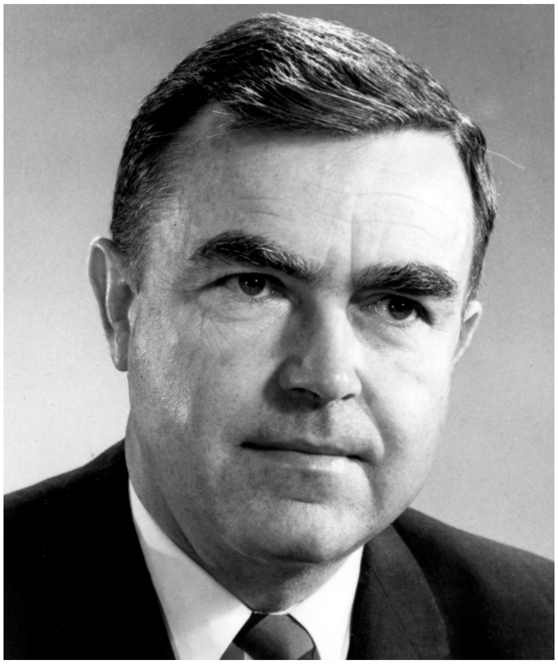

Verner Schomaker, c. 1984

VERNER F. H. SCHOMAKER
(1914 – 1997)

INTERVIEWED BY
SHIRLEY K. COHEN

February 1993

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Chemistry

Abstract
An interview in four sessions in February 1993 with the physical chemist Verner F. H. Schomaker, professor emeritus at the University of Washington in Seattle. Dr. Schomaker received his BS (1934) and MS (1935) from the University of Nebraska and his PhD (1938) from Caltech. He remained at Caltech, in the Division of Chemistry and Chemical Engineering, as a George Ellery Hale Fellow (1938-40), senior research fellow (1940-45), assistant professor (1945-46), associate professor (1946-50), and professor (1950-58), before leaving to join Union Carbide's research division. In 1965, he moved to the University of Washington, where he chaired the Department of Chemistry for five years. He died in Pasadena, on March 30, 1997.

In this interview, he describes the Caltech milieu in the 1930s; his graduate work with Donald Yost; and the operation of the chemistry division under Linus

Pauling (1937-1957). Discusses his own work in electron diffraction and collaboration with such colleagues as Jürg Waser, William Lipscomb, David Shoemaker, Roy Glauber, Kenneth Trueblood, and Richard Marsh; his work for Union Carbide; and his eventual move to the University of Washington. Comments on Pauling's career at Caltech, his deep insight, his wide-ranging interests, his political activism, and his eventual departure from Caltech.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH VERNER F. H. SCHOMAKER

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

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Pasadena, California

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SCHOMAKER: I was born on a farm in Eastern Nebraska, the same farm on which my grandmother, my father's mother, had been born in 1860. My grandmother was an orphan. I don't exactly know the circumstances, but she was brought up by her aunt and uncle. Her mother had died, I think, and her father had moved to Chicago or maybe Kansas City. Anyway, my earliest memory of the farm is the great flu epidemic [1918-1919]. As far as I knew, everyone was sick in the house except my grandmother and me. My mother, my father, my two aunts, two of my brothers and my sisters—they all were ill. And what I remember most about that was my grandmother running back and forth through the house taking care of people.

COHEN: Do you recall your early educational experiences?

SCHOMAKER: I went to school in Nehawka, Nebraska, which is a little town about three miles from the farm. A school bus picked us up and took us to the consolidated school. Recalling those early days made me extremely anti-busing for the poor kids in Seattle and elsewhere who had to spend a couple of hours a day riding great distances on the bus. In Nebraska, when the weather was good, our two-and-three-quarter miles bus ride took only a few minutes.

COHEN: Well, a few minutes are quicker than two hours.

SCHOMAKER: True, but when the rural Nebraska roads were deep with mud, clay mud, it sometimes took an hour or more to get to school. Walking would have been quicker in bad weather.

COHEN: Did you attend a one-room schoolhouse?

SCHOMAKER: Oh, no. It was a brick building, and, as I recall, the school had eight schoolrooms in a two-storey square. I went to kindergarten for a year, starting when I was five years old. A couple of times, they pushed me ahead a year. School was easy and fun, especially compared to life on the farm. Even when they pushed me ahead, the schoolwork wasn't really harder. It was great to get ahead.

COHEN: The way that you kept being pushed ahead—it didn't bother you that you were with older children, the social part?

SCHOMAKER: I think it did bother me. I think I was a misfit that way. I was a nerd, I guess; I enjoyed studying. And yet, while there were elements of school that were all perfectly good, I remember that there was something primitive. For example, when we were in our senior year, we took some kind of admission exams for the University of Nebraska. There was one in algebra, and one of the questions was to plot a graph, two plus four X. But a graph was something that I'd never even heard of.

COHEN: So you didn't have a lot of mathematics?

SCHOMAKER: Well, I had *some* mathematics, but the algebra was primitive, and the geometry class was ruined.

COHEN: Ruined?

SCHOMAKER: By the older boys. The teacher, as I remember, was a pretty girl, and the older boys in the class were feeling their young oats. So she had to spend all her time rapping their knuckles. I can remember no instruction in geometry. I think my favorite teacher was a fellow named Sam Lingo. He was the football coach—no, they didn't have football—but Sam was the basketball coach, the vocational agriculture instructor, and the teacher of chemistry and physics.

COHEN: It's interesting that you remember his name.

SCHOMAKER: Oh, I remember Sam's name. If I saw him, I might possibly recognize him. At any rate, I enjoyed the chemistry and I enjoyed the physics especially—the pulleys and inclined planes, all the things that worked. And we did experiments; they were all simple and straightforward, not really very complicated, but it was great when they worked. It wasn't any very fancy stuff, but doing those experiments got me interested in science.

COHEN: You had an early interest in science?

SCHOMAKER: Yes, but I'd decided that I was going to be a farmer, going to farm my father's place. There was a lot of friction between my father and myself, so I took the agriculture class.

COHEN: This was during your high school period?

SCHOMAKER: Yes. We had some interesting projects. For example, we had a project that was supposed to make the pigs a little more free of disease. This was really the breaking point about my wanting to be on the farm, because my father refused to have anything whatsoever to do with that project. It was too much work to go out to the back eighty, or the back forty, to do the simple acts of taking care of the pigs, and so the pigs were always sickly, sickly, sickly.

COHEN: So even at that point, you were rather more interested in technical things than farming?

SCHOMAKER: Well, I don't know *what* I was interested in. I was still interested in math. There were eighteen of us in our math class; it was the biggest class that Nehawka Consolidated School had up to that time. But there was only one other fellow in that class who liked studying and so on. His name was Vilas Sheldon.

COHEN: Did Vilas and you want to be mathematicians?

SCHOMAKER: I don't know about Vilas, but by that time I knew that I didn't want to go to ag college. I wanted to leave the farm to become an engineer or something. I'd also heard about chemical engineers and geologists. I didn't have any serious interest in these things, but they did drive away the idea of being a farmer.

COHEN: So you took the entrance exam for the university?

SCHOMAKER: I can't really remember what entrance exam the university had; I think everybody could get in. So there I was at the University of Nebraska, taking endless engineering orientation courses. Chemical engineering was a joke; there was no chemical engineering, except for the one course that was taught by a fellow who'd been a colonel in World War I, assigned to Edgewood Arsenal. All that guy ever talked about was mixing salt with water, which is why the chemical engineering course was such a joke. But it was good to have a little physics and some electrical engineering. And the chemistry courses were good.

COHEN: You enjoyed chemistry.

SCHOMAKER: Very much, so my instructor at the University of Nebraska talked to me about where I'd like to go for graduate school. But I didn't think I could do that, so I stayed a fifth year at Nebraska with the thought that it would prepare me to go somewhere else.

COHEN: So very early on, people around you realized that you were a budding chemist?

SCHOMAKER: Well, I don't know *what* they knew. I certainly didn't have intense interests in anything. I didn't know enough to take better advantage of the library. All I seemed to do was to try to do my best in my classes.

COHEN: But you didn't have any career plans?

SCHOMAKER: Well, I thought maybe I'd like to be an organic chemist. I also thought I might like to be a physical chemist. I didn't know anything at all about modern physics and chemistry. Back in those days, at least at Nebraska, it was the [Arthur] Eddington dictum of electrons or particles on Mondays, Wednesdays, and Fridays, and it was waves on Tuesdays and Thursdays and Saturdays. In five years there, that's about all I ever learned. But there were a lot of other things that were perfectly good, so I applied to all the Midwestern schools, and to Princeton, Caltech, and Berkeley.

COHEN: Was this suggested to you by your professors—to apply to a place such as Caltech or Princeton?

SCHOMAKER: Yes. But I decided to take advantage of the ominous opportunity to go to the University of Illinois and work for a man named George L. Clark, who was famous for being the most incompetent crystallographer in the country.

COHEN: Did you even know the name [Linus] "Pauling" in those days?

SCHOMAKER: I was told about him—told that he was a wonderful fellow. I didn't know anything about him personally, but I was aware of him through the *Journal of the American Chemical Society* and other journals in the library right there in the chemistry building. So, yes, I knew of Pauling. But I'd heard a lot more about the University of California at Berkeley. There'd been a fellow several years ahead of me at the University of Nebraska [who] went to Berkeley as a graduate student after his fifth year at Nebraska. We corresponded for several years, and he kept writing about the glories of San Francisco.

COHEN: What was his name?

SCHOMAKER: Robert [D.] Vold, Bob Vold. He worked with [Joel] Hildebrand at Berkeley, and his first job was with [James W.] McBain at Stanford. I didn't know Bob's wife but she was the University Medalist there. Her name was Marjorie Young. Bob was a smart fellow, but she was a brilliant student. After they worked with McBain at Stanford a couple of years, they worked for Procter & Gamble for a good number of years. Then they returned to Stanford, where they finished up, and finally they came down here, to the University of Southern California.

COHEN: But they sort of mentored you in the early days?

SCHOMAKER: Well, they had me interested in G. N. [Gilbert Newton] Lewis. I had a window into physical chemistry that way, and to the West Coast. So I applied to Berkeley, and I applied to Caltech. But I also applied to Illinois and to Minnesota and all these other places.

COHEN: I gather your applications were successful.

SCHOMAKER: Some were. I got a letter from Roscoe Dickinson [then an associate professor of physical chemistry] here at Caltech, offering me this wonderful fellowship with a value of $1,500. Well, I didn't realize what it meant. I had board and room at the Athenaeum, which was used as a dormitory. The Athenaeum was a wonderful place in those days. You go up there now and you find that it's all dirty stuff with broken-down furniture. It's terrible. In fact, these days it's probably not good enough for graduate students. They're a lot more prosperous than we were then.

Anyway, Dickinson offered me that wonderful fellowship, and I accepted it the next day. Now I had reasons to be interested in Caltech other than just the mention of Pauling. The next day I had a letter from G. N. Lewis offering me a position at Berkeley. Had the letter not come from Caltech, I would certainly have accepted that job.

COHEN: You could have gone somewhere else.

SCHOMAKER: Yes. Some of the other places turned me down; others accepted me too late. So that's how I got to Caltech.

COHEN: Were you familiar with the area?

SCHOMAKER: A little bit. I had a cousin who was a year behind me in school, and he and I took a trip around the country in a terrible old automobile that his parents bought him—a 1930 Model A Ford. This was 1934, but I remember his car as being terribly old. But we drove around the whole West and back to New Orleans and Chicago. That was a wonderful trip.

COHEN: Did you visit Caltech on that trip?

SCHOMAKER: Well, we were in Los Angeles, and one day somebody took us out here to Pasadena. But did I come and visit the campus? No. We rode around California Street and back. It was a hot summer day, about the 10th of July, but did I see Caltech? No. Was I that sensible? No.

COHEN: But you got here. Why did you apply to Caltech?

SCHOMAKER: One reason was that the dean of the graduate school at the University of Nebraska—an organic chemist named F. W. Upson—was a friend of [Richard Chace] Tolman's [then dean of Caltech's graduate school, and a professor of physical chemistry and mathematical physics]. I never learned about it from Tolman, but the word was that Upson and Tolman were friends from their days at Illinois. Upson was ill during my time there, but he was a pipeline to Caltech. And there was Lawrence Brockway, who died a few years ago. Lawrence also had been a student at the University of Nebraska, and he was at Caltech [as a senior research fellow, 1933-1937] for a couple of my graduate years. Then he went on a fellowship to England. He was supposed to come back here, but he got a job at the University of Michigan. So Brockway was from Nebraska, and he was successful. In fact, he might have even known Robert Millikan [head of Caltech 1921-1945].

COHEN: How did that come about?

SCHOMAKER: Millikan was the commencement speaker at the University of Nebraska in 1934, if I remember it right. So the great man talked there, and I was told the next year by one of my professors, I think, that someone from Nebraska actually had spoken to Millikan here on campus. It was probably Brockway. He had built the electron-diffraction machine and was doing very fine work indeed. So I suppose it was possible, back in those days, for Millikan and Brockway to have known each other. The chance of Everhart [Thomas E. Everhart, Caltech president 1987-1997] getting to know some young postdoc is less probable these days. Caltech was much smaller in Millikan's day.

Another connection to Caltech was Robert [E.] Rundle [Caltech PhD 1941], who was my successor at the [University of Nebraska] laboratory. He and I worked together and wrote several papers. From my point of view, our point of view here, Rundle cracked the problem of the nature of starch.

COHEN: The nature of starch?

SCHOMAKER: Yes, the structural nature of it. Rundle had found that the blue color that occurs when iodine is mixed with starch comes from iodine molecules. They go into the starch polymer molecules, and a number of things develop.

COHEN: You say there was a string of people who came to Caltech from Nebraska?

SCHOMAKER: Brockway was the first one. He must have come in 1930. Then I came [1935]. Then Rundle arrived [1938]. Walt [Walter A.] Schroeder came after that [1940]; and then there was Earl [W.] Malmberg, who worked here during the war. He went back to Nebraska, or maybe Ohio State, to get his PhD; I lost track of him eventually. In any case, after Earl Malmberg, I don't know whether there's ever been a graduate student in chemistry who came to Caltech from the University of Nebraska.

COHEN: But you were here, rooming in the Athenaeum.

SCHOMAKER: When I was first here, I stayed in the older dormitory; it was the oldest building on campus. It was located where Chandler [Harry Chandler Dining Hall] is now, and there it sat until it was pulled down. My roommate was Ed [Edwin W.] Paxson, a mathematician who worked for [Aristotle D.] Michal. Ed was a very bright man. I don't know whether he finished in one year or not, maybe two years. [Dr. Paxson received his PhD in 1937.—ed.] After leaving Caltech, he worked for the RAND Corporation.

COHEN: Didn't you meet Willy [William A.] Fowler [Institute Professor of Physics, d. 1995] fairly early?

SCHOMAKER: Yes. There's a story that I always tell Willy; he refuses to believe it, but it's true. About the time I became acquainted with him, I became acquainted with J. Y. Beach, John Young Beach, who was Pauling's student [PhD 1936]. Beach did electron diffraction for

Pauling, some quantum mechanical calculations, too. Now, Beach was a tall fellow. He stood very straight, and liked to put on airs. I can't describe him very well, but here's the story: One day Beach pointed out Willy—and of course I knew that was Willy—and he said, "That's Willy Fowler. He's just a graduate student, but he tells Charlie Lauritsen what to do!" Willy denies that he ever told Charlie [C. C. Lauritsen, director of Kellogg Radiation Laboratory, 1931-1962] what to do, but I think Beach was a pretty smart guy and probably knew what was going on. My impression is that Charlie Lauritsen was a wonderful guy, but I think he was a very practical guy; his fingers told him what to do, too. Of course, it may be that Willy was already planning what should be done in [Kellogg] at that time. I don't know. He denies it, but—

COHEN: So that was your first impression of Willy Fowler. How about Pauling?

SCHOMAKER: When I came out here to go to graduate school in '35, I spent a month with my cousin in San Francisco, and there I was, in that glorious place I ended up *not* going to. It so happens that there was an American Chemical Society meeting in San Francisco, so I went to some of the sessions and saw some of the famous people. That's when I discovered that Pauling had a wonderful red beard. I didn't meet him that day, but he was up there. In fact, Pauling was sitting with Ava Helen [Mrs. Pauling]—cuddling right there during the talks! Well, as a young fellow from Nebraska, I didn't consider that proper behavior. I came down here after that, and one day I saw a fellow in Gates [Laboratory of Chemistry], bustling around, and I thought it was Pauling because he had the red beard. But it turned out that it was Charles D. Coryell [PhD 1935], who was a wonderful, wonderful man. At any rate, I finally did meet Pauling. After I'd known him a few days, I told him that I'd been in San Francisco. He upbraided me for not coming around and introducing myself.

COHEN: So Pauling had a red beard in those days?

SCHOMAKER: Well, he did every now and then. Pauling would grow it and everybody would get used to it. Then he would make a seminar tour or go to a meeting in the East. We would take him to the train, or be there to see him off, and sometimes we'd pick him up on his return. And when he'd come back, the beard would be gone.

COHEN: So you were working with Pauling by this time, and doing serious research.

SCHOMAKER: No, serious research was one of the things that I really was not prepared for. But I was working. In those days—here and in Berkeley, too—the thing to do was to get in the laboratory and get busy.

COHEN: As soon as you arrived on campus?

SCHOMAKER: Right away, yes. There was none of this nonsense of fooling around for a year, or for a quarter, to decide what you wanted to do.

COHEN: What did you do?

SCHOMAKER: Well, I wanted to do electron diffraction. I'd heard about it from Brockway, and it was the one thing in the world that I knew about. So I met with Pauling to discuss electron diffraction, and he said, "Well, that's too complicated. You should do something easier first." Or something simpler at first. So it was arranged for me to work with Yost.

COHEN: Yost?

SCHOMAKER: Don [M.] Yost [then an associate professor of inorganic chemistry]. Don did a lot of work with Roscoe Dickinson. In fact, the Raman spectrograph that Yost and his students used had been built by Dickinson. I don't know whether he'd ground the lenses, but he put it together and the Yost group did some work on it. Yost was already very active by the time I came to Caltech, and he was a guy who always rose to a challenge. So one time Don decided to do low-temperature calorimetric work. Why did he want to do that? Well, he told me—perhaps not quite seriously—that he just wanted to prove that he *could* do it. And Don Yost didn't merely want to prove he could do it, he wanted to get results as precise as those of [William F.] Giauque at Berkeley in the process!

So Don and his students got a laboratory running, down in Gates. Then some graduate students and postdocs obtained some results. Things seemed promising, but then the war came, and Yost had to go off and spend a period of time in Evanston [Northwestern University]. When

Don came back, he was a completely changed man. He was desperately ill, suffering from osteomyelitis of the jaw. I heard that Joe [Joseph B.] Koepfli [senior research associate in chemistry, emeritus; d. 2004] was able to save Don's life by getting some of the first penicillin in this country. After taking the medicine, Don returned to being somewhat the same man he'd been before. In fact, he even undertook an entirely new project: He bought up surplus equipment, a lot of radar stuff, and got into microwave spectroscopy. Yost also took a series of graduate students under his wing, bright fellows who were eager to work on their own. One of Yost's students was John [S.] Waugh. John was back at Caltech recently, as a Fairchild scholar, and he's one of our most distinguished alums. Considering all the prizes he's won, John Waugh is probably the most accomplished and most famous of Yost's students.

James [N.] Shoolery was another very successful Yost student. James went to work for Varian Associates. He was the guy who used to come here to service NMR [Nuclear Magnetic Resonance] for John D. [Jack] Roberts [Institute Professor of Chemistry, emeritus]. Of course, John D. Roberts did most of the servicing himself.

COHEN: And you were one of Yost's graduate students?

SCHOMAKER: Yes. Pauling sent me to see him, and we started making compounds and blowing glass.

COHEN: Blowing glass?

SCHOMAKER: Don Yost was a good glassblower—not a fancy one, but a good one, and he tried to teach me the art of glassblowing. Of course, I'd tried to learn how to blow glass when I was in my fifth year at the University of Nebraska, but I wasn't much good at it. I improved under Don's tutelage, and glassblowing was easier with Pyrex than it was with soft glass, which was hopeless. After you worked with it, it would always crack; with Pyrex, you could do almost anything. So I worked hard at that and managed to do quite a few things.

COHEN: So you were a successful glass blower?

SCHOMAKER: Reasonably. But as time went on, I got very discouraged, because I didn't understand anything about the mechanics of vibrating molecules. I read about the problem and talked about it, but it seemed infinitely complicated. It was far beyond anything I'd ever actually learned. Then, too, Don was the right guy for some people but he was not the right guy for me. I wanted someone to hold my hand and teach me things about vibrating molecules and so on, but all I ever got out of Don by way of interpreting these things was, "Oh, that looks like a Fermi doublet." Now, a Fermi doublet was one of Fermi's little triumphs, but Don didn't tell me what a Fermi doublet *was*!

But I thought the whole business was marvelous. And Yost was a great guy, and he'd tackle anything. "Verner," he told me one time, "don't ever be a pencil-and-paper chemist." Of course, his advice didn't do me any good at all.

COHEN: What was Yost tackling at the time?

SCHOMAKER: I suppose this was in 1935 or '36, and Don and Roscoe Dickinson were infinitely busy down in the basement of Gates, doing experiments on neutron diffusion. Charlie Lauritsen provided them with a neutron source, which was sitting in a great big paraffin sphere. They put foils in various places to measure the activity that was generated. They radiated this foil for a certain time and then dashed down the hallway to make measurements on the Lauritsen electroscope to determine what the activity was. They eventually published a paper on the diffusion of neutrons.[1]

COHEN: Was there interaction among chemists and physicists?

SCHOMAKER: In those days, there was an ongoing Friday afternoon or Friday night seminar. Everyone carried around these thick notebooks with articles written by Hans Bethe and several review articles. We called them Bethe Bibles. I don't know whether Dickinson was part of that group, but Yost was. So was a physicist named Tommy Lauritsen [son of C. C. Lauritsen], I think he was in high school. I'm not absolutely sure, but I think Charlie Lauritsen was there. A lot of good people were there, and we had some fine parties.

[1] Don M. Yost & Roscoe G. Dickinson, "The Diffusion and Absorption of Neutrons in Paraffin Spheres," *Phys. Rev.* 50:128-32 (1936).

Begin Tape 1, Side 2

COHEN: Besides the seminar and the parties, what was your calling in those days?

SCHOMAKER: I'm not sure I *had* a calling. My only practical ambition was to work on electron diffraction, so in my first year at Caltech I prepared one or two samples of things for the guys over there. Then—this was after about a year—I went to the astrophysics building [Henry M. Robinson Laboratory of Astrophysics]. I'm not sure why I left; maybe Yost wanted to get rid of me.

COHEN: You found the astrophysics group more compatible?

SCHOMAKER: There was nothing incompatible with Yost and company; it was all wonderful. But the astrophysics work was something that I could do, whereas I felt buffaloed with Don. Plus, I didn't have the gumption to sit down and learn what I needed to learn to work with Yost. So I think it was a good move. In the astrophysics building, I understood some of what was going on. In electron diffraction, there are high vacuums, and the main electron scattering is in the form of rings. The rings are very subtle; they look much better than they are. Their measurement by physical means was too hard to be practical in those days. But you could see the rings, and Pauling and others could decipher information about the structure. We had some pictures. I don't know whether they were mine or those of others, but we made measurements. One time I made a measurement and Pauling made a measurement. Then after we'd looked at the sample for five minutes, he said, "Well, I guess you've got the hang of it." That was all he said.

COHEN: So that was a lesson in trusting one's instincts?

SCHOMAKER: In trusting Pauling's instincts. My success in that field was mostly a matter of learning, of pursuing. Using your eyes alone was for the most part an obsolescent technique, but it was a valuable experience for me. I spent a lot of time doing it and learned something from it The important task wasn't merely observing, it was making a correlation between what you can perceive and what kinds of theoretical intensities you can calculate from those observations. There were countless subtleties to learn, but I kept trying. It took several years before Pauling and I got to the point where we decided that this was what I was going to do. I wanted to learn how to interpret these things. And the basis for interpretation, really, is to find some other method to construct a structure—the accepted structure, the accepted pattern—and to look at those pictures and to count relationships intelligently. It takes time and experience to perceive one relationship here and another relationship there and to use the technique to comprehend the big picture.

COHEN: But you were able to see these relationships?

SCHOMAKER: Pauling thought so. Most of the time.

COHEN: How did you see them?

SCHOMAKER: I'm not sure; but Pauling said, "Verner, you can do that because your eyes are better." My eyes *were* better—at least, better than they are now—but I don't think the success had that much to do with my eyesight. I think my success was due to the fact that I had a vision of what was possible—a concept of what maybe could be done. I didn't have this conception right away, and it came from Pauling in the first place. But I felt I was on the right track, and I was pretty certain of it when he said, "Verner, I guess you've got the hang of it."

COHEN: So from that point forward, things ran smoothly?

SCHOMAKER: In many ways, yes, but I had too great a richness of opportunity! People came by the dozens, wanting to work with Pauling. And what did he do? Well, naturally, he sent them down to work in the electron-diffraction lab. It made a lot of sense—you had these darn pictures—but typically nothing would ever get done. There were lots of things we did that never got finished. I don't blame Pauling; it was bad management on my part.

COHEN: But aren't you saying that Pauling gave his permission for a hoard of people to wander in and out of your laboratory?

SCHOMAKER: They would come to work for six months or a year or three months, and of course they wanted to do something. But they didn't necessarily come because they wanted to work on electron diffraction, and I think it's fair to say that most of them weren't intensely interested in the structure of molecules. They came to Caltech, and came to my lab, because they wanted to work here, especially with Linus Pauling. But he couldn't possibly work with everyone who came here.

COHEN: But Pauling accepted them anyway?

SCHOMAKER: Don't misunderstand. I don't want to cast blame; I'm trying to describe my own insufficiency about it, I think. I had too many opportunities, all these people to work with, but I lacked enough down-to-earth responsibility to get jobs done. So we had lots and lots and lots of stuff that never got published. In all fairness, I should say that we did publish some work, and some of it was actually important work.

COHEN: But in some sense you were being a service person to all these people coming through.

SCHOMAKER: I didn't think of it that way, but that's what it amounted to, and it wasn't very good service, because the important part was puzzling out what to do, figuring out the nature of the problem and its solution. Sometimes that's a bit difficult. It takes a certain devotion to the problem.

COHEN: But it took a lot of this Pauling-and-Schomaker kind of insight. This vision and instinct and feeling.

SCHOMAKER: It took a lot of work. It took a lot of serious introspection. And it took a lot of second-guessing. The second-guessing was the time-consuming part of our work, and the mixed blessing. You could always go back and look at those damn photographs. You could do it again and again. And you could ask yourself, "Was I really right about that, or was it this way?" I referred earlier to having too many riches, and this is another example. You look at the photographs and come up with an answer, but your initial answer doesn't have to be *the* answer. The photographs aren't going anywhere; you can always go back and interpret them again. So *the* answer, whatever it is, can remain elusive. Your initial answer makes little difference, and there's no question about the dates being final. Like the photographs, the dates remain there, still on the record. They'd no doubt remain there until, in a hundred years or so, traces of the original interpretations, maybe, would fade. So, wanting to be careful and correct, you could always change your mind.

COHEN: And reinterpret what you saw?

SCHOMAKER: If it seemed appropriate.

COHEN: But you're saying that, based on the original feeling that you had—that Pauling and you had—your results were almost always correct?

SCHOMAKER: Yes, almost always correct. But I'm talking about our desire to make sure that our results were correct beyond a shadow of a scientific doubt. I suppose I'm alluding to the fact that not everyone's results were uniformly correct.

COHEN: For example?

SCHOMAKER: Must I?

COHEN: For the record.

SCHOMAKER: Well, there was a wonderful guy who came to Caltech in my first year here—a wonderful guy from the University of Chicago named Simon [H.] Bauer.

COHEN: Simon Bauer?

SCHOMAKER: You know Simon?

COHEN: Well, we were at Cornell for quite a few years.

SCHOMAKER: So I should bite my tongue.

COHEN: Don't.

SCHOMAKER: I don't think we had a problem, because Simon was a nifty fellow and very industrious. When he came out here from the University of Chicago, he did a monstrous amount of work for Dick [Richard M.] Badger [then an assistant professor of chemistry], and an even more monstrous amount of work for Linus Pauling. Simon got [H. I.] Schlesinger and A. [Anton] Burg at the University of Chicago to let him come down to USC, the University of Southern California. They sent along some boron hydrates so that Bauer could work on diffraction. This was an interesting problem, because, as it turned out, there were four or five structures and they were all unconventional. The guesses that were made weren't what you'd expect them to be. Nonetheless, that didn't mean that there weren't signs or hints or clues or *something* to suggest the true nature of the structures. But it didn't matter: In every case, Simon Bauer, absolutely and uniformly, managed to guess the wrong answer. Well, Simon and I remained friends and colleagues, but we had scientific disputes after that. He always said to me, "You're too subjective!" That might have been true, but what he didn't quite understand is that the whole thing is subjective.

On the other hand, Simon was innovative and creative. For example, one thing he did that nobody else did was to draw a representation of what he saw. Such renderings weren't a bad idea. On the contrary, the problems arose because Bauer saw what he thought was *supposed* to be seen, as opposed to what we *wanted* to see. In other words, he failed to get the idea that a representation has to correspond with what you want to see in the first place. This is an intuitive thing. Perhaps it isn't two-plus-two logical, but that's the way it works. But Bauer didn't get it, even though he was a bright fellow. Instead, he drew his representations and insisted that they were the god's truth. As a consequence, it seems Simon Bauer had great difficulty in getting any answer at all.

COHEN: Unlike Linus Pauling.

SCHOMAKER: Pauling's approach was different. No matter how many drawings he looked at or how many papers he'd read or discussions he'd had, Pauling had the knack of just picking out what the answer had to be. And in this case he decided that the structure of B_5H_9 had to have some relation to the crystal structure of calcium boride, in which there are octahedral groups of boron atoms joined together in a three-dimensional network structure. So when Pauling got involved and suggested that Bauer should look at the chemistry in those terms, they were on the verge of success. In fact, success seemed to be an immediate and foregone conclusion, because Pauling's insight fit pretty well. But then Simon worked it around and worked it around until it was something that no longer had anything to do with the actual, original structure. That was a shame, because Pauling was pretty close. B_5H_9 does have a structure with a square pyramid, just the way Pauling suggested.

COHEN: Somebody once told me that Pauling could see around the back of the crystal.

SCHOMAKER: I think Pauling could do that, but Bauer made a number of contributions, too. Simon was always such a bright, bubbling guy, and he was always sure that he'd discovered something wonderful. And if this or that discovery were outlandish, so much the better.

COHEN: But you and Pauling got on just fine as far as this type of looking at things?

SCHOMAKER: Well, actually I was pretty much on my own, so we learned to work effectively. Pauling always said that these results were so important here. I've never been able to see it that way, but I'm sure he was right. To me, it seemed that these results were just the ones that he knew they would be anyhow. The tetraboron theory is something like that.

COHEN: Pauling uses instinct.

SCHOMAKER: From thinking about all the other things he knew, as to what this structure ought to be, Pauling had a better idea than he had from his knowledge of the data alone. But Bauer got stuck. He couldn't see what the answer had to be, and he couldn't understand that a suggestion from Pauling could immediately lead to success.

VERNER SCHOMAKER

SESSION 2

February 4, 1993

Begin Tape 2, Side 1

COHEN: We're interested in Pauling's successes, but in the meantime tell us about some of the earlier greats, and your introduction to them.

SCHOMAKER: When I got here in the fall of 1935, A. A. Noyes was still alive and chairman of chemistry, but he was so ill that I didn't even see him until sometime in the spring. When we finally met, Noyes wanted me to work in the library, gathering notes and so forth for him. I worked a little bit at that, ineffectively, I think. Nonetheless, I was given the opportunity to be one of the people to go down to Corona Del Mar and, I think, to stay in Noyes's house and work in the marine laboratory down there. Charles Coryell, Don Yost, Fred Stitt, Cliff [Clifford S.] Garner—there were many, many guys I learned to respect, as students, postdocs, and the like during that period. It was very grand, particularly when I met Noyes one or two times before he died [1936]. Noyes was a great fellow.

COHEN: You said you came to Caltech in 1935?

SCHOMAKER: Yes, and it was an almost overwhelming experience. Everything was at Caltech. Everybody knew everything; it was all quantum mechanics.

COHEN: Everyone affiliated with the chemistry division was working on that?

SCHOMAKER: Well, that was my impression. Anyway, when we arrived as graduate students, we came upon a remarkable situation. It was no doubt a wise provision, but there were some of us who couldn't understand it: We were *forbidden* to take more than forty-five units—I think it was a maximum of forty-five—and half of the units had to be in research. This requirement permitted us to take one of those wonderful courses in quantum mechanics or statistical mechanics or relativity or whatnot. Maybe we could take more than one course, I don't remember exactly. But some of us signed up for what we could, and then we audited several more courses. And some of the time, I just overwhelmed myself. That first fall, I think I went to Tolman's course in relativity, thermodynamics, and cosmology. I also went to Pauling's "Nature of the Chemical Bond." I took a course from Millikan the first quarter, [Ira S.] Bowen the second quarter, and [Carl D.] Anderson the third quarter. From Bowen, I learned a lot about atomic spectroscopy. From Anderson, I learned a lot about a lot of things; his was sort of a talk-talk-talk course. From Millikan, well, he talked about the old days and had his little book, *Electrons (+ and -) Protons Photons Neutrons Mesotrons and Cosmic Rays* [1934].

COHEN: That was his own book?

SCHOMAKER: Yes. I don't know how my copy disappeared. I was pleased to have Millikan's book; it had a lot of interesting things in it and a series of maybe a dozen appendices, in which he had boiled down the essential derivations of the beginning relations—the real relations that people use and remember, not the fancy guys but the guys who really want to know the essence. The essence—that was something we learned from Millikan, all right. Those were wonderful things. Every noon at the Athenaeum—remember, that's where I had board and room—students and teaching fellows would get together. Oh, there would be such a buzz of talk there! It was never about football, or tennis, or any of that. We were all excitedly trying to understand some of what we'd heard in the lectures.

COHEN: Those must have been wonderful lunches.

SCHOMAKER: They were extremely exciting. I thought they were wonderful; whether they really were wonderful in everyone's judgment, I don't know. Not everybody agreed on everything, and certainly everyone didn't always agree with me.

Some people said Pauling was not a good teacher. I've heard it said that [Richard P.] Feynman wasn't a good teacher, and that [J. Robert] Oppenheimer was lousy! But I was always utterly fascinated by whatever I heard when Feynman was talking. Oppenheimer, too. I went to a course of Oppenheimer's after the war, when he was here full-time. It was terrific! But there were snide things said. Maybe they weren't snide; maybe they were true. Anyway, sometime later I took a course or two from [Paul] Epstein. He was quite wonderful and sort of amusing or funny sometimes. I went to his thermodynamics a little while. It was thermodynamics, electricity, and magnetism, and I'm sorry but I just really didn't understand it very well. All the physics graduate students were there, and it was so easy for them. Even so, the physics students said that it was as if they were really understanding physics for the first time. They'd taken [W. R.] Smythe's course [in electricity and magnetism], where they'd worked all those problems that I couldn't even have begun to work. But they did say—sincerely, I think—that they had never quite understood what they were doing when they worked on Smythe's problems. On the other hand, when they went to Epstein's class, they learned what physics is all about.

COHEN: How many people would be in one of these classes?

SCHOMAKER: That all depended. When Pauling or Tolman lectured, the room was full, fifty or sixty people. Epstein? I think he held his class in 201 East Bridge [Norman Bridge Laboratory of Physics]; it didn't fill up, but it would be pretty full. The Millikan and Anderson lectures were in an ordinary classroom, perhaps twenty or thirty people.

COHEN: Would that have been both the chemists and the physicists?

SCHOMAKER: I don't know whether any physicists took that course; they may have had it already. On the other hand, maybe not. By 1935, physicists no longer cared much about atomic spectroscopy; its heyday was in the 1920s and early 1930s. But for the chemists and other people who wanted to understand the language and some of the facts about the atoms that you deal with in physical terms, atomic spectroscopy was still very important. In any case, I don't remember whether the physicists were in that course; they were likely slaving away on Smythe's problems.

COHEN: But you'd talk with the physics students at the Athenaeum, during those lunch gatherings?

SCHOMAKER: Yes. Several chemistry students had been listening to Tolman, and some of the physics students may have been to Pauling's lectures as well. So we saw a good deal of many of the physics and math students. Some of the engineers, too. But I was thinking now in terms of

Gene [Eugene H.] Eyster, Ken [Kenneth J.] Palmer, Henri Lévy, some of those fellows.

COHEN: Your chemist colleagues. Including Dickinson?

SCHOMAKER: Yes. Roscoe Dickinson was a marvelous guy. And sometime or another—this may have been a couple of years later, after I got my degree—Dickinson and Yost and Badger conducted a sort of seminar class on what they regarded as the foundations of thermodynamics and practical statistical mechanics. And of course Yost was playing his role in there.

COHEN: Yost played a role?

SCHOMAKER: It was the same role he always played in Tolman's class. Don was probably acting in all sincerity, but he sounded as if he were a shill for Tolman—a guy employed to ask questions, to ask questions, to ask questions.

COHEN: And yet he was actually on the faculty?

SCHOMAKER: Except for Tolman and Noyes, Don Yost may have been the oldest member of the faculty. But he came to this course whenever Tolman was talking, and he asked probing questions. It took me many years to figure out what was going on. So often I'd sit there and think, "That's a dumb question. We already *know* the answer to that question." Well, two-thirds or three-quarters of the time, we didn't really understand what Tolman was driving at. So Don would ask the question—ask it right away, at the time that it *had* to be asked.

COHEN: So with Yost's help, you were getting along pretty well.

SCHOMAKER: Fairly well, but somewhere along the line, my ignorance in mathematics became painfully obvious. I'd taken calculus and algebra and trigonometry—eventually I even found out what a graph was—but all this advanced and wonderful mathematical stuff was beyond me. So Morgan Ward taught a course out of the Courant and Hilbert textbook [*Methods of Mathematical Physics*]. My copy of that book is worn out, but at the time I never got beyond the first two chapters. Ward's lecture was at eight o'clock in the morning. I'd sit there and try to listen. He talked very fast and wrote very fast—left-handed as I recall—and I'm sure the lectures were wonderful. But it would have taken full time for me to master what was going on. There was so much that I needed to know, I thought, and so little time to learn it. It was assumed that I'd spend a third of my time being a teaching assistant, a third of my time doing research, and that there would be ample time left over for me to learn something.

COHEN: What was your assignment as a research assistant?

SCHOMAKER: To work as hard as I could, I guess.

COHEN: You weren't involved in taking care of any undergraduates or anything?

SCHOMAKER: Not in research. I believe I was called a teaching fellow, and I worked with eight or nine laboratory assistants from the freshman chemistry course. In my second year, after working my first year with Yost, I was allowed to go into electron diffraction, which seemed easier because at least I'd heard about it. I got on with that fairly well. We were working as hard as we could—at least it seemed to me we were.

COHEN: And still attending lectures?

SCHOMAKER: Always. In my second year—maybe it was the third year—I took a sequence of courses Pauling had worked up and taught: quantum mechanics, the chemical bond, and crystal structure. At this time, Pauling was teaching two of the courses, but the third was turned over entirely to [a senior research fellow]—Sturdivant, James Holmes Sturdivant. He became my mentor over the years, and I respected him very much. He took us through this course very slowly, very systematically. We worked problems, so it wasn't a burden to try to learn what he was telling me about. And it was important to us to try to do well.

The first days I was here, I had learned about [professor of physics William V.] Houston, and here was his book, *Principles of Mathematical Physics* [1934]. It was a rather thin volume, and I thought, "Well, gee, this is big print, and it contains things that I know a little bit about." So I read through all of Houston's book in one week. I didn't do all the problems—I was trying to do things faster than I could—but I thought I was doing pretty well even though I wasn't. I didn't know very much, it seemed; I could have profited by studying like an undergraduate for several more years.

COHEN: You still felt at a disadvantage in mathematics?

SCHOMAKER: Well, compared with some of the students who were here, I was at a disadvantage. They came from such places as Berkeley and the University of Minnesota. And a lot of people around were in the actual business of physics and chemistry. At the University of Nebraska, I had no indication of anything in the modern realm, except in organic chemistry. In my view, the main man there was Cliff Struthers Hamilton, an important organic chemist. And of course there was Tolman's friend whom I've mentioned, F. W. Upson. He was an important fellow, but he wasn't part of our laboratory. I never did get to meet him.

COHEN: But you did catch up on mathematics and modern ideas?

SCHOMAKER: Oh, I *never* did! After all these years, that's what I'm still trying to do! [Laughter] I thought that I knew about thermodynamics, because I'd had several courses from [H. G.] Deming in chemical thermodynamics at the University of Nebraska. But then I was taken around to see that wonderful guy, Roscoe Dickinson. Dickinson asked me about thermodynamics, and I thought I gave him a perfectly straightforward and correct answer. He said, "Well, what is entropy?" Well, I thought entropy was difficult to talk about. So Dickinson said, "How do you measure it?" So I said, "[equation, inaudible]." And he said, "What? What? What?" You see, I never tumbled to what I perfectly well knew he wanted me to say: "It's reversible; the increment of heat is a reversible process." That is what Dickinson wanted to hear, but I didn't say it. So he decided that I had to take the undergraduate physical chemistry course. So I went to Stuart Bates's course.

Now, there I was in [Professor] Bates's physical chemistry course, surrounded by some undergraduates and a few of the graduate students, and it seemed to me everyone else had come from Berkeley and such places—places where they'd already learned this stuff solidly. I don't know whether that was true—that they knew everything—or if I just respected them so much because of where they'd come from. At any rate, I took that course, and the experience saved my student esteem. I pretty much knew the stuff beforehand and had no trouble getting along. I didn't need to do it, I'm sure, but I did.

COHEN: If it saved your esteem, there was a reason for it.

SCHOMAKER: It kept me from thinking about myself as being utterly inconsequential as a Caltech graduate student.

COHEN: When you finished [1938], you made the decision to stay here?

SCHOMAKER: I was asked to stay, which was wonderful, because I was working on electron diffraction and couldn't dream of doing anything other than staying at Caltech. I was only a postdoc. My first year, I got $100 a month—I think that's what it was. And another student—David Stevenson from Princeton—had a National Research Council Fellowship and got about $110 or $115 a month. That ten or fifteen dollars seemed like a very important difference! But I continued receiving fellowships—I was a [George Ellery] Hale Fellow [1938-40] and a research fellow [Senior Fellow in Chemical Research, 1940-45] during the war—even though my future was sometimes in doubt.

COHEN: It wasn't your goal to teach at Caltech?

SCHOMAKER: In the beginning, I suppose I thought about getting an appointment here teaching chemistry, working on interesting problems, and so on, but I didn't really aspire to anything like that. It came about over the course of time. I remember sitting with Pauling on the stairway over in Crellin [Laboratory of Chemistry]. He said that I ought to go into industry. Well, I didn't want to do that. There was no question of any other job I had asked for. And so eventually, when the war was over, I was given an appointment—it was assistant professor, a slight demotion in pay—but I managed to stay on at Caltech. But at that point, I couldn't think of anything better to do, and I'd really never had any other dream.

COHEN: Was it Pauling who asked you to stay on as an assistant professor and did what work it took to make it possible?

SCHOMAKER: I'm sure it was. I didn't have to do anything. I get no credit for it at all.

COHEN: Were you married by this time? Or did you meet your wife a little later?

SCHOMAKER: Well, we were married in 1944, and this was 1946.

COHEN: And in those years, were you doing mostly the same thing, electron diffraction?

SCHOMAKER: In one way or another, I was mostly improving on things I had learned or worked on in the lab. But there were some different opportunities. For example, shortly before Pearl Harbor, I was going to work on a project near Pittsburgh [Bruceton, Pennsylvania]. George Kistiakowsky was in charge of that project. But then the war broke out, and I stayed on here to work on military-related research jobs. I don't have much to say about that.

COHEN: Well, OK. Do you remember Pearl Harbor?

SCHOMAKER: Who doesn't? That Sunday, a group of us had gone out into the desert, in the Caltech style. I had a new car at the time and drove out with June Astor and some other people whose names I can't remember at the moment. We may have been with the Paulings, but I don't remember it that way. At any rate, we made camp and then stopped for coffee or something. That's when we heard that the Japanese had snuck in with a few planes and bombed Pearl Harbor, but that everything was going to be all right. That's what the story was.

COHEN: Now, how about a recollection or two about your work during the war?

SCHOMAKER: Well, let's see. The first job was a contract that Pauling had obtained. The idea was to put electron diffraction to work for the war effort. I don't know how good this idea was, but we were supposed to investigate substances to protect against chemical warfare agents. In the summer of 1943, Pauling sent me to discuss this project at the University of Chicago. I also visited Homer Smith in New York. Dr. Smith was a famous nephrologist, whom Pauling knew because of his own nephritis. I went to see Carl Niemann's [then an assistant professor of organic chemistry] former boss at the Rockefeller Institute, and I went to Edgewood Arsenal. So

Pauling sent me all over the country because, I guess, he thought I'd be smart enough to figure out something that would be useful against chemical warfare agents. But I never had any productive thought about that at all. I tried, but nothing ever came of it.

COHEN: Did *anybody* have a productive thought about that?

SCHOMAKER: I don't know whether anything substantial was ever accomplished. I'm not saying it was a bad idea, but timing was crucial during the war, and we couldn't accomplish something practical in a reasonable amount of time. Perhaps if Pauling had become more directly involved in that project, or any number of other people had worked on it, something important might have been accomplished. From my point of view, Pauling could succeed at just about anything he attempted. On the other hand, maybe it wasn't a great idea.

COHEN: The best-laid plans. Do any others come to mind?

SCHOMAKER: Well, after that project wound down, I joined an enterprise that Don Yost called the Gunpowder Project. But he was always very derisive toward it, perhaps because he'd come back from Evanston suffering from osteomyelitis. Whatever the reason, the Gunpowder Project fizzled out. But hope sprang eternal during the war years. One time Pauling tried to get me interested in an idea that had been put into practice—controlling the burning rate of solid propellants in jet-assisted takeoff, or JATO. This was an interesting problem. Pauling's idea was that you could control the propellant's rate of burning by integrating strands of a different kind of propellant. The propellant charge was supposed to burn at a uniform rate of speed from the back to the front, which was the direction that the plane was going. The trouble was that ordinary double-base powder—if that's what it was that they were using—is hypergolic. The higher the pressure, the faster it burns. The resulting tendency is to burn uncontrollably and to explode! But here are Pauling's strands, placed in there somehow.

COHEN: Was Pauling disappointed if you didn't pursue exactly those ideas that came to his mind?

SCHOMAKER: He never said so. Pauling hoped I'd get something done; he hoped the same for lots of people. So he wanted me to do something with this theory, to understand it further. But I could never get beyond thinking that his idea was exactly right to begin with, if it could be implemented. This stuff burns faster, and it's a material that isn't hypergolic. Its burning rate depends directly on the pressure.

COHEN: As you were working on such experiments during the war, were people still coming to learn things in your laboratory?

SCHOMAKER: I think that was always going on. There may have been fewer visitors during the war, but there were always some people around who wanted to do something in chemistry at Caltech, especially with Pauling. They wanted to determine the structure of molecules. I was trying and trying and trying to understand the procedure from my point of view, reading those electron-diffraction pictures visually and learning from them. And I finally got to my rationale.

COHEN: Well, it must have been pretty good. It's fortunate that research could continue even during the war.

SCHOMAKER: Well, a lot of graduate students' regular work was interrupted by the war, but only partially, because they got deferments for their war work. In other words, even when they worked for the war effort, they often had some time to keep working on their other pursuits. For example, Bill [William N.] Lipscomb, who's retired now [d. 2011]. Bill came to Caltech in 1941, I think, as a physics student, but then he decided he wanted to work with Pauling. Pauling sent him down to work with me. Of course, Lipscomb really was interested in working with Pauling, but he was now my student, and we worked together quite a bit. He continued on down the line in electron diffraction and then working on crystal structure with [research fellow] Eddie [Edward W.] Hughes. Then Lipscomb became involved with a classified project, one of the problems concerning the double-base powder used in the propellants. So I talked to him about that problem, and think I made some contribution, although I'm not sure anything ever came out of it.

COHEN: Let's return to Pauling. As I understand it, Pauling's original idea very often had the solution in it, and he just wanted you people to give him the experimental proof of it.

SCHOMAKER: Well, you know, there's always more to be done. Very often Pauling did understand an idea so well that the only thing left was to prove it. Sometimes he didn't have it quite right; sometimes it didn't work, but it almost always really did work. I must add that Pauling didn't come up with *all* the ideas. For example, I'm not sure that the idea of putting dye into the rocket propellant originally or directly came from Pauling. In fact, it probably didn't. Remember, a lot of the problem already had been solved. Pauling or [senior research fellow Robert B.] Corey or even someone else may well have brought the remaining problem to Lipscomb. But in any case, Pauling was a continual source of great ideas, and they worked most of the time. For instance, about half of my thesis—the radial distribution method in electron diffraction—was just a matter of working out a thing he told me to try.[2]

COHEN: How did that come about?

SCHOMAKER: There was a man by the name of Charles Degard who had come here from Belgium to work with Pauling. Pauling had put Degard onto trying to carry through the instrumental reading of the electron-diffraction photographs of carbon tetrachloride and to make a radial distribution function by using those data. Degard found what Pauling and Brockway had wanted to know to begin with, but they didn't respond to it.

COHEN: At this point, was Pauling still saying, "Why don't you do this, why don't you do that?" Or were you more or less left to yourself by then?

SCHOMAKER: Well, a very big part of our lives around here was that Pauling came down with this terrible nephritis. This was in 1941, I believe. By comparison with what he was before, he was out of action for all that time. Yet Pauling was still very busy. There was only a certain period when Mrs. Pauling kept him in bed; she had him weaving little squares for a bedspread, things like that. She found that if she could keep him occupied with those little projects, she could keep him from thinking about things so much. So we'd go visit him on occasion, just to say hello.

[2] Schomaker, Verner (1938) *I. Modifications of the radial distribution method of interpretation of electron diffraction photographs of gas molecules. II. The electron diffraction investigation of the structure of benzene, pyridine, pyrazine, butadine, cyclopentadiene, furan, pyrrole, and theophene.* Dissertation (Ph.D.), California Institute of Technology.

COHEN: How long did Mrs. Pauling keep him confined to quarters?

SCHOMAKER: I don't know, maybe six months. He was mainly bedridden for a modest period. He was just resting, in order to survive the trouble with his kidneys. There was no dialysis in those days, so it was a pretty serious situation. And for a long time Pauling's condition remained somewhat delicate.

The first sign of Pauling's illness—I mean, the first time we realized that something was wrong—was a photograph. Pauling had gone East to receive the Nichols Medal of the New York Section of the American Chemical Society, and someone took a photograph of him receiving this award. I think that in the photograph people actually saw that he was edematous—that his face was swollen. Some people may have known about his illness before this, but we didn't. In any case, by the time Pauling got back from New York he was a very sick man.

COHEN: But the work at Caltech went on, through the war and beyond. And you remained involved with electron diffraction?

SCHOMAKER: Always. And for a decade—from about 1940 to 1949—I was interested in doing our calculations with punched-card machines—a different method from the way we'd been doing it before the war. One day, Eddie Hughes was looking at my program, and he said something such as, "This is silly. It doesn't make sense to have a table of value of the sine of a certain frequency, or of the cosine of a certain frequency, on these thirteen different cards that you've got to handle and manipulate." Eddie decided that we should have all these data on one card, so that we could handle these cards the way we handle the Beevers Lipson strips, in which one strip represents a sine function or a cosine function for one value of the amplitude and one value of the frequency.

Well, I thought Eddie had come up with a great idea! And I found a way to make it work. It wasn't immediately obvious, but I found a way of getting sixteen four-place fields on one card and on the tabulator at one time. So my wife, Judy, punched up the cards according to this scheme, and I found out how to wire the tabulating machine to get sixteen different totals, even though there were not that many different four-place-or-more counters on the machine. This was pretty nifty stuff.

COHEN: And it kept you pretty busy.

SCHOMAKER: I always tried to stay busy, even when somebody suggested I was a busybody.

COHEN: You'll have to explain that.

SCHOMAKER: Just about this time, at about the beginning of the war, David [P.] Shoemaker came to Caltech [1942] as a [graduate] student from Reed College in Portland, Oregon. David, who's somewhat younger than I am, was one of Corey's students and a marvelously productive fellow. One day David and I were discussing a problem he was working on with Corey, and I told David he should use a three-dimensional Patterson function in his calculations. I suggested that if they used it, it should be possible to deduce the threonine structure reasonably easily. As I continued to suggest the three-dimensional approach to David, Corey and he struggled, unsuccessfully, to get the structure by two-dimensional calculations. Eventually, Corey decided that my little suggestion to David wasn't all that constructive. Corey and I had a little heart-to-heart talk, and he said he didn't want me to continue discouraging his student. Well, Corey probably thought that sincerely, but I always thought it was very funny. I wasn't discouraging David, and I believed that the three-dimensional method would work. So we worked day and night for three days to make this calculation. As it happened, I was on the button about what we might do in order to get the structure solved. We managed to interpret the Patterson function and to find the structure. We didn't have it all finished, but later Corey, Jerry Donohue [PhD 1947], and some others refined it, and we finished it all up after we came back from Copenhagen.

COHEN: Copenhagen? Why did you go to Copenhagen?

SCHOMAKER: One day I was in Throop Hall, working on a program for this new machine. Pauling walked in and said, "I thought you were done with all that stuff!" Well, I *wasn't* done with all that stuff; in fact at that point I was just getting interested in it strongly. But Pauling had forced me to apply for the Guggenheim Fellowship back in 1941, and now it was duly awarded.

COHEN: Because of Pauling's influence?

SCHOMAKER: I'm sure of it. The fellowship had been put on hold because of the war, so I didn't go until 1947.

COHEN: Did you have your professorial rank by 1947?

SCHOMAKER: Yes. [Schomaker received tenure as an associate professor in 1946—ed.] I changed my plans somewhat from what they had been to begin with. A lot of time had passed. The war was over. So we spent some time visiting: Brockway in Michigan, Bob Livingston at Purdue, T. R. Rubin at Ohio State, our old friend Simon Bauer at Cornell, Maurice Huggins at Eastman Kodak, Bob Langmuir at General Electric in Schenectady, R. W. Dodson and others at Columbia and Brookhaven; several friends at Yale. And for a month or so, E. B. Wilson at Harvard.

Begin Tape 2, Side 2

SCHOMAKER: So without too much proper planning, I found myself in Copenhagen, at the Bohr Institute, without a work plan. Bohr wasn't even in Denmark; he was at Princeton at the time.

COHEN: This was still in 1947?

SCHOMAKER: This was now the spring of 1948. Bohr came back to Copenhagen in the summer. I met him a couple of times, once at a wonderful occasion at his summer place. As I recall, it isn't on the North Sea but it faces toward the north. Bohr told me the story about the Atlantic eels. They came from some then unknown place in the Sargasso Sea, which is along the Gulf Stream. Bohr also told his story of the horseshoe that he had nailed up above his front door. One day a visitor said something such as, "Why, that's just superstition! You don't really believe that a horseshoe brings good luck, do you?" And Bohr replied, "Of course not. But, you know, they say it works even if you *don't* believe it."

Niels Bohr was a wonderful fellow. In fact, everyone in Copenhagen was very kind to me. When I got to the Bohr Institute, people asked, "What are you doing here? Are you going to work on the theory of electron diffraction?" I'd say, "Sure." I didn't know much about it. As it turned out, however, it was one of the biggest little scientific events of my entire life.

COHEN: So they were aware of electron diffraction at the Bohr Institute?

SCHOMAKER: Oh, yes. They were nice, wonderful strangers, and they were very interested in what my plans were. When I said, “Sure, I plan to work on the theory of electron diffraction,” they said, “Well, are you going to work on the *problem* of the theory of electron diffraction?” That was an excellent question, because I didn’t realize there *was* a problem. But there was a problem, all right. Actually, I had been aware of it, but I didn’t completely understand the theory of electron diffraction, so I didn’t understand the problem.

COHEN: What was the problem?

SCHOMAKER: Well, there was a fantastic thing about the reported structures of the hexafluorides. Sulfur, selenium, and tellurium hexafluoride had been investigated by electron diffraction by Pauling and Brockway as well as some other authors. These hexa-fluorides were found to be fully symmetrical octahedra, just as Pauling and others had expected they would be. On the other hand, there were these people in Germany—Braune and Pinnow—who had studied the hexa-fluorides of uranium, tungsten, and molybdenum, and they had found that the structures were different. These structures had some bonds that were longer, some shorter, and some of the bonds were intermediate. The amounts of these differences were considerable—different by several tenths of an angstrom. And the difference was absolutely remarkable. Now, this work had been done before the war, and because uranium hexafluoride was very important, it had become a great issue. Many people had been put to work studying uranium hexafluoride, so that they would know how to use it for the separation of uranium-238 from uranium-235.

As it turned out, all the lines of evidence, except for electron diffraction, seemed to show that uranium fluoride is a regular octahedron. Consequently, Simon Bauer was asked to reinvestigate this, and his electron-diffraction work confirmed the previous findings of the German investigators, Braune and Pinnow. Then J. L. Hoard, a crystallographer at Cornell, determined the crystal structure. He found that the crystal structure contains unsymmetrical molecules all right, but they weren’t quite the same as those that had been proposed by Braune and Pinnow.

So there it was, an unresolved problem. And we worried over it quite a bit after the war. I kept saying—by this point, I knew enough to say this—I kept saying that we ought to make pictures of this phenomenon at different electron wavelengths. I thought that if there were something complicated about the physics, changing the wavelength would change the character of the diffraction pattern. However, before we got around to making those pictures, a student of mine, Bill [William F.] Sheehan, and I talked a great deal about the problem of the unsymmetrical molecule—about what it was supposed to be, and so forth. Bill and I had a lot of ideas—and doubts—about the consequences related to the reported lack of symmetry, but we couldn't explain why the molecule should be unsymmetrical. Neither could Pauling. So everybody was perplexed for a while there, but it was the best theory that existed. Then in December of 1951, Bill Sheehan took his final examination for his doctorate. As it turned out, there was some discussion of this problem, and the great physicist Robert Christy was on the committee. Sheehan asked Christy, "Do you think the present theory is correct?" Bob Christy was good enough to say that, well, perhaps we shouldn't trust that particular theory—not too much, certainly not totally. Christy's remark stimulated, or encouraged, me to think more constructively about this matter. And very soon I had worked out the simple notion that there is a shift—a phase shift—on scattering that increases with the angle of scattering, and I realized that this phase shift is greater for heavy atoms than it is for light atoms.

COHEN: This insight was a breakthrough?

SCHOMAKER: This insight made it possible—no, easy—to determine the approximate law to explain this shift phase from the considerable number of other cases of unsymmetrical electron diffraction that we had accumulated by this time. There was a paradox of symmetrical structures for sulfur fluoride, selenium fluoride, and tellurium fluoride. By now, I had begun to perceive the mystery of that paradox: Simply put, the diffraction patterns had not been seen to be big enough angles to show the effect, and of course sulfur and selenium aren't very heavy atoms anyhow. However, I could find nothing in the books, especially [N. F.] Mott and [H. S. W.] Massey's *Theory of Atomic Collisions*, that could support my hypothesis. So I went back to see Bob Christy. He said, "Well, don't worry too much about the prevalent ideas." Christy encouraged me to pursue my own ideas along this line, and his encouragement gave me some hope. It's funny how these things happen. A few days later, I happened to be at a physics seminar. Roy Glauber, who was a Caltech postdoc at the time, came up to me and asked about what Christy had been telling me about my work. I told Roy what the facts were, as I understood them up until that point. Almost immediately, within about a week or so, Roy came up with a pretty good theory. [Hans] Bethe's first approximation, which everyone had used until then—at least, in gas electron diffraction—is inadequate. Glauber's work thus far had justified what I had meanwhile discovered as a description of the thing, and he made further theoretical studies of the problem. Jean [A.] Hoerni and Jim [James A.] Ibers made some moderately extensive practical theoretical calculations. That was it for us here, so the question remained, What to do next? We did make pictures of a few more molecules with both light and heavy atoms. It turned out that the phase shift is indeed a universal thing. And of course nowadays it's always taken into account.

Now, did Pauling have something like this in mind when he sent me to Copenhagen? Did the people there already realize that I ought to be consulting them about the theory of electron diffraction as we used it at Caltech? I don't know. But I do know that we worried a lot about the puzzle that had been presented by the unsymmetrical hexa-fluorides. I also know that I came close to failing to see the answer. So, as I said, it's funny how these things work. If Bill Sheehan, the doctoral candidate, hadn't asked his question of Bob Christy, I might never have gone to Christy for help. In any case, now I was in Copenhagen, spending most of my time working with Shoemaker.

COHEN: David Shoemaker, who had been Corey's student?

SCHOMAKER: Correct. In Copenhagen, I found myself sitting next to David, and both he and I were still interested in the Patterson function. So we did some work, talked a lot about how we might be able to use it more effectively, and wrote a manuscript about our thoughts. By this time, Corey had come around to accepting this three-dimensional scheme. He read our manuscript and encouraged us strongly; Corey said, "You should publish this." But in the summer—July or maybe August—David set out for home. I accompanied him down to Langelinie, the dock in Copenhagen. So there was David Shoemaker, loaded down with all his things, holding our manuscript in hand. He sailed for home on the *Batory*. But that's how I spent my time in Copenhagen, enjoying everything, being entertained by marvelous people.

COHEN: Then you returned to Caltech.

SCHOMAKER: It was good to be back, much better than returning to some other place. At some universities, you have the burden of having to teach classes with 500 students or 300 students, as well as all the usual administrative duties. Here at Caltech, people who were good at teaching and liked to do it taught two courses all the time. On the other hand, some people didn't ever have to teach anything at all.

COHEN: Should we gather that you didn't enjoy teaching?

SCHOMAKER: I didn't teach that often. I usually taught one course, sometimes a special course, for a quarter. Maybe I taught more than that, but it never seemed like a great burden. So I didn't get too much experience here at Caltech, except for the wonderful but devastating experience of teaching Pauling's course—either the Nature of the Chemical Bond or his Introduction to Quantum Mechanics. If it was the Nature of the Chemical Bond course, the lecture room would be full. When Pauling taught it, the room would also be full—at least in the first few days. When I took over for him, which I often did when he traveled, attendance dwindled a great deal.

Also, when Stuart Bates traveled in Germany, sometimes I took over teaching his course in thermodynamics. I enjoyed it and found it very enlightening. For example, one day as I was talking to the students I finally understood the Gibbs phase rule in a way that I thought was entirely new. I hunted in all the books in the library and I couldn't find anyone who ever had explained it so clearly and simply—my way. When I told Pauling about my new insight, he was very enthusiastic. He went around telling everybody about it. He didn't quite bother to convey it correctly, however, so I had to explain it to them over and over again. But that didn't matter. I had come up with an entirely new insight, and I thought that was great! But then I discovered that a man named Otto Redlich had published on this, first in the 1920s, then again about 1945. And this Redlich fellow had it just right.

The typical thing in science is that important general concepts get streamlined; they become clearer and clearer as they're taught, taught, and re-taught. But that hadn't been the case with the phase rule. Redlich had explained it very beautifully from the start, more elegantly than I could have. But his view of it was essentially the same as the one I'd come to that day in Bates' class; the only difference is that he'd seen things so clearly some years earlier than I had.

COHEN: There's nothing like teaching to make you understand something yourself.

SCHOMAKER: I've always found that so. Unfortunately, too often the students didn't seem to agree.

COHEN: Should we deduce that teaching is not your first love?

SCHOMAKER: I enjoyed the learning part. And arguing with people. And talking about our work. I used to do an awful lot of that—talking about what we were working on. I don't know why people came, because I'd be just talking through my hat. But they'd come in the door hour after hour after hour.

COHEN: You preferred explaining things to your colleagues?

SCHOMAKER: I believe that's what I mostly did. I only wish that I knew more, so that I could explain things more deeply. Nevertheless, Pauling would come around and talk to me sometimes as if I were his advisor, rather than the other way around. One day he came in and said he thought we ought to get [John G.] Kirkwood. I nodded or something, I suppose, and Pauling just walked out. Then he went about trying to get Kirkwood to come to Caltech, and Kirkwood came [1947]. One time, somebody said that the reason Kirkwood left Cornell was that he'd had some problems with [Peter] Debye. I don't know what Debye had done that was so unpleasant to Kirkwood, but there might have been something to the story. My own experience with Debye was quite good. He flattered me by remembering me after having met me just once. The next time I saw him in Baker Lab [at Cornell], he remembered who I was, or at least he was kind enough to pretend to remember me. I also enjoyed knowing Kirkwood. He was very inspiring to many people at Caltech, but then he decided that Yale was the place to be, because that's where [J. Willard] Gibbs had been. Now that Kirkwood was gone, Pauling asked me to teach his course in statistical mechanics, and I enjoyed that very, very much. I didn't have much experience working in statistical mechanics, but I'd listened to Tolman's course several times over. Tolman would talk about a subject until he had finished it completely. His course was scheduled for a year and took five quarters. It was a very thoughtful, very thorough presentation of all the fundamentals, without an emphasis only on applications. We learned a lot. So I was fairly prepared to teach statistical mechanics, and there were some perfectly tremendous guys in that class. Martin Karplus and Gary Felsenfeld were the stars. Martin knew everything.

Everyone was so bright and curious and motivated. That was wonderful.

COHEN: And how was your research progressing?

SCHOMAKER: There were some pretty exciting things going on. For example, the boron hydrides. I didn't tell you that we eventually got ourselves into a dispute with Simon Bauer. In his time here, he worked on four or five boron hydrides and he managed, I think, to get the wrong answer for every one of them.

VERNER SCHOMAKER

SESSION 3

February 8, 1993

Begin Tape 3, Side 1

COHEN: Should we gather that Dr. Bauer's work was mediocre?

SCHOMAKER: Certainly not, not in the long run. Simon went on to Cornell; and in any case, he was very creative, and creative thinkers are always an asset in science. Sometimes creative scientists move on so they can find a good outlet.

COHEN: You moved on. Eventually you entered industry, then joined the faculty at the University of Washington. Before we relate those experiences, let's cover your early Caltech days.

SCHOMAKER: Well, Millikan was called the Chief, wasn't he? Or something like that? And I was told that Noyes was called the King. But I never knew much of Noyes in his role as leader of the chemistry division. As I've said, Noyes was ill when I arrived in 1935. I did some work in the library for him, not too much and not particularly well. That was just before I was given the opportunity to go down and work at the marine lab with some of these other heroes, most of whom were also students of Don Yost.

COHEN: So most of your time was spent under the chairmanship of Linus Pauling [1937-1957]? Were you involved with that appointment, the politics and all?

SCHOMAKER: Oh, no. After all, I was just a graduate student. But I'm sure there was some politics surrounding Linus being chosen as acting chairman and then as chairman. But again, my perspective was that of a student; all I knew, or thought I knew, came from the rumor mill.

COHEN: How about a juicy rumor?

SCHOMAKER: Well, as I try to reflect on it now, it may be that Tolman was considered a candidate for the chairmanship job.

COHEN: Do you think many people wanted Tolman rather than Pauling to be chairman?

SCHOMAKER: No, no! I don't think many people preferred Tolman. Probably his candidacy was more rumor than fact, and I didn't reflect on it too much one way or the other. As I recall, we thought it was obvious that Pauling was going to be leader. But Pauling was just about the youngest guy in the department, so I guess there was some uneasiness about that.

COHEN: Was Pauling a strong leader?

SCHOMAKER: I had the impression that Pauling pretty much did what he pleased as chairman. He certainly must have consulted his colleagues, but it was our impression that he really did what he wanted to do. For example, later on, when I knew something about how decisions got made, I'm pretty sure that one day Pauling just called up Jürg Waser, asked him to teach freshman chemistry, and that was that.

Then there was the time that Jack Roberts, Norman Davidson [Chandler Professor of Chemical Biology, d. 2002], and I went over to see Pauling about hiring Harden McConnell. Harden had been Norman Davidson's student, and we all thought his returning to Caltech was a wonderful idea. I don't know whether Jack or Norman was the leader of this excursion, but they brought me along. We said to Pauling, "You've just got to try to hire Harden McConnell. You've got to get him to come back here." Oh, I suppose there was some talk, maybe even heated discussion as if there were something to fight about. But my impression is that Pauling more or less agreed about Harden on the spot, that they called up Harden and appointed him [1956]. I don't know whether that's true, but that's my impression.

COHEN: That hiring procedure seems rather informal, for such a high-powered academic institution.

SCHOMAKER: Oh, I don't know whether it was really as informal and almost dictatorial as that, but it was certainly a lot different from the later years. Things were very different at Caltech in those days. For example, as I recall it now, Norman Davidson sent me a postcard asking whether there was any opportunity for him in the department. I mentioned Norman's casual little query to Pauling. It seems to me now that Pauling nodded and said, "Yeah, that's a good idea." Then maybe after Pauling talked with someone else—I'm not sure who, I can't remember whether Kirkwood was here at this point—Norman was given an appointment to Caltech [1946]. The process might not have been that simple; it probably wasn't, but certainly things were simpler in those days than they are now.

COHEN: Were these people asked to come to teach chemistry or to do research? Were they appointed because the department had needs to fill or simply because they were good chemists?

SCHOMAKER: Well, in Harden McConnell's case, we went to see Pauling because we thought Harden was going to be a leader, because we thought we ought to have him here. It may also have had something to do with Jack Roberts's interest in magnetic resonance. By the way, when Jack came here [as professor of organic chemistry, in 1953], the process seemed to be somewhat informal. [Research associate Edwin R.] Buchman had some manuscript or something he'd gotten from Jack, or from Jack's associates at MIT or at Harvard, and he was very impressed. So Buchman came around and said, "Look, this Roberts guy is really very good." Pauling agreed, saying something such as, "Yes, we ought to try to get Jack Roberts to come here." And of course, Jack Roberts did. Again, maybe it wasn't that easy. I may be spinning you stuff that's utterly false, but that was my impression at the time.

COHEN: During Pauling's tenure as chairman, were there faculty meetings on these matters?

SCHOMAKER: Well, after I was on the faculty, there were occasional faculty meetings; of course there were. But as often as not, my impression of it was that Pauling said something about his plans, or said something about what had been done. Linus Pauling knew how to get things done.

COHEN: Not a lot of discussion in the faculty meetings?

SCHOMAKER: Not too much. And certainly there was not at that time, in my impression, any matter of voting on it or of having a formal search or anything like that. Later, there were formal searches for new faculty. In the year just before I left, for example, George Hammond was appointed as an organic chemist [1958]. Six other wonderful organic chemists had been considered—there was a lot of careful consideration—but George was the chosen one.

COHEN: How did such decisions come about? Did they decide, "Hey, we've got to beef up the program in organic chemistry?"

SCHOMAKER: Something like that, I suppose. As I recall, Howard Lucas had died recently. [Howard J. Lucas died five years later, in 1963, but by 1958 he had ceased to teach.—ed.] Carl Niemann was there, Jack Roberts was there. I can't say that Lucas could have been, you know, replaced exactly; all those guys were unique. But, yes, there was a decision to strengthen organic chemistry. And then just before I left, there was another formal search; that time, it was for a physical chemist.

COHEN: But Pauling was still chair, in these last years of your first incarnation here?

SCHOMAKER: As I recall it, he resigned from being chairman the same day that I left.

COHEN: What led up to this?

SCHOMAKER: The department changed. Some of the changes were good, I think; some weren't. The department was still dominated by structural chemistry and molecular structural determination, much as it had been in all my time there. Of course, before Tolman came [1921], it had been different, but Noyes was responsible for starting crystallography here. He got one of those early guys to go study and travel, to learn what it was all about, and then Dickinson came out to Caltech.

A lot of important things happened in those days. The greatness of the Caltech chemistry division didn't originate with Pauling; it goes back to Noyes. He established vital programs. For many years, in chemistry Caltech was the most important place in the country, perhaps the most important place in the world.

COHEN: That's quite a claim.

SCHOMAKER: It is Noyes, I think, who deserves credit for being the first one to make it so. But it was true: Caltech was the place to be. Arthur A. Noyes was a pioneer and a giant.

COHEN: Yet it seems that in the outside world, or the public imagination, Linus Pauling has the more famous name. Does that have anything to do with his politics?

SCHOMAKER: Well, Pauling certainly made a name for himself in the political world. But not in the early days. In fact, I never had the slightest glimmer of Pauling's interest in politics until about 1940—I think that was the year. In any case, that was when Pauling accepted the invitation to debate [Caltech professor of history and economics Ray E.] Untereiner. The debate was over whether Wendell Willkie or Roosevelt should be president, and it was my first evidence that Pauling was interested in politics. Before that, it seemed to me that his interests were purely scientific. Pauling's so-called liberalism, from my point of view, was confined to being open to interesting new problems and ideas in chemistry.

COHEN: When Pauling did make his politics known, did it bother the Caltech trustees, who tended to be quite conservative?

SCHOMAKER: I feel sure that not everybody welcomed Pauling's public views with open arms. On the other hand, the trustees didn't consult me on these matters. My feelings were very intense, but we never knew what the trustees may have thought.

COHEN: But Pauling made waves?

SCHOMAKER: He was a factor in world politics for a long time. Pauling made an impact on the world—the Nobel Prize in chemistry [1954], the Peace Prize [1962], the controversy about vitamin C, and so on and on. But it seems as if we're trying to encapsulate three decades here, and that's not easy.

COHEN: Was Caltech a politically active place?

SCHOMAKER: There were so many ideas in the air. We talked about all kinds of political philosophies, even the tenets of communism. And the conservative people ran around calling Roosevelt a communist. And that's the way it was around here—all these ideas, left and right, good and bad, productive and silly, et cetera, et cetera, et cetera. But as I think I've said, not too much was said about Pauling one way or the other, until the Second World War. After Pearl Harbor, it just so happened that Pauling had a Japanese gardener. Somebody came along and painted a sign on Pauling's garage: "Jap Lover."

COHEN: "Jap Lover." That graffiti suggests that Pauling was not universally popular.

SCHOMAKER: Pauling had strong views about things, and he became increasingly vocal, but I don't think it ever entered his mind to become universally popular. Debates took place. He said and did many things that were of great concern to his colleagues in the chemistry division. Pauling took risks.

COHEN: What were the consequences?

SCHOMAKER: Well, for one thing, the government rescinded his passport [1952]. A lot of people say, and perhaps with real reason, that if Pauling had kept his passport, he would have attended some meetings in Europe. In England, he probably would have seen the photographs of Rosalind Franklin, as Watson and Crick eventually did. In other words, if his passport hadn't been rescinded, Pauling might have known enough of the simple facts to get the DNA structure.

COHEN: Oh, so we can directly trace the success of Watson and Crick to this political action of our government?

SCHOMAKER: That has been suggested, I'm sure, many times.

COHEN: But Pauling's passport was returned?

SCHOMAKER: In 1954, I think. They gave his passport back to him so that he could go get the Nobel Prize in Stockholm.

COHEN: What were the reactions to Pauling winning?

SCHOMAKER: They varied. We were enormously proud, of course, and overjoyed. But the State Department said something to Pauling such as "We're not angry, if you're not angry," and I think that was one of the terrible things.

COHEN: But not the worst?

SCHOMAKER: The *Wall Street Journal*—maybe it was the *Saturday Evening Post*, I can't recall—published an editorial when Pauling got the second Nobel Prize [i.e., the Peace Prize, 1962]. As I recall it, the headline said, "SWEDEN SLAPS AMERICA IN THE FACE!" [A headline in *Life* read, "A WEIRD INSULT FROM NORWAY." *Morgenbladet*, a conservative pro-American Norwegian newspaper, called it "a slap in the face." Note that the Peace Prize is not awarded by the Swedes; it is awarded by the Norwegian Nobel Committee.—ed.] I thought that was absolutely godawful, saying that just because Pauling had been awarded the Nobel Peace Prize.

COHEN: How did Mrs. Pauling react to all this?

SCHOMAKER: Well, Linus always said that she had encouraged him, strongly encouraged him, to be active. And life went on. We, the students, were often invited over to the Paulings' home for parties or dinners, and they used to take people out to the desert, in the Caltech style. At Caltech, we seem to have the habit of doing things like that; it's the influence of the young kids around here. We really had some great times.

COHEN: Since it was so enjoyable, why did you decide to go into industry?

SCHOMAKER: The smog in Pasadena was a factor. The growth of the smog always made me very angry and unhappy.

COHEN: Was smog here as early as the 1950s?

SCHOMAKER: Earlier than that, although the [Western] Oil and Gas Association didn't want to admit it. When I returned from that boondoggle trip to the University of Chicago that Pauling had sent me on, people were talking about how the smog was showing up. This was in August of 1943, and the Oil and Gas Association had an advertising campaign to convince people that smog had nothing to do with automobiles! They claimed smog had nothing to do with internal-combustion engines, nothing to do with the sloppy chemical engineering practices of the refineries. It was amazing. The Oil and Gas Association tried to convince people that smog was just here, that it *always* had been here, that the Los Angeles Basin had been the valley of smokes from time immemorial. Maybe the most outrageous thing was the Oil and Gas Association's so-called research program. It was a sham, a program for disinformation. Every time [professor of chemistry Arie J.] Haagen-Smit would come up with a new point about what was going on—and he was substantially always right—the Oil and Gas Association would go into action. Their program seemed to be just an organized effort to talk down anything that Haagen-Smit discovered about this smog.

Some people even said that there wasn't smog here, but I had a lot of personal evidence to the contrary. For example, sometime during the war the people in chemistry and biology played some overhand softball games down in Brookside Park. I was never anything of an athlete, but I took these games very seriously for a number of years and really enjoyed them. So we'd go practice in Tournament Park. And as time went on, if we went out at noon, after only a little bit of practice we would be gasping. Just from exercising mildly. Typically, the smog would come in at eleven or twelve. If we finished practice before it came in, then fine and dandy, but if the smog was already there, that was it! It was horrible. And the smog didn't show up only outside. I really became incensed when it entered the buildings on campus. In Crellin, the ventilating system would suck it in, and you could tell that you'd been invaded within about ten minutes. I suppose the crowning blow came when our son went on a school trip to Pomona, which was far away and supposedly smog-free. But our son came home and told us he'd seen the smog. We talked a lot about moving to Santa Barbara or Bishop—anywhere there was no smog would be just wonderful!

COHEN: So smog sent you packing?

SCHOMAKER: There were other factors in our decision to leave Caltech. A lot of my friends had gone to work at a lab in Berkeley. I accepted an invitation to go up there and found it to be very attractive. As things turned out, however, Berkeley wasn't a stable place to go. But now I was thinking seriously about making a move into industry. Money was another factor. I had heard from some people here that our students were going into industry or government and getting better initial pay than we got here—more money than we earned even after we were supposed to be somebody, even after quite a number of years of service. So money had something to do with my eventual move into industry. Also, I had personal disappointment, in that a number of the interests in the department had changed. I can't say that the changes were wrong, but I was disappointed by the way we started to do certain things around here.

COHEN: For instance?

SCHOMAKER: Well, for years Pauling had encouraged us to build a new machine, a new vacuum apparatus. So we agreed to copy the machine they had at the University of Michigan, changing it a little bit. My friend, my mentor, James Holmes Sturdivant, was in charge. He went out and got a PCC [Pasadena City College] engineering student to make drawings for this apparatus, and we were supposed to approve his drawings. That was all right, but we weren't even permitted to talk to him about his work! And we weren't allowed to discuss things with people in the machine shop, either. I was incensed. Sturdivant said something such as, "You guys aren't experts in machine work or in building things, and their time in the shop is too precious for you to go in there and talk." This made me very sad.

COHEN: Was the apparatus constructed?

SCHOMAKER: Eventually, but not without a lot of unnecessary headaches. If we had known that this apparatus was difficult to fabricate, we could have said, "Oh, it can be done another way, another way, another way." Some of our ideas would have helped them build a better machine than the one we were trying to copy. And we used gas, at a time when it was absolutely stupid to use gas in constructing a vacuum apparatus. But our hands were tied. There was this young fellow, the engineering student. He was a nice enough guy, but we couldn't discuss things intelligently with him; we could only sign the drawing. People have told me, "You should have refused to sign the drawing." I complain about it now, but at the time it never occurred to me to think that I had the authority that I was supposed to have.

COHEN: Was this machine ultimately used here?

SCHOMAKER: Oh, it was used here. We used it for a couple of years, did a few things with it. But it leaked! Leaked, leaked, leaked. Of course, that's what you get when you use gas. Using gas was obsolete by then, but we couldn't tell them it was stupid to use it. We couldn't *talk* to them!

COHEN: People who didn't know science were making the machine?

SCHOMAKER: We had a good shop and we had good, friendly relations with the guys working there. But Sturdivant made it clear: "You're not to take up their time talking with them about what they're doing for us." I think it was terrible.

COHEN: Were you a professor already at this time?

SCHOMAKER: Oh sure, yes.

COHEN: And this was another professor telling you this?

SCHOMAKER: Well, Sturdivant was in charge of these things. Besides, he was doing things very properly, by the book. This is what I'm talking about, an example of how things had changed around here. And I felt it was absolutely absurd that he didn't behave intelligently about how the machine was built. We certainly complained often enough. I can't say he never took our complaints seriously—I think he did—but he never supported our views, and I took it to heart.

COHEN: So that was an unhappy experience for you.

SCHOMAKER: Well, that was an incidental thing, but it was important to me. And more and more, in my view, nothing ever worked out as well as it should have.

COHEN: So that was it: You bade farewell to Caltech.

SCHOMAKER: I could have stayed here and continued to do well, but I sort of thought that I ought to do something else. I wanted to do other experiments. I wanted to do something very important. So one way or another, I ended up at Union Carbide.

COHEN: That was a good move?

SCHOMAKER: That was a catastrophe. Union Carbide is a good company, but on more than one occasion it dawned on me that I might have been better off no farther than a mile or two from the corner of Wilson and San Pasqual. But now there I was, working as the director of some darn project at Union Carbide. I didn't really have any competence or knowledge about this thing. I suppose I didn't have much interest in it at all. But that didn't seem to matter too much.

COHEN: What did you actually do at Union Carbide?

SCHOMAKER: I spent a lot of my time traveling around talking to people in various labs, and traveling around trying to recruit bright young graduate students, and traveling around trying to raise money for my project. And it was a major boondoggle, because if you asked for money you could get it.

COHEN: What was the project?

SCHOMAKER: I told you: a boondoggle.

COHEN: But you stayed with that work for the whole time you were at Union Carbide?

SCHOMAKER: I stayed with it, yes. I struggled with it, talked with the people at the lab. But it was a miserable, useless thing, just a waste of time and money. And it would have been difficult to accomplish much under the circumstances.

COHEN: Circumstances?

SCHOMAKER: A Caltech engineer was a consultant to Union Carbide. He visited our lab and said, "Working the way you do is as if you were working with both hands tied behind your back." Maybe he said only one hand, but both hands is what it amounted to.

COHEN: There must have been some positive things about working in industry. Didn't you ever have any fun?

SCHOMAKER: Sometimes. For example, we hired a guy to do X-ray crystallography, a student from the University of California named Richard P. Dodge. Dick and I did some work on a few crystal structures and had a good time with that. Dick Dodge was a smart kid, all right, but he was a perfect villain! He was interested in correspondence chess; he was interested in traveling around to look at eclipses. He was interested in spectator sports, football, baseball, and so on. He was interested in contract bridge. Yes, sir, young Richard P. Dodge was interested in lots and lots of things.

COHEN: Did he make any mark on the scientific world?

SCHOMAKER: As a teacher or administrator, he did. But no, he's never made any mark in scientific research. He could have, but he didn't. Oh, there was one thing, I suppose. I had one idea that I came to as a result of working with Dick Dodge on a crystal structure. I came to realize that the orientation of the molecule was not quite what it ought to be. So I wrote up some notes about it and sent them around to friends of mine whom I thought I could talk into working with me on it and write a program for examining it. I tried to get [Kenneth N.] Trueblood, here at Caltech [Dr. Trueblood, a 1947 Caltech PhD, was then at UCLA.—ed.], and he said, "All right." I was out here on a trip, when we were trying to sell the air force or somebody on some new project we were working on at Union Carbide. So I got half a day, or maybe a day to spend with Trueblood. We worked up our ideas and published a paper in 1968.[3]

COHEN: Well, some good did come out of Union Carbide, didn't it? I mean, in addition to young Richard P. Dodge.

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[3] Verner Schomaker & K. N. Trueblood, "On the Rigid-Body Motion of Molecules in Crystals," *Acta Cryst.*B 24, 63-76 (1968).

SCHOMAKER: It came out of there in spite of everything. Anyway, the most fabulous thing about Dick Dodge was this game he played with cronies scattered all over the United States. They were interested in college athletics, mainly track and field, which they pretended was like professional sports. And in their imaginations, they bought the performers from each other and kept track of how well these performers did in competition. So Dodge traveled some distance, all the way across the country sometimes, to follow his athletes.

COHEN: What became of this young man?

SCHOMAKER: When Richard left Union Carbide—he was encouraged to go, finally—he went to the College of the Pacific, or the University of the Pacific, in Stockton. For a while, he was associated with a research lab and did a little bit there. I understand he became chairman of the department there, but I never saw him after he left Union Carbide—except once. One time, when I was visiting and consulting at the [Union Carbide] lab in Westchester, Richard turned up and we had a conversation. He was carrying a tape recorder, and I asked what he was doing. He was interviewing pioneers of the radio business, the guys who played records on the radio. Richard was on his way to interview some former great disc jockey, a guy in a sanitarium in Westchester—a rest home, or a charity center—to get this fellow's precious words for the record.

COHEN: So you did consulting work for Union Carbide?

SCHOMAKER: Sometimes it seemed as if I did little else. Most of the time I was sent around to the various laboratories, because most of the time there was nothing else to do. I would say the silly things I had to say, and the guys in the labs would say the silly things they had to say, but I can't say that these discussions were particularly useful. But then I met a wild young Hungarian at the Linde laboratory. His name is Rabo, Jule [A.] Rabo, and he had wild ideas! Jule's ideas really were wild, but he was very inventive, very imaginative.

COHEN: A good man?

SCHOMAKER: An extraordinary man! Jule Rabo got to Linde in 1957. He was lucky enough, or bold enough, to flee the Hungarian Revolution. He had been going back and forth across the border, doing some work in Germany, I think. Jule had won the Order of Stalin or something, for something that he'd done, but that didn't do him much good when the tanks rolled into Hungary. He managed to escape and ended up in the United States with a number of patents he wanted to peddle. Nobody was interested in his patents, he told me, but a lot of people were interested in hiring him. So he decided to work at Linde, which is part of Union Carbide. When I met him he was working with zeolites. And as I said, Jule had lots of ideas, seemed to know everything, and was *very* creative. Sometimes he would pervert an argument a little bit away from dead solid convention, but always in an intelligent and imaginative way. So Jule was always a treat for me, and we'd talk for hours on end.

COHEN: An interesting fellow?

SCHOMAKER: And very successful. Jule Rabo was a key guy on cracking the catalyst based on molecular structure that was used by the oil companies—I'm talking about ninety-eight percent of the gasoline that was used in the world. Union Carbide didn't do all of this, because it was too big a thing, but they managed to hold onto the process and profited a good deal from it.

COHEN: This catalyst was Rabo's idea?

SCHOMAKER: Well, the single original source is hard to pin down. But it came from the line of work that Jule had drawn up and wanted to do. I suppose, from an industrial perspective, the idea was a product of the "expertise," to use that darn word, that existed in the Linde laboratory. But in Jule's hands, the idea was developed very fast. That's the way it happens sometimes, even in industry.

Begin Tape 3, Side 2

COHEN: Professor Schomaker, do I detect a hint of cynicism?

SCHOMAKER: Perhaps a touch of bemusement. I know it sounds as if I'm making fun of working in industry, particularly at Union Carbide, but there were a lot of fine young chemists in my lab. And as it turned out, about half of them were organic chemists, which was an extraordinary development. It's self-important to say that, but that was indeed the case.

COHEN: Why so many organic chemists?

SCHOMAKER: If Pauling had been in that environment, he would have explained the situation as an example of imitation. Pauling never made a big deal about the influence of his ideas, about how his ideas made such an impact on so many people, about how his ideas revolutionized the way so many people thought about chemistry. But Pauling wasn't afflicted with false modesty, and he was never blind to what was going on. A lot of bright young people were thinking and talking about organic chemistry, and Pauling was aware of this trend.

I think my group at Union Carbide was a prime example: Many of the things we worked with were not minerals, they were organic compounds—not crystals but volatile materials that we put into the electron-diffraction apparatus. Of course, this was quite a stretch for me. I'd had some wonderful discussions about organic chemistry with Saul Winstein when he was in our PhD class at Caltech, and we carried on some great arguments after he went back to UCLA. And then later I discussed organic chemistry with Jack Roberts to a certain extent. But I can almost honestly say that I'd never been an organic chemist in my life, and that I might not even recognize one in the real world. But there I was at Union Carbide, directing all these bright young organic chemists. And that was fine; it went on for some time.

COHEN: It sounds as if the Caltech style existed there, and that it wasn't so bad after all. So why did you leave?

SCHOMAKER: Well, it wasn't bad, but it wasn't perfect either. Then, too, I got the idea that they wanted to get rid of me. I had built a laboratory, and eventually it seemed pretty clear that they had a genuine opposition to it; in fact, they got rid of that laboratory. They finally managed to rent it, or maybe to sell it, to IBM for an office building.

COHEN: So that's why you left Union Carbide?

SCHOMAKER: Well, the prospect of losing my lab was a factor. But the big thing was the phone calls. I wasn't actively pursuing another job, but I kept getting phone calls. "Are you interested in coming here?" a caller would ask. "Are you interested in coming here?" another caller would ask. And since I hadn't put out any feelers for another job, it dawned on me that Union Carbide was trying to get rid of me.

COHEN: Were they?

SCHOMAKER: I don't know. I asked about these occasional queries. "Are you trying to get rid of me?" I said. "Oh, no," they said. "We're just trying to be helpful."

COHEN: Were they helpful?

SCHOMAKER: Perhaps the University of Washington thought so. Probably inspired by my bosses at Union Carbide, one day George called.

COHEN: George?

SCHOMAKER: George Halsey. George is a brilliant fellow, a physical chemist who had just received his degree when, in 1948, I was visiting for a few weeks with George Kistiakowsky at Harvard. One of the highlights of that visit was meeting George Halsey. I remembered him vividly, partly because he'd been a graduate student at Princeton, but mostly because he had many interesting stories. Now here he was many years later, calling to say that the University of Washington was seeking a modern physical chemist.

COHEN: And saying that you were the man for the job?

SCHOMAKER: Well, I suppose that I was one of the men or women for the job, but by that point in my life I was simply too old to pick up stakes, truck up there to Seattle, roll up my sleeves, and take my place in the physical chemistry lab. I'm not trying to put on airs, but I wasn't a kid anymore, and I couldn't go to the University of Washington or anywhere else as an ordinary faculty member. If I were going to accept a job, it had to be the job of chairman. Or something worse!

COHEN: You'd gotten yourself up into the upper echelons?

SCHOMAKER: Well, I don't know whether it was a matter of having gotten into the upper echelons or of having been degraded to the extent that you're not good for anything else.

COHEN: So George Halsey was your connection to Washington?

SCHOMAKER: Yes, but George wasn't the only link. There was also Paul C. Cross, who'd done a lot of good work in chemistry at the University of Washington. I never met him when he was here at Caltech—he'd been a postdoc with Pauling. But when Cross was chairman of chemistry at Washington, he had some connection with the Mellon Institute, which had a strong connection with Union Carbide, so he visited us occasionally. Anyhow, [in 1964] Paul Cross became president of the Mellon Institute in Pittsburgh, which left an opening at the University of Washington. So I went up there to Seattle. It was a day in March, and the temperature was 80°F. It was absolutely beautiful, the way it sometimes is.

COHEN: Had you been to Seattle before this?

SCHOMAKER: Once or twice. My cousin and I, on our trip around the country, visited Seattle in July of 1934. The weather was beautiful. It was very impressive. I'd never seen mountains before I started on this trip, or the ocean—well, we didn't see the ocean, we saw Puget Sound.

COHEN: Now you returned to Seattle, this time as a faculty member at the University of Washington. Did you enjoy that?

SCHOMAKER: I enjoyed seeing Puget Sound again. I didn't enjoy being chairman of the chemistry department.

COHEN: So you went immediately into that position?

SCHOMAKER: Yes. And I didn't know what I was getting into.

COHEN: How big was the faculty of the chemistry department?

SCHOMAKER: About twenty members, something like that.

COHEN: That's a good-sized department.

SCHOMAKER: Well, there were 4,000 or 5,000 freshman chemistry registrations a year and, I think, lots and lots of students. And at the time they were not swimming in research support. On the other hand, there were a lot more fellowships then than there are now—national fellowships—and so there were about 200 graduate students. Compared to Caltech, that was really something. When I was a student here, it seemed as if we had a lot of graduate students, when in fact there were only fifteen or twenty. In later years here, we still had only about forty or fifty graduate students, something like that. But at Washington, there were 200 of them, and life in a department like that is bitter and hard, compared to the rich mental life at Caltech.

COHEN: Did you continue with your research at Washington?

SCHOMAKER: Well, I didn't have any research to continue. When I was at Union Carbide, I had a lot of conversation with Jule Rabo, but none of that came to anything. My primary duty was serving as chairman of the chemistry department.

COHEN: Was it a rotating job?

SCHOMAKER: No, I was chairman for five years. I don't know how long Paul Cross had been chairman, but it hadn't been a rotating job in his day. It's a rotating position these days, however. The rotation thing started after I was kicked out of the job as chairman of the chemistry department.

COHEN: Kicked out? What do you mean by that?

SCHOMAKER: The dean said, well, he didn't know whether he'd want to ask me to continue. I didn't urge him. I don't know what he would have done if I had tried to stay on as chairman.

COHEN: So then you were just a professor in the department?

SCHOMAKER: Yes. And I now tried to do some research. I had a couple of students there, good people, but never got a thing going with them. So mostly I taught—much to my regret.

COHEN: Teaching there could have been a better experience?

SCHOMAKER: Oh, well, whereas I had incredibly little teaching experience here at Caltech, I had quite a lot of it there. I enjoyed teaching when I was chairman, because I assigned myself to small classes and taught what I was interested in. But after I was no longer chairman, I was assigned to teaching these big classes. And I tried and tried, but except for the occasional student—one guy here, another guy there—I couldn't get a good student response.

COHEN: Unlike at Caltech.

SCHOMAKER: Chemistry was pretty easy for Caltech students. The best situations were the ones when Pauling was giving some inspiring lecture on some topic, and the students could read the textbooks, learn a lot of the chemistry on their own. On the other hand, the situation at the University of Washington was so dismal. The students come from high school pretending to have learned something but not having learned anything. There were some excellent students; some of them were as good as any I'd ever encountered. But most of them weren't very good students—most of the nursing students, for example. Typically, they were very dutiful about pretending that they were devoted to learning chemistry. But that's what most of them were doing—pretending. I don't think many of them were in the least bit devoted to learning chemistry. Then, too, the department chose a textbook that from my point of view was mostly a lousy textbook. To make things worse, because chemistry is difficult, the instructors have to spend time reading the book to the students, or do something to that effect. And again, there are so many students at Washington. Some people enjoy teaching those large classes and are successful at it, but I wasn't one of those people. And I wasn't fond of all the administrative work that I had to handle, all the student files. I didn't like that at all.

VERNER SCHOMAKER

SESSION 4

February 10, 1993

Begin Tape 4, Side 1

COHEN: But you enjoyed living in Seattle, whether or not you enjoyed the students at the University of Washington.

SCHOMAKER: I think we should keep things in perspective. I did enjoy some of the students but not the majority. And eventually the thing I enjoyed most about the University of Washington was my sabbatical to Caltech.

COHEN: What year was that?

SCHOMAKER: It was 1977-78. It was great being back in Pasadena, particularly since Dick [Richard E.] Marsh [senior research associate, emeritus] and I came across something that we both thought was very important. We did some work and wrote a paper about it.[4]

COHEN: What was that?

SCHOMAKER: It had to do with interpreting crystal structures. I don't know whether the theory of the crystal structure was properly understood, but one thing seemed certain: There are many people out there in the world who do crystal structures and do them incorrectly. They push buttons on the modern machines, and if the program on the machine doesn't happen to get the right answer, they don't bother to notice. Every now and then, one of these structures is really interesting, because it's a drawing of an important chemical. But often enough—too often—it's not done right; one time in ten, something like that, the guy did the thing carelessly. Depending on what the guy's doing, often the mistake doesn't make too much difference, but the work

[4] Verner Schomaker & Richard E. Marsh, "The crystal structure of bis(tetraethylammonium) tetrachlorodioxouranate(VI): correction from *P*1 to $P2_1/n$," *Acta Cryst.* B35, 1094-99 (1979).

ought to be right on the record. So I joined Dick in setting things straight, and it's still going on to some extent. Dick Marsh has published hundreds of corrections, and continues to do so.

COHEN: How does it work?

SCHOMAKER: Dick reads the crystal structure in relation to the tables, and the relationships tell him that the fellow had got them wrong. Dick just sort of perceives the differences.

COHEN: This is that intuitive way of looking at things?

SCHOMAKER: It's intuitive. It's a special thing. Dick has the ability for it, and he's cultivated it so that he just senses what other people probably don't understand. It seems to me that Dick looks at these things as if they were a crossword puzzle, only in reverse. Usually people read the clues and fill in the word, the answer. But Dick sees the answer right away, and then spends hours trying to figure out what the clues were supposed to mean.

COHEN: Only at Caltech.

SCHOMAKER: Well, I've enjoyed it. And when I was at Washington, and even before that, at Union Carbide, I enjoyed returning several times over the years. I managed to keep up with my old friends, and I got to participate in some important Caltech events, particularly the opening of the Noyes Laboratory [1967]. Then there was a memorial meeting [1975] for Holmes Sturdivant [a few years] after he died, and that occasion was an opportunity to keep some connections.

COHEN: That year you were here on sabbatical, did things seem much different from when you'd been on the faculty?

SCHOMAKER: Oh, it certainly was a lot different, yes. For one thing, I don't think Dick Marsh still had graduate students. Crystallography was no longer considered a respectable subject for PhD study in chemistry.

COHEN: As a result of new machines and new techniques?

SCHOMAKER: I don't know what it was. It’s a problem of hubris on the part of the chemists, I think. Not everybody thought the crystallographers were that important. Of course, without their help, there would be many more red faces than there are.

COHEN: So where was crystallography being done?

SCHOMAKER: Everywhere. But—and the situation was similar in other places—the support for crystallography was drying up. The people who wanted to practice that art found that they couldn’t get professorial jobs in the universities—not unless they pretended to focus on things other than crystal structure. On the other hand, there were big prizes to be had if people worked on protein structures, things like that.

COHEN: Protein structures?

SCHOMAKER: Determining the structures of protein. There is a practitioner in the field here now, [professor of chemistry] Doug [Douglas C.] Rees, a very engaging fellow who works over in the Braun Laboratories. Doug was a student of Bill Lipscomb, who was a student of Pauling and Ed Hughes. Doug was my student, too.

COHEN: So protein structures is on the cutting edge?

SCHOMAKER: In some ways, but the research has been going on for years. In fact, as far back as 1935 or ’36, Pauling was already trying to imagine what the structures of protein are. He got into a big controversy with a woman—I never met her—named Dorothy Wrinch. Apparently Dorothy Wrinch was a very able person. She even sold Irving Langmuir on her idea, and he was a very remarkable fellow, all right—Joe [Robert V.] Langmuir’s uncle. But Dorothy Wrinch’s idea was a crazy chemical idea about how amino acids are put together in the protein molecule. Still, even though Dorothy Wrinch was wrong, Pauling saw something important in her work: her imaginative speculations. And so Pauling and Corey and I spent years trying to guess the structures of proteins and suchlike in different ways. On the other hand, I think Pauling knew what he wanted all along, as usual.

COHEN: He knew what direction things were going to go in.

SCHOMAKER: That was one of the directions. Of course, another big side of it was the double helix—the DNA molecular structures and so forth. And, darn it, Pauling ought to have got that, too.

COHEN: Pauling not discovering the double helix before Watson and Crick—that was a controversy in itself, wasn't it?

SCHOMAKER: Well, there was some public discussion, but not too much of it was very useful. For example, somewhere or another Peter Pauling, Linus Pauling's son, appeared on a television program. And Peter blurted out, the way he does, that his father made a mistake in the DNA search by thinking that DNA was another chemical, that this was another routine problem.

COHEN: Was that true?

SCHOMAKER: I'm not sure. But I have to admit that in all those years I don't think that Pauling ever said "Boo" about his trying to get some guesses or hints out of the likely genetics or biological reasons for the importance of DNA. He was using his knowledge about his diffraction experiment and building his own model. So I think Peter was probably right in saying his father saw DNA only in chemical terms, although I don't know exactly why he said it on TV.

COHEN: Did Pauling get around to seeing his DNA model in biological terms?

SCHOMAKER: To a certain extent, yes. Gary Felsenfeld told me an interesting story about that, and about something I'd said and utterly forgotten. One time I attended a seminar Pauling gave on his DNA model to a group of biologists. Well, Pauling described this model in which the phosphate groups—things going down the center and the famous bases on the outside—were just exactly inverted. I don't know why Pauling said that. Later, I thought he was trying to interest me in something important, or maybe he said it out of enthusiasm, I'm not sure. At any rate I just couldn't believe Pauling's model. If only for structural reasons, it didn't make any sense to me. So I raised my hand and said, "If you had that structure, it would blow up!"

COHEN: No!

SCHOMAKER: Felsenfeld says it happened.

COHEN: Well, then I bet you said it.

SCHOMAKER: Maybe I did. In any case, that was my chance to make a contribution, by arguing the point successfully instead of ineffectually.

COHEN: Why do you think Pauling didn't go ahead with the structure that really won out.

SCHOMAKER: That's hard to understand. Peter Pauling suggested that his father just didn't try hard enough. There might have been something to that notion. Then, too, Linus was always interested in so many ideas. The DNA structure was only one of the ideas that interested Linus Pauling.

COHEN: Was there much talk in those days among the biologists and the chemists, as there had been in your early days among chemists and physicists?

SCHOMAKER: Oh, there was a great deal of discussion, I'm sure. Biology was always, in my memory, a big division at Caltech. I suspect that biology might have been as big a division as chemistry. And Pauling was enormously interested in pushing and helping in biology. For example, he played a major role in getting George Beadle to come back to Caltech.

COHEN: He'd been here before he came as chairman of biology?

SCHOMAKER: Beadle was a postdoc or something or other here when I first came in 1935. I didn't get to know him, but he was one of the people in the Athenaeum group.

COHEN: Living with you young fellows in the dormitory?

SCHOMAKER: No, no. He must have been married at the time. I suppose he was, and I'm not sure of the postdoc position. [Beadle was a National Research Council Fellow at Caltech in the early thirties—ed.] But when Beadle left Caltech, he went to Harvard and then to Stanford, and he made a big, big contribution. So naturally a lot of people around here thought he belonged in the biology division. And it seems Pauling was one of those persons. I'm not sure of the details, but apparently Norm [Norman H.] Horowitz [professor of biology, d. 2005] wrote an article about the circumstances surrounding Beadle's return. Well, Pauling took exception to Horowitz's article and wrote him a letter complaining that he didn't have all the facts quite right, that Norm had overlooked Pauling's role.

COHEN: So Pauling, the chemist, was a major player in the return of Beadle, the biologist?

SCHOMAKER: Well, Beadle did come back [1946]. And according to Norm, Pauling must have made an important contribution—arranging for Beadle to come back here and help the biology division. Norm told me that he was just astounded that after so many years Pauling had picked up everything in the article and remembered what the situation had been. But that was Pauling.

COHEN: So he was really interested in biology?

SCHOMAKER: Genetics, too. Somehow or another, Pauling had found the time to study up on genetics before I came to Caltech. I don't know when it was—maybe 1925; I don't know when in the dickens it was—but Pauling gave a seminar on genetics. He clearly had an interest in genetics very early and had written up some ideas about crossing over, a paper about the chemical and biological processes of genetics. So in hindsight, this is interesting—another piece of the puzzle. Pauling certainly had an interest in such things, biology and genetics, things that would have been enough to help guide him along the other direction when he worked on DNA.

COHEN: So then, clearly, the biological connection was there?

SCHOMAKER: It was there, all along. For example, there was a great deal of interest in [A. L.] Hodgkin and [A. F.] Huxley and nerve induction. Pretty soon after their famous paper[5] appeared in 1950 or so, there was an informal evening seminar that, as I recall, Norman Davidson had

[5] A. L. Hodgkin and A. F. Huxley, "A Quantitative Description of Membrane Current and Its Application to Conduction and Excitation in Nerve," *J. Physiol*. 117, 500-44 (1952).

something to do with arranging. Anyway, after that seminar Norman went to Pauling and asked his opinions on the Hodgkin-Huxley contribution, and Pauling asked me what I thought. I was proud that he'd asked me, but I wasn't quite sure what I thought. So Pauling said, "Why don't you get interested in this problem? What is it that the carotenoids do in the photosynthetic arrangements? Why are they there?" So Pauling had an interest in these problems and wanted me to share it. At the same time, Frits Went [professor of plant physiology 1933-1958] was interested in photosynthesis. I believe it is understood now, but it was seen as an important problem in those days, and Pauling was interested when Went came to ask him about photosynthesis.

COHEN: So there was an ongoing intellectual link between biology and chemistry?

SCHOMAKER: An architectural link as well. In those days, you could walk straight from Kerckhoff [William G. Kerckhoff Laboratories of the Biological Sciences] to Crellin. The doors in one corner of one laboratory opened directly into the doors of the other one, from the corner of one lab to the corner of the next one. Maybe that direct passage doesn't still exist, but I used to be able to walk straight through.

COHEN: An open-door policy, right? That's one of the great strengths of Caltech, scholars having access to each other.

SCHOMAKER: I think so. We had lots of connections to biology.

COHEN: And lots of Caltech connections in general. Was that why, when you retired from the University of Washington [1984], you decided to make annual visits back to Pasadena?

SCHOMAKER: Well, my friends and colleagues were important factors. Also, we have most of our family here. And my wife and I aren't fond of Seattle in the winter.

COHEN: Too much rain?

SCHOMAKER: The rain doesn't matter too much. I'm bothered by all that damn darkness up there. I don't understand how civilization profited so much living at latitudes as far north as Seattle, or farther north.

COHEN: But you've remained active in the Northwest?

SCHOMAKER: When I first got to Seattle, I went to a local section meeting of the American Chemical Society and was asked to become active. I said OK. And that mistake caused me to be elected vice president and president and so on. But there were some good aspects. For example, shortly after I got there in 1965, I was asked to play a part in setting up the Pauling award—the Oregon and Puget Sound Section. My task was to get Pauling to agree to have his name attached to this and to be the first recipient of the award.

COHEN: I gather he was agreeable.

SCHOMAKER: He was agreeable, yes. I had the good fortune of being able to attend the first award ceremonies, and they're an ongoing thing. So are the meetings. They alternate or rotate between the Puget Sound Section, which reaches from Olympia, I think, all the way into Canada—maybe on up to Alaska. It's an enormous geographical area. The award ceremonies for the Puget Sound Section and Oregon Section have been held mainly in Seattle, although last fall it was in Vancouver. In Oregon, the ceremony is usually in Corvallis or Eugene—except for the first year [1966]; it was held in Portland that first year, in view of the fact that Pauling had been born there.

COHEN: Do they get Pauling to take part in these meetings?

SCHOMAKER: He's been faithful about coming for a long time. And I've attended most of them.

COHEN: How about the Pauling Lectures at Caltech?

SCHOMAKER: That series was started very recently [1989]; I think there have been only three lectures so far. But I managed to attend the one last year, when [Yuan T.] Lee talked. Lee got his PhD at the University of California at Berkeley, but he was a postdoc with Dudley

Herschbach, and they shared the Nobel Prize [in chemistry in 1986] with someone else [John C. Polanyi]. The third guy's name slips my mind at the moment.

COHEN: Well, what about your own honors? Surely you haven't forgotten them.

SCHOMAKER: My honors?

COHEN: Oh, come on!

SCHOMAKER: Well, Pauling talked me into applying for a Guggenheim Fellowship, and that was a wonderful if not very productive time. No, I guess it was somewhat productive; I saw the light about the phase-shift problem in diffraction. But that didn't happen due to my efforts alone. Pauling pushed hard for it. All I have, I owe to Linus Pauling.

COHEN: I suppose a lot of people here feel that way, yet Pauling left Caltech. Do you recall the circumstances?

SCHOMAKER: When I was on a recruiting trip for Union Carbide, I went to see Pauling in his second-floor office in Church [Norman W. Church Laboratory for Chemical Biology]. He talked about the Nobel Peace Prize, about changes in the chemistry division, about democratic institutions, about a number of important things. And that was the only time I ever heard Pauling really express unhappiness about the fact that he'd found himself on the other side from the trustees, and perhaps at odds with the faculty and the administration as well. He told me that he was going to Santa Barbara. [To the Center for the Study of Democratic Institutions—ed.].

COHEN: You're saying he was unhappy about going there?

SCHOMAKER: Well, unhappy about feeling that he *had* to go. I'm sure there was some disrespect in the chemistry division, but I don't know how real or extensive it was. This was a complicated situation. For example, I think that Pauling was under some pressure at the time to give up some of his laboratory space, but he wanted to keep that space for his people who were pursuing one thing or another, some important problems in chemistry. I know that Jack Roberts was required by the division to go see Pauling in an effort to rescue some space. It couldn't have been a happy time, particularly since Pauling was really extremely enthusiastic about his work on immunology. As always, Pauling was interested in a lot of problems, and as is the case with great scientists, he was often on the right track—but not every time. For example, he once worked out a notion for how he would be able to make artificial antibodies. He had a little series of test tubes of immunological [preparations in which] the reaction had been carried out. The tests were very promising, and he talked about this a couple of times here at Caltech and at the American Chemical Society meetings. But it turned out that antibodies were not made that way at all. Pauling did find out a lot about the antibodies, but his whole rationale was not nearly so complicated and wonderful and outlandish as what actually happens.

COHEN: So this was not one of his successes?

SCHOMAKER: It was not one of his successes. There's a remarkable thing about great people—some of them, at any rate—like Pauling. They don't want to retract something that might be wrong unless they really *know* that it's wrong. I had some correspondence about this with Joshua Lederberg, whom I've never met, but he's a famous guy, the president of Rockefeller University [1978-1990]. Somehow or another, Lederberg got wind of Pauling's stubbornness about his views on how antibodies are made, and he wrote me a letter asking whether I could explain it. I tried to answer, but only Pauling can speak for Pauling. Lederberg said he just couldn't understand why Linus would never give up on this! Who knows? To the best of my knowledge, Pauling never has seen fit to say, "I was wrong about how antibodies are made."

Begin Tape 4, Side 2

COHEN: You think that comes from being right so often?

SCHOMAKER: I do. It's also a matter of pride, I suppose, or ego. You can work on a problem for years, put everything you have in it, and then to be proved wrong can be, personally, extremely uncomfortable or disappointing or devastating. I think I've had one little touch of it, and in this case we were right even though we suffered as if we were wrong.

COHEN: I think that will take a little explaining.

SCHOMAKER: Well, it's a little complicated but I'll try. Back in the early days, I met a fellow named Jürg Waser, and he was very important to me. Jürg came here from Zürich [1939] to be some kind of an exchange student. Anyway, Pauling urged Jürg and me to investigate the chemistry of arsenomethane.

COHEN: Arsenomethane?

SCHOMAKER: It's arsenic double-bonded to arsenic with two methanes, which is a dimer. Now, azomethane is also a dimer, so azomethane and arsenomethane were reputed to have the same structure, but they didn't; they were arranged differently. So Pauling suggested that Jürg study this arsenomethane stuff. Jürg made vapor-density determinations before he made some studies of freezing point and solution. And by and large it looked as if this stuff was a pentamer—that is, five molecules, not two. Jürg and I studied the structure by electron diffraction of gas molecules, and we discovered some interesting evidence that arsenomethane is a pentamer, all right. So we wrote a paper about it.[6]

I don't remember whether Jürg ever published his chemical investigation data, the kind of report that just repeats what people had reported earlier. But our study of the structure seemed to prove that the methane did not have arsenic-arsenic double bonds. I don't know who first observed that double bonds are uncommon, but Pauling, based on the first few elements of the periodic table, knew they should be.

Some time passed. I worked on the Guggenheim. Jürg got his PhD [1944], went to Switzerland for a year on a fellowship [1948], then [eventually] returned here to work with Eddie Hughes, and something very interesting came out of that. Jürg and Eddie worked on crystal structure of azobenzene, which is not a dimer like arsenobenzene with two arsenics—it's got six. So that proved to me that, even though Jürg Waser and I thought we were right about the electron diffraction, we really *weren't*! I concluded that instead of being a pentamer, arsenomethane is a hexamer. So I wrote Jürg and said that we ought to publish a retraction, that our only option was to admit that we were wrong. Well, he knew that we should do that, but he wouldn't do it. He simply would not admit we'd made a mistake. Instead, Jürg got crystals of the stuff, and he and a student worked out the crystal structure. And you know what? It *was* the

[6] Jurg Waser & Verner Schomaker, "On Arsenomethane," *J. A. Chem. Soc.*, 67:11, 2014-18 (1945).

pentamer. As it turned out, we'd been right all along; it was the pentamer just the way we reported it in the gas phase.

So you see? Making a mistake and not owning up to it isn't the only bad thing we can do in science. In fact, it may be worse to do something right but not have faith in it.

COHEN: But how do you know the difference?

SCHOMAKER: Well, if you're Linus Pauling, almost all the time you know the difference very well!

COHEN: So you're excusing him for not retracting anything.

SCHOMAKER: I think there must be a very strong reason to retract something that you believe to be true. I've never talked with Pauling about it, but it's impressed me deeply that no matter how controversial, he doesn't want to be wrong about things. But I *have* talked to Pauling about controversial matters, such as the big rivalry he was supposed to have with Eyring.

COHEN: Eyring?

SCHOMAKER: Henry Eyring. He had an interesting background. Eyring was born down in Chihuahua, Mexico, and his family moved to Arizona. He was about to be a geologist, or geological mining engineer. Apparently he was down in a shaft one day, and rocks came crashing down! He said that little incident convinced him he didn't want to be a miner anymore. So he got his PhD in chemistry—at the University of California at Berkeley, I think—and went on from there to Princeton. Later he went to Salt Lake City [to the University of Utah—ed.] , presumably because he was a Mormon. I can't remember all the details, but Eyring had an inspiring career. A very impressive guy.

COHEN: But this rivalry business—I gather Pauling was somewhat less than impressed.

SCHOMAKER: Let me put it this way: One day we were discussing Eyring, and I swear that Pauling made the following statement: "The difference between Henry and me is that he has lots of wonderful ideas, but he can't make solid arguments for them. Henry comes up with a fine idea and tries to work it out theoretically but finds that's impossible to do. It's too complicated. So he makes an assumption, chooses an element to focus on. Now, Henry tries to work that out theoretically, but that part of the idea is also too complicated. So now he has to make another assumption. Pretty soon he has a long chain of five or six mathematical or physical assumptions, but there's no longer any real connection between the original fine idea and what he's claimed as being a physical argument at the end. Whereas when I look at such a situation, I think about it and, making all the adjustments, I form it in such a way that I *can* make an argument that is rigorous and supportable."

COHEN: Pauling said that?

SCHOMAKER: Oh, perhaps not quote-unquote, but I swear that's what he told me. Nonetheless, a few years ago I confronted Linus Pauling about this, and he said something such as, "No, I don't think I ever said anything of the kind." But he had compared himself to Eyring just like that; that's what I remember, and I don't think I'm distorting it.

COHEN: Why do you think Pauling said it?

SCHOMAKER: I don't know why he would say it. Certainly Eyring was a respected chemist. A statement like that didn't make a lot of sense, and it wasn't characteristic of Pauling to make it. Perhaps he was being a little playful—he wasn't speaking in public—and of course we'd been friends for many, many years. At the same time, I think there was something essentially honest about what Pauling said. I think he was honestly expressing a difference between Eyring and himself—and remember, we're talking about differences, about making or not making mistakes, and about how great men handle their careers.

COHEN: And Pauling and Eyring were different.

SCHOMAKER: Here's an important difference. Both men had many ideas, but unlike Pauling, Eyring had many students. And those students wrote numerous papers that got published with Eyring's name on them. Now, I'm not suggesting anything wrong with that; universities encourage productivity, and that's how academic and scientific careers are made. But it became pretty clear that Eyring's actual contribution to many of these papers was minimal. Why? Because a lot of these papers turned out to be utter nonsense.

COHEN: Utter nonsense?

SCHOMAKER: I'm afraid so. A paper came out under the official authorship of Bing and Eyring, and because that paper was such utter nonsense it seemed fairly clear that Eyring hadn't been all that involved. If Eyring had paid more attention, he never would have allowed publication of that paper. Pauling's stance on these matters was always very, very different. He would not put his name on a paper unless he had been a substantial contributor to the work. A *substantial* contributor—not just the guy who got the money or who assigned the space for the experiment. If Linus Pauling didn't make a substantial contribution, he wouldn't take credit even when the idea for the paper had been Pauling's in the first place. And in my case, fortunately, it often *was* his idea.