CHEMICAL HERITAGE FOUNDATION

LINUS PAULING

Transcript of an Interview
Conducted by

Jeffrey L. Sturchio

in

Denver, Colorado

on

6 April 1987

(With Subsequent Additions and Corrections)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Date) 5 April 1993

(Revised 20 February 1989)

Linus Pauling, interview by Jeffrey L. Sturchio at Executive Tower Inn, Denver, Colorado, 6 April 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0067).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

LINUS PAULING

1901	Born in Portland, Oregon on 28 February

Education

1922	B.S., chemical engineering, Oregon State College
1925	Ph.D., physical chemistry and mathematical physics, summa cum laude, California Institute of Technology

Professional Experience

1925-1926	National Research Council Fellow
1926-1927	Guggenheim Fellow, Universities of Münich, Zürich, and Copenhagen
	California Institute of Technology
1922-1925	Teaching Fellow
1923-1927	Research Associate
1927-1929	Assistant Professor
1929-1931	Associate Professor
1931-1964	Professor
1936-1958	Chairman, Division of Chemistry and Chemical Engineering
1936-1958	Director, Gates and Crellin Chemical Laboratories
1945-1948	Member, Executive Committee, Board of Trustees
1963-1967	Research Professor, Center for Study of Democratic Institutions
1967-1969	Professor of Chemistry, University of California, San Diego
	Stanford University
1969-1974	Professor of Chemistry
1974-	Professor Emeritus
	Linus Pauling Institute of Science and Medicine
1973-1975	President
1978-1979	President
1973-	Research professor

Honors

Among the numerous awards in chemistry are:

1931	Langmuir Prize, American Chemical Society
1941	Nichols Medal, New York Section, American Chemical Society
1947	Davy Medal, Royal Society
1948	United States Presidential Medal for Merit
1952	Pasteur Medal, Biochemical Society of France
1954	Nobel Prize, Chemistry
1955	Addis Medal, National Nephrosis Foundation

1955	Phillips Memorial Award, American College of Physicians
1956	Avogadro Medal, Italian Academy of Science
1957	Paul Sabatier Medal
1957	Pierre Fermat Medal in Mathematics
1957	International Grotius Medal
1963	Nobel Peace Prize
1965	Order of Merit, Republic of Italy
1965	Medal, Academy of the Rumanian People's Republic
1966	Linus Pauling Medal
1966	Silver Medal, Institute of France
1966	Supreme Peace Sponsor, World Fellowship of Religion
1972	United States National Medal of Science
1972	International Lenin Peace Prize
1978	Lomonosov Medal, USSR Academy of Science
1979	Medal for Chemical Sciences, National Academy of Science
1984	Priestley Medal, American Chemical Society
1984	Award for Chemistry, Arthur M. Sackler Foundation
1987	Award in Chemical Education, American Chemical Society
1989	Vannevar Bush Award, National Science Board
1990	Richard C. Tolman Medal, Southern California Section, American Chemical Society

ABSTRACT

Linus Pauling begins this interview by describing his early interest in science. While growing up in Portland, Oregon, he collected laboratory equipment and carried out chemistry experiments in his home. He also worked in the chemistry laboratory of his high school. Pauling supported himself through his undergraduate years at Oregon State Agricultural College by working in the chemistry department stockroom and assisting an engineering professor. During graduate school at Caltech, he learned x-ray crystallography from Roscoe Dickinson and published his first paper. Pauling continued to use crystallography to attack more complex chemical problems. In 1926, Pauling was awarded a Guggenheim Fellowships to study in Europe. In Zürich, he carried out research on the interaction of two helium atoms which later led him to develop the theory of the three-electron bond. Pauling concludes this interview with his return to Caltech as assistant professor of chemistry.

INTERVIEWER

Jeffrey L. Sturchio received an A.B. in history from Princeton University and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He was Associate Director of the Beckman Center for the History of Chemistry from 1984 to 1988, and has held teaching appointments at the New Jersey Institute of Technology, Rutgers University, and the University of Pennsylvania as well as a fellowship at the Smithsonian Institution's National Museum of American History. After a sojourn on the senior staff of the AT&T Archives, Dr. Sturchio joined Merck & Co., Inc. as Corporate Archivist in June 1989. He is currently Director, Science & Technology Policy, in the Public Affairs Department at Merck.

