his way. He died of cancer; I don't know whether that was what stopped the whole thing. There were a lot of technical problems, of course. You put a drill down through the water for a couple of miles at least, and then you've got to drill the hole, and then pull the drill out; and then there's the problem of how you're going to get back in. Not trivial problems. And how do you take a vessel and hold it in exact position two miles up from the bottom? They're doing a lot of that drilling now, and it's been very interesting, but then the technology wasn't so far along, and it was sort of a great step forward.

Chemistry finally began to do pretty well, and some of the NSF programs got pretty well established. I enjoyed it partly because it was the first time I'd really had much association with these upper-level guys. Walt Kirner got me into the Cosmos Club. We had lots of arguments about how you could evaluate projects and proposals. One thing that I innovated at one stage was in response to what they kept saying, "Look, you guys have to take the word of the physicists or whoever." I initiated a study, taking about twenty proposals, three or four from each of the areas of science in our committee's purview, and distributing them to the whole committee, and letting the committee members evaluate the proposals and come back and see how well we had done by looking at other people's proposals. It was interesting that we could agree very well on everything but math. Math was something that everybody who was not a mathematician just threw up their hands over. We had no way of telling whether a math proposal was a good one. Apparently the mathematicians are pretty casual about proposals, anyway. A thing would be handed in essentially on the back of an envelope and it seemed like it could be recognized by somebody as great math, but the non-mathematicians on that committee couldn't do it.

PRUD'HOMME: What do you think about the government financing faculty salaries, in effect?

ROBERTS: Well, when they started, I remember specifically the first time that a chemistry proposal came to the foundation for the advisory panel to deal with, involving faculty salary. It was a proposal for summer salary by Louis Fieser, one of the great organic chemists at Harvard during that period. Harvard professors were well known to be receiving rather high salaries, and here this guy was asking for two months of very high salary. The panel was pretty much aghast. Then it was basically taken out of our hands to make decisions about that, because the university presidents who were strong on the NSF board were saying that after all, this was part of the cost of doing research. "You don't want these people to go out and work in industry." I can't imagine Louis Fieser doing that. His salary was paid over twelve months; it wasn't as if he didn't get any money during the summer, and this was going to give him more. I guess [President] Kennedy really believed that universities ought to be supported in any way they could be supported, and he really didn't care what label you put on it. And so the great faculty salary thing got started. It's been a very difficult problem over the years, because it has involved different levels of enforcement by different panels. An advisory panel that doesn't believe in that kind of thing can maneuver in all kinds of ways to subvert the administrative fiat from above, that you've *got* to do it.

I'd say chemists didn't go very heavily for it for a long time, but when it became an important way of university financing, then virtually everybody got into the act. When I came to Caltech, there was a man named George Green who was vice president for business—the position that Mr. Morrisroe holds now. He was an excellent administrator, and a good man to talk with the faculty; a very pragmatic person. We got into a lot of problems over this issue, because George believed very firmly that the institute ought to be getting faculty salary money. I didn't, partly from my experience with the NSF. I used to argue, "Well, I think you're going to ruin this place in the final analysis, because we're going to be so dependent on the federal government." Our salaries were on a twelve-month basis, and that wasn't true at the other universities. He kept saying, "It's not your business to worry about that; you just get us some money and we'll be sure that it's properly used." Well, over a period of time, he wore me down; but I've never been very comfortable about it. I've always gotten what I thought I could reasonably get on my own.

I guess in the final analysis it'll turn out to be a bad thing. On the other hand, it certainly has helped a lot with the university finances. Much of the early postwar Caltech was built on money that the physicists brought in from AEC [Atomic Energy Commission] grants—overhead and faculty salaries—when other divisions were not getting much grant money. Part of the difficulty is that summer salaries are one thing, in principle, and academic-year salaries are another, in principle. It's true that someone who would not be receiving a summer salary, even though he got the same amount of money every month, *could* go out and get a job at DuPont or something, and give up his summer and earn money and thereby deprive the institution of all this research. I actually don't believe that many of them would do it. So you could argue, and it's been argued over and over again, that these guys have got to have summer salaries.

I think Caltech can make an argument—and we made an argument for a long time—that we could get academic-year salaries and really spread them out over the whole year; we weren't really thinking of it as summer salaries. We were thinking of it as money spread out over the whole year, and the other places were really doing something different. But the institutions got hooked on the faculty salary thing, and when you add the dollar total up, that amounts to a lot of science that's not going to be done. Of course, one could assume that enough money comes in to do all the worthwhile science, and then congressmen decide this is a reasonable add-on to financing universities. In other words, if we said we're not going to pay any summer salaries, the congressmen would immediately say, "OK, we'll just take all that money out." I don't know how that would work, and I don't know whether they really know much about the details of the NSF budgets. Now, it was certainly true, in the early days of the National Science Foundation, that the line-item stuff was scrutinized hard. The foundation and its bureaucracy amused me in one particular way. Albert Thomas would say something, and the whole NSF would quiver. But it was like quivering Jell-O, it quivered but didn't really move. [Laughter]

DuBridge could argue just unbelievably eloquently, and he did, over and over again, that the way the agencies were going was the right way. And yet, I don't like what has developed in the medical schools and other places where people have *de facto* tenure as long as they've got a grant. The Harvard faculty claims that at least they bit the bullet and can say, "We are not going to get academic-year salaries." But there's a certain amount of puffery in that, because institutions have different ways of figuring overhead. And what the Harvard professors either did not know or dismissed is that the overhead rate includes the contribution for faculty time spent in administration. So they were, in fact, getting paid part of academic-year salaries, but it wouldn't appear on their budgets in that way; but they, as a result, wound up with a larger overhead rate than they might have otherwise. This kind of thing was what led to the famous time-accounting controversy, because the auditors were saying, "OK, you're paying these guys' salaries out of overhead, but how do we know what they're doing?" And then they wanted people to keep track of their time. The academics went berserk, because the last thing they wanted to do was to be on a time-clock basis. And that battle raged. A lot depended on whose auditors you were being subjected to. The navy didn't seem to care much. And we had a very gentle system, where we'd sign every now and then, once every three or four months, that we had indeed spent the time. Other places expected something, I suppose, almost every week, because they were audited by the National Institutes of Health. So it's been a very difficult problem. And I'm worried about the universities, because salary contributions—not overhead, which is audited—are in effect general institutional support. They can be used for any purpose, not just for science and engineering; to hire another professor of English if desired.

PRUD'HOMME: The National Academy of Sciences, you were deeply involved in that, too.

ROBERTS: Yes. I was elected in 1956, which is almost thirty years ago. I was very fortunate, because being at Caltech and having been at MIT and UCLA, and being on lecture tours, I was elected pretty young. I'd hardly known of the National Academy, except that when I was at MIT, Cope had asked me to put together some stuff for him; he was going to nominate both Sheehan and me for the academy. And when I got out here, consciousness of the academy was not all that high either. But the academy did have a meeting here in 1955, I guess it was, in the fall. Carl Niemann asked me to give a paper at that, which I assume was a way of exposing me more to the members. Shortly after I was elected, Carl said I really should go to the meetings. And I haven't missed many meetings over the years—the spring meetings at least; the fall meetings never amounted to much and were finally abandoned in that form. And I liked it. It was another opportunity to get to know people in other fields. Its members are divided, as you might expect; there is a very large group that has absolutely nothing to do with the academy except vote for people in elections. Then there's the type that come once every four or five years; and usually a lot of people came, in the old days, especially when a luncheon or something was scheduled for the wives at the White House. The NSF was a natural connection for me—I started there about the same time I got elected to the academy. I remember very, very vividly going to one of the academy meetings just after Kennedy was elected, when the number of members was still relatively small. They used what is now called the Lecture Room for the members, and it was reasonably full. Kennedy came with Jerry [Jerome B.] Wiesner; and Kennedy gave a talk, and I was in the second row or so. I was really impressed with him—his vitality, what he had to say, his humor; he seemed to really be strong for science and education.

At some stage along the way, Bryce Crawford, who is now home secretary of the academy, was going to go to Japan for a sabbatical year, and so I took over from him as chairman of the Chemistry Section. One of the things I liked about the academy was it was one of the few places left in the country where a single person could really have an influence. An editor of the *Proceedings of the National Academy* and editor of some other journals, John Edsel, who was a biologist at Harvard, really helped to put a stop to the supersonic transport project in the earlier days. It was exciting to watch that happen. And yet the business of the academy, writing the reports and so on, is done by the National Research Council. Ted Cairns, who was director of the Central Research Department at DuPont, liked to work with the NRC; he used to say it was the business of the section to just elect members and not get involved with other issues. And I felt, particularly because of the crisis in funding, that the chemistry section ought to identify itself more with questions that were of interest to chemists. So I started a campaign and got them to give us some money, and I tried to arrange for the section to have some real business at its meeting in the fall, when they would talk about the election and so on.

Well, it worked a little. About this time, the academy was expanding into the social sciences and reorganizing its structure to have classes of members: physical science and mathematics, biology and biochemistry, engineering, medicine. And then Class V was economics. I remember Roger Adams, who was sort of the pope of organic chemistry at that time and had been very influential during the war, was absolutely convinced that taking in social science was going to ruin the academy. As they created new classes, there was a terrible pulling and hauling among members: “If you’re going to elect all these people, why can’t we elect more chemists than physicists?” And they went ahead and elected 100 members a year for a while. I’ve always felt that it was a mistake that numbers don’t solve the problems, and they dilute the influence of individuals. It was clear that the academy then and now is largely run by the president and so on, with a council, to do the things that they think are important, and not necessarily the things the members think are important.

We went through some very interesting crises. There was the [William] Shockley affair—devastating in many ways. There was the [Richard] Lewontin affair. There was also the whole argument about how big the membership was going to get. I was involved in sort of a crazy way, first as chairman of the Chemistry Section, and then I was often on the Class I Membership Committee, which helped to arrange the preference list of people who were to be voted on. I was not always a voting member of the committee, but often. I was secretary and then chairman of Class I, and that’s a pretty long association in minor offices that took care of the elections and that kind of thing. I was regarded as an election expert and protocol expert, but never really was involved very heavily in some of these other things. Later I was elected a member of the NAS Council.

I was secretary of Class I when Murph [Marvin L.] Goldberger was chairman, and that was an amusing experience. Murph wasn’t all that in tune to the nitty-gritty of ranking people for elections. He’d doodle away at his mathematical formulas and not really pay a lot of attention. A couple of times, he didn’t show up, and I acted as chairman. So I was involved with that part of the academy bureaucracy for a long time; and when I finally got on the council, that was a very different experience.

The Shockley business was interesting because Shockley, you know, is an old Caltech man. I had a lot of interaction with him. I really respected him as a physicist.

PRUD’HOMME: Describe the Shockley business.

ROBERTS: Well, you know Shockley was the guy who had taken the position that race was important in determining intelligence. And he was determined that the academy should endorse his views on this by making a study or whatever. So at academy meetings, Shockley was always put down as "other business." In the meantime, he would be talking to people in the halls and all this kind of stuff. The academy was intensely nervous about this whole business. There was a question of science involved, but there was also a terrible question of politics, and they tried every possible means to evade facing the issue. So Shockley would always try to work up in the order of transacting business, knowing that everybody would be gone by the time they got around to other business. In those days, people would come to the business meeting, and as soon as the election was over, they'd leave. Of course, the people wanted to settle the Shockley thing, but there was no way to fit it in, and they were worried about what was going to happen, too. And when Shockley would get the floor, you know, there'd be impassioned speeches on all sides, and then they would vote usually to table the thing.

I spent a lot of time talking with Shockley. I guess I expected scientists to be scientific about all kinds of matters of science. Shockley wasn't very scientific about this. And he was unwilling to have the proposition put up for a vote in a way that people could vote for or against it. He wanted to have the academy endorse his call for an investigation showing that his idea was right. He always phrased it in that way, right down almost to the end. At that point, some people said, "Look, we've been messing around with this thing too long." Shockley came up with a semi-reasonable proposal. And while they voted it down, the members turned around and commissioned the president to set up a committee under a man named Kingsley Davis to investigate Shockley's challenge. Kingsley Davis came out with a wonderfully ambiguous report, so that any side could claim they won. Kingsley is a very smart guy. And the thing finally straggled away in the sandy wastes. Anyway, it certainly was a messy business and took a lot of academy time and never wound up anywhere.

The Lewontin affair was different, a deep-seated thing that involved a lot of academy members. Lewontin had been at the University of Chicago when he was elected, and he objected violently to the idea that the academy was doing classified studies that the members didn't know about. He was a pretty persuasive guy, although very dogmatic in the way that he presented his arguments. And they worked out, in my view, a pretty good compromise—that the members could find out, and all the titles of the things should be unclassified, and so on. And I thought

Lewontin could have been a real power in the academy, but he chose to say it wasn't enough. And he just out and out resigned. But he really had a big effect on the academy; he was a vocal guy, and he had a good argument. And it was interesting to see how he scared them. A lot happened as a result, and he quit anyway.

**Begin Tape 7, Side 1**

PRUD'HOMME: How did you get interested in NMR?

ROBERTS: That is an amusing story, unfortunately spread out over a few years. I heard about NMR early, but it didn't take. When I was at MIT about 1949 or 1950, there was a visiting professor of chemistry at Harvard named Richard Ogg, who came from Stanford, which is one of the places where NMR was developed. Ogg was a brilliant and rather erratic person. He was striking looking, sort of like a knight out of armor. Although he was born in Colorado and had gone through Stanford to the PhD, he had spent a couple of years in England at Manchester and had a rather strong English flavor about him, even to his speech. He was a physical chemist, but he seemed to have an affinity for physical organic chemistry and was a popular speaker and participant at conferences on reaction mechanisms which he loved to discuss—too much for some, because he often had unsettling and difficult ideas about what was going on. I liked him very much indeed. Anyway, Ogg went to lunch with me in MIT's dreadful Walker Memorial and gave me a long lecture about this wonderful new technique for studying molecules involving radio waves, magnetic fields and resonance (not Pauling's kind), which was going to revolutionize chemistry. It made an impression on me because of his eloquence, but I couldn't understand enough to see how or where I could use it and so I missed the opportunity then. About the same time, an MIT physicist, Francis Bitter, heard I had a sample of methyl iodide enriched in C-13 and requested permission to borrow it. I was aghast at the idea. The stuff was very expensive; you had to have special permission to buy it, and it was very volatile. If the glass tube broke, it would vaporize at once and be gone. Bitter assured me he was not going to open it up, only wrap a coil around it, and then measure the magnetic moment of the C-13 nucleus. Just how, he didn't say. After a few days he came back with the tube, thanked me and said, "Well, we pushed out the nuclear moment by another four decimal places!" He was using NMR for the purpose, but he was not really measuring the nuclear moment as accurately as he thought, but a composite of the moment and the magnetic shielding peculiar to the carbon of methyl iodide.

PRUD'HOMME: I want to get started talking about your own work. We talked a little bit about where you first heard of NMR—over a lunch table at MIT. And then you were at DuPont. What happened at DuPont?

ROBERTS: Well, Du Pont was where I really got exposed to what was possible with organic structure analysis and reaction rates by a man named William Phillips, who's now vice president of Mallinckrodt. Then I got so excited about it that I came back here and talked to Pauling. Pauling's view surprised me in a way, because he must have known about what Yost had done in the field. And I actually didn't know what Yost had done; I found out about it later. But it was easy to be persuasive with my colleagues that NMR was going to help solve some of their problems. Pauling really felt that the division should have an expert in this field to take the best advantage of it. I saw the thing differently, because when I was at MIT they had put in a new infrared spectrometer at Harvard, and while they had infrared experts around—[E. Bright] Wilson and people like that—it was set up for use by the organic chemists. They could go in and put their samples in; they didn't have to ask Wilson how to do it. They then took the spectra away, and they could talk to people about the spectra as their own thing. I saw NMR in terms like that, not as the kind of thing that you had to have an expert to carry out for you, and then tell you what the results meant, in your context. I wanted results much faster than I knew they could be gotten that way.

And we did have at Caltech already, right across the hall from my office door, a little room for a PerkinElmer infrared spectrometer that I was delegated to watch from my office and be sure it was used right. We were using infrared very, very heavily for everything we were doing. So I saw NMR as a technique for organic chemists or chemists in general, to use in their own way. I kept beating on Pauling unsuccessfully, and, well, Davidson put up some money, and some of the others. And so we finally went to Pauling and said we had $10,000—the machine was going to cost $25,000. Pauling said there wasn't any money; there wasn't $15,000 around. So I suggested he ask the Board of Trustees. I guess in more or less the way he operated in Dorothy Semenow's case, he decided that he'd give it a whirl. Anyway, he got all the money from them, and we had $10,000 to get some auxiliary equipment. The Church lab was under construction then; Pauling had made a decision with Sturdivant's backing that they'd build the Church lab with a sub-basement. Sturdivant liked sub-basements; he liked quiet places. But they had a certain fixed sum of money to build a building. The biologists wanted something they could move into ready to do research, but Pauling said, "We're going to build our building with a sub-basement, even if we don't have anything in the rooms." And so they got a lab which was essentially empty. We were able to move the shops and a few things like that in, but the rest of the building was really bare.

The NMR equipment was going to be heavy because of the electromagnet, and, fearful that the floors in Crellin could not be strong enough, they let us be the first ones to move into the Church lab, and right over there, across the way, we got a very convenient NMR lab, not quite in with the rest of our stuff but reasonably close by, and I was in charge. When they came to install it, I really didn't know how the thing worked. And so the guy from Varian Associates came to install it—Jim Shoolery, who I think was one of Don Yost's PhDs—he worked with Don, anyway. Shoolery was a very good NMR man—still is. I looked over his shoulder, trying hard not to look too ignorant; the machine was covered with dials and lights and switches, really complicated. And the instruction book was frightful; it had the most marginal instructions I've ever seen for a major piece of instrumentation. Jim was adjusting the machine, and finally I said we were doing some stuff in the lab; I had this sample, and I wanted to see how much he could find out about it. So he put it in the machine, and he looked at the spectrum, and he said, "Oh, that's got a methyl group on a double bond." And I said, "*No*, it doesn't. Your spectra have really got that wrong." Well, it turned out Shoolery was right. And that was even more impressive; so I became even more eager to get in there and really figure out what was going on.

After Jim left, I did my best to learn how to run that thing myself without a good instruction book. The operation was pretty complicated, and the instrument very unstable. Furthermore, I didn't know anything about electronics. It was the first instrument Varian had installed in a university. And we did not really have an electronics shop. We had a super machine shop; Pauling really believed in that. But Sturdivant had not really entered the electronic age, and this NMR spectrometer was packed with vacuum tubes. We had troubles piled on troubles and so the Varian Associates people used to come down most weekends and work on it. One time I called them on Friday, and said, "Well, the whole thing has collapsed, and

I don't know what to do. I've tried everything." So Forrest Nelson, one of the designers, came down and messed around with it for a while, and in the process shorted out and almost blew up my voltmeter. After an hour, I said, "Well, I'm glad you didn't have to come down to replace a blown fuse." And he said, "A blown fuse! I forgot to check that." And that was what was wrong. [Laughter] It was still getting electricity, but one leg of the three-phase was gone. And he was embarrassed and I was embarrassed. It was a wonderful experience to deal with those Varian NMR people—they were all physicists and engineers.

It was not always like that with instrument companies. There was a Caltech graduate, named Howard Cary, who had worked with [Arnold] Beckman for a time. Cary set up his own instrument company, Applied Physics, in Pasadena. And I had bought a machine from Applied Physics to do carbon-14 analysis. And I remember when I had an urgent problem, I went over to the company and nobody was there but Howard Cary. And I said, "Look, the thing doesn't work." And he said, "Well, all you have to do is work out the problem from the circuit diagram in the instruction book." He was rather incensed that somebody would use his instrument who really didn't know what it was all about. That kind of attitude was slow in changing. But now people are not expected to really know all about each part of the instrument.

Anyway, the NMR thing really came along very fast, but I was having a very hard time understanding the theory of it. Toward the end of the year [1955], I remember there was an unbelievable blizzard. I was invited to Rochester to give some lectures, and they were all hot there to learn about NMR. I went on the train across the country; and I kept worrying about one really critical point that I did not understand. It was amusing that late the night before the lecture, somewhere, something clicked and I got the idea. I finally got up and said confidently, "Well, this is the way it happens." It was great. But it was sure a worry up to then. I'd no experience in electronics either, so I built a lot of Heathkits to better understand what was going on.

PRUD'HOMME: Did the physicists think that you were getting into their turf?

ROBERTS: Oh, no. I never had that feeling. Edward Purcell, who was a Nobel Prize winner for his work in NMR, came out and gave some physics seminars. I went to those; and as far as I could understand them, they were a revelation. Purcell was a marvelous teacher. And a lot of what I learned about the theory, I finally got from those lectures. Also a lot from Felix Bloch's original NMR papers; he was always wonderfully encouraging to me. And there was a man named George Pake, who's now a big-shot at Xerox, who worked in NMR very early; he'd written some fine elementary papers. And I began to get the idea of how the thing really functioned from all of these things.

The chemists and the physicists in NMR area have, I think on the whole, gotten along pretty well. I think the reason is that what the chemists have been interested in, has been quite a bit different from what the physicists are interested in. There was enough stuff for the physicists to do, stuff that they didn't need the chemists for. There wasn't much competition. Now, where the problems came with respect to competition, came primarily from people who were in the field early. A physical chemist at Illinois named [Herbert S.] Gutowsky, who was a graduate student with E. Bright Wilson at Harvard when I was there, was one of the very early people in chemical NMR. I think he never got his due, and I've always felt that Gutowsky should have gotten a Nobel Prize for his work in chemical NMR. However, he did share the Wolf Prize last year. The problem with Gutowsky was that he sort of got washed away by the fantastic influx of organic chemists into the field that he was one of the principal founders of—namely, NMR applications to organic chemistry.

I had to learn a lot more of the basis of NMR for the book that I wrote in Durango in 1960. Harden McConnell gave me some expert help. I didn't understand what "spin-spin splitting" was all about. Harden kept saying, "Well, you can understand it if you just read the available literature." But I didn't. Finally I got him to go through the mathematics in front of me on a Fourth of July morning, just as Bill McMillan had done with the molecular orbital theory. He did a great job. So that worked out just fine, subsequently.

PRUD'HOMME: You got a new spectrometer in 1975.

ROBERTS: Yes. Well, before that, actually. When we got our first spectrometer, we were turning out spectra every seven seconds. The stability of the machine was so bad that we would run off twenty or thirty, and we'd try to find two that were the same, because the electronics were not stable, the magnetic field wasn't stable, the temperature wasn't stable—everything was wiggling around. One day, I went up to Varian for some reason, and Jim Shoolery showed me the first spectra that they'd taken with something called a Super-Stabilizer, which permitted one to take spectra over minutes instead of seconds. And the difference in the spectra you got were just incredible. I was in Pauling's office the next morning, trying to get some more money. We put in an order so fast that we got serial number 1, and that served us very well for a long time. So we were cranking out stuff. And that really brought NMR into a different kind of age.

Then I went up there another time, and they said, "Well, we have a new, relatively inexpensive instrument coming out. Would you care to predict what it will be like?" I gave them a list of specifications for what I thought they could and should do. I got most of it right. I knew pretty much what the market wanted in the way of capabilities. Anyway, it was a dandy machine, because for the first time you did not need an expert to run it, you could essentially put a technician to work on it. It was called the A-60 and it had a lot of capabilities. Its problem was it was full of vacuum tubes. We got one and set it up over in Church. I spent an awful lot of time trying to keep that machine working and tuned up. All of the graduate students could use it. You could come in, drop a tube in, put in a sample, make a few simple adjustments, and run off a spectrum—provided everything worked. So I had charge of that. We did a lot with it.

We got another of the same series later with many extended capabilities. And then I really started fluorine NMR. And we started using fluorine NMR for conformational analysis. E. J. Corey at Harvard had urged me to do something about that, but his idea was to use compounds with one fluorine atom that were hard to make and relatively unstable. When I was consulting at DuPont, they discovered a really neat way, using sulfur tetrafluoride, of putting two fluorine atoms on one carbon atom, which was just where I wanted them. So that simplified the problem. It wasn't very easy to make the compounds, but they were stable when you got them. So we began to use that technique to get appropriate compounds to study conformational equilibrium and equilibration. A lot of what we did, then, I guess wasn't popular with the organic chemists, because we were using the fluorine as a tracer to find out what would happen if the fluorines weren't there. We sort of hypothesized that would give useful results. And indeed, most of the stuff we did really could be applied to the hydrogen compounds. In the process of working with these fluorine compounds, we discovered—a little ahead of Phillips, I guess—two important phenomena: that we can measure the rates of rotation around single bonds in alkane derivatives, particularly when we used fluorine as a tracer. And then later we also discovered some very unusual effects which depended on molecular asymmetry, which is the NMR effects of asymmetry in molecules. And those were very productive kinds of things.

Well, the fluorine business began to run down, and around 1964-65 I began to start to think about natural-abundance carbon NMR. I tried some experiments with our old, original spectrometer with carbon, and I was not pleased at all with the results. A man named [Paul] Lauterbur, who was then somewhere in New York and later at Stony Brook, had run carbon NMR in a very important experiment, which was not a very practical way of getting at it. The important thing was he had been able to run it on carbon at its natural-abundance level. The problem with carbon-13 is there's not much of it present in molecules; and carbon-12 doesn't give any NMR signals. You get about 1.1 percent of carbon-13 in carbon normally. It gives a relatively weak signal, and there's not much of it, and that makes it very tough. But that's what we wanted to do, to be able to run carbon the same way you could run hydrogen. Now, it was well understood in principle that you could operate an NMR machine in the same way that you take a photograph in faint light; you can use a longer time exposure in order to get a better signal. But when you get the signal, it's like listening to a radio with static. And so you have to decide, when somebody's saying something, whether it's making sense. You can ask them to speak more slowly, which sometimes helps, or you can ask them to say it again. In either case, you're improving the signal-to-noise ratio by increasing the time of observation. So the original idea was that if you run through the spectrum exceedingly slowly, you can increase the time of observation and improve the signal noise. The other way is to run it over and over again, and pile up the signals. But that involves a lot of coordination. You can't have drift, and you can't have other things. But it has a tremendous advantage that you can run the thing for five minutes, and if it's not good enough, you run it for half an hour, or you can run it for two hours, or whatever. The other way, you're committed to a very slow process. Some people were doing it for twenty—four hours. The problem was that everything was drifting. And we didn't then have the wherewithal to keep things steady.

# JOHN D. ROBERTS

## SESSION 5

## March 25, 1985

**Begin Tape 7, Side 2**

PRUD'HOMME: We were talking about NMR. And you were talking about how to get people to build spectrometers that were stable.

ROBERTS: Yes. We were having grave difficulties running something that you might say was the equivalent of taking a time exposure with film that was not sensitive. You can help this a lot by doing what's called time-averaging, when the noise is random but the signal is coherent. You keep adding up the signal over many, many scans; the random noise then tends to cancel out and the coherent signal tends to grow. That involved a special apparatus which we didn't have at the time. I had been trying with a number of people to devise ways of getting past this obstacle. One was to do more time-averaging; another one was to use a technique for sensitivity enhancement on the basis of what is called electron double resonance. Richards, now Sir Rex Richards, had worked on electron double resonance in England. And he had been so enthusiastic about that when I met him, that I negotiated with one of Harden McConnell's people, a very capable guy, who did electron resonance work here and to help me set up a spectrometer to use electron double resonance. But then *Scientific American* ran an ad for a new Hewlett-Packard instrument called a frequency synthesizer, which seemed to have all the characteristics that we wanted: a super stable instrument with which one could generate different frequencies by simply pushing buttons, or the buttons could be pushed electrically. I had a feeling that this was really the right way to build a spectrometer and would be easier than electron double resonance. The people at Varian were hesitant at first; in fact, they said it wouldn't work—phase problems—but I bought a frequency synthesizer anyway. Then they decided they would try to incorporate my synthesizer into a spectrometer and test it, and they did a marvelous job, actually. I'd already gotten the money from the National Institutes of Health to put something together so we could go ahead with Varian. And the result was a great technical triumph, an enormous NMR machine, about fifteen feet long. It was just a fantastic instrument. And it really worked for natural-abundance carbon-13 NMR spectra.

Just that summer, Frank Weigert came from MIT as a potential graduate student, and he just took that whole business over. His PhD work was done in two years and nine months, and involved fifteen publications, a remarkable student who has done remarkable work at DuPont's Central Research since—not on NMR, but on developing new and imaginative industrial processes. Anyway, Frank helped to get that program off to a great start. We showed a lot of people that this was a really useful new structural technique in the organic laboratory. I can't claim a lot of credit for originality here, but we did some great development work and demonstrated some new tricks that were very useful in working on the structures of natural products. It was the first time C-13 NMR had ever been really applied to large natural-product molecules. So we had that field all to ourselves for a while. With carbohydrates and steroids, it was a very exciting time. Steroids have some twenty-seven or so carbon atoms, all of them different. And we were getting twenty-six different resonances—two of them were on top of one another. I remember Frank Weigert, when he left, just as this really got going, saying, "Well, you'll never work out what carbons all those resonances belong to for at least another five years." But we had some good organic chemists around, and they solved that business in about six months. It was one of the best things we did in the NMR area.

Then everybody started to get C-13 NMR machines and use them, and we had to look for something else. During my lifetime as a chemist, I had always been a little intrigued by nitrogen chemistry, but things never quite worked out the way I expected. And then, even before we were into C-13 NMR, I got this idea that we should try to use nitrogen-15 NMR, which was at least 200 times harder to do than carbon-13 NMR. And actually, we did quite a bit with isotopically-enriched N-15 compounds, but this was very slow and very hard because labeling can be very difficult in chemistry. What we needed was natural-abundance N-15, and this took, again, another step forward in technical instrumental capabilities.

So for quite a long time carbon-13 NMR got better and better, and the potential for nitrogen improved so much that I put in a proposal with NSF to get a dedicated N-15 spectrometer. It took them about three years to decide that it was worthwhile. Perhaps, because of our work with nitrogen earlier, not at the natural-abundance level, most people might think that it would be an obvious extension of the art. But they finally gave us some money to get such a machine. And then we dickered hard with the various companies on different concepts of how we could make N-15 NMR work better. Finally, David Grant, who's a professor at the

University of Utah, suggested that we use large samples—the milk-bottle approach. That was a very good suggestion. While I wasn't very confident that magnets to take large samples could be made with the desired necessary one part in $10^8$ magnetic-field homogeneity, Bruker was able to supply a spectrometer which met our specifications and indeed was even better than we hoped for. Again, I had very good people; I was fortunate to have one of [John H.] Richards' graduate students, Richard Moon, come as a postdoc and help get our N-15 NMR going. And I was also lucky to have a couple of other motivated people, and we started doing really big molecules, even enzymes, right away. That program developed very, very nicely, especially because of a Japanese woman, Keiko Kanamori, the wife of one of our professors in geophysics, who came in as a graduate student. She did her undergraduate work, after having two children, at LA State College. She turned out to be just fantastic. And so we had a good program going in N-15 NMR for quite a long time; I give her the credit for keeping it going, particularly during the time I was provost.

PRUD'HOMME: What are you like to work with?

ROBERTS: Probably exasperating. [Laughter] Well, as I told you, I found that directing students' research closely wasn't all that successful. And so I developed into a consultant, primarily, and money-raiser; and I spent a lot of time working on papers and getting the word around. Now, I've never really been able to get from anybody the real candid story of what they thought about it. George Whitesides is a humorist; whether what he wrote in my thirty-year Festschrift volume really represents everybody, I have no idea. I know I had some people who were extremely frustrated, and others who just blossomed completely. And Keiko Kanamori is one who, in my view, really blossomed. When she came, she really wanted to be a biochemist. She would come in at various junctures and ask a lot of questions and so on, and then when she went out, she did what she thought was important to do. She was never a slave in any sense of the word.

I guess people respond in different ways. But I'm sure many people felt they didn't have enough direct personal interactions. But you know, actually, I think the Dorothy Semenow episode really got me worried about getting too involved with people's personal problems, because those things are usually evolved in a way in which you can't really deal with them very effectively.

PRUD'HOMME: David Morrisroe said that you expect considerably more from people than they think they can do. And that this has a very positive effect. And you "shore it up with a certain tenacity in follow-up that pretty much assures achievement."

ROBERTS: Well, I like to have people do more than they think they can do, that's right. There's a woman here now who came from England; her father is a leading NMR man there. She came as a Boswell Fellow to work at the Huntington Medical Research Institute, and I'm her sponsor at Caltech. She came into a really difficult situation, although we told her in advance that the lab wasn't set up, and equipment was not all there. She was in here today. She was worried that she won't have enough money to go for research, and I was trying to convince her that for the time she's been here and in a situation where she doesn't really have an active research supervisor to do all this stuff for her, she's done unbelievably well. And she shouldn't be worrying so much about the future because, while she's worried that people aren't going to take care of her, she has to remember that they've got an enormous investment in her. We're putting a lot of money into the operation. And if that investment doesn't pay off, they're not going to look very good. I think she's risen to this occasion unbelievably well. But she's still uncertain about it, and I don't blame her; it's a tough business. But when you see the lab that she's got now and the progress she's making—that's just incredible.

PRUD'HOMME: When you became chairman of the division, what did you see was needed in the way of changes?

ROBERTS: Well, Pauling really, I think, left things in pretty good shape. And Swift was a very logical successor. Swift was a fine scientist; very well loved by the students, but he didn't have much of a national reputation. He was sort of doing a caretaker's job, but doing it very well. But we were getting a lot of pressure to broaden the division. Organic was slender. When I came, there was only Niemann, Zechmeister, and Buchman, just four of us. The physical chem group had been small, and Badger and people like that were getting on. So there was a lot of pressure to enlarge.

Harden McConnell was a very, very strong acquisition. He had been a graduate student here. At first, I wasn't particularly in favor of getting him back. I remember the first time he came down, when he was at Shell, he was telling us about some fancy work that he was doing, and Verner Schomaker and I were both very skeptical about it. And yet McConnell was doing quite pioneering work, and by the second time he came down, I was really impressed. He was doing very fine work in NMR then. It was stuff that I was just getting into and I understood. And he could provide a lot for the division. So we got him on the staff. He was very strong for broadening the division and getting into more than just the things Pauling was interested in. During Swift's chairmanship, we made a lot of appointments. And when I became division chairman, McConnell had been offered a job at Stanford, and when they asked me to be division chairman, I had to try to talk him out of it, not very successfully. You couldn't believe how crowded it was then. We didn't have the Noyes lab at all. The Church lab was occupied by the X-ray people, and Gates was jammed with people; every inch of space had been taken up. Robinson and Davidson were doing physical chemistry then, and the teaching labs were there. The upper floors here were the organic teaching labs, and so a lot of the space that we now have for research was really teaching space, too. Shortly before I became division chairman, the NSF had announced a matching program to build buildings. They were willing to entertain the prospect not only of building a building but also of fixing up the space that was vacated. So Sturdivant and I got together. He drew up the plans, and I wrote up the proposal to the NSF. I guess the administration was pretty well worried about the commitment that the institute had to make for half of the funds, because they didn't have a donor in sight. But they did let us get the proposal in, deciding they needed to make that commitment. The NSF sent out some people, and we took them around through the labs. In McConnell's lab, a board being used as a writing desk had to be moved up out of the way so they could walk through. One of the visitors nudged me on the way out and said, "Well, you didn't really have to put on quite that kind of a show." [Laughter] I think we got essentially everything they asked for, including a lot of money to fix up the old space, which was really remarkable. I think it was a million-and-a-half or so that the institute had to commit. I tried all kinds of things to raise more money. I got some money from DuPont.

PRUD'HOMME: You went around to Upjohn and Kodak, too, didn't you?

ROBERTS: Yes. I wasn't very successful at this, but anyway, we did get some money from these other people. And I think the institute's promise of doing that made a big difference to the people who were already here. It was a period in which universities were really expanding, and they all wanted to have superstar people. And we had a good collection of them. That was when I was working on the first edition of *Basic Principles*, coming right up to the wire on the index. And I remember being in the division chairman's office and spending two hours on the phone, correcting index proofs over the phone to New York. Dr. Caserio was here then, and she was very, very helpful, helping in running the lab and so on. She had a good way about her, which Keiko has too. Neither one of them really tried to act as first lieutenant; they acted to be available to help, and never to tried to direct. So that kept relations among our people in the lab very, very good. Both of them were tremendously well liked. But the division really was in trouble at that point.

PRUD'HOMME: Because of the people leaving?

ROBERTS: Well, the potential of people leaving. I think the only one we lost then was Harden McConnell. It was during the time when the smog was very bad, and Stanford looked pretty green. I hated to lose him, because I enjoyed him and his science.

PRUD'HOMME: What kind of faculty changes did you want to make or did you feel were needed?

ROBERTS: Well, we didn't have any inorganic chemistry. Yost had retired and Swift was retiring. And he represented analytical chemistry. Swift actually proposed to the faculty that it was time to get rid of the analytical chemistry course, put the best features of it in other courses and move organic chemistry down to the sophomore year. I think I was chairman of the committee that worked on that problem; it was a chemistry division problem, of course. In the process, we lost a lot of emphasis on inorganic chemistry in the undergraduate curriculum, and it was a field which needed to be revived at Caltech. Since the war, people had done some very good stuff in inorganic chemistry, and there were lots of books about the subject; but those in the field didn't really try to tie it together much or try to understand the details of the mechanisms of the inorganic reactions. However, a young man at Chicago named Henry Taube had really started to move in the direction of getting organic chemists to study mechanisms in inorganic chemistry in something like the same way the organic chemists were; to really use all the tools they could use. And this was beginning to come on very fast. But we needed an inorganic chemist and Bill Benjamin kept telling me, Harry Gray is your man. Harry, who was then at Columbia, had written a book for Bill and I was asked to review it and I covered it with red-ink corrections, but Harry took it well. Finally, we got him to come out here for a while as visiting professor. George Hammond was here, and they got along very well. So we offered a job to Harry. It was a great disappointment to me as division chairman, when Harry turned us down, but later George somehow talked Harry into it. He immediately stepped in and got the freshman chemistry going well, and really got inorganic chemistry here. So now it's one of the most vital parts of the division.

PRUD'HOMME: Did he bring a group with him, as you had when you came?

ROBERTS: Yes, a few, but he was more or less by himself for a while. So that was a big need for the division; Harry had the personality and so on to really make inorganic sing here, and we needed somebody like that.

**Begin Tape 8, Side 1**

ROBERTS: [Continuing] I think I learned at some stage that I had to let other people really help; by letting them get the feeling they were accomplishing things, and I found that it helped them to work very hard. We had some great people in the office. We got Lea Sterrett, who was a marvelous person for the division for many, many years. She came when I was chairman and rapidly took over a lot of the administrative work. Sturdivant and I had some battles, particularly about secretaries. Sturdivant's idea was that you put a bunch of very competent women typists in a room all by themselves, and then you put in handwritten manuscripts through one slot in the wall, and they came typed out of another slot like sticks of chewing gum. [Laughter] It sort of reminded me of the cigarette factory in *Carmen*. However, the division's faculty wanted a more personalized kind of service; they wanted people to help them do their filing and that kind of thing. You couldn't get that out of a secretarial pool. When Harry came, it wasn't long before Sturdivant and I reached a showdown over how the secretarial work would get done. I won, but the cost was high. After this and the struggle to get the Noyes Lab going, I began to get sort of tired of the chairmanship.

There was a lot involved. I really enjoyed dealing with DuBridge and Bacher and those people in the division chairmen meetings on matters of institute policy. But I was particularly depressed by the fact that just as I got everything going on the Noyes lab, the faculty started talking about wanting another building, and they wanted us to redo Crellin, make it the equivalent of Noyes. However, Sturdivant was convinced nothing could be done with Crellin without building a shell around it, six feet wide, to handle the changes needed for the utilities, air-conditioning, and so on. The windows would stay as they were but be set back six feet from the outside wall. Then there was a plan to build another building across the end of Crellin, which did not fly. The problem there was related to the original planning of Noyes. I wanted to have the building be here on this side of San Pasqual, so we could all be closely connected to one another. The trustees absolutely refused to do that, so we went across San Pasqual. It was amusing that the institute spent a lot of money on the design of the outside, and the trustees were incensed that Sturdivant had gone ahead and produced a design of the inside. They wanted to have their architect come use his own imagination both inside and outside. Well, it worked out that the architect wound up with Sturdivant's plan anyway; Sturdivant knew his stuff and was persuasive. I was at a cocktail party a few weeks ago where the architect came up and introduced himself. He sure remembered Sturdivant. But of course, now, without Sturdivant to keep things in the way he intended them, much of the building has been diverted to uses Sturdivant did not plan. For example, the little low building in front was to be the ultimate secretarial pool; well, now it is all research space. I guess I had to let Sturdivant and the architect do that part of it. But I did insist that the lecture rooms be very good. And I think they did a good job there.

PRUD'HOMME: You also had Aron Kuppermann, and Richard Dickerson, and Sunney Chan.

ROBERTS: Yes. A lot of people came in around that period.

PRUD'HOMME: And then when Niemann died, you tried to appoint [Howard W.] Whitlock in organic, who was only twenty-eight.

ROBERTS: Well, Whitlock was a very bright guy. He looked like he was going to be a world leader at that point. But, instead we got George Hammond and later Bob Ireland, both of whom were great additions. Anyway, as I said, the thought of doing another building was just killing me. [Laughter] I had told DuBridge and Bacher I wanted out; and they kept saying, "Well, we'll think about it," and so on. Then my wife introduced the children to skiing, and by the next Christmas they were really getting pretty good. So we went to Aspen, and the day before Christmas, the day before we were supposed to come home, I broke my leg. That made me feel maybe this was a good way to get out of the chairmanship. [Laughter] I almost knew that I was going to break my leg. I didn't particularly like it when it happened; but it did give me an excuse to get somebody in as acting chairman. Well, they got George Hammond in. When I finally came back to work, I remember giving the rest of my Chem 41 course. I couldn't get up to the blackboard because I had a cast up to my hip, so I used a viewgraph instead. I think George wanted to be chairman; by that time, he was in a position where he wanted to get his hands on the levers. I guess he didn't know levers are elusive; sometimes when you get your hands on them, you find they aren't connected to anything! [Laughter]

PRUD'HOMME: Did you make course changes?

ROBERTS: As chairman? Well, I really didn't do much myself. George and Harry had gone off on a big kick to revitalize chemistry, particularly George, who had the feeling that everything we were doing was sort of illogical. He thought we should talk first about structure and then about reactions, and after we put all of that together, we could talk about synthesis. George got quite a hearing in those days. He was a national figure; he was on the American Chemical Society Education Committee, and prominent in the NSF. The faculty, on the other hand, was pretty conservative, and they weren't all that convinced. But they were willing to let George and Harry take on Chem 2, which is different from Chem 1, in that the students with advanced preparation are allowed to take Chem 2. So George and Harry taught Chem 2. There were maybe twenty-five students, and the two of them worked out together a new way of teaching the course. And then George was very anxious to see whether organic chemistry could be taught in the same kind of sequence of structure, reactions, and synthesis. Bill Benjamin was encouraging this trend by wanting to get in the publishing part of it. I agreed to teach organic chemistry that way; I taught probably two quarters of it, and I didn't like it. The problem was that it focused too much on structure, then too much on reaction. The students got bored. And the Hammond curriculum never really took off, although a lot of people tried it. And Benjamin was willing to help finance people trying it.

PRUD'HOMME: You sent some wonderful memos to Bob Bacher, about how you spent your time. You asked to be relieved of your chairmanship in February, in May, in July. They gave you a raise at one point, I think, to keep you quiet. And by July of 1966, you said, "I should like to reiterate my, by now, shrill request" to be relieved of these duties. Do you remember that?

ROBERTS: [Laughter] Yes.

PRUD'HOMME: Did you figure that with the courses and the research and the government work and lectureships and national meetings, you had no time to do anything of your own?

ROBERTS: Well, then and now, I can still get in a lot of work—in airports. It doesn't take much activation, and I'm not easily distracted.

PRUD'HOMME: Tell me about DuBridge. Did you like DuBridge?

ROBERTS: Oh, immensely. I was quite impressed with him.

PRUD'HOMME: And Bacher?

ROBERTS: Yes, and Bacher, too. The two of them were a marvelous combination.

PRUD'HOMME: In what sense?

ROBERTS: DuBridge was the ideal front man; he was a marvelous public speaker. Apparently he was not when he came. I keep hearing reports from old grads saying, "Gee, DuBridge was a terrible speaker when he came." Well, by the time I got around to hearing him, I just loved to hear him talk. He used to write out his speeches in longhand. I'd sit on a plane with him sometimes and talk to him about his speeches, because I was really envious of the way that man could get up and give a talk. He gave one to an Associates luncheon that they had in honor of him a few months ago. When he talked he knew, like most old people don't, when to stop. On this occasion, he said maybe for three or four minutes how he felt about things—life, Caltech, and so on. God, it was really impressive! A marvelous statement!

As president, he was a good speaker. And he was also an interesting administrator. We used to have our meetings over in his office in Throop. We'd get around a small conference table. He would sit at one end and Bacher would sit at the other end. Bacher really ran the academic show, and DuBridge was willing to let him. And DuBridge ran the other end, and Bacher was willing to let him. They were very close. They complemented each other, and apparently they spent a lot of time together. You used to see them at lunch together at the Athenaeum a lot. It was nice the way they divided the job and the way they worked. DuBridge would come to meetings and he'd bring an old, beaten-up notebook, where he kept a record of what went on. He'd have already written in pencil maybe four or five items for the agenda. Nothing was really worked out very much in advance, except that I'm sure that he and Bacher had talked about it a lot. So then he would start down the list and throw out some ideas, and we'd discuss them.

It was an interesting group at that time. Bob Sharp was there, and Ray Owen and Fred Lindvall and Hallett Smith. And Bob Gilmore, who was the vice president for finance at that time. And Carl Anderson. I'd never known Carl Anderson before. It seemed to me I always sat in the same place, next to DuBridge. I probably did an awful lot more talking than the rest of the guys. Some of them didn't respond very well to that. Hallett Smith, particularly, did not seem to like my attitude and things that I wanted to press on. Most of the others were reasonably quiet. I think Bob Sharp and I agreed a lot on what was going on. Carl Anderson never said very much, until we got around to physics. Ray Owen was always very solicitous—still is—of the students and their concerns. And Hallett was just terribly disorganized, it seemed to me. Every year they would settle the salaries. First, they would have to get their lists approved by Bacher. This was a fascinating business, because Bacher liked to be pretty tough; he'd put on his hard face in these negotiations. You knew, once you'd gotten a smile, you were OK. I found the way to get to Bacher was to wait him out. First he'd give you a lecture. Then you'd get down to business, and if he said no, and you really felt that you had to have it, the only thing to do was to wait. And he'd go back to more lecturing. Then you had to decide whether it was worth waiting to get the smile. Usually you could get what you wanted if you were rather reasonable, and it was a good idea to negotiate things with Bacher ahead of time. Anyway, with the salaries you'd come in with your list of suggestions; and the list would be passed around. Well, Hallett Smith would never bring a list. He'd have his proposals written on the back of an envelope, and he'd mumble it, nobody could see it. It was really funny to watch the group work.

They were informal. They expected everybody was going to run their own divisions. Money was no real problem. I remember they made some decision about doing something in the way of benefits. And DuBridge said to Gilmore, “Well, we’ve got money for that, haven’t we?” And Gilmore looked, “Oh, yes, yes.” And they did well.

And the thing I liked about DuBridge was that he responded to what it was the faculty themselves wanted to do. He wanted to get the best out of them. And if you think I was busy, DuBridge was just unbelievable in all the things he got mixed up in. You know, he was running the California Air Pollution Foundation, he was president of KCET for a while, he was in the NSF and on the President’s Science Advisory Committee. And Bacher was much more the homebody who kept a good grip on things. Bacher, too, never seemed to push his ideas on people for their programs. But he tried to keep people reasonable. I always had the feeling that maybe I got more for the division than our share, somewhat the same way that Pauling did. This happened because, in the first place, Sturdivant was buddy-buddy with the physical plant. He could deal with them, always had good ideas, and really respected them. And that was a big help. And the other thing was that DuBridge and Bacher knew almost from day one that I was willing to quit being chairman any day. But when they made the commitment for the Noyes lab, that was something that really had to be finished. I guess George finally became chairman about the time Noyes was finished, and ran the celebration for the opening of it.

PRUD’HOMME: What do you think was your chief accomplishment as chairman?

ROBERTS: We got some good people in. We at least arrested the potential erosion of the good people. The Noyes lab was certainly a great achievement, to have that going. On the whole, those were the things that I remember the most. I think we set up a pretty good administrative system. Getting Lea Sterrett in here was certainly a very important thing for the division.

Institute-wise, you see, the chairman is very much a sort of a general officer, too, in that you have a responsibility for the institute as a whole. I always took that fairly seriously, and I was really pleased to find that DuBridge and Bacher really wanted to talk about things and be sure they had the faculty point of view. I think this was one of Bacher’s strengths, that he would never go ahead with anything without being sure first that the faculty were not going to get up on their hind legs. That's been one of the problems we've had in later years, that people haven't always done that. I used to hear a lot about how Bacher and the chairman of the board, Jim Page, had fought to keep Pauling from being fired by the trustees. It was clear that DuBridge had been pretty annoyed by Pauling. And Pauling I think had done some bad-mouthing of DuBridge or the institute, that DuBridge was unhappy about. Pauling felt that the faculty was not really behind the ban-the-bomb-testing thing that he was working on very vigorously. And I don't really think that's quite fair. I'm sure there were a lot of faculty, particularly in engineering, who then, as now, were in favor of any technical kind of advances of warfare, or seemingly so. But I think, by and large, the chemistry and physics faculty, particularly, were really behind Pauling, especially the younger people. A lot of the older people who had to deal with Pauling on a day-to-day basis, had had their problems. There's one Pauling who was a public figure, and that is, in some respects, a façade. There is also a more private figure, and sometimes one that people don't like. I never saw much of that myself.

PRUD'HOMME: What was it in him that they didn't like?

ROBERTS: Well, I'm not sure I can find the right word to describe it. There were some things that he could be pretty small about. I didn't see much of that, but I think that's how he came across to DuBridge, who felt that Pauling was not doing the best for the institute as a whole, while chemistry was certainly getting its share. I think he wanted more support from Pauling. Of course another problem in the nuclear bomb problem was that DuBridge and Bacher had been very heavily involved in the war effort. Bacher particularly was very involved in the atomic bomb project. And I think they agreed fundamentally with Pauling that it would be nice to get rid of nuclear testing, but they had their own private worries about it. And Pauling, I think, put them down because he couldn't get their whole-hearted support. Pauling could be pretty tolerant, but when he really felt deeply about something, he wasn't going to take anybody else's point of view easily. And so when Pauling got his second Nobel Prize [1962], I was still chairman. And I remember he came in to my office and told me he was going to resign. I was the first person, I guess, that he told, except possibly his wife. He didn't really say much, but he gave me the strong impression that he was dissatisfied with the institute, and that he was definitely going to leave. I don't remember how long it was after the prize was announced, but not long, as I remember.

Anyway, I called DuBridge and Bacher and said I wanted to talk to them. So I went over there and told them. I remember DuBridge said, "Hot diggety-damn!" [Laughter] And Bacher was saying, "Oh, wait a minute now. We have to think about all aspects of this," including what the faculty thinks. DuBridge calmed down, but it was sort of funny that he felt that he'd paid his dues, and it was OK with him to have Pauling leave. There was some talk. Herbert Hahn, who I got to know and like very much—he was a local trustee—said that Pauling had cost them millions of dollars in donations. I don't know whether any of it's true or not. My own feeling is that if somebody is trying to decide whether to give you money or not, they'll seize on any excuse, and that's a very good excuse. But I personally think that by and large, you do better by sticking to your guns. On the whole, Harvard has certainly shown that over the years.

PRUD'HOMME: Well, Pauling certainly was a controversial figure.

ROBERTS: Well, yes. But, you know, the people who were more removed, who had to deal with him in other terms, the young people now almost idolize him, and there's no reason why they shouldn't, because he really had an enormous influence on chemistry and an enormous influence in the peace movement as well. I personally agree with him all the way down the line. On the other hand, a lot of people don't.

Anyway, I don't know whether I made any contribution at all as chairman to the larger things around the institute. We were involved in several fund-raising campaigns. We had a big problem with what we were going to raise funds for; this was a really thorny thing. Each division chairman was encouraged to bring in plans and tell about all the things that his division was going to do. I was told to predict for the next five years, which seemed to be completely unreasonable. So Sturdivant and I took the same old rate of growth of 5 percent per year, and projected it out to the next five years. I wasn't very happy about it, but we had a lot of ideas for things we wanted to do—faculty members that we might add, whatever. So DuBridge and Bacher decided we would have a retreat. We went up to Santa Barbara; everybody sat around and talked about what they wanted to do for their divisions. I made an impassioned presentation for our division and what we wanted to do. There were hardly any questions. I thought that was all settled then, but it didn't turn out that way. I guess all you could say is that they sat there and they heard us, but they didn't decide anything. That made it kind of unsatisfying; yet I don't know any other way for them to operate. The institute would respond to what looked like the most attractive opportunities, and those people who pushed hard for something usually got something.

I was on the Educational Policies Committee at the institute for a year or two before I was division chairman. I had two big projects then, one that worked very well, and one that was a disaster. The first was to make a rather simple change in the institute's academic regulations, that a student who was a sophomore in good standing at the end of the second year could change to any other discipline in the institute without going back and taking required first- or second-year courses in that discipline. The students had been complaining a lot about the iron mold of the institute. I think the change was really a great success. Every now and then I have to remind the faculty of it, especially when somebody starts wanting to put back required engineering in the sophomore year.

The other one I wanted to try was the result of listening to my colleagues in humanities—to see if we could raise the level by setting up a graduate program in humanities. The Educational Policies Committee was a real close-knit group, and almost everybody on the committee thought this was a great idea. Finally, we had a big meeting at our house. And my wife had a big dinner. I'd talked to Hallett Smith ahead of time, of course, and it sounded like it was really going to go. Then we got started and Hallett said, "Well, we've decided we don't want to have graduate work in humanities."

PRUD'HOMME: Why not?

ROBERTS: I don't know. It was an amazing thing for me. Here I thought I'd touched all the bases. Bacher and DuBridge were not what I'd call convinced, but they were willing to be convinced. The committee was enthusiastic. And then *bam*, the whole thing went like a balloon that had a hole in it.

# JOHN D. ROBERTS

## SESSION 6

## April 12, 1985

**Begin Tape 9, Side 1**

PRUD'HOMME: Let's move up to 1972, when George Hammond went to UC Santa Cruz and you agreed to act as acting chairman of the division. Previously, you had asked endlessly to be relieved of your position as division chairman.

ROBERTS: Well, not endlessly, but frequently. You know, administrators don't like to change administrators, because they don't know who they might get. But I did get out and, when I broke my leg, George Hammond took over for a while and then later decided he wanted to do bigger and better things because he wasn't being really all that successful.

PRUD'HOMME: In what sense was he not being successful?

ROBERTS: Well, he had this Hammond curriculum which, as I said, I had found wasn't the way students liked to learn. They preferred to learn a lot of different things a little bit at a time, and then integrate it, and then keep adding on. George's idea was to be very logical—teach one subject very thoroughly and then move to the next one; and you had the whole background behind you. And he was frustrated, I think, by that.

But as an administrator, he had Lea Sterrett working for him, who, from my own experience, could make up for people's inadequacies of administration very, very well. So, when he decided to go for bigger and better things in Santa Cruz, he took her with him. When she got up there, she realized they didn't have enough secretaries or enough money to have an efficient operation. In the meantime, while I was acting chairman, we had to find somebody to take her place. When she came back, it was impossible to put her back where she was. Fortunately, [Robert] Christy, who was then the provost, knew about her skills, and he was happy to hire her as the administrator of the Fairchild program, which was just starting up. I guess she began to help him in some other things, too. George wasn't all that successful at Santa Cruz either. I think he would have liked to come back here, but he finally went off into industry.

PRUD'HOMME: How do they go about finding a replacement to be the chairman of the division?

ROBERTS: That was an interesting process. There were a certain number who did not want to take any risk; they said we really should have somebody from the inside who understood Caltech, but that meant to me what they wanted was a "chairman who understands me." I think about a third of the faculty, who were the most confident group, were willing to look outside. And so we did quite a bit of searching. Finally, we came up with John Baldeschwieler, who was at that time at Stanford. Of course, Baldeschwieler had been a very, very capable scientist. At that time, I think he was functioning in some kind of a position in the Nixon or Ford administration. He had done some really excellent NMR work at Harvard, and at Stanford he did some extraordinary things. One of my prize undergraduate students, Jack Beauchamp, worked with Baldeschwieler; they developed what's called ion cyclotron resonance. John deserves a Nobel Prize for that; it's certainly in that league. After a certain amount of hassling around, we offered him a job and he came in as chairman of the division. This was fairly typical, but I must say that in my experience, at least, chairmen of divisions who have been brought in from the outside have not been outstanding successes for all kinds of different reasons. Engineering tried it a couple of times without great success, although they had good people.

PRUD'HOMME: Do you think this is specific to the nature of Caltech as an institution? Or is it true of all institutions?

ROBERTS: I don't know. Baldeschwieler was a very efficient guy himself. Somehow he didn't have the human touch to go with it. He had the division well organized, good committees, and did a lot; but he sure didn't make it with many of the division's faculty. So when his five years were up, his term was not renewed. It was traumatic for him and for the division, too. To be fair, there were a lot of differences of opinion. However, Bob Bergman, who was on the staff then, seemed always at odds with Baldeschwieler. This was when Christy was provost and [Harold] Brown was president. The administration loved Baldeschwieler, because he was well organized. The three of them—Brown, Christy, and Baldeschwieler—were, well, not terribly good on the human side. Bergman was a very fine scientist and pretty disenchanted that he couldn't get what he wanted. So he left and went to Berkeley. That whole business, I think, sort of soured things for John. I wasn't unhappy with him, but I could see a lot of other people were. So it was a tough thing.

PRUD'HOMME: You were named Institute Professor when Hammond was still here.

ROBERTS: Yes. Well, that was an attempt by the institute before they had many endowed professorships, to try to establish people with some kind of a special title. I wrote Harold Brown a memo at some point and said, "Well, now what? What do I get out of this?" It took him quite a while. They made up a perk that if you contribute money to your salary from your grants or contracts, the institute would put back that money into an unrestricted research account. And so you could use that for research as you pleased, because the overhead had already been collected out of it and it was overhead-free. That was a very advantageous thing, although at the end of the first year I remember the accounting department tried to take the money back because I had not spent it all. [Laughter] Boy, I really went tearing into them on that one. So then they let it accumulate. And actually, about the time I got out of [being] provost, I had something like over $100,000 in there. And that's what my research is living on today. Well, it was really a very worthwhile idea. And I guess they do that now for all endowed professorships.

PRUD'HOMME: You were appointed vice president, provost, and dean of faculty in 1980. That's everything!

ROBERTS: Well, that's a longer story.

PRUD'HOMME: I'd like to hear that story.

ROBERTS: The story really went back a long way. Harold Brown left very suddenly. And I was appointed a member of the Presidential Search Committee, with Fred Anson as the chairman of the faculty part of the search committee. There were concurrent faculty and trustee committees, and we would have joint meetings very occasionally; I think Si Ramo was the chairman of the trustee committee. So we set about the job in a systematic way—a very good committee, on the whole; [Gerald J.] Wasserburg from geology was on it, Bruce Murray from JPL [Jet Propulsion Laboratory], Peter Fay, Robbie [Rochus E.] Vogt, Jim Morgan, Lee [Leroy E.] Hood, and Zach [Fredrik] Zachariasen. This group had the responsibility of getting up candidates and so on. It was a tough job, particularly because Harold had left precipitously. He was at a faculty meeting in December where he gave sort of a State of the Union address about Caltech. And then he left for Washington to be secretary of defense about a day after that, and I don't think he came back for months. So Christy took over. Christy is a very good friend of mine. I like him very much. But as an administrator for the institute, he had his problems, too. So we were under some pressure to get the job done, but, on the other hand, everybody wanted somebody good and that took time. The trustees were impatient and pushing on the committee during this period, too. They insisted on having a firm come in to conduct an executive search. That firm was really a riot. At least we could use them as a filter to be able to say that we had looked at all the applicants. Hundreds of people wrote in. So they filtered out those people and came up with a couple of names. Well, I knew one of them very well; he was not my idea of the man who should be president of Caltech. They didn't seem to know that this guy was three paces to the right of Barry Goldwater, which would not have gone over very well, including his being a white supremacist. [Laughter]

It was pretty much of a rush; there was a lot of pressure to get things going.

At one point, somewhere down the line, [Marvin L.] Goldberger was recommended. As mentioned earlier, I had known him before a little bit at the academy, when I was secretary of the Class I Membership Committee and he was chairman.

When he came out for a visit, he talked to me at one point about Christy and what I thought of him as provost and whether he should keep him on. I guess I said that he should do things in whatever way he wanted to. He really wanted to establish how popular Christy was and how effective he was. We just talked about that, but he didn't say anything to me about becoming provost. After he came, why, things went for quite a little while with that regime. And I remember one day—when was this, 1980?

PRUD'HOMME: Yes.

ROBERTS: Well, one day—it was a good six or eight months before I actually started, it probably was early in 1979—I was walking back with him from the Athenaeum on the Olive Walk, and he said, "I'd like to have you be provost." At that point, I was sixty-one years old. I said, "I don't think you ought to do that. I'm too old and I think the provost ought to retire at sixty-five as a matter of principle." He said, "Well, that doesn't make any difference. I'd like to have you do it." I asked him if he'd consulted with the faculty, because I hadn't heard anything about a provost search committee. He said, no, he hadn't, but he thought it would be OK. And I said, "Well, I don't think you can assume that it's OK. I think you'd better get a faculty search committee together and find out who they want as provost. If you appoint a provost they don't want, you're going to have more trouble than you really want." So he said, OK. And I didn't hear anything more for months.

Around September or October, after school started in the fall of '79, he came around again. And he said, "Well, the search committee's decided that they'd like to have you be provost." I don't know whether they'd asked anybody else in the meantime, or whether they decided not to go outside, or what. I was still reluctant to do it, because I thought they ought to have somebody who'd be there for quite a while. Well, Goldberger said he didn't care. I talked about it for a while, and then I finally decided, well, OK, I might do it. Or at least I was willing to think about it.

This wasn't so easy. There had been some problems between Goldberger and Christy. I suspect more on Goldberger's side than on Christy's, because Bob seemed to be personally rather oblivious. He's a rather exemplary person. He could take the best shots from somebody without seeming to be greatly affected by it. His wife was much more affected; she wanted Bob to be president and had seen what it was like living in the president's house. She seemed disappointed that Bob wasn't appointed, but I think she was realistic about it, because Bob was already sixty. One problem was that she did not have the best relations with Mrs. Goldberger, and I understand that. Mrs. Christy is a very attractive woman and a fine astrophysicist in her own right. Bob had been divorced; he had two grown sons. He married Juliana in the seventies; I was the best man at the wedding. My theory is that Christy got married again after many years after he went skiing with my wife and me and decided that maybe marriage wasn't all that bad. [Laughter] In any case, Juliana got to know Murph quite a while before I did, and she was not impressed; in fact, I remember she came over and urged me not to accept the appointment. I gathered she felt that Goldberger really wasn't going to make it as president, and she thought I should avoid being involved in a rescue operation. I never knew what Bob thought about it; it was the kind of thing that Bob would never talk about. In fact, Bob, in all the time I've known him, has always been reserved in saying anything about Goldberger's abilities or how he was doing.

I spent a lot of time thrashing around as to whether I wanted to do it or not. I finally decided that I would go ahead, but I always felt that I always had the option—that I wasn't committed to an indefinite period. I was teaching a class at the time, and I pulled out of that in the midyear because I just felt there was so much to do. The situation was really very complicated. At that time, Mrs. Sterrett was working in chemistry and also doing work for Christy as a Fairchild administrator. Christy operated from a desk piled full of papers like mine here. He had somebody who took care of the faculty appointments. But otherwise he did almost all the work himself.

PRUD'HOMME: How did he get along with the faculty?

ROBERTS: Some of them found him hard to deal with. That was part of the problem, particularly the brash guys like Bergman, who were young and full of enthusiasm, who wanted to see things get moving. But Christy was anxious not to rock the boat. He was a very fair guy; he didn't want to commit his successor to programs that would get out of hand and embarrass somebody later on. But it was a long interregnum; it took eighteen months to get Goldberger here, starting from the January when Brown left, and quite a bit longer before I started as provost. There was a lot of reorganizing to be done, and I believed that Mrs. Sterrett would be the ideal person to do that. But Dr. [Harry] Gray, who was then chairman over in chemistry, knew of her talents, too, and was not eager to give her up. So we had a bit of a problem getting that straightened out. She was able to run both operations for a time, but it got harder and harder. So we finally had to make an arrangement for her to be full time in Millikan [Library, in the provost's office].

When I talked to Goldberger about being provost, I laid out my view of what I wanted to do as the chief academic officer of the institute. I said that I expected that he would take care of JPL, fund-raising, all the outside operations, and then I would run the academic part. He said, "Well, yes, I guess so. But you'll hear from me from time to time, about the academic part." So it wasn't really the same kind of an arrangement that DuBridge and Bacher had. The two of them together were a great combination and had brought to Caltech a wealth of wartime administrative experience. And as I've told Bacher, I admired so much the way they did the job, that I made up my mind to try to do it as close to that as possible, because it was a very fine arrangement.

We had to rebuild some of the Millikan Library to make a better place for us to have a group of people working. I moved over to an office on the north side with a wonderful view of the mountains, and we rearranged the rooms so that Mrs. Sterrett could act as a guardian of the gate. And we took on a lot of faculty staffing work which was being done piecemeal elsewhere. We ran the Fairchild program, ran all the appointments of the Scholars and so on. The Fairchild program at that time was going great guns; but it was a lot of detailed work. Lea Sterrett was really marvelous at running things like that and gained the affection of the people she helped.

I think things started off reasonably well. But the problem really was that neither Goldberger nor I had the right kind of previous experience for that kind of thing. He didn't know how to administer and, in fact, I didn't either.

PRUD'HOMME: What had his administrative experience been?

ROBERTS: Well, he had been chairman of the department of physics at Princeton. I guess he had done some things for consulting. I think he'd been chairman on committees for the government, the Defense Department, and so on, but never really, as far as I know, running an operation on a day-to-day basis. He really didn't know how to use people. I at least had Mrs. Sterrett that I could really count on to take responsibility. She would take care of a vast amount of stuff; and I would spend very little time on it.

PRUD'HOMME: He couldn't delegate, or he just couldn't comprehend the job?

ROBERTS: Well, he found it hard to know how to get anybody to do anything in a useful way. Hardy Martel, who was professor of electrical engineering, had been Harold Brown's sort of right-hand man. Harold would tell Hardy what to do, and I guess he expected it would get done. Well, Goldberger didn't know what to tell Hardy to do in the first place. [Laughter] Or if he told him, then he would forget. Hardy would come in and say, "Well, I've done this." And Goldberger would have forgotten that he'd ever asked him. The worst was in connection with W. H. Corcoran.

**Begin Tape 9, Side 2**

ROBERTS: [Continuing] Bill Corcoran was professor of chemical engineering. He was extremely honest, very competent, very hard-working, and dedicated to Caltech. He'd been an undergraduate and graduate student here; he'd been off in industry for a while, then had come back. He was a very large man, quite handsome and impressive-looking, stubborn as a mule. He and Christy used to get on opposite sides, in the early days. At two or three faculty meetings, if Christy was presenting a report Corcoran would be criticizing it, or vice versa. Christy seemed to win those battles hands down. At one point, Corcoran was made vice president for institute affairs by Brown. His job, in part, was to raise money from industry and so on, and he really worked at it. I remember his telling me about having breakfast with Lee Iacocca in the days before he was out at Ford. Goldberger inherited Corcoran, but he could not figure out what to do with him. He couldn't direct him; he couldn't undirect him. Corcoran disliked Goldberger and just felt that he had no real feeling for what Corcoran's job was and what he was trying to do. So finally Corcoran got eased out, the first of several vice presidents for institute affairs.

Then Goldberger decided that the institute needed an energy program, and I guess as some kind of sop to Corcoran, he made him administrator of it. Corcoran went right to work, started having conferences and so on. But Goldberger didn't know what to do with any of the results or anything. [Laughter] But it was all organized and the wheels rolling. I guess Goldberger thought that if you told somebody to do something, unless you kept after them, it would never happen and just sort of fade away. Well, Corcoran had integrity and by God, if somebody gave him a job, he was going to do it. It was amusing and tragic at the same time, and unfortunately not much of anything ever really came from his work.

A really key player in the administration was [David] Morrisroe, because Morrisroe was a master strategist, master administrator, who knew how to let physical plant run itself under his philosophy and general guidance, without having to deal with everyone on a day-to-day basis. He did the same thing with the fiscal operation, accounting and so on—and seemed to be the one guy in the whole administration that the trustees liked and understood. I think they're fundamentally a little suspicious of the academics as being not pragmatic enough or not understanding enough or what have you. Morrisroe would sit in his little narrow office in Millikan and be aware of what was going on by watching the traffic going back and forth. So you'd go by and then he'd sing out, "Hey wait a minute, I want to talk to you." So you'd go in and talk. And he really had all the threads in his hands. He worked very, very hard.

Well, a few division chairmen were appointed. Vogt took over in physics, math and astronomy. [Barclay] Kamb was already division chairman of geological and planetary sciences during the Brown era, and Gray was also already in there. [Robert] Huttenback, who is now chancellor at Santa Barbara, was too. And I remember Goldberger used to stop me before I was provost and ask me what I thought about [Roger] Noll being division chairman, or [Roy] Gould. And I'd tell him what I thought, particularly for engineering. Engineering is a very tough thing to run.

The division chairmen group was a very strong group of people. Lee Hood had already been appointed as chairman of biology. And those were the chairmen I had to really deal with as provost—Kamb, Gray, Noll, Gould, Vogt, and Hood. It was interesting because they're all vastly different people, but very tough and occasionally almost uncontrollable. So I spent a lot of time at being provost, and it didn't take me long to realize that it wasn't what I really expected. In the first place, I didn't know who was really in charge of the money, or who kept track of what we were spending. Well, I soon found that I couldn't find out. The president seemed to have the authority to spend money. I remember his saying at one point, when he was being interviewed for the job here, that Morrisroe had told him about the general fiscal state of the institute. He was impressed with Morrisroe. And I'm impressed with Morrisroe, particularly because of the way he seemed to be able to keep us from spending much money; he'd put on an awfully poor face when we suggested doing it. Even Murph had a problem with that, even though he had a special situation as president. And on my side, I was frustrated because I couldn't really find out where the money was and what and where I could spend anything.

PRUD'HOMME: You were in an untenable position because you couldn't operate, you couldn't affect anything.

ROBERTS: Well, not altogether, but we'd go through these budgeting sessions with Morrisroe. We'd run through all of these accounting documents, and there never seemed to be any excess for anything that the provost wanted to do. Finally, after a lot of pressure, I got a $100,000 commitment. Then there was a problem later about carrying over what was unspent in a given year. I insisted on carrying it over, and that gave me something to work with, although it wasn't a whole lot. And I'd done a little fund-raising on my own by writing proposals. Ivan Sutherland, who was a brilliant man, highly regarded as a computer scientist—in fact, almost the father of certain kinds of computer graphics—was here then as a professor. And he was very unhappy, because he was used to seeing how things were run in companies. He was always wanting to appoint somebody in computer science. And he didn't care about the academic process; he just wanted to appoint them. He and Carver Mead, who's a computer scientist, a brilliant man too—the father, or at least some kind of an uncle, of the design of the large-scale integrated circuit—they came to me one day with a grandiose idea about computers for the whole institute. They really had a good idea. They felt that what we were trying to do was wrong; that everybody should have computing power at their own location. And the computing center should be where you archived things, where you used special laser printers and did special graphics, and that kind of stuff. But there had to be a connection to connect it all together. So they urged me to get to work on getting the connection. Well, it was a great idea. So I got the computer center people and people at JPL to make a recommendation about what we ought to get. And they suggested we go for Ethernet. So I wrote a proposal and got some money from one of the foundations—$400,000 to put in the Ethernet connections. That worked very well. When I look back on it, I think that's one of the really most positive things I did as provost.

You asked about all the titles. Well, I didn't really like the sound of being vice president, but then everybody said, well, the corresponding officer at all these other universities is a vice president. I knew that Earnest Watson had been dean of the faculty, and I liked the idea of the senior academic officer being dean of the faculty. I guess it finally came out provost and vice president, or vice president and provost?

PRUD'HOMME: Vice president, provost, and dean of the faculty.

ROBERTS: Well, I guess I really wanted to make it provost, vice president, and dean of the faculty, but it didn't come out that way, because of this alleged political problem. I did want to establish relations with the faculty. So I started a series of lunch appointments with three faculty members every week. And that was fun. I started with assistant professors and worked up. About the time I got out, I'd had lunch with just about everybody. Sometimes it was hard to arrange. The ground rules were that they shouldn't be from the same division, because I didn't want to have people there that gang up on their division chairman, and they could ask us about anything they wanted. We had some rather memorable luncheons and I learned a lot about what the faculty thought about the way things were going.

The library was a common source of luncheon complaints. People were generally of two sorts: they'd either complain about the library or they'd complain about the computers. Well, we managed to make some progress on both, I think, in the final analysis. We got Glenn Brudvig in as head of the library, and I think Glenn's been very good, although he hasn't been as visible to the faculty as I would have liked, but much better than his predecessor.

Another place I made some progress was in administration-faculty relations. Before I was provost, I kept harassing Christy about having a new set of institute regulations published for the benefit of the faculty. There were regulations, with a pretty strong name, *Policies and Procedures*, published in 1960 or so. Bacher was the last one, I guess, to supervise a revision of it. Christy had tried to get some revisions out. In his revisions, they had tried to detail the whole appointment process. This came about because he had had problems with Jenijoy La Belle, who had difficulties in getting tenure here but finally did. The revisions tried to cover every possible detail in nauseating length. I wanted to get that out. So Lea suggested that we call it the *Faculty Handbook*, which was a good idea, rather than trying to be the laws and regulations. I cut it down a lot, and then I had the experience of trying to get it through all the division chairmen and the trustees and everybody else. And we finally made it. It was a massive job. Since then, I think that's gone along pretty well. So that was very positive along the way.

Another thing that bothered me a great deal was that, in Harold Brown's day, he had decided that he wanted to have an Administrative Council, which included a lot of extraneous people besides the division chairmen. And in deciding about appointments, it is a very, very tricky business to open the process up and discuss appointments and promotions among people who are not principals. When I was acting division chairman after Hammond left, I had a real problem in that kind of meeting opposing an appointment in physics, because so many people got involved. I don't know how they handled the salaries in the Brown days, but I assume that would involve just the division chairmen. But the appointments went through the whole group. I made up my mind that we would reinstate the division chairmen meeting, and then follow that meeting with an Administrative Council meeting to talk about general matters, later in the day—have the one in the morning and the other in the afternoon. And that was pretty tough, because a lot of people felt cut out of the appointment process by that. I decided it would be good to have

Morrisroe at the division chairmen's meetings to keep him informed.

It was the toughest, I think, on Neal [Cornelius J.] Pings, who was vice provost first under Christy and then me and is now provost at USC. And I had some problems there, because it was difficult for me to know just how to use Neal's considerable talents. He liked to go to Washington and to deal with educational matters on that kind of level, and he did a good job on that. But Neal would be away for four or five days, or doing his professorial work. So I really didn't know how to use him very well, because I didn't have time to keep him up to speed on what the provost's office was doing. And it was not appropriate for him to be in the division chairmen meetings, partly because he was not a principal and partly because he had vested interests in chemical engineering. He wasn't too happy about that, because he really wanted to be involved, but he was good enough to go along with it and never really made a big issue out of it. But we did make progress on the character of the division chairmen meetings. It was a step in the right direction, and I guess they're still doing it that way.

PRUD'HOMME: Could you do any chemistry?

ROBERTS: Oh, yes, I had a good group of people in the lab. Dr. Kanamori was senior person in the group. And she kept things going very well, because I didn't really direct research in the ordinary way; and it worked extremely well.

PRUD'HOMME: Can you contrast Goldberger and Brown for me? Their administrative styles?

ROBERTS: Totally different. People thought Brown was very unresponsive, but I never found him so. If you wrote Brown a memorandum, you got a reply back the next day, with crabbed little handwriting along the margins. And then the secretary would type that up and put a little note on it. I liked him on the whole. He was not a terribly communicative guy in some kinds of situations, and whenever I could get at him at lunch at one of the faculty tables, I used to harass him to tell the faculty sitting around the table about things that I thought they wanted to hear. But I probably didn't know many of the right questions, because I was not in the administration then. I think we were good friends. He gave the appearance of being cold, and his speeches were pretty dull. But I have heard that a lot of that was a façade; that he was eager, when tough problems came up where he thought he had an answer, to answer; and that Morrisroe and others would counsel him, “Well, hang on a bit.”

PRUD’HOMME: Do you think he was too efficient for Caltech, in that he took too many decisions on himself and wasn’t a committee person?

ROBERTS: No, I don’t think it was question of being too efficient. I view Harold as a very, very capable guy. I think Harold’s problem was that—and I hold the same view of him as secretary of defense—I feel that he was much more of a technician than a statesman. He didn’t really have a long view of the academic world. I had some disagreements with him, because he seemed to feel that you couldn’t go in a new direction without setting up some new administrative unit, an institute, a center or something like that. I got him pretty sensitized about it. I felt Caltech could do new things without having more administration. In some ways, I think Harold was more reactive than creative. Then, maybe that’s the way Caltech presidents in some ways have to be. DuBridge was more or less reactive; he reacted to what he thought the faculty wanted. I don’t think Harold reacted quite so well that way. I have heard that some of the trustees were nettled with Harold because they didn’t feel they had enough control over him. Harold had a background of success that matched many of theirs. And I assumed that Harold really knew where the money was around the place and knew how to spend it. I never really found out; I really didn’t want to keep the books, but I wanted at least to know how much I could spend. I remember one memorable occasion where I wanted to spend some money on something I felt was important, and when Morrisroe said, “Well, we don’t have any money to spend,” I said I would insist on going through the books. “I want to see every dollar this institute’s got, how much they’ve got to spend. And if you can demonstrate to me that we can’t spend this money, then I will say, OK. Otherwise, I’m going to spend it. So it’s up to you to decide whether you want to take me through the books or give me the money.” And I got the money. [Laughter] That was pretty funny.

Well, I think we worked out a good understanding. I liked Morrisroe. The problem was that Goldberger, in contrast to Brown, had a pretty short attention span, particularly on fiscal matters.

PRUD’HOMME: Is he more interested in his own field than he is in administering Caltech?

ROBERTS: His own field? I didn't get the impression that he was especially partial to physics; although he really did want to be appointed professor of physics, and I don't know how he engineered that, but it finally worked out. DuBridge was never professor of physics. In fact, one of DuBridge's favorite stories was the fact that when he came here, he didn't have tenure, as president. If he got kicked out, he was out altogether. And I think Goldberger didn't want to be in that position.

PRUD'HOMME: Goldberger has passed his five-year mark.

ROBERTS: Yes. And he's never had a review. But his style was very different. Goldberger's a smart guy, an interesting man, but he has some problems in deciding, "OK, I want to do this, and I'm going to stick with it to see that it gets done." And he lately came out with a big blast about undergraduate education, about how he feels the students were being shortchanged by the faculty, and he was really going to do something about it. Then three or four sentences later, he said, "I've delegated the whole thing to Robbie Vogt." Well, that, I think, is sort of typical of what he's done. I think he's certainly smart enough to do all of these things if he worked on them.

PRUD'HOMME: What is the faculty's general opinion of him?

ROBERTS: I don't know what the general faculty opinion is. Those who know him reasonably well are likely to say that they don't think he's a strong and effective president. Morrisroe is respected as having a lot of power in this administration and the only one who made it from the Brown era.

Now, Vogt is a different proposition. Vogt is an extraordinary guy. I don't know whether your people are ever going to get an oral history out of him. But I had several of them that were never recorded. [Laughter] His life and times, and where he came from. He was very, very difficult to deal with as a division chairman. Very organized; a superb organizer and micromanager. Radio astronomy was in trouble when he came in as division chairman and Vogt in effect put it in receivership for a while. They had trouble with all kinds of things, with regard to the money that they'd spent and in getting renewals, and he micromanaged that operation in an unbelievable way. But it must have been very traumatic for the people who were involved.

Vogt is a very volatile person. When we were on the presidential search committee, he didn't seem to have much of a sense of humor. The give and take was pretty heavy; he would blow his stack and leave, determined he would resign. Then Anson would talk him back into coming to meetings. But on the other hand, as I said, when I dealt with him as provost, he knew what he wanted to do; he came in with lists of things and explained in great detail why they were necessary; he made an excellent case. Efficient. Pretty economical. But very, very tough to deal with. And we had some incredibly stormy administrative meetings with Vogt, Noll, Gray, Kamb, and Hood as protagonists. Murph was not too good in keeping them under control, but perhaps nobody could.

The division chairmen operated with me in very different ways. I told you about Vogt. Hood would come in—when he did come in, he was usually traveling around the world and doing great research things—every now and then and with a set of basically non-negotiable demands for appointments or research funds for appointments. He was strong for getting good people. But his appointments were very expensive. To my surprise, it turned out that getting in biologists was much more expensive than the physicists, actually. It took enormous sums of money to get biologists set up, but the physicists somehow took care of their appointments mostly out of their own grant funds. Noll was a lot like Vogt, although less organized. His colleagues in humanities were afraid he would push only for social science. But, in fact, he tried very hard to build up humanities, especially in literature and philosophy.

PRUD'HOMME: Why did you leave the provost's job?

ROBERTS: Well, it was a little bit like the experience I had being division chairman, a very cyclic kind of thing where the same old problems kept coming up at the end, but on top of that, administration in that period had some special problems. One of the most vexing of these was with Stephen Wolfram.

PRUD'HOMME: What was that?

ROBERTS: Stephen was, in principle, a minor actor on the Caltech stage. He was a graduate student here, and I think he got his degree in a year or something like that, doing some work in theoretical physics. At a very early stage, he got a MacArthur Award, which was, I'm sure, strongly supported by Murray Gell-Mann. Stephen, I think, wanted to be on the faculty here. He was an Englishman, absolutely humorless, and absolutely determined to get his own way. And apparently very brilliant. He'd been working in fields that I had no understanding of.

Well, during this period, when I was provost, the great conflict-of-interest battles started. Faculty members all over the country, especially in biology and computing, were going out and starting companies and so on. It was a national thing and received a lot of national attention. Caltech had to reexamine its role in this kind of thing. The institute principles were in fact pretty tough. We had some rules, and one of them tucked away in the *Policies and Procedures* book was that you couldn't be a full-time, operating officer in a company. This was not true at MIT or Stanford.

PRUD'HOMME: You could be a participant in terms of owning stock, though, could you not?

ROBERTS: Oh, yes, you could own stock, and you could be on the board of directors or a scientific advisor. But that's not the same as having line responsibility for day-to-day operations. This question had not been looked at very closely during the Christy era, because then it was not much of a problem. It was later when it began to be a big thing. So I had a faculty committee, and got into this conflict-of-interest thing. The people most concerned were in computer science, biotechnology, and some engineering. Hood was one of the principal actors, because he and one of his people had invented a device for helping determine the structures of proteins, which worked on enormously smaller quantities than had ever been done before. One of the unfortunate things about this is that Arnold Beckman and his company were selling a similar machine, only Hood's was perhaps a factor of ten better. Hood was impatient. He and his people had developed this machine and were eager to have someone commercialize it. It was offered to Arnold's company; this was handled through our patent office and legal counsel and so on. But it went slowly. Whereas Arnold seemed to think his people were doing a great job, the impression of Hood's group was rather that the lower echelons of the Beckman company were throwing every obstacle in their pathway and didn't really want Beckman to make this machine. It's hard for me to know the truth, because Hood's people were impatient, and the negotiations were slow and difficult.

Anyway, finally we seemed to get a clear indication that the Beckman people did not want this machine. Then a company got formed, and I am not sure when, called Applied Bio-Systems. Then it became an issue of people in Caltech being involved in the company and licensing Hood's machine to Applied Bio-Systems.

**Begin Tape 10, Side 1**

ROBERTS: [Continuing] Hood and his man, a man named Mike Hunkapillar, who got a PhD at Caltech with Jack Richards in chemistry, were involved with this company. I felt I just had to make clear what the rules were to Hood—and here I felt that while in principle Goldberger was supportive, in practice, he never seemed so when I really needed him. Morrisroe, Goldberger, and I had agreed that Hood just couldn't own stock in this company, have it be the principal licensee of the patent, and be chairman of the biology division. There was opposition in the division to the commercialization of this thing, anyway. A lot of professors were almost livid about having this kind of operation, that we were hearing about so much about at Harvard and so on, going on at Caltech. So I finally delivered the bad news to Hood that we were just not going to want him to be a stockholder in this thing.

PRUD'HOMME: And what was his reaction?

ROBERTS: His reaction was pretty good, I think, considering everything. I think he understood the ethical problem. But, on the other hand, my experience with the faculty is that they see conflict of interest most easily if they're not involved. [Laughter] But once they're involved, my God, is it tough! Hood was a bit grudging, but he agreed that he probably shouldn't do it. And I must admit that I'm sure it's kept him from being a millionaire a couple of times over. Because Hunkapillar, I understand, is a millionaire now. That machine and Applied Bio-Systems really went over well. But in the meantime, when Arnold found out about it, he was in an absolute snit. And I really feel badly about that. The problem is, because I wasn't involved with the Beckman negotiations per se, I didn't really know what the Beckman people were doing. According to Hood, his side were just angels in trying to deal with them, making every accommodation. Yet, I know they were impatient as hell. I suspect the truth is somewhere in between. But Arnold has never forgiven Caltech or Hood for that. And yet Arnold had his own conflict of interest between Beckman Instruments and as Caltech trustee for the licensing of the machine. In any case, Arnold gave Hood more of the blame than he deserved, because, actually, Goldberger, Morrisroe, and I were the ones who approved the deal.

Well, the trustees were up in arms about the whole conflict-of-interest thing; it was a very, very painful period. Then I found that Carver Mead was involved in running a company to some extent; I never really got that whole story quite straight. Computer guys were accusing each other of all kinds of stuff in this area. I got the faculty committee working harder on the problems. They were coming up with some recommendations, and I was trying to implement some kind of policy. I had a consulting group of just a few faculty that I would call in and hear their opinions on a problem before making a decision.

While all this was going on, Vogt came in one day and said, “Well, we’ve got a company in physics. They’re having board meetings in the building. It’s wrong. What do we do now?” It turned out that two faculty members in particular, Barry Barish and Stephen Wolfram, were involved in what became known as SMP, which is a symbolic manipulation computer program. There’s an article about SMP in *Scientific American* in September 1984. This program does very complex algebra on the computer. It doesn’t do the arithmetic, it does the algebra. So it will tell you, if you’ve got *x* squared plus *x* is equal to three, what the factors are, if any, and that kind of stuff.

PRUD’HOMME: Incredible!

ROBERTS: It is incredible. It did a lot of things. Geoffrey Fox, who was in charge of one part of the program in high-energy physics, had been interested in the problem of doing math with a computer. Geoffrey is a really interesting, very talented person. He apparently started this program with a group of students, and Wolfram came along and got in it. Wolfram, who had been doing high-energy physics theory, became completely enamored of this programming effort. And later, in a *Scientific American* article, which he authored, he says he developed this program. Well, I don’t know; a lot of people were involved. But at some stage, the group, but not Fox, decided they wanted to commercialize it, and they formed a company. And they got the interest of a software company with an office in Culver City whose specialty was developing programs. Wolfram and Barish and several students were involved. And then the question came up about who owns the copyright on the SMP program. This was a difficult and very frustrating problem.

We had a patent officer, a man named Stam, an older man who handled the licensing of patents for Caltech. These people had gone to Stam, and here let me say part of the reason why I hated being provost was that when you talked to different people, what looked like a cat to one person would look like an elephant to somebody else. The story was that Stam encouraged them to go start a company, but Stam later denied that he'd ever said that. So it wasn't very scientific. Apparently each one had made up his mind what the outcome ought to be ahead of time.

Well, Vogt was upset. Vogt was like Corcoran in that both were very dedicated as far as Caltech was concerned. Vogt didn't think this was going to be good for physics and he wanted to get it stopped. And we had to work out some kind of a license agreement in a situation where Wolfram at all times claimed the program belonged to him, period. I had to be pretty actively involved in this. The man who ran the company in Culver City, Jacobson, was eager to get a license agreement, because he could see a big market for this thing; he got TRW and other places, presumably, lined up. Unfortunately, the program, like all complicated programs like this, was full of bugs. It was written in a not-too-common computer language at that time—C, which has become more common since. There was a lot of pressure on us. To be fair, it wasn't really clear in the faculty laws about just what was supposed to happen to copyrights on computer programs. Books were specified to be the property of the author, but everything else was, in a sense, in limbo, because those bylaws had been last revised in 1962, you know, when nobody ever thought about marketing scientific computer programs. Then, because the work was ostensibly supported by the AEC, there was the problem of an agreement with them. Wolfram and his buddies tried to prove that the AEC didn't have any claim on the program because it wasn't really supported by them; in fact, they wrote some letters to the AEC claiming that, that none of us knew about. And by the time I got involved with it, it was really pretty far along, and a terrible mess with both Gell-Mann and Feynman involved in trying to settle things down.

Well, the major difficulty I had was that Goldberger could not keep his mind made up. He was a theoretical physicist; he had admiration for Wolfram's intellect. When I talked to him, he would be in complete agreement with what I was going to do. When Wolfram came and talked to me on a couple of occasions, I simply laid it out for him: "OK, this is the way it is." Well, he soon found that he could get attention by going to Goldberger. He would get responses from Goldberger and so could Gell-Mann, who was pushing for Wolfram's way. And I think the biggest problem I'd always had with Goldberger is that he tends to reflect the last person that he's talked to. He is not very good at delivering the message to anybody that we're not going to do what he wants to do.

I kept getting conflicting messages, and I didn't like it; it was really tough, and I was losing a lot of sleep trying to figure out how we were going to get out of the box we were in. I would get support from Goldberger, but then I kept getting messages from other quarters that it had eroded. But I think I had support of the faculty committee. Barish was pretty bitter about the way we made him get off the board of directors if he were involved as a licensee. But we felt that we could not license it to somebody who was that closely mixed up with it—the company that licensed SMP.

So finally, we got Barish out of it, and he's never been especially friendly since—about the only member of the faculty that I had that kind of trouble with, except Wolfram. I finally gave Wolfram the option of resigning from the faculty or the board of directors, or we wouldn't license it. So he decided to resign from Caltech. And I guess he worked it into a story in *Science*, finally. I was pretty unhappy with the story because I was only asked a few questions over the phone. I wrote a letter to them that they published, and, and of course, Wolfram was unhappy about the whole thing. Anyway, that was a very traumatic period, and it helped me to decide I'd had it as provost.

And something like the same kind of thing was happening in the academic program. People would go and talk to Goldberger and get one assurance. And then I would talk to him, and I'd get something else. And then, when they came around to me, why they would say, "Well, the president says…" And I'd say, "Well, that's not my understanding." To make matters worse in this period, Goldberger and Vogt were not getting along well. It got so that the one thing I wanted to do at that time was to reaffirm the rule that administration should get out when they're sixty-five. So I gave Goldberger a year's notice. I said, "Look, I'm going to be sixty-five as of next June, and I want to get out either then or before." He said, "Well, let's wait; you can rethink about it." But I guess after he thought about it, he decided it was a good idea.

I get along with him fine now. He seems very good about not holding a grudge. He always surprised me that whenever I would get really mad he never reacted quite the way most people would react. He has sort of a protective shell.

PRUD'HOMME: I think there's always a period when the faculty have certain expectations of a new president. And then there comes a point when you realize that he is what he is.

ROBERTS: That's right. Well, several times Morrisroe and I felt that we really ought to do our damndest to help back him up and move along and so on. Once I discussed my problems with Bacher, and he said, "Well, you've just got to go and talk to him. Lee and I used to talk." Well, God, I talked to him all right. But I never quite had the feeling that what we seemed to agree upon was going to stick.

The Parson-Gates battle was quite amusing. Murph and I had it planned that he would take the northwest corner and I'd take the northeast corner. We both wanted to be able to see the mountains, or at least be on the shady side. And we set it up to have a short private corridor connecting our offices so that we could communicate with each other reasonably directly. It was something we almost had in Millikan and seemed like a good idea to plan into the building, even though we were disagreeing on a lot of things, because we would be able to share things and be able to communicate. The building was not finished when I retired, and one of the first things that Robbie Vogt did when he became provost was to have the plans changed to take out the passageway. He wasn't going to have it so that Goldberger or anyone else could walk into his office unannounced. And he made the office bigger by moving the wall back six feet. [Laughter] It was a difference in style. And it was clear, I think, that he didn't want to do things the way I had done them. He changed the staff in the office almost completely and terminated Mrs. Sterrett. I was sorry about that, because I hoped she could help him just as she helped me. But clearly that was his privilege. Provosts are not going to do things the same way. His style is different, and so was Christy's. They were both extremely capable people in their own ways, but I wish that they had made themselves more visible to the faculty—particularly Robbie. But then he didn't want to be dean of the faculty; he dropped that title from the beginning.

From my experiences with administration, I have come to believe that to be successful at the upper levels of a university, you should get experience with administrating early. I'm not very sanguine about people who have been fine scientists until fifty-five or so and they say to themselves, "Well, I've shown that I can do science; now, it's time to take that experience and be a college president." The scientific frame of mind is not, in my view, good training for the administrative frame of mind. I think both Goldberger and I showed a lack of the good administrator frame of mind. And I suspect that that had a lot to do with my disenchantment with being provost. In my science, I was just too used to finishing off problems and then turning to new ones. In administration, you don't finish off problems; they may die sometimes, but usually not through any action of yours, and there aren't many new problems—just old ones in new disguises.

**JOHN D. ROBERTS**

**SESSION 7**

**May 10, 1985**

**Begin Tape 11, Side 1**

PRUD'HOMME: What do you think the future of the institute is? And what do you think it should be?

ROBERTS: Well, I hope that it can go on in basically the same modes. I believe it's important for it to be an educational institution, and not just a research institution. As I think I told you, at the research institutes abroad and similar places in this country, you don't get enough turnover of people. And I think it's important to have students around insisting on new ideas all the time. I hope Caltech doesn't grow a great deal more. One of the problems with the academic world, particularly now, I think, is that things are moving so fast that people often don't keep up. In order to stay at the forefront, the tendency is to hire more people, build more stuff. And I find that very unfortunate, because I think this place is a good size. I think it's getting too big; but I think it's still a good size. There's not a lot of restraint possible; there's been some, but it's just very difficult. Nowadays, the buzz words are "target of opportunity," or statements like "We don't have a critical mass in some area or the other." I hate this critical-mass stuff; I don't really believe in it.

PRUD'HOMME: Do you think the conservatism of the present student body will diminish?

ROBERTS: What kind of conservatism?

PRUD'HOMME: I thought you had said that they tend to be a little more conservative in this decade.

ROBERTS: Well, I think they may be more job-oriented than they were.

PRUD'HOMME: But then there's no implication that that would limit their scientific horizons?

ROBERTS: No, I don't think so. I think you have to keep looking for new ways to keep in the center of things. The institute has done well in that, although if you talk to people outside, they see physics, for example, particularly in theoretical physics, as not as hot as it used to be. Geochemistry was one of the greatest, and probably still is compared to anywhere; but on the other hand, they're not yet renewing themselves. Somehow they don't seem to see a new way to go on up. Organic chemistry was sort of getting in that mode, but I think they're now well out of it, by the addition of Peter Dervan and Bob [Robert H.] Grubbs, and people like that. They're pursuing new directions that are very significant in organic chemistry, very important, and they've got a long way to go in those directions. They're not going to run against a brick wall. And our new young guy, Dennis Dougherty, I think is going to do some very good things in what we call grassroots kinds of organic chemistry. Physical chemistry was a little more of a worry, but it also is looking better. Inorganic's doing fine. Chemical biology [in chemistry] is having troubles, I guess, because they can't make up their mind whether they're going to be biologists or chemists. That's going to be a problem for them for quite a while. They've got some very good people, but again, they're not renewing themselves very much. Anyhow, they're trying, and they don't often sit back and say, "We're OK, we don't need to do much." That's never been a real problem—except possibly in some areas of engineering, but generally speaking I think the institute is always frenetically looking for new things to do and new directions to go. But I'd hate to see it get bigger. I was interested in seeing in a paper the other day that somebody was claiming that corporations, when they have about 200 people, seem to work best; and the really big ones not so good.

Anyway, that's what I think we have to do, is to keep actively looking for new areas, and part of it, one would hope, would be to be able to get back somehow into areas which are not the most complex. I think we can do a lot, for example, with the super high-energy physics, but also I think we ought to try to find areas of physics in which you can basically sort of start from scratch with simpler ideas, simpler things, in new directions where there hasn't been much exploring. I don't think Caltech can sustain an identity very much in some areas in which things are being done on what you might call a super-massive scale. The Keck telescope is an example of how it will work the other way, in that it's basically Caltech's telescope. And the University of California will have half of it, but that's a heck of a lot to get. It's when the individual investigator has to get his own time out of it as an individual, that the institution is not quite involved in the same way. I think that makes for a very different kind of thing.

And one of the things I am concerned about is the degree to which those people who work as components of large projects somewhere else, a mostly detached kind of thing, can contribute to the teaching here. And furthermore, and very important, how are they really going to feel about Caltech? You know, they're not going to be a part of the place. And I've seen that with some of the physicists now, who work in Germany or CERN. Some of them don't seem to me to be all that really deeply involved.

PRUD'HOMME: We had discussed DuBridge as president; then came Brown, and then came Goldberger. What are your opinions of each of them as president?

ROBERTS: Well, I think I told you a lot about them. I think DuBridge was extremely successful, partly because he had Bacher to help out. He was the outside guy, and Bacher was the inside guy; and they worked together extremely well. And they presided over an enormous expansion of the physical plant and facilities. DuBridge had a tremendous influence in Washington and so on, at a time when science was changing very, very rapidly—all for the good, in my view, except possibly in this question of faculty salaries that we talked about. And I think that DuBridge and Bacher made out extremely well.

Now Brown, with Christy as the provost, was a different sort of administrator. Brown was a much cooler—outwardly anyway—personality. And as the provost, Christy was much different from Bacher. The two of them had a much more efficient kind of operation in some ways, but a colder one. I think that a lot of the faculty didn't feel close either to Brown or to Christy. I didn't have that problem myself because I knew both of them well—although Brown not as well as Christy. They're hard guys to get to know, to know how they really feel about things. But I thought Harold did a great job of getting the institute back to more efficient methods of operating a physical plant, things like that, getting the budget more under control. I think what usually happens is that things start to slip a little bit, and DuBridge and Bacher had been at it for a long time.

Goldberger, I think, is the weakest of the three, in terms of lack of follow-through. I don't think the faculty has confidence that he can be counted on to carry anything through. I think that Vogt is a very different person, and if the two of them work together like DuBridge and Bacher did, they're going to get a lot more done. But the image for the faculty is going to be a difficult one. I don't think the faculty is going to be happy, because in some ways Vogt's a purely invisible guy to them. And furthermore, he is really collecting power in a way which makes his office much stronger than the way we used to run it. It used to be that the division chairmen really had most of the power. And to some extent, he's been draining some of the power away from them and doing a lot of micro-managing. I think he'll do very well, but I don't think it's going to be terribly popular. And he's gone out, for example, and said publicly, "We're going to have a hiring freeze on several divisions." And that's had a terrible effect on some faculty. They figure that their divisions ought to be able to make a case when something unusual comes along.

PRUD'HOMME: Also, once you've been decentralized, it's very hard to then centralize.

ROBERTS: That's true. But the provost has always had control over appointments. The real question is whether the provost should really say to any division, "OK, you can't hire anybody for *x* number of years." And even at Harvard, which has had a much tougher policy than Caltech, you can borrow on the future if you have a good case, and there are ways around it. I don't know whether that's still official policy, but I know a lot of people were very unhappy about Vogt's pronouncement when he made it. And yet it turns out that even though the pronouncement was made, in chemistry, for example, we've got a shortage of people and we've got a real problem.

PRUD'HOMME: How does chemistry research in the United States compare with the rest of the world?

ROBERTS: Well, I can only speak about chemistry. The Swiss do very, very well, considering the number of people and the size of the country. They have very high standards, particularly in Zurich and Basel. The British are having troubles, but they still do good work at many places. Germany is still very good in chemistry; they're not leading the world the way they were. Except in a few areas, the Soviet Union hasn't had much effect on the world, as far as chemistry goes. The Japanese, however, have really learned a lot. And the Australians do pretty well, considering the fact that they have no real industrial base. The trouble with Australia is that they have fine universities, but they don't have any place to put their people. They don't have enough industry to do research for. They don't even do research really in the industries they have down there. They import most of their developments. It's unfortunate in that they have really very fine universities and yet they don't have much of a market for their trained scientists. And the Canadians, to some extent, are that way, too. Canadians are turning out PhDs at an unbelievable rate for the size of their industry; so most of them come down here to work. It's been great for us, though.

PRUD'HOMME: What do you think the role of scientists should be, speaking out on matters of public concern?

ROBERTS: I think they ought to speak out. I was burned to a crisp by George Shultz's recent speech that he made at the National Academy of Sciences—I wasn't there, but I read about it—in which he claims that scientists don't have any special expertise or any special infallibility. But he didn't say who he thought did have infallibility—I presume he meant George Shultz. I think everybody should try to speak out. It's quite true that scientists are not dispassionate when they come to these kinds of affairs. But that doesn't mean they aren't informed. When you read about what happened, say in the Tonkin Gulf thing, it seems that the people in Washington weren't very infallible, or the navy wasn't very infallible; and that's what worries me. I think that scientists have every right to speak and sound just as infallible as the politicians. I don't think scientists are very good politicians, in general. They have a very tough time making decisions in situations where it's difficult to weigh things objectively. And I think scientists often get nervous about making decisions in which they can't be absolutely objective, or can't decide on a scientific basis. But I think they have a duty to speak out. The fact that people listen is what makes these other guys mad.

PRUD'HOMME: What is your current work? And what are your future plans?

ROBERTS: Well, I'm getting ready to give some talks. I'm doing a lot of computing; I'm trying to set up a facility here for the organic chemists. I'm working on nine different computing systems at the time, most of which are completely incompatible. It would be nice to have some slaves to do some of this. But on the other hand, the problem is that it's almost as much work to explain to somebody what you want done as to do it yourself in this area. So we're remodeling the building here, and we're going to put in a computer room for me with a lot of stuff. What we've had to do is demonstrate that we really have programs. So I am the programmer.

PRUD'HOMME: I gather there's no basic computer language that is used by all chemists or all scientists, but that everybody uses what they're accustomed to or trained in?

ROBERTS: Well, yes, in effect, that's pretty true. It's a very difficult area from that standpoint. I find it extraordinarily annoying to deal with this. There are very good languages. For example, Hewlett Packard has a language in one of their machines which I find extraordinarily easy to program, much better than almost anything I've ever used. But unfortunately, it's not an acceptable language for other computer manufacturers, and they're not willing to use it. I've spent almost a month trying to get a program fixed up on an IBM computer, which is a rewrite in FORTRAN of a program I wrote in another language on my Hewlett Packard machine in two or three weeks, and I've only got about 20 percent of it done. And that's when I already know how the program works and I don't have to fiddle around with exploring a lot, just translate—but, well, you have to explore some, because the system's a new one. I'm doing a lot of this programming to see how useful the system really is.

And then I'm working with the Huntington Medical Research Institute on NMR imaging, and getting up their capabilities in nuclear magnetic resonance generally; and that's coming along pretty well. They've invested several million dollars to set up a going concern in imaging and related processes. And now that we've got the equipment over there, we're going to have to get the people. I'm hoping that Caltech will be more involved with that. I'm not sure that's going to work; but the facilities there look very good.

PRUD'HOMME: What are you most proud of in your career?

ROBERTS: Well, I think one really has to look at what's happened to the people who worked with you more than anything else. And I've had some very fine students; some are doing extremely well. I think that's probably the most important thing. On the other hand, I'm very happy to have been here and contributed what I can to help this place get and keep going on through a tremendous area of expansion. Science fades away and blends in to other things. I've been happy about the books I've done, things like that.

PRUD'HOMME: And your students.

ROBERTS: The students and the postdoctorals I've help train, I think they are the most important part of it.

PRUD'HOMME: Do you have any regrets about being associated with Caltech as opposed to any other place?

ROBERTS: No. I don't have any at all. I feel the institute's been very good to me, and I've tried to give it a lot in return. I have no complaint about that. I guess I would like to have had somewhat more interaction with the undergraduates. That's difficult here, partly because of the way the undergraduates' ethic is. And yet, you have to blame a lot on the shortage of time; you've only got so many hours in a day. And I guess if someone wanted to really take his attention and spread it over the undergraduates, why, you could do that. But at the same time, there's an awful lot of other things that you couldn't do.

PRUD'HOMME: What do you mean by their ethic?

ROBERTS: Well, most of them are not very aggressive, it seems to me, about trying to find out more. That may be the professor's fault—I'm not sure. But they don't often come up and ask about more after a lecture, or come around to see a professor and say, "Well, I didn't really understand what you said in class." I don't know whether it's all that clear in class and I find it hard to understand why they seem to show so little extra interest. So I'm a little disappointed in the undergraduates in that respect. I don't know what I expect of them or what I expect of the faculty, but I think there has not been enough interaction, unless you become something like a resident of student houses or dean of students or a Feynman or something like that, which is certainly not my bag. But I don't think it's really different at many other places. But it did seem different when I was an undergraduate at UCLA. We hounded the professors a good deal more than they do here. And we weren't paying any tuition! [Laughter]

PRUD'HOMME: It may be that this is another silent generation.

ROBERTS: No. I think it's something different; I don't quite know what it is.

The other thing about the student ethic is this business about working nights. They tend to be night people. Eight o'clock classes are anathema; there's just no question about it.

PRUD'HOMME: Is there anything else you'd like to add? We've not discussed the Athenaeum, and the Associates.

ROBERTS: The Athenaeum is something I can get some satisfaction out of, because I got Bob Ireland to go in there and run the Athenaeum. That was an interesting thing.

I was chairman of the Board of Governors. And the Athenaeum in that period, when I started, was not doing very well. You'd go there for dinner at night, and maybe six or eight people would be there. It was really depressing. And the lunches were so-so. But it was still a great place to go for lunch, because of the people. So when I got Ireland involved, I didn't realize he would do so much more than I expected. The Athenaeum was a drain on the institute; and I remember DuBridge saying at a division chairmen meeting in the sixties, "We're not going to put more than $25,000 a year into the Athenaeum!" And yet, it's a great advertisement for the institute in many, many ways. By the time we came along, it was a much worse drain financially, and they were talking about not serving dinner anymore. That was the pattern of what had been happening to faculty clubs around the country. So I got Ireland involved. We went and talked to Morrisroe about changing it. At that time, a vice president named Bob Gilmore, who had worked with DuBridge before, had basically taken charge of the Athenaeum. It was being run by Business Services, or whatever they called it. And I had been on the House Committee once; and Gilmore's emissary to the committee would say what you could do and what you couldn't do, particularly with regard to spending money. Gilmore wanted to keep running the thing as part of the Business Services. So I finally had a shoot-out with Bob on this, and I pointed out that the constitution of the Athenaeum said that the Athenaeum was to be run by the Board of Governors. He wasn't very happy about that—and I don't know whether it was the cause or not, but he took early retirement shortly thereafter. Anyway, we did get that principle established, and this was when Brown had left and Christy was acting president. And Christy was willing to let us go ahead. I don't think Brown would have had that much faith in professors running the Athenaeum.

Anyway, Morrisroe, Ireland, and I had lunch at the University Club one day to thrash all this out. Morrisroe gave us six months to show that we could get the Athenaeum on track. Ireland was able to get a new manager and a new chef and a new second chef and a new hostess. I hadn't realized before that Ireland was such a club man; he belonged to Annandale and he belonged to the Jonathan Club; and he really wanted to make the Athenaeum a full-fledged high-style club, far beyond where I really wanted to go. I wanted to keep it reasonably informal. But anyway, he marched down that road, and that made a lot of the faculty mad, because they felt it was becoming way too starchy for them. So I was trying to hold him back. Anyway, they wound up with new uniforms on the people, all kinds of ideas, and excellent dinners.

Then we tried to do a lot of remodeling, and we really had to raise money. We did a lot of planning, not much of which ever was carried out. There's a wonderful room up above the dining room, which was at one time used as a dormitory. It's a big open room, the loggia. And we wanted to make that into a fancy dining room, but there were too many problems. It would have cost millions of dollars. But we did get quality of the service and the food way up. Then people started coming to dinner; and then it became so attractive that the Associates were using it as a major attraction to get people to join the Associates. During the last couple of years, some of the faculty has been complaining that the Associates are taking the Athenaeum over. That's not really fair; but the faculty feels, particularly at lunch, that things are crowded; and at dinner, things are crowded. The trouble is the faculty doesn't know a lot of people from the administration, the Huntington Library, alumni, and JPL who come to use the Athenaeum; they don't recognize them and think of all of them as Associates. And on top of that, there's been an enormous increase in graduate student use. All of those things have conspired to make it look as though it's not a faculty club anymore. Well, it never really was.

But the momentum has been terrific. The nice thing about it is that, in my view, it has sort of settled down to being about the right kind of place. You know, in the old days, the Athenaeum had a coat-and-tie rule for lunch, except in the summertime—at least when I came, it did. And dinner, absolutely! So, when things changed during the Ireland regime, he wanted to get people to wear coats and ties for dinner. And I was determined to prove that if there was a rule they were going to have to throw me out before it was enforced. [Laughter] But they're really getting their money's worth out of it. I feel it's a great facility and a very fine thing for Caltech; I'm very happy about that.

PRUD'HOMME: You've gotten so many honors and awards. Have any particular ones meant anything really special to you?

ROBERTS: Yes, a few. Well, when I started with the American Chemical Society back in the forties, there was a lot of competition for the American Chemical Society Award in Pure Chemistry, which was given to people thirty-five or under. It was sort of like the Nobel Prize for young chemists. Linus Pauling and a lot of really fine people got that. It was $1,000, which was a third of a year's income or so. So a lot of us really worked to try to get the stuff that would do that. I don't think this award was ever a matter of politics; it was perceived as being awarded on the basis of ability. So that was one that I would have liked to get when I was at MIT, but that didn't happen. They had a tradition of alternating organic and physical chemists, and when I came out here, they had just given the award to an organic chemist at Yale. I was quite surprised, and very happy, when I got it the next year. I had just done the work on benzyne; the award was not for that, although I used the topic in the award address. It really created a stir, because most people hadn't heard about it then; it was a really new thing.

After that, the Roger Adams Award was a very special one. I had been sort of involved in Organic Syntheses, an organization which now has a lot of money; it's a group of people who do books. They were set up after the war to publish recipes for making organic compounds that weren't easily available; and each recipe is tested by a member of the editorial board.

**Begin Tape 11, Side 2**

ROBERTS: [Continuing] Organic Syntheses was a very important endeavor for organic chemistry, particularly up through the thirties. The enterprise had accumulated funds through the royalties on its books, and then through a fantastic series of investments, had increased the corpus amazingly. And when Roger Adams was at the point of retiring, or maybe even afterwards, they decided that they should honor his starting the series by naming an award for him, which was for research in organic chemistry. I think Roger liked that very much.

Anyway, I got that award fairly late, but it was a very satisfying one—particularly satisfying, as I remember it now, because the year before I got it, they doubled the amount of money. [Laughter] I got a lot of kidding from my friends about having pulled that off, because I was on the board of Organic Syntheses—although the award is actually made through the

American Chemical Society, which picks the people involved.

Then the other one which I felt strongly about was the Theodore William Richards Medal, given by the Northeastern Section of the American Chemical Society in the Boston area. That award had a long history, and very distinguished people got it when I was at MIT. Most notably, the one I remember the best was Linus Pauling, who gave a fantastic lecture at a time when his notoriety was high from the McCarthy era, and his well-known liberal tendencies. But the lecture, which was given to a mixed crowd of people, including Richards's widow, was fantastically popular. He told some amusing stories that I can still remember, and in addition gave a very inspiring scientific lecture. That was my first experience with Linus. But there'd been a big change in the American Chemical Society by the time this award came along for me, in that the local ACS section meetings are no longer major events in the academic world. It was a fine occasion, but it wasn't really like it was in the old days, at all. I think this has been a general experience in metropolitan areas. But if you go to the boondocks, you'll find that the local ACS section can be a very important influence.

PRUD'HOMME: You were talking about the fact that when you were an undergraduate, and even at Caltech in the beginning when lecturers came, students would go and listen to lectures, they would listen to guest speakers. But now there's too much.

ROBERTS: Yes. This week, for example, we've had just an unbelievable series of seminars. And sometimes we have had two or three in a day. They may have to have seminars at two o'clock, at three o'clock, and at four o'clock, in order to accommodate the visitors that come. And this has meant that people are much more selective about the seminars that they go to. And that's unfortunate, because they tend to pick the ones that they are interested in, and those that they feel they'll understand more of. And that's not always so good. I remember Ross Robertson at UCLA one time telling me that he was so impressed because he'd come over to Caltech—this was before the war—and he had run into Arthur Amos Noycs. And Noyes had told him, "Well, I've got to leave you now; I'm going to go and listen to a seminar that Linus Pauling is giving. I'm not going to understand very much of it, but I'm going to go anyway." And I think that's very important, to get people to do that. And when Linus was chairman, he used to come to the seminars himself religiously when he was in town. And he always sparked the discussion, always interested in that.