



**DAVID S. WOOD**  
(1920–1998)

**INTERVIEWED BY**  
**SHIRLEY K. COHEN**

**May 25 and June 1, 1994**

**ARCHIVES**  
**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**Pasadena, California**



### **Subject area**

Engineering, materials science

### **Abstract**

An interview in two sessions in 1994 with David S. Wood, Caltech professor of materials science (1950-1988), associate dean of students (1968-1974), and alumnus (BS, 1941; MS, 1946; PhD, 1949). He recalls growing up in Sierra Madre, California, and attending school in Pasadena; family friendship with Russell Porter leads to application to Caltech; bachelor's in 1941. Recalls engineering program in the late 1930s: professors F. Thomas, D. Clark, R. Knapp, F. Converse, H. Clapp; employment with B. Sage and Knapp; Caltech's Pump Lab. Develops interest in metallurgy; work with D. Clark in Impact Lab to study properties of metals. Wartime work on metals with P. Duwez; towards end of war goes to Los Alamos to work on mechanical design of uranium 235 (atomic) bomb; meets later colleagues R. Christy, R. Walker, R. Bacher, and R. Feynman. Postwar return to Caltech; graduate study and thesis on rapid load testing machine; cutaway drawing by R. Porter. Begins collaboration with Thad Vreeland; theory of dislocations in crystals. Recalls Caltech in postwar period and Lee A. DuBridge's presidency. Becomes associate dean of students (1968, under dean P. Eaton); organizes Freshman Camp; involvement with minority students program. Recalls participation with his wife Connie in campus musicals written by Kent Clark and Elliot Davis. Consulting work; work on stress analysis

for industry. Remarks about improvement in pedagogy at Caltech; notes Feynman's lectures in physics as starting point of that trend.

## **Administrative information**

### **Access**

The interview is unrestricted.

### **Copyright**

Copyright has been assigned to the California Institute of Technology © 1998, 2005. All requests for permission to publish or quote from the transcript must be submitted in writing to the University Archivist.

### **Preferred citation**

Wood, David S. Interview by Shirley K. Cohen. Pasadena, California, May 25 and June 1, 1994. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: [http://resolver.caltech.edu/CaltechOH:OH\\_Wood\\_D](http://resolver.caltech.edu/CaltechOH:OH_Wood_D)

### **Contact information**

Archives, California Institute of Technology  
Mail Code 015A-74  
Pasadena, CA 91125  
Phone: (626)395-2704 Fax: (626)793-8756  
Email: [archives@caltech.edu](mailto:archives@caltech.edu)

Graphics and content © 2005 California Institute of Technology.



Dave Wood (third from left) sings "The Richter Scale," with other members of the Caltech Stock Company (from left), Bob Oliver, Jim Knowles and Bill Corcoran. From the Lee A. DuBridge 20<sup>th</sup> anniversary show, *Lee and Sympathy*, 1966. Caltech Archives.

**CALIFORNIA INSTITUTE OF TECHNOLOGY**

**ORAL HISTORY PROJECT**

**INTERVIEW WITH DAVID S. WOOD**

**BY SHIRLEY K. COHEN**

**PASADENA, CALIFORNIA**

**Caltech Archives, 1998**

**Copyright © 1998, 2005 by the California Institute of Technology**

**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**ORAL HISTORY PROJECT**

**Interview with David S. Wood**  
**Pasadena, California**

**by Shirley K. Cohen**

Session 1	May 25, 1994
Session 2	June 1, 1994

**Begin Tape 1, Side 1**

COHEN: Good morning. I'm glad to see you here with us. You want to introduce yourself, and we'll proceed with this interview.

WOOD: OK. My name is David Wood. I'm professor of materials science, emeritus at the present time.

COHEN: And I know you've been here a long time, but why don't we really start from the beginning? And let's talk a little bit about your growing up, because I see you were born in Ohio.

WOOD: I was born in Ohio, I'm told, but I was moved to Southern California at the great age of two. So I really don't remember Ohio.

COHEN: I see, OK. Ohio always piques my interest, because I'm from Cleveland. OK, so you came to California early on. What did your parents do? Can you say something about your father?

WOOD: Yes, he was an engineer. He worked in industry all his life. The reason I happened to be born in Ohio, he got a job with the Goodyear Tire & Rubber Company way back then.

COHEN: Were you in Akron then?

WOOD: Akron, right. I was born in Akron, Ohio. But both my father and my mother came from

California. They were both born in San Francisco, actually. They were in Akron I guess maybe four or five years before I came along. They got tired of it. They liked California, so they came back. [Laughter] As a matter of fact, I'm the only member of my immediate family who was not born in California. Both my parents and both my brothers—two brothers—were born here. So I'm the only outlander in the family, so to speak.

COHEN: So it was a San Francisco family?

WOOD: You mean my parents?

COHEN: Yes.

WOOD: OK. My father's father was also an engineer. He was in the dredging business. You know, digging out harbors and stuff like that. And so he moved around the country. He got jobs doing some dredging in San Francisco Bay. Previously, he dredged Galveston harbor. He had dredging jobs in New Jersey. So my father, when he was growing up, moved all around the country from time to time. It just happened that he was born at the time my grandfather was in San Francisco.

COHEN: I see, I see.

WOOD: Now, my mother never lived for any length of time in San Francisco. She was born there because her mother had married a Scotsman named Stevenson, who came to this country more or less just in time for the Spanish-American War. And he got shipped out to the Philippines quickly. And he died there of malaria or something like that. So my grandmother was in San Francisco, waiting for him to come back. Which he never did—but, anyway, my mother was born there. Now, my mother's family, actually, going beyond that, was involved in San Francisco earlier, I think. One branch of her family went through the 1906 earthquake—lost their home in the great fire. And there was some of the family already down here in the orange-growing business. And the branch that was still in San Francisco came down here to be taken care of while they rearranged things in San Francisco. So there's a connection with San Francisco anyway.

COHEN: Well, so you're really from a very old California family.

WOOD: Yes, my mother's family moved here from Wisconsin in the 1880s. At that time, the popular thing to do to get "rich" was to raise oranges. They never got rich, of course, but they went into the orange-growing business. They had forty acres of oranges up in Sierra Madre, where I live now.

COHEN: Very good. So you actually grew up here in Los Angeles. Where did you go to school? Public schools in Los Angeles?

WOOD: Well, no, in Sierra Madre and Pasadena. When I was a boy, Sierra Madre was a separate elementary school district. At that time, it was K-6 [kindergarten through sixth grade]. Then we started to come to junior high school, as we called it then—the seventh grade.

COHEN: What junior high school?

WOOD: Wilson—out here on Del Mar. At that time, they had the so-called 6-4-4 system. You went six years to elementary school, four years to junior high school, which takes you through tenth. Then there was pre-JC [junior college], which was eleventh and twelfth grades, and then two years college.

COHEN: Right.

WOOD: I didn't do the two-years-college business, because I came to Caltech.

COHEN: I see. So did you go right here, where PCC [Pasadena City College] is?

WOOD: Right here, yeah. I finished pre-JC—I finished high school, in other words—in 1937.

COHEN: Were you influenced by your teachers in school to go into engineering and science, or was that something at home?

WOOD: I guess it was largely home. But the teachers were also encouraging, and so on. Mathematics teachers—and even in junior high, I recall—were encouraging and very good. And a general science teacher, a guy who taught biology class. And then at pre-JC—eleventh and twelfth grades—they had some very good people. Guy named Skinner, who taught physics, and he came down here to Caltech and learned about what Caltech does in physics and what they would like to have high school students know. And so he taught a physics course designed for people who wanted to go to Caltech. And other places, too, but I mean that sort of thing. And the same with mathematics. There was a woman mathematics teacher there, I remember, named Plummer, who was very good. Another teacher I recall was a physicist actually from Caltech. You know, that was the Great Depression, and a lot of people had a hard time getting work. This was a fellow named Jack McMorris, who had recently gotten his PhD in physics at Caltech and couldn't get a job. And so he was teaching chemistry at pre-JC. And so I had chemistry from Jack McMorris. He was later, during the Second World War, killed in some sort of airplane accident off the East Coast. I've forgotten where.

COHEN: So you really had very fine teachers and a good high school.

WOOD: Oh, absolutely excellent teachers, yes. Can't complain. I had some good teachers in grade school, too.

COHEN: So then it just seemed natural for you to come over to Caltech, being so close?

WOOD: Yes, and I was interested in engineering. At that time, I thought I would like to design bridges and stuff like that. I changed that, but anyway I wanted to be an engineer—you know, because my father was, and my grandfather, and so on. But I also liked that sort of stuff. And then, at the time, the family happened to know Russell Porter.

COHEN: We all know about Russell Porter.

WOOD: You know all about Russell Porter. [Laughter] That was a social connection, actually. My

parents were musicians, amateur musicians, and they played trios frequently with another friend in town who played the cello. And sometimes Russell Porter would come to the house and listen to them. Or with some other friends who were there, and so on. So he suggested that I should apply to Caltech if I was interested in engineering. And I think he actually brought me down to the campus and introduced me to the admissions officer or registrar or somebody like that. You know; where you get the applications and get going on the paperwork and that sort of thing.

COHEN: Was he already doing his sketches?

WOOD: Oh, sure, sure. I've forgotten what year he came here. You can look that up. [Ed. note: 1928]

COHEN: Those pictures were from the late thirties already, weren't they? Some of them?

WOOD: Yes, some earlier than that. He was already here, sure. He was at Caltech working on the 200-inch Palomar telescope, doing all those beautiful drawings. Many years later, after the Second World War, I designed a piece of machinery for my own PhD research and built it, and we got him to make one of those drawings of my machine.

COHEN: Ah! Do you have that?

WOOD: It's hanging in the hall up in Keck, just opposite my office door. It's the same sort of thing. It's not nearly as fancy, of course, as the Hale telescope drawings, but it's a very nice drawing. It's the same sort of deal.

COHEN: Very good. So you went to school here at Caltech, and you went directly into engineering.

WOOD: Yes, well, of course, as you know, I wasn't an engineer until later. You know, you're a Caltech student to start with.

COHEN: Ah, so you did the standard physics, mathematics—

WOOD: Oh, yes, you did all that standard stuff that we largely still have.

COHEN: What year was this?

WOOD: I started here in '37. Got my bachelor's degree in June of '41, just before this country got into the Second World War.

COHEN: Let's talk about those few years when you were a student here. Do you particularly remember any of your professors who were outstanding?

WOOD: Well, I remember being in freshman physics—or sophomore, or both, I don't know—with Earnest Watson. What other people did I have? Morgan Ward, as I recall, in mathematics, and probably others. And a lot of the engineers. Franklin Thomas, and of course Don Clark, whom I later worked with for many years. Bob Knapp in hydraulics; Fred Converse in testing and materials, and so on. Many people. And I had a few little part-time student jobs while I was an undergraduate, to earn a little money. I did some design work. The first job, I guess, was for Professor Howard Clapp. He taught mechanical design. He had that beautiful office in the tower of Throop Hall. You have the picture out there—that marvelous rotunda with glass all around it—that was his office. Anyway, he was doing some research on the life of ball bearings, and so on. And I helped him. I did the detail design work on a machine for testing ball bearings. Later on, I did some design work for Bruce Sage. Do you know him? He was a chemical engineer. He and William Lacey headed for many years a large project funded by the American Petroleum Institute to measure all the thermodynamic properties of all kinds of hydrocarbons that come out of oil, which the petroleum people need to run their refineries properly, and all that sort of business. And that involved apparatus for subjecting these things to fairly high pressure and change in temperatures. So I helped him one time in designing some of that sort of apparatus.

COHEN: These were not summer jobs?

WOOD: No, they were mostly jobs during school. You know, a few hours a week. Students still do

that, all the time. So that was sort of standard procedure.

I was always impressed by Bruce Sage, because he had a big office. Not quite as big as this one [Ed. note: the rare book room of the Archives], but almost as big. And I was offered one corner and a drawing table. And he was over there, and his secretary was there, and I've forgotten her name. But Sage was a guy who did everything with memos. People were always getting memos from Bruce Sage on this, that, and the other thing. And the way that worked was, his secretary was exceedingly good. He would just sit beside her and just talk. And *bingo!* It went right on the page—her hands on the typewriter. She didn't transcribe it or anything. No shorthand. It went right on the typewriter. Right now. Right on the printed page, *boom!*

COHEN: And then she would send it out to everybody.

WOOD: So anyway, that was kind of interesting. Another part-time undergraduate student job I had for a while was in the Pump Lab. I don't know if you remember that. It's at the west end of Guggenheim. And that was Professor Robert Knapp's big deal. He ran that affair. The funding and so on for that, as I understand it, was from the Metropolitan Water District of Southern California, when they were building the Colorado River Aqueduct. And so there was a lot of pumping of the water, and they were interested in getting the most efficient pumps they could. A lot of electrical energy was involved. So they had a scheme whereby the pump manufacturers, as part of the bidding process, would submit a model pump. And Knapp's Pump Lab would then test it, to see how efficient it was. Knapp had developed some very accurate and fancy equipment for testing pumps—measuring accurately how much energy it took to run them, how much pressure. You know; all that stuff. So I worked there.

COHEN: So this was in some sense a commercial operation?

WOOD: Well, they also took the opportunity to do a lot of research on the side. [Laughter] For example, one of the things I did as a student was take—I don't know if you know what a centrifugal pump is. It's got a so-called impeller inside, which is actually the thing that makes the water go. So one of my jobs was to test changes in the veins inside the impeller. One time I was busy—this is just with a file—filing off the tips of certain veins to make them a little shorter. Change them just a

little bit, to see what effect that would have. So they did a lot of basic research on fluid turbomachinery. They learned, for example, that if you have an efficient pump and you run it backwards, it's a very efficient turbine. In fact, Knapp got very nice results for pumps running under all possible conditions—all directions of rotation and pressures. So you could have positive pressure and positive flow, and negative pressure and positive—all that sort of stuff. And whether the energy is going in or coming out, depending on whether it's a turbine or a pump. So they did a lot on that—a whole lot of stuff on that. Learned a lot. And made very efficient pumps in the end. Ninety percent of the energy got put into the water, where it's supposed to be. And the reason the Metropolitan Water District was willing to put money into that operation, of course, was that a one-percent gain in efficiency of a pump on the Colorado River Aqueduct—that's fifty years ago or more—it's an awful lot of money. So that's why they did it that way.

COHEN: So all that pump testing was going on right here at Caltech?

WOOD: That's right. Then after that project was over, then came from the Bureau of Reclamation a big project up in the state of Washington, on the Columbia River—the Grand Coulee Dam. The major reason for the Grand Coulee Dam up there in eastern Washington is basically to pump water out of the Columbia River up onto the high plateau up there. There's a vast area that's irrigated now. It's just basically a desert—eastern Washington, from the Columbia River. It's irrigated by water pumped out of the big lake behind Grand Coulee Dam. Those are very big pumps. Much bigger than even...

COHEN: So those pumps were tested here also?

WOOD: Those models were tested in a similar way, right here. They had this beautiful facility for doing all that sort of thing. So that was done.

COHEN: And you had some job there?

WOOD: Yes, undergraduate student jobs there.

COHEN: But that was not your field of engineering?

WOOD: No, not particularly—I just worked there for a while.

COHEN: Who were some of your classmates? Was Willy Fowler here about that time, too?

WOOD: Oh, no, he was before me.

COHEN: He was before you. He was a graduate student.

WOOD: He may have been a professor by then. He may have been on the faculty by then. I don't really know. He was certainly around here. [Ed. note: Willy Fowler was a research fellow in 1937. He became assistant professor of physics in 1938.]

One thing I can recall as an undergraduate is seeing an ambulance bringing cancer patients to Kellogg Lab to be irradiated. The Van de Graaff accelerator they had at that time. That was in the early days of trying to deal with cancer by radiation.

COHEN: So patients were brought over from the [Huntington Memorial] hospital here to Kellogg?

WOOD: Yes, to irradiate them, and then they took them back. It was basically, as far as I know, an experimental sort of thing. Let's see, who was the doctor that was...

COHEN: Well, I think it was Mrs. [Charles] Lauritsen, wasn't it? Wasn't she a doctor?

WOOD: I didn't know that. Could well be. There's another name floating around back there that I don't have.

COHEN: [Richard] Harrison?

WOOD: That's right! That's right! That's it! You got it! Harrison, right.

COHEN: OK, so that was an experiment going on from the hospital.

WOOD: That was going on at that time. Well, there were lots of other—

COHEN: And you were aware of that as an undergraduate because it's a small place, and one finds out what's going on.

WOOD: That's right. Sure.

COHEN: So then you graduated, and then what did you...

WOOD: And then I graduated. But before that, the last job I had as an undergraduate was with Don Clark and his group. He had set up the Impact Lab. He was interested in the behavior of metal strength—behavior when you really pulled fast, it went bang. So by the time I came along, he had quite a substantial laboratory with several fancy impact-testing machines set up. It was in a room under the front steps of Throop Hall. Anyway, I went to work there, especially in the summer of '41, after I graduated. Don Clark had a graduate student who had just finished his PhD, named LeVan Griffis. There were a few little experiments he wanted to do in addition to what LeVan had done for his PhD thesis. And he wanted some help doing these experiments—machining test specimens, and so on. By that time, I knew a little bit about machine-shop work. So anyway, I worked that summer with him, doing that sort of thing. And then in the fall of '41, I started being a master's degree student.

COHEN: Right here at Caltech?

WOOD: Right here, yes.

COHEN: Who was head of engineering at that time? Was Lindvall here yet?

WOOD: I've forgotten exactly when Fred became chairman. I don't think Fred was chairman of engineering yet, at that time. I think he became chairman of engineering after the Second World

War—shortly after. It might have been Franklin Thomas, for all I know. [Ed. note: Fred Lindvall succeeded Franklin Thomas as chairman of the Engineering Division in 1945.] But I just don't remember. I wasn't involved in those high echelons. [Laughter]

COHEN: So then you were still in mechanical—or was there such a thing as civil engineering then?

WOOD: No, I was really in metallurgy, but we didn't have anything called that at the time. So I was called a mechanical engineer. But Don Clark was interested in metallurgy—in various aspects of it. So the actual work—well, it was mechanical properties of metal stuff that we worked on. But it was all under the rubric of mechanical engineering. So all of my degrees say I'm a mechanical engineer. [Laughter]

COHEN: A coverall, right. So did you finish your master's degree then?

WOOD: No, no, because, as you recall, on December 7, 1941, something happened. And instantly I no longer did graduate work. Right about then, a new research project had started in the Impact Lab, promoted by a theory of Theodore von Kármán about the propagation of plastic waves in metals. And the point was that this apparatus that Don Clark had developed was suitable for testing that theory, checking on it.

COHEN: What do you mean by plastic waves?

WOOD: All right, if you take a piece of metal or rubber, anything else, and you stretch it a little bit and let go, it goes back. That's called elastic deformation. But especially metals, if you stretch them a little bit farther, they don't come back all the way. They're permanently deformed. You can see it when you bend a paper clip, right? That's plastic deformation.

COHEN: Oh, that's plastic? I know about elastic, but I didn't know about plastic.

WOOD: Yeah, that's plastic. And this theory of von Kármán's had to do with—say you take a long rod of material. You suddenly set one end in motion at high speed. That generates elastic waves in

it, but also, if you do it fast enough, it generates plastic waves. And the project was to study those things. And in that connection, there was a European refugee that Kármán knew and got to come over here named Pol Duwez. And so Pol came, I think, in the fall of '41. Of course, Belgium had been overrun already, and he was already a refugee in Europe. Kármán made arrangements for him to enter this country. And he came as the head scientific person on this project. Don Clark ran the business end of the whole thing and hired people and so on. Pol Duwez was the idea man, and I became the engineer who did the experiments and designed new equipment and all that sort of stuff. And that project was sponsored by the government, by the NDRC—the National Defense Research Committee—when the war started. And we did a great deal of work and built up quite a group of people.

COHEN: Now was this all still in Throop basement?

WOOD: That was in Throop, that's right. The laboratory itself, and then we had one of the rooms down there, where we all had desks, and we were working in a room about this size, different shape—developed other kinds of impact testing, with plastic waves, and so on. And incidentally, another person who came a little bit later into that project was a mathematician who came here, H. F. Bohnenblust, from Princeton.

COHEN: He came to work on that project?

WOOD: He came and worked on that project. He cleaned up the mathematics involved and did that very nicely. So that part was interesting. Well, that kept on going, and then, in early 1944, somebody came from Los Alamos to talk to me about going there.

COHEN: Who was he?

WOOD: A fellow named Cornog. Robert Cornog.

COHEN: They were recruiting engineers?

WOOD: They were recruiting people for work at Los Alamos.

COHEN: Did you know anything about Los Alamos?

WOOD: Not at that time. I learned immediately a little bit after this guy came to talk to me. And then a little later, the famous mathematician John von Neumann happened to come to the campus. And I met him very briefly over at the Athenaeum, and he essentially taught me what the project was all about. Why should you go there? You know, why should you think about this? And then they asked me to come and visit the place, which I did in January, as I recall.

COHEN: Of 1944?

WOOD: Of 1944, and I decided to go there. And by the time I actually got there, it was about March '44.

COHEN: Now all this time you were really just working. You weren't still working on a master's degree?

WOOD: No, I was doing no academic stuff; just working—war work. Sure. Sure.

COHEN: So this was really, in some ways, a great adventure.

WOOD: Oh, absolutely. Absolutely. Yeah, sure. That's where I met Connie, of course. And we were married then. [Laughter] So then I stayed there until the end of the war.

COHEN: I see, and what did you do at Los Alamos?

WOOD: Well, I worked on the uranium 235 bomb—the mechanical design of it, which involved shooting one piece of uranium into another.

COHEN: So you were still doing the same impact work?

WOOD: That's exactly—that's why they got me to come down there. There were impact problems with material, and all that stuff. So that's why they recruited me.

COHEN: You must have been really pretty good at what you were doing.

WOOD: Well, we had a good time, and it worked. [Laughter] But that was the old bomb, which was never tested, except over Hiroshima, and was instantly obsolete. There was one, period. End. As far as I know, that's the only bomb of that design that was ever built, because it used uranium—essentially pure uranium 235. Extremely expensive. Natural uranium has—I'm not sure I remember this number correctly, but something of the order of 0.7 percent of it is uranium 235.

COHEN: I know it's a very small amount.

WOOD: It's a small amount.

COHEN: And hard to get it out of there.

WOOD: Yes. You can't do it chemically because it's the same chemical element. It's an isotope. So that's why they built Oak Ridge—this vast factory.

COHEN: Just to get that uranium out.

WOOD: They tried at least two methods simultaneously. Ernest Lawrence had essentially built a giant production cyclotron. Separate with the cyclotron. The other method—at Oak Ridge—was a gaseous-diffusion plant. But that separation just makes that sort of a bomb exceedingly expensive to do. At the same time, of course, they were making a plutonium bomb by putting in a big power reactor at Hanford in Washington. And then shutting it down after a while, and when it converted some of the U-239 into plutonium, then you could do chemical separation—separate the plutonium from all the other junk in there chemically.

COHEN: And when you got out to Los Alamos, were there people you recognized from Caltech who were already there?

WOOD: Well, there were people from Caltech—not that I recognized them, because I hadn't had contact. Notably Robert Oppenheimer, of course. Of course, I got to know other people who later came to Caltech, like Bob Christy, for example. And Bob Walker.

COHEN: So you first met these people at Los Alamos, and later became friends and colleagues with them here?

WOOD: That's right. So that was a big thing. So we stayed there until after the end of the war. But then I wanted to come back and do my graduate work. We left Los Alamos in December '45. In other words, a few months after the end of the war and the bombs dropping and all that business. So I landed back here at Caltech as a graduate student in January '46 basically, working with Don Clark again, although we did some new kinds of experiments that I developed as a graduate student. So I got my PhD.

COHEN: Don Clark was your professor then?

WOOD: Right, in '49. Finished up. Well, I got a master's degree along the way, too.

COHEN: That was just incidentally?

WOOD: Yes, incidentally.

COHEN: And you continued doing the same sort of work on your PhD thesis? You continued with this same line of work after you came back from Los Alamos?

WOOD: Well, we went into something called rapid load testing, which is a little bit different from impact testing. And I developed this machine that I mentioned, the one in the Russell Porter drawing that shows, you know, cutaways, so that you can see the inside of it. Those lovely Russell

Porter drawings! Maybe the Archives would like that.

COHEN: Oh, they'd love it. Judy particularly loves Russell Porter.

WOOD: Well, OK, that's a good point. We should get that over here. It doesn't serve any useful purpose anymore, hanging on a wall in Keck.

COHEN: Oh, it'll get an honored place.

WOOD: That was interesting. I knew Russell, of course, already, as I mentioned. But we decided it would be nice if we had one of his drawings of this piece of machinery. And so I went and talked to him. And it's amazing how he did that. I brought him the actual construction drawings I had made—the design drawings for telling the machine shop how to make this piece of machinery. And he selected the ones he needed. And he had a big drawing table. He put one view over there, and another view other there, and he'd make these perspective projection lines. It's a geometrical proposition, making a perspective drawing accurately. And he did that. And then the artistic part comes in. He'd put shading in. You can see it. But it's a geometrically accurate perspective projection based upon the working drawings. And it's the same as the method he used on the Hale telescope. It's the same sort of proposition. So I learned a little bit about how he did that. That was interesting.

COHEN: How big is his drawing?

WOOD: Oh, this is not that big—three feet by two feet, maybe, something of that order. It's framed and has a piece of glass in front.

COHEN: Look forward to seeing it. So then you finished up your degree. And stayed on?

WOOD: Right. Right. The first year, I got an appointment as a lecturer in mechanical engineering, for the 1949-50 academic year. And then, starting in 1950, I was an assistant professor, et cetera, et cetera.

COHEN: Meanwhile, the engineering school was going through various kinds of organization. Were you involved in any of that?

WOOD: Oh, yes. By that time, Fred Lindvall was the chairman. At some stage, I've forgotten exactly when, I moved over to an office in Thomas Building.

COHEN: Out of Throop?

WOOD: Yes. And I had some graduate students. The main one, still the most important one, is Thad Vreeland. He's now retired and run off to Montana. He and I worked together for a good many years. And there were others who went hither and yon—industry and academe, and so on.

COHEN: So how would you describe the changes that have taken place since you came here? I mean, you've been here the whole time.

WOOD: Well, of course, the big change came with the Second World War, as you well know. Shortly after the Second World War, the federal government started to sponsor all kinds of research, including engineering as well as pure science. And [Lee A.] DuBridge came [as president]. And Caltech expanded in terms of buildings, and all that business. And all this federal support came into the picture. It kept growing until the late sixties. And all these federal agencies were set up. First off, we started under ONR, the Office of Naval Research.

COHEN: Right. Well, I know they funded the Owens Valley Observatory.

WOOD: Right. Then as time went on, various other federal agencies were set up, like NSF [National Science Foundation] and NIH [National Institutes of Health], et cetera. But that was the big change: most of the research, whether it was science or engineering, became government sponsored. Before the war, it was all mom-and-pop, catch as catch can.

**Begin Tape 1, Side 2**

COHEN: OK, continue.

WOOD: Well, one example of the by-your-bootstraps way of supporting research in those days was Carl Anderson's work with cosmic rays, when he eventually discovered the positron and mesons and so on. I guess through Millikan's connections with the navy or something, they got some old generators that he used for power, and some old magnets—I don't know. You'd find old junk. And in this lab of Don Clark's that I worked in so much—the Impact Lab—one of the main impact testing machines was an old direct-current generator that he got from the Southern California Edison Company. They had essentially discarded it—didn't need it anymore. It had been up in the high Sierras, with a pump-wheel thing on it, as part of their hydroelectric operation up there from Huntington Lake and all that area. But now it was sitting in their yard somewhere down around here. So the Edison Company gave it to Caltech. That's the way it was done in those days. And the Pump Lab was similar. Well, that was a government lab, but it was a local thing, not the federal government, and associated with a specific construction project, and so on. So, that was the big change after the Second World War: from that sort of local support to this big, continuous federal funding.

COHEN: Well, now that would have really affected graduate students. But how about the undergraduate training? Did that stay the same?

WOOD: Oh, that's pretty much the same. Well, eventually there were changes. For example, having spent quite a few years helping the physicists teach freshman and sophomore physics, I found out about that. There've been big pedagogical changes in the syllabus in that course since when I was an undergraduate, tremendous changes.

COHEN: Well, of course, atomic physics came in.

WOOD: Yes, quantum mechanics, for example. [Laughter] Well, and the teaching of everything is much more advanced, much more sophisticated. And these new areas; when I was an

undergraduate, nobody did quantum mechanics, except super-advanced PhD students. First they had to learn all sorts of fancy Hamiltonian mechanics and all sorts of stuff, it was thought then. You had to go through all that monkey business before you could hope to even begin quantum mechanics. Well, nowadays they teach quantum mechanics to the sophomores.

COHEN: They don't worry about the other stuff.

WOOD: Richard Feynman, among other people, showed that you don't have to go through that other stuff. You just start talking about quantum mechanics and the kids lap it up.

COHEN: I see. So was that an innovation of Dick Feynman's? Or was he just part of the trend?

WOOD: I don't really know the answer to that. But that's my impression. These are the big red books—*The Feynman Lectures on Physics*. As far as I know, that's when quantum mechanics got put into the freshman and sophomore required courses. And the teaching of other aspects of physics was vastly improved, in my opinion, and made more sophisticated. Electricity and magnetism, for example. The freshmen now, they learn all the mathematics used for that, and so on. We never heard about that when I was an undergraduate.

COHEN: But what's interesting is that you guys doing it had no trouble going into it.

WOOD: Well, you learn it, yeah. [Laughter]

COHEN: You didn't say, "Well, we can't do this—that's not how we did it." You just went ahead and did it.

WOOD: That's right. You go ahead and do it. There have been very big advances in pedagogy, I would say, in spite of the fact that the basic Caltech undergraduate curriculum looks the same as it did when I was an undergrad. Two years of physics, two years of mathematics, and a year of chemistry, plus the humanities requirements. You know, those basic requirements are very little changed. But the contents are tremendously changed.

COHEN: Yes, of course. And the students are probably just as bright as they were then, too.

WOOD: Or brighter.

COHEN: But more is demanded of them.

WOOD: Sure. Sure. Oh, yeah—definitely!

COHEN: So, why don't we stop here?

**DAVID S. WOOD**

**SESSION 2**

**June 1, 1994**

**Begin Tape 2, Side 1:**

COHEN: Well, good morning. I was just thinking that maybe you had a little bit more you might want to say about some of the people you were with at Los Alamos who later became your colleagues here at Caltech.

WOOD: Yes, well, one person we got to know very well there was Bob Walker, a physicist who was here for many years. He's now retired also and living in New Mexico. Another one was Robert Christy, of course. And Richard Feynman was there. And then Bob Bacher.

COHEN: Now some of these, like Bob Christy or Bob Bacher, must have been in much more—in leadership positions.

WOOD: Oh, absolutely—yes, absolutely. [Laughter]

COHEN: Was this your first meeting with these people at Los Alamos—Christy and Bacher?

WOOD: I think so, yes; and Walker and Feynman.

COHEN: Of course, they didn't come directly to Caltech, did they? They went to Cornell, first?

WOOD: Yes. Yes. Yes. I don't think any of them came directly to Caltech after Los Alamos. As you say, they came via various other places, but Cornell is certainly one of them. I'm not sure about Christy. I know that Bob Walker and Feynman were elsewhere for a while before they came here. And Bacher, too.

COHEN: You all became quite close friends when you were at Los Alamos.

WOOD: Well, mostly with Bob Walker. I knew the other people.

COHEN: Of course, your wives were close friends. So that made a difference with Bob Walker.

WOOD: That's right.

COHEN: You know, it's interesting—this week we're getting such a deluge of fifty-year anniversaries, D day and all. Actually, it's fifty years for Los Alamos, too. Are there any kinds of celebrations being planned or anything like that?

WOOD: Apparently not. They may have had something, but Los Alamos put on, ten years ago, a fortieth anniversary. But they didn't do anything now. I guess they thought, maybe we better do it at forty, before all these guys die. But that was very nice, and interesting.

COHEN: Did you go to that?

WOOD: Yeah, we went to that. And saw lots of people we used to know there—well, we still see from time to time. And the Walkers were there, of course. And the Christys went from here. I don't remember whether the Bachers went or not. I don't think the Feynmans did, no. I didn't think Bacher did.

COHEN: Was it just a celebration, or was there actually a symposium?

WOOD: There was a symposium, yes. It was very interesting. Oh, I'm sorry—Feynman was there, because he gave a talk at the symposium. He gave a talk about what fundamental limitations there are in how small you can make electronic devices.

COHEN: Did he think there were any such limitations?

WOOD: Well, he thought there weren't any until you got down to the size of individual molecules.

Of course, that was a question that was originally put to him by [inaudible], I think. But he talked about that. And there were a lot of other interesting talks there. Hans Bethe talked about new calculations of how supernovas work. So that part was very interesting. But there were lots of social events, things like that.

COHEN: Of course, there must have been many changes, because it's now a big university.

WOOD: Oh, yes, of course. Los Alamos has changed a great deal since the war. Well, it was built during the war by General [Leslie R.] Groves, so it looked like an army camp, basically.

COHEN: Well, in some sense it was.

WOOD: Right, it was. It's more like a regular small American city now, except it's in a lovely place up there. Have you ever been there?

COHEN: Yes, but not actually up to the lab. We've been close, but we didn't drive all the way up. So OK, that's interesting. They did a big fortieth and the fiftieth is quiet.

WOOD: Right. They may have had some celebration amongst themselves, down there, but they didn't invite people from far away to come.

COHEN: So after the war, you came back to Pasadena. That seemed the natural thing for you to do. And you went into—were you a graduate student immediately when you got back here?

WOOD: Yes.

COHEN: And I think we did talk about that last time. So, you finished your PhD, and you stayed here. Did you continue with the same research that you had been doing?

WOOD: No. Oh, well, for quite a while I continued with extensions of the research I had done for my thesis work. And then I gradually moved into various other things, along with Thad Vreeland.

And at that time, early in the fifties, a new theory involving mechanical strength properties of crystalline materials—metals and other crystalline materials—came along called the theory of dislocations in crystals.

COHEN: Theory of...?

WOOD: Theory of dislocations in crystals. Well, the theory had been proposed and gotten a little bit started just before the war, in 1939 and 1940. But of course the war stopped all that sort of stuff. Then in the early fifties, lots more people around the world, got involved in this idea. And in 1955 and '56, people started to develop experimental methods of actually seeing these dislocations in crystals on an atomic scale. Features of crystals, their faults—that is, their dislocations. And so that was very interesting. We started doing experiments using single crystal materials, where you can study this. And we did various techniques of direct experimental observation of dislocations and how they move. Particularly, we used one called etch pits. You can etch a crystal in a certain way, and you see where the dislocation—dislocations are a line, like a worm going through the crystal. And when they meet the surface, the atoms are messed up, so to speak—right where the dislocation meets the surface. And then if you attack the surface of the material with delicately adjusted chemical attack acids, it'll cause a little pit there, because chemistry goes faster where the atoms are already mucked up. So the etch pits are one method of seeing. Another one we used extensively was a certain special form of X-ray diffraction called X-ray topography. Then you can see dislocations that are close under the surface, like one or two microns under the surface. You can see the line. You can see the whole thing. And we developed methods of finding out how fast dislocations can move in crystals.

COHEN: Did you make your own crystals?

WOOD: We grew the crystals, right, starting out with zinc, which is fairly easy to grow, because it has a low melting point. And then we graduated to copper. We did copper crystals and we did aluminum. That's intermediate. So Thad and I and our various students worked on those kinds of things. And we developed methods for sending stress waves into the crystals, such that we could basically push on the dislocation for a known length of time with a known force. And we'd take

these pictures and see where the dislocations were before we did that, and then we'd push on them for, oh, ten microseconds or something, and we'd see how far they got. And so we could measure their velocity, as a function of how hard you push on them—that is the magnitude of the stress. And the other important factor, of course, is the temperature. Leaving out many other factors, like how pure the crystal is, and things of that sort.

COHEN: Now, would any of you have been consulting in the meanwhile?

WOOD: Oh, yes, of course.

COHEN: I mean, were there commercial applications for this that were immediate?

WOOD: Oh, no, there weren't commercial applications of this—still aren't, as far as I know. Specific dislocations is a field which gives you background, good ideas of a way to do things. It's similar to—well, not quite, but it's comparable—to the geologists' idea of plate tectonics, which helps them in all kinds of ways in understanding what's going on in geology.

COHEN: I read a very interesting article about a blinking microscope.

WOOD: Oh, yeah. Well, that's the etch pit business. You see the dislocations by etch pit. What you end up doing is looking through a microscope, and you see these little pits on the surface of the crystal. And then you—like these other experiments—you push on them for a short time, like ten microseconds, and then you want to know where they got to. But out of all these little pits, only a few of them move. And if you look in a normal sort of a way at the picture beforehand and the picture after, it's impossible—you can't see any difference. But then I heard about Fritz Zwicky. And your husband's in the astronomy business, you know about this. You take a picture of the sky at one time, and then sometime later you take another picture. And you want to know what's changed—it's the same problem. So I went over and talked to Fritz, and he had this blinking microscope. You look in the eyepiece and alternately you see this picture and that picture.

COHEN: Both at the same time?

WOOD: No, no; alternately in time. One second, or a half a second, you see this one and then you see that one. And you register them. If nothing has happened, you won't see any change. But the amazing thing about how the human eye and brain work—if out of all those little spots, one of them has changed a little bit, and you blink it like that—*bingo!* You see it, right away. It's amazing. And that's the way that Fritz discovered all those supernovas and other things like that. It's the same problem. So we adapted that idea. Instead of looking at big astronomical plates, though, our thing...

COHEN: The crystals were very small.

WOOD: Were very small. But it worked. It was great.

COHEN: So you had something to do with Fritz Zwicky?

WOOD: That's right. We learned from Fritz and got this technique from him. I don't know whether he invented that technique, but he was using it, and he showed me how it worked.

COHEN: And so he was nice to you, and hospitable.

WOOD: Oh, yeah, sure.

COHEN: So then did other people start using this technique? I mean, were you really an innovator in this?

WOOD: I haven't heard that other people have used that technique for dislocation studies. They may have.

COHEN: Now, I also have here that you then started to do a bit more administrative work. You became the acting associate dean of students after some time.

WOOD: Yes, that was later on. Well, it came as usual around here. Some of those kinds of positions come along. Somebody's going to retire, or leave for whatever reason, and they need somebody. And so DuBridge asked me if I would do that. So I did. That was when Paul Eaton was the dean of students. Foster Strong had been the associate dean of students for many years. I took over his job when he retired.

COHEN: Now there seem to be many deans of students.

WOOD: Yes, well, over the years they've added more people to that operation. For example, now there's a vice-president for student affairs, which we didn't have back at that time. Just a dean of students and associate dean of students, that's all there was.

COHEN: And that was it. So, what sort of thing—was that a full-time job for you?

WOOD: Oh, no, no. I would say about half-time I spent on it.

COHEN: Did you enjoy it?

WOOD: Sure, sure! It was fun. One of the main activities for the associate dean of students was to organize the freshman camp every year—orientation for the incoming freshmen. That used to be, when I first came into that job, held up in the San Bernardino Mountains, near—well, not too far from Barton Flats and that area—at a camp owned and operated by the City of Los Angeles, Camp Radford, because they had the physical facilities to handle that sort of a group of people. We're talking about, all together, roughly three hundred people at the time. They had roughly two hundred freshmen and a bunch of faculty members and some upper-class students.

COHEN: Right. That number probably hasn't changed.

WOOD: Not much, no. That's the order of magnitude. But several years later, various people thought it would be nice to change the place, for various and sundry reasons. So I got involved in figuring out how to do that. So I was involved in getting the freshman camp moved to the one

they've been using since—Catalina Island.

COHEN: What year are we talking about?

WOOD: Oh, gosh, this is early seventies—early seventies or something along in there.

COHEN: OK, I have your appointment as being 1968.

WOOD: Really? Was it that early? I've forgotten. All right, OK.

COHEN: How long did you do that?

WOOD: Oh, I got out from under that job in '75 or '76.

COHEN: So you actually did it for a good ten years.

WOOD: I think, yeah, something like that.

COHEN: So you got the camp moved to...

WOOD: Catalina, yeah. It's a facility owned by—still, I suppose, as far as I know—the Glendale YMCA. But they'll rent it out to various people. There are, it turns out, not very many facilities of that kind around Southern California that will handle that many people. You don't have many facilities able to feed that crowd, and so on. But people have enjoyed that place, of course, because of the swimming in the ocean and all that sort of business.

COHEN: Sure. And being on an island, one is detached.

WOOD: Yes. We tried one year not to go away, and to have a freshman orientation on the campus. And that was basically a disaster. For the reason you mentioned—people didn't stay around. We'd have some formal, or less formal, event, and people would come and listen and do what they were

supposed to do. As soon as it was over, everybody would take off and go home, wherever home was, whether it was into the student houses or home to their wives and so on. So it wasn't good in that respect. The students didn't like it either. [Laughter]

COHEN: So you went back to Catalina.

WOOD: So we went back. And then, another thing, we put on some talks at the camp for the benefit of the freshmen by some faculty members about things they were doing. And some little field trips. I remember getting Lee Silver to take the kids on a little field trip right there in Catalina. Because there's some interesting rocks there, and he could talk about all that. And I also got Wheeler North to take them scuba diving and show them all the stuff under the water that's interesting over there.

COHEN: That was an innovation you introduced?

WOOD: Yes. So we had fun that way.

COHEN: Well, I think they've continued that sort of thing.

WOOD: I think so. So at various times, various faculty members have been over there. Given the students something inherently interesting, and given them some little idea of how Caltech works and what happens.

COHEN: That I think has been a very successful part of starting them here. So then finally in '75, you said, "Enough!"

WOOD: I had had enough of it, yes. [Laughter] I had had enough of it. I was heavily involved also, when it got started, in the minority students program.

COHEN: Is that right? When did that actually get started? Because I think you asked me if I knew Lee Browne?

WOOD: Right. I worked closely with Lee Browne on that project. I guess that got started around 1970, plus or minus a little bit. But I've forgotten. We set up special programs to bring these kids here in the summer, before school started, and got them going a little bit on some of the academic work ahead of time, so they could essentially catch up.

COHEN: So these were minority students that were admitted to Caltech.

WOOD: Oh, yes, already admitted, but it was recognized that they—many of them, at least—their background was deficient by Caltech standards. And so we tried to bring them up to speed in mathematics, physics, and I've forgotten whether we did chemistry or not. And a little humanities, too, you know—English, and so on. But the mathematics and the physics were the big things.

COHEN: What is your feeling about it? Do you think it was successful?

WOOD: Oh, yes. Sure, I think so. We did very well, I think. It was recognized, of course, that the chances of survival of some of these students were not the best. Their school background and things like that were not what the typical Caltech student has. You know, a kid from South Central L.A. doesn't have an opportunity to learn the kind of things that you taught kids in Pasadena. And so the admissions committee had to make some kind of judgment on some of these kids—that in spite of those handicaps they had the basic ability to do things. Of course, that's a tough judgment to make, and you can make mistakes. Anyway, so it was recognized that we were going to have a certain amount of failures. But it turned out, at least in the first few years of that program—and I think it went on—that our success ratio, as measured by how many of them got the bachelor's degree here, was like forty-five percent. Which we thought was pretty good under those circumstances.

COHEN: How many students would you take?

WOOD: Oh, not very many. The first year, we had maybe ten. But it was a number like that. It wasn't large numbers.

COHEN: Well, considering the size of the class, that's not bad. And was that when Lee Browne

came in, to do that program? To be in charge of it? I told you I got Lee's job [at John Muir high school].

WOOD: Right, right. You took over from Lee. Yes, he was brought here to help in that program. I think Lyman Bonner was the guy who looked him up and found him and talked him into coming here.

COHEN: So how much of your time did the minority program take? What did you actually do?

WOOD: Oh, we set up the summer program, for one thing. And that involved talking other faculty members into helping with it—plus undergraduate students. We used a lot of undergraduates.

COHEN: To do the tutoring.

WOOD: To do the tutoring—students who were going to be here in the summer, working on somebody's research project or whatever. I remember one such student who's become a very famous physicist—Michael Turner. He's a guy who's done a lot of theory on the big bang and that sort of stuff. He was a very good student here. He was one of our minority summer program teachers.

And there was a lot of stuff during the year, of course, with those students. From the dean's office point of view, you spent a lot more time worrying about those students, because of their special problems. To give you one example that I remember very strongly, there was a young man named Haywood Robinson. I don't know if you ever heard of him. He came from South Central L.A, a young black man, very personable and handsome and so on. Actually, I think he still holds one of the Caltech records in some running event—I've forgotten what. And he came here and he failed the first quarter of freshman physics.

COHEN: Not a rare event.

WOOD: It's not a rare event. That's right. And at that time, the physicists had decided that if a freshman failed the first quarter, he could not continue in the course, because they thought that if you

didn't learn much in the first quarter, you're going to be dead in the second quarter. So they wouldn't let Haywood register for the second quarter. But Haywood said, "All right, that's the way you guys want to play the game. That's your business." And he went to class anyway, even though he wasn't registered. No credit for it, and so on. He went to all the lectures. He did all the homework. He took the exams. And he did well. You know, he was a persistent young man. And so, when I found out about that, I said, "The heck with all this nonsense! We're going to get him registered and get him credit for this. And he's going to do the third term." And he did. Of course, later on, he came back and did the first term. But he was persistent. Those are the kind of problems you deal with in working with those students.

COHEN: Yes, but of course those are the kind of students who are going to be successful.

WOOD: Right. He later went to medical school. He's a doctor now. And he served his internship down at Martin Luther King Hospital, in South Central L.A. I think he's now moved somewhere on the East Coast. I've forgotten. I've lost track of him now, but he was quite a guy. I don't say that all these students were like him. He was a real example of the kind of person we were trying to find.

COHEN: He did well. What year was that? Was that the middle seventies or so?

WOOD: I think he was in our first bunch. So that was '70 or '71 or somewhere, somewhere along in there.

COHEN: Now are they—that program is not going on anymore?

WOOD: It's much decreased, as far as I understand. Now that Lee is retired and is no longer here.

COHEN: Yes, they've gone off in other directions.

WOOD: I think so.

COHEN: So I don't hear too much about it. I know I was more involved when I was teaching high

school. I was much more aware of what was going on. OK, so you did that, and that was certainly administrative work in some sense. And then I was going to ask you about the social life here, because I remember that you sang with a group that did all those wonderful programs.

WOOD: Which?

COHEN: The group that sang all the songs for parties.

WOOD: Oh, Kent Clark's.

COHEN: That's right. When did that get organized?

WOOD: Oh, Kent Clark wrote these little shows from time to time, starting way back. I think the first one—in which Connie and I were not involved—was one of those musical shows when Linus Pauling got his Nobel Prize for chemistry. I wasn't involved in that particular one, but I think Kent started there. Later on, the American Association for the Advancement of Science had an annual meeting here in Pasadena, and Kent wrote a show for that called *The AAAS Show*. The lead song was called "Let's Advance on Science Before It Advances on Us." [Laughter] Another song that Kent wrote for that particular show is called "It's Not Gneiss." G-N-E-I-S-S. And he goes on about all kinds of rocks.

COHEN: I think I remember that one. So then you participated?

WOOD: And then he wrote a number of other shows over the years. There was the Watson show. About the famous event when Earnest Watson, who was then the dean of the faculty, corresponding to the provost, or whatever you want to call it, went off on a sabbatical. And on a cruise ship in the Mediterranean he met this woman and married her, this old bachelor. What's her name? She wrote children's books, Golden books. Jane Warner. Jane Warner's her name. So that was a very special event at Caltech. So Kent wrote a show in commemoration of that event.

COHEN: And you and Connie were in that one?

WOOD: We were in that one, yeah. Another early one was when George Beadle got the Nobel Prize in biology. And there's one song that involved, that was just after Watson and Crick discovered the structure of the DNA molecule. And then there was another—he wrote one song called “Blue Genes,” which was great.

COHEN: Did he present the group with the show intact, or did the group itself help?

WOOD: Oh, no, the show would be intact. From Kent and his lawyer friend, Elliot Davis, who helped him with the music. Elliot Davis is a jazz piano player. So he and Kent together did the music. Kent did the words, and he and Elliot together worked out the music. That was all done by the time they got some faculty group together.

COHEN: Was it the same group all the time, or was it sort of a floating group?

WOOD: It was somewhat floating, but Kent tended to ask the same people. You know, he had experience with them. It was often the same people. For example, Ward Whaling was practically always in the group. And Ed Hutchings. I don't know if you remember him.

COHEN: Oh, sure.

WOOD: Ray Owen. Bill Corcoran, when he was alive. And various others. But from time to time, he'd ask new people in.

COHEN: But it was his show?

WOOD: Oh, it's his show, absolutely! No question about that. [Laughter]

COHEN: I have seen some of them. They're terrific.

WOOD: They were a lot of fun. And there were two shows that he wrote for DuBridge. One after

DuBridge had been president for ten years and another one after twenty years. “Give Me a View from DuBridge,” [laughter] one of the songs in that one. And the show in honor of Arnold Beckman called *Beautiful Beckman*, in the Beckman Auditorium, of course. Probably others that I forget, but there were quite a few of them. That was a lot of fun.

COHEN: So you saw many changes. In these past years, what would you say the major changes in engineering have been? Of course, we have Lindvall leaving. And then who was the head?

WOOD: Let’s see, who took over from Fred? Was that Francis Clauser, I guess? [Ed. note: Francis Clauser succeeded Lindvall as chairman in 1969.]

COHEN: Did that affect you very much, as far as your own program and work went?

WOOD: Not really. No. No. The Engineering Division is very good. They don’t bother you too much. They let you do your thing. [Laughter]

COHEN: And then, I guess, we had Roy Gould coming.

WOOD: There was another fellow, Bob Cannon, for a while.

COHEN: Oh, I see.

WOOD: A non-Caltech person. He’d had some position in the government in Washington. So he was chairman for a while. And then Roy Gould came after Cannon. And then probably Paul Jennings.

COHEN: But meanwhile, the mechanical engineering, your materials science, all that just proceeded. It didn’t really affect you very much.

WOOD: Not tremendously, no.

COHEN: And have you done anything that interests you a great deal, say, on the national scene with your discipline?

WOOD: No, not really on the national scene. After I retired from the dean's office—well, actually before—I did a certain teaching activity that I enjoyed very much and got a kick out of. That was to help the physicists teach freshman and sophomore physics. I did that quite a few times. So that was fun. I think during my time on the faculty at Caltech, the interesting thing to me there was the great movement forward in the pedagogy. In teaching those core courses, so-called. In terms of physics, I think it was largely due to Feynman. It used to be thought that a student, before he could have a hope of even starting to learn anything about quantum mechanics, had to go all through a great long rigmarole of classical mechanics at very sophisticated levels and so on. And Feynman showed that that's not true. You don't have to do that. And mathematicians have done similar things in mathematics. So I think in my time at Caltech great strides have been made in the pedagogy. In other words, how you teach students and what you teach them. The material they're given and what they learn here now is far, far advanced, compared to when I was an undergraduate. You know, it's like the difference between the Model T Ford and the latest car, or the Wright brothers' airplane and a 747.

COHEN: There's just more information around. OK, let's see. Have you ever done any consulting? I mean away from Caltech?

WOOD: Oh, yes, sure, quite a bit. Various companies and so on—around the area and far away. I consulted for several years in the seventies and eighties with Sandia Corporation, in Albuquerque, New Mexico. And I've consulted with lots of companies around this area from time to time on various kinds of problems.

COHEN: And this would be directly related to the work you were doing here?

WOOD: Yes, well, I've done a lot of work. The work I did at Sandia was in connection with the phenomenon of wave propagation in solids, which goes back to Second World War-type research we were doing. I wrote a paper after I became a faculty member which got me into that—another

application of those basic ideas. And some of the people at Sandia Corporation were interested in that, doing some very nice experimental work on that. So they got me to come down and consult with them about that program of theirs. Other work I've done is in so-called stress analysis of things. I did a lot of work one time for the Byron Jackson Pump Company, down here in Los Angeles. They're a company that would take a contract to develop a new kind of pump for some new application—a nonstandard kind of thing. One time, for example, in the early days of nuclear power reactors, they had a contract to build pumps to circulate liquid sodium, hot, through a sodium-cooled nuclear reactor. So these were very special problems that hadn't been encountered before, including a lot of nasty strength problems. And so I helped them with that sort of thing.

COHEN: Now, I know sabbatic leaves are not a big deal at Caltech, but have you taken any?

WOOD: Yes, I had one, one time at MIT. Not for a full year. About six months, I think. At that time, there was an arrangement—sort of an exchange arrangement—between MIT and Caltech.

COHEN: What year would that have been?

WOOD: It was the late fifties—'58, '59—somewhere along in there. Anyway, I worked with a fellow about my age. He had been a graduate student here when I was, named Frank McClintock. And he was interested in various problems of fractures. Still is. And I was interested in that also at the time. So I had a good time. It was very interesting there. And I taught a course, a special course of my own.

COHEN: Now, when you say “exchange,” was that sort of a formal thing? People from MIT would come to Caltech, and people here would go to MIT?

WOOD: Right. And you just got the same salary. They did put in a little more money for moving expenses and that sort of thing.

COHEN: Did that go on for very long?

WOOD: Oh, I don't remember how long that program went on. I knew the particular person who was exchanged for me, so to speak, although we didn't exchange at the same time. A fellow named Egon Orowan. He was one of the original inventors of the idea of dislocations in crystals. But he had very wide interests in materials and similar things. He actually came here to work in geophysics—how rocks creak under high temperature, and all that sort of business. So he had already been here, and then I went to MIT later, when he was back there. And I did some work with him there, experimental work.

COHEN: So that was a change for you, living on the East Coast.

WOOD: Oh, sure, right. We enjoyed that, had a good time there.

COHEN: Did you take any other leaves?

WOOD: No, I think that's the only leave I ever had.

COHEN: Well, now just in looking back at all this—your experiences here in engineering—are there any sort of observations you'd like to make?

WOOD: Oh, well, of course, as everybody knows, Caltech is a great place in all kinds of ways. I've been very lucky, I think, to have been here—and to be here still, as far as that's concerned. They're nice to us when we retire, among other things. [Laughter] No, it's a great place. As I think I said before, I'm impressed by this improvement in pedagogy. A lot of people claim that Caltech doesn't worry much about teaching—that everybody's doing their research and the heck with the students. But that's not true. It's just absolutely not the case. I know from my experiences in helping physicists teach physics for the beginning students, and my own teaching in material sciences, and other people's teaching in the Engineering Division. Pedagogy has increased tremendously, in my opinion.

COHEN: Concern with what the students are learning?

WOOD: Yes. And teaching them more and better stuff, basically.

COHEN: So this business of saying, well, teaching has nothing to do with somebody being promoted or getting tenure at all...

WOOD: Oh, it's true that you have to do research to get promoted. There's no question about that. But it's very desirable to do teaching. My point is, however, regardless of that, that Caltech has really improved pedagogy in the teaching of science and engineering over the years, while I've been here.

COHEN: And it's been a good place to be.

WOOD: Absolutely. No question about it.