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INTERVIEWER: Jeffrey L. Sturchio

LOCATION: Executive Tower Inn, Denver, Colorado

DATE: 6 April 1987

STURCHIO: You have written a number of articles on your early career and there certainly have been plenty of articles about you, so from that we know some of the details of your early interest in science (1). I know you were born in Portland, Oregon on 28 February 1901, and that you went to Oregon State Agricultural College, but I wonder if you would talk a little more about the origins of your interest in science. Why did you decide to study science at that age?

PAULING: Well, my father was a druggist, and I used to hang around the store. He died when I was nine years old, so he can't have had a very great effect on me. When I was eleven, I started collecting insects. So far as I can remember this was spontaneous, in that there was no one who suggested to me that I do it. I think I ran across a book about insects which interested me. And there were many insects that one could find in the Willamette Valley. So I made a considerable collection. I think it was left in the basement of the house when my mother died and I was in Europe when my two sisters sold the house. The next year I became interested in rocks and minerals, mainly minerals. I got books on mineralogy from the library and copied tables out of the books before returning them. About the only minerals I could collect were agates. I did not know enough to find zeolites and didn't have instruments to permit me to identify small crystals. So it was mainly reading about minerals and thinking about them that engrossed my attention for a year. That was when I was twelve, my first year in high school.

Then when I was thirteen, my second year, Lloyd Alexander Jeffress, my age, and I, were walking home from Washington High School, when he asked if I would like to come to his room and see some chemical experiments. So we went up to the second floor of his house which was at about 28th street. My house was on 40th street, so this was about half way. And he carried out some experiments which I found extremely interesting. I think I hadn't thought about the fact that substances can be converted into other substances. Combustion, the burning of wood in the stove, I just accepted as part of the world. I hadn't thought about the general phenomenon of conversion of one substance to another substance. (The science writer in the _London Times_ has a rather vague idea about this also. Last week in London I read about a new substance that had surprising magnetic properties and the science writer described it by saying it was called Bipo, and it was made by taking a yellow substance which exploded to form a black powder. That was it. That was all the chemical information that was given.) So, I began carrying out experiments starting with a chemistry book that my father had.

STURCHIO: Do you know what book that was?

PAULING: I think Williams, but it got lost, left I guess when my mother died. I am not sure about it. I can remember the book but not the title page.

STURCHIO: Were you using your friend's equipment, or did you start to get your own equipment at that time?

PAULING: I started getting my own equipment, such as flasks and beakers. There was a druggist who had been a friend of my father's in Portland who gave me chemicals. When I was eleven he gave me some potassium cyanide and some plaster of Paris. I had read about making a killing bottle. He gave me perhaps thirty grams of potassium cyanide. I went home and got a mason jar and went out on the back porch because I knew it was poisonous. I put the potassium cyanide in the jar, and put some water on the plaster of Paris, and put that in on top of it and made my killing bottle. So he gave me chemicals and some pieces of apparatus. A man living next door had been a photographer and a guide on Mount Hood and was retired. He was working as the stockroom keeper at North Pacific Dental College. He brought home many pieces of glassware for me. They had been chipped so that they would have been discarded, and he brought them for me.

My grandfather was a night watchman in the foundry in Oswego. I would go the seven miles to Oswego on the train. It was usually every week, going Saturday to see my grandparents and coming back Sunday. I could go down with him or without him. Actually, I went to the smelter, which was abandoned, and was about a quarter mile away from the foundry. The smelter was a wooden structure that was falling down. The roof of the laboratory had fallen in. There were hundreds of bottles of ore samples and bottles of chemicals. So I would take a suitcase and fill the suitcase with bottles of chemicals, get on the train, take them back to Portland, get on the street car, and go up two miles to my home. For example, there was a two and half liter bottle of sulfuric acid and similar bottles of nitric acid and hydrochloric acid. The sulfuric acid was black. I suppose it had been opened and a little organic material had fallen in and been dehydrated. There was a big bottle of potassium permanganate. It was really great to have these chemicals.

STURCHIO: Were other friends of yours interested in science at that time?

PAULING: Well, I saw a good bit of Lloyd Jeffress, but we didn't do any more chemical experiments together. We were together at his country home which was close by, or at my grandmother's house. He became professor of psychology at the University of Texas and died a few years ago. He was a psychologist all right. When we were about fifteen, and he was with me at my grandmother's home, my grandmother said, "Linus, what are you going to be when you grow up?" I said, "I am going to be a chemical engineer." And Lloyd said, "No, he isn't. He is going to be a professor." I had no expectation of being a professor. I thought I would be a chemical engineer.

STURCHIO: You mention in one of your articles that at that time you thought that a chemical engineer was the profession somebody interested in chemistry followed (1d). Why was that?

PAULING: I didn't have much knowledge about professions. I did know about engineering and chemical engineering, because I had a cousin, Mervyn Stephenson, who was studying highway engineering at Oregon Agricultural College, and I knew that they taught chemical engineering. So I thought that was the profession.

STURCHIO: Did you have brothers and sisters?

PAULING: Two sisters. One is a year and a half younger, the other three years younger than I. They're both alive. [The younger one died in 1992.]

STURCHIO: Were they interested in science at all?

PAULING: No. The elder later in life married Paul Emmett. So I think she had much more contact with Paul Emmett than she had with me. After I went to college at age sixteen I didn't see my sisters very much.

STURCHIO: Basically she had a secondary interest in science. So the fact that your cousin Mervyn Stephenson was at Oregon Agricultural College studying engineering is probably an important factor that led you in that direction.

PAULING: Yes. Well, we didn't have any money. Oregon

Agricultural College was the only place I could have gone, I think. I drove on my bicycle two miles across the east side of Portland to Reed College a few months before I went to Corvallis. I knew about Reed College and was sort of interested in it. I drove around the campus, but that was all. I didn't go in, and I didn't see anybody or speak to anybody. But I probably couldn't have gone to Reed because of the tuition, even though I could have lived at home then. But I just didn't know that at the University of Oregon I could have majored in chemistry, or that I could have majored in chemistry at Reed College. But it wasn't too bad to go to Oregon Agricultural College.

The main way in which I suffered scientifically, I suppose, is that for four years I had no courses in mathematics. I took all the mathematics in my freshman year. I had had four years of mathematics in high school. So I could take sophomore mathematics in my freshman year and then they didn't teach any more math courses. And I taught full time for one year, which is why I say for four years. When I got to CIT [California Institute of Technology] of course I signed up for many mathematics courses. For a while I thought, because of faulty memory, that I had a minor in mathematics as well as a minor in physics at CIT. But in fact, I found my diploma which says physical chemistry and mathematical physics. They made it mathematical physics.

STURCHIO: I wanted to ask you a little more about the atmosphere you found when you got to Caltech in 1922. But first, do you recall the textbook you used when you taught quantitative analysis back in Oregon about 1919?

PAULING: It was written by George M. Smith in the Midwest who then became a professor at the University of Washington in Seattle (2). I wrote a letter to him complaining about a statement that I said I thought was not true or misleading. He wrote back.

STURCHIO: Was he an analytical chemist?

PAULING: That was quantitative analysis.

STURCHIO: By the time you got to college in Oregon you already had a fairly good background in traditional methods of chemistry, just from your own investigations.

PAULING: Well, my junior year in high school I took chemistry from William V. Green, who I think had a master's degree from

Harvard. He was a very good teacher. He had me stay after school hours (I think 2:30 is when my school stopped) and help him to run bomb calorimeter measurements of the heat value of oil and coal that was bought by the Portland school district. I think it was probably largely as a way of giving me a little extra instruction. And when I got my high school transcript I had an extra year, I believe (or perhaps only a semester) of high school chemistry. After finishing the one year he allowed me to come in the laboratory and he gave me problems. There were some organic preparations that I carried out using Remson (3). I spilled some sulfuric acid on the book, and I think I still have it with pages eaten away by acid.

And there was analytical chemistry, qualitative analysis. I remember this because he gave me an unknown and said that I should remember the words "hoi holoi strategoi" and tell him when I reported to him. I hadn't started studying Greek yet. About that time, I did decide to study Greek and got a Greek book and learned some Greek from it. It was [John] White's Elements of Greek (4). So I learned later "hoi holoi strategoi" meant "all the soldiers". He had put all of the metals into the sample.

STURCHIO: You were very lucky to come across somebody with that kind of a background in high school.

PAULING: Yes. I had a one semester course in physics with a very good physics teacher too. He was a smart fellow. We used [Robert] Millikan and [Henry] Gale as the textbook for high school physics (5).

STURCHIO: Do you recall the other texts you were using when you got to college?

PAULING: There was [William] Granville's Differential and Integral Calculus (6). At some stage I used Alexander Smith's General Chemistry (7). Maybe it was Smith and Kendall. [James] Kendall was at Columbia for a while, where Alexander Smith was, and then went back to Scotland. I liked Alexander Smith's big book. I am not sure I ever had it as a textbook in chemistry. It may be that somebody gave me a copy. From Alexander Smith's freshman chemistry text I got the word "stochastic." In my papers I quoted several sentences from Alexander Smith's General Chemistry. I used it when I said that the method that I am using to find the crystal structure of a substance is the stochastic method, from the Greek "stochastikos", apt to divine the truth by conjecture. And Alexander Smith went on to say that you make this stochastic hypothesis, (for example, that a substance is a hexahydrate) and then you can immediately test the hypothesis by carrying out an analysis. In this case you had to remove the water to find how much water was present. He may have given a better example than that, but I think I am now confabulating.

STURCHIO: While you were in college was there much contact in the curriculum with physical and theoretical chemistry? You mentioned in some of your earlier papers how you stumbled across [Gilbert N.] Lewis and [Irving] Langmuir in the journals while you were studying (1d). Was that entirely extracurricular?

PAULING: Yes. I had one year of very simple organic chemistry. That is all I had. I've forgotten the writer of the textbook that we used. I had a year of physical chemistry with [Earl] Millard as the text. I am not sure that is right, but he was at MIT (8). [Arthur A.] Noyes had a very poor opinion of the book, so he sent to me and Paul Emmett, in the beginning of the summer of 1922, proof sheets of the first nine chapters of Noyes and [Miles] Sherrill's <u>Chemical Principles</u>, which was then available in the printed form in the fall (9). He asked us to work the problems from all nine chapters during the summer, in order to make up for the deficiency in our physical chemistry.

The physical chemistry book that we had used was essentially a cookbook that would give equations without saying clearly where they came from, and you just substituted in numbers. It was taught by Frederick J. Allen, who later got his Ph.D. at Purdue. He has said that this course was the only course in physical chemistry that he ever gave, and it was hardly his field. He got his Ph.D. at Purdue and remained there all his life and taught chemistry to football students. He said that it was the football student's course in chemistry that he was in charge of. When my general chemistry text came out he used it as the textbook (10). They wouldn't use it in the other course because they thought it was too hard. But he used it with the football players!

They have all the correspondence between Fred and me over the years there at Purdue. Some of the letters have been published in the <u>Journal of Chemical Education</u> (11). There was correspondence about my idea that we could make xenon fluoride. Freddy sent me 200 milliliters of gaseous xenon for the experiment. I wasn't the experimenter. I got Don Yost to try to make it, and he reported that he didn't succeed. He had a nickel apparatus, and he couldn't see inside it. The man who later made the xenon compounds in one experiment was shown in a photograph holding a Pyrex flask with crystals of xenon difluoride. He said he was sure that Yost had made xenon difluoride, but had failed to recognize that he had. I think he may have been measuring the change in pressure but I don't remember just how the experiment was carried out. So Yost reported in a paper that you couldn't make xenon fluoride (12).

STURCHIO: That's not the only example of that kind of report.

PAULING: Someone at CIT said that he thought this was about the most unenthusiastic investigator who ever carried out an investigation. I judge that Don did this just because I asked him to, but perhaps he was convinced that it would be a failure.

STURCHIO: He missed a big discovery. I was interested in how you fixed on going to Caltech. Was it just by Noyes's reputation, or did Allen suggest it?

PAULING: No, I don't remember discussing this matter with Allen. He perhaps had got a master's degree already at Purdue. He was about at that stage, a few years older than I. When I was teaching quant full time, the head of the chemistry department, John Fulton, showed me a little placard sent out from CIT offering fellowships in chemistry to graduate students. He said that perhaps in two years I ought to go there. I then thought that I perhaps ought to go next year for my junior and senior years. So I wrote to A. A. Noyes, including a statement that I would have to make my own living. My mother did not have any money, and I was giving her the money I earned that year and other summers too. She had a hard time after my father died. I got a letter back from Stuart Bates, saying that A. A. Noyes was not there. He pointed out that he thought it would be impossible for me to attend CIT (it had just taken the name CIT in 1918, I think) and to support myself at the same time.

Well, I did it at Oregon State. When I was a freshman I worked at odd jobs in the kitchen of the girls' dormitory, chopping wood for the wood stoves and cutting the sides of beef into steaks and roasts. When I was a sophomore the chemistry department gave me a job in the stockroom so I got enough money to live on. In my freshman year I had a hard time. Then when I was a junior, the professor of engineering, Sam [Samuel H.] Graf, gave me a job. I took the course in metallography from him, and he may have given me the job during that first semester of my junior year. It seems to me I worked for two years for him and got $25 dollars a month or 25 cents an hour for a hundred hours a month. I corrected papers in the statics and dynamics course, bridge structure, strength of materials, and helped him in the laboratory.

I took the course in metallography and enjoyed it and began thinking about metals. I am pretty sure I still have the textbook we used, but I don't remember its name. In this book they discussed the slip interference theory of hardening. The grain boundaries provide the slip interference. Little crystals of a magnesium-aluminum compound, $MgAl_2$, or $MgCu_2$ form and interfere with the slip in the aluminum grains.

So Johnny Fulton, who also loaned me some money, which I paid back after I got to graduate school, suggested that I go to CIT. Well of course I was interested in Berkeley, because of Lewis and the nature of the chemical bond. I applied to Berkeley, CIT, Illinois, and Harvard. There was a professor of chemical engineering who had got his Ph.D. at Illinois named Floyd E. Rowland. He was an enthusiast about graduate work. He was one of the few people on the faculty at Oregon Agricultural College who had a doctor's degree.

[END OF TAPE, SIDE 1]

PAULING: There were about twelve graduates in chemical engineering in the class of 1922. Because of his influence, largely, seven of them went on to graduate work. I think perhaps six of them got Ph.D. degrees.

STURCHIO: Did they go into chemistry as well, or into chemical engineering?

PAULING: They were all in chemistry. Well, one of them, I think, got a master's degree at Harvard, perhaps in metallurgy, and got a job with an iron and steel company. So perhaps he was more of a metallurgical engineer. I am not sure. I think I saw him once many years later in Pittsburgh. Paul Emmett was one of them. Alfred Robertson had a National Research Council fellowship after getting his Ph.D. at Wisconsin. He worked most of his life for Eastman Kodak in industrial chemistry. I don't know what he was doing in the photographic business. Bill [William F.] Tuley got a Ph.D. at Illinois and worked for a chemical company in Stamford, Connecticut. Oz [Oscar M.] Helmer worked in medicinal chemistry at Eli Lilly after getting his Ph.D. I think there may have been one or two others. Well, chemical engineers were the smartest students in Oregon Agricultural College.

STURCHIO: That's a remarkable record for 1922 to have that many.

PAULING: There were seven out of twelve chemical engineers from a small cow college who got a Ph.D. in chemistry.

When I gave some general university lectures at Berkeley about four years ago three members of the chemistry department came up to me to tell the same story, which I had never heard before. I think it is apocryphal. See, I didn't get a reply to my application to Berkeley. Harvard offered me a half time instructorship and said it would take six years to get a Ph.D. So I turned it down.

STURCHIO: Was that from [Theodore W.] Richards? Did he write to you at that time?

PAULING: No. The editor of JACS [Journal of the American Chemical Society], Arthur B. Lamb, wrote to me. He was professor and director of the chemical laboratories. I turned that down. And Noyes said that I should reply immediately. In a few years there was much complaint about that sort of action. The chemistry departments decided that they should say that this offer is open until the first of April, so they could all have a chance. Well, I wrote to Berkeley saying that I had accepted the CIT appointment. The story is that G. N. Lewis was looking over the applications in the spring of 1922 and there weren't very many, perhaps twenty five. Departments were small, as I pointed out in my talk today. So he came to one and looked at it, the story goes, and said, "Linus Pauling, Oregon Agricultural College. I have never heard of that place." And down it went in the discard file.

STURCHIO: It does sound apocryphal to me.

PAULING: I think it is apocryphal. Here I'd been around Berkeley for fifty years before I'd heard that story, so I'd surely have heard it earlier on.

STURCHIO: What did Illinois say? Did you go through with that application or did you just withdraw it?

PAULING: I think I hadn't heard from them when I accepted the CIT offer. Well Noyes was pretty clever, I think, to pick Paul Emmett and me from Oregon Agricultural College to offer fellowships to.

STURCHIO: It is interesting to note that had you gone to Illinois Arnold Beckman would have also been there.

PAULING: I believe he came to CIT two years later, in 1924. But we are fraternity brothers in that he was in Delta Upsilon fraternity at Illinois and I was in Delta Upsilon fraternity at Oregon State, one of the founding members of the chapter. I said to him last year that I had decided that his spectrophotometer was named DU because of his being a member of the DU fraternity.

He said that wasn't so, that DU meant something but I don't remember now what it is.

STURCHIO: It was his fourth prototype model in ultraviolet. Of course it certainly is an assumption that it was for Delta Upsilon. I wanted to ask you a little later about some Arnold Beckman connections, but incidentally, he had a very similar introduction to chemistry. He had a high school teacher who had him do analyses on the side of similar kinds of industrial applications, and he took a course in metallography while he was still in high school, and then went on to study at Illinois. It is very interesting how you in Oregon and he in Illinois had similar backgrounds at the time in chemistry.

Well, Caltech must have been a completely different environment from the Oregon Agricultural College in Corvallis.

PAULING: Yes, indeed.

STURCHIO: You mentioned in one of your autobiographical articles (1d), about working the problems in Noyes and Sherrill (9) over the summer and reading the Braggs' book on X-rays and crystal structure (13). Did that help bring you up to speed when you got to Pasadena in the fall?

PAULING: Well, I suppose so. I don't remember very clearly. It didn't take me very long to learn. Within three months I had been taught the technique of determining the structure of crystals as it had been developed up to that time. In fact, I had studied fourteen crystals by the first of November or fifteenth of November, which I had made in the laboratory. I looked up cubic crystals, since Roscoe Dickinson told me that cubic crystals were the ones that can be analyzed, or hexagonal crystals or tetragonal, but mainly cubic. So I hunted through the crystallographic literature, [Paul] Groth's Chemische Krystallographie, five volumes, page by page, looking for cubic crystals (14).

One that I made was $CaHgBr_4$. As I made each of these I subjected it to preliminary X-ray examination to determine the size of the unit and the number of atoms in the unit cell. $CaHgBr_4$ turned out to contain 32 calcium atoms, 32 mercury atoms, and 128 bromine atoms. This was just an impossible problem. I dehydrated nickel sulfate hexahydrate to get nickel sulfate and mixed it with potassium sulfate. I built an electric furnace, melted them together, and let the melt cool slowly. I got crystals of $K_2Ni_2(SO_4)_3$ and determined the unit cell and space group for it. I found the structure depended on 19 parameters. Dickinson was working on a structure that depended on five parameters, and he succeeded, but most successful structure determinations involved zero or one or two parameters.

STURCHIO: Nineteen would have been biting off a little more than anyone can chew.

PAULING: Yes. The structure was determined twenty-five years later by somebody.

So then I made Mg_2Sn by melting magnesium and tin together and letting it cool slowly. It was brittle, so I just broke the pellet, which was about an inch diameter, with a hammer and got cleavage fragments which were well suited to X-ray study. And I melted sodium and dissolved cadmium in it and let it cool slowly. I put the product into absolute ethanol, which dissolves the sodium and doesn't attack sodium dicadmide, and got very nice octahedral sodium dicadmide crystals. The Mg_2Sn, of course, I determined, and it was a no-parameter structure. It was the first intermetallic compound reported in the literature to have its structure determined (15). I mentioned $NaCd_2$ in the same paper, but not in the title, as having a very large unit structure, at least a thousand atoms in the unit cube. Roscoe Dickinson then got some samples of molybdenite from the stock room, just molybdenum disulfide water-worn pebbles, but they were single crystals. You could cleave these pebbles and get cleavage sheets, which gave good X-ray photographs. So that was my first paper (16).

STURCHIO: And you wrote in [Paul] Ewald's book Fifty Years of X-Ray Diffraction that looking back on it you still found that a pleasing structure, finding the surprising result that it was a trigonal prism rather than the structure people expected (1a).

PAULING: Yes. X-ray crystallography was a lot of fun.

I began attacking more difficult problems by this stochastic method. I was getting to the point where, by 1927, after I got back from Europe, I had enough knowledge, perhaps vague sometimes, about the principles of the structure of crystals, so that I could say ahead of time what I thought the structure would be. And then I could check the proposed structure, by the X-ray diffraction pattern, and if it agreed I would say this is the structure, and of course it could even be refined to give better agreement, although that wasn't done very much because it involved too much calculation.

Or I could build a model and measure the model. That's what we did with Brookite, with one of my first graduate students, Holmes Sturdivant (17). I remember going for an automobile ride with Roscoe in 1928 near the Institute, just at the corner of Hill and San Pasquale streets, where Roscoe said that he thought it was fine that I was doing these structures that I was doing, but it was something that he could never do. I've mentioned this several times in interviews——he taught me to be rigorous, to know at every stage in whatever I was working on, whatever I was thinking about, what conclusions I'd reached that could be supported rigorously, and what conclusions involved an assumption somewhere along the line.

I was fortunate to have [Richard C.] Tolman teaching there. He was enthusiastic about teaching science. He gave a course my first semester on the principles of physical science. It was not like any other course that I have ever heard about. He discussed dimensional analysis, for example, and he even brought in the principle of relativity of size, which is something that he had formulated. He had asked, what restraints are there on physics, on our understanding of the physical world, if we make the assumption that the dimensional scale of the universe could be changed, a millionfold say, without changing the nature of physics, without changing the values of the dimensionless physical constants, such as the fine structure constant or others. And he tried to draw conclusions from that. He wasn't successful enough for it to have remained a part of physics.

STURCHIO: It is interesting that that kind of speculation and abstract intellectual activity was going on at Caltech.

PAULING: Yes. Then we had a course that Tolman gave on atomic and molecular physics, old quantum theory, using a book called The Origin of Spectra by [Paul D.] Foote and [F. L.] Mohler (18). It was a very good book. I learned a lot of basic quantum physics from it. Then the next year Tolman gave a course on Atombau und Spektrallinien using [Arnold] Sommerfelds's book in German (19). It hadn't been translated yet during the first year. In the first quarter Noyes gave a course on chemical thermodynamics, using the last four chapters of Noyes and Sherrill (9). I took this course, and it was the last course that A. A. Noyes taught. Very soon, and I don't remember when, but I think the second semester in my freshman year, there was a course on chemical thermodynamics using Lewis and [Merle] Randall (20). So if Lewis and Randall came out in time it was that year; if not, it may have been the following year. But my memory is, and I'm not sure about this, that I had a copy of Lewis and Randall along, when I went to Oregon to be married in 1923. That may be true. Then the following year, 1924, Tolman gave another course on atomic physics using the English edition of Atomic Structure and Spectral Lines which had come out in an English translation (21). So you can see how much emphasis there was on modern quantum science in the chemistry department, to say nothing of the physics department.

STURCHIO: What kind of mathematics were you taking at that time? I noticed in your first few papers that you were already very familiar with group theory and were using all of the space group analyses that [Ralph W. G.] Wyckoff had come up with.

PAULING: Yes. Dickinson taught me space group theory. That was hardly anything that I had done. It become routine to use group theory. The selection rules for space groups had not been tabulated yet, so I had to tabulate these rules myself for the space groups that I was interested in. If I make statements, my biographer, Bob Paradowski, is apt to say that the documents showed that what I said isn't right. He has all this stuff.

STURCHIO: Yes, I would like to get in touch with him at some point and compare notes.

PAULING: The first year, I think, I had a course in advanced algebra using the book by [Edouard] Goursat and [Raymond H.] Hedrick (22). My first graduate year Harry Bateman, a great mathematician, gave a course in vector analysis, and that was one of the most valuable courses I have ever had. Later I studied mathematical analysis, using [Edmund T.] Whittaker and [George N.] Watson, Modern Analysis, which is pretty advanced classical mathematics (23). And I had a course in integral equations from Harry Bateman. I had a very valuable course on Newtonian potential theory given by Harry Bateman. So very rapidly I built a pretty good mathematical background.

STURCHIO: You've talked about Dickinson, and some of Tolman's courses, and Noyes gave you his last course. Could you talk a bit about personal interactions with the faculty and your fellow graduate students in those first couple of years at Caltech?

PAULING: Well, the first year I wasn't married. I lived with Paul Emmett and his mother. His mother had come down to take care of Paul. He was sort of a mother's boy, and I lived with them. Paul and I went to some of the same courses. Dickinson and his wife took me to the desert a couple of times. They liked going out to the Colorado desert, the Palm Springs area, or Painted Canyon about fifty miles beyond Palm Springs on the other side of the Coachella Valley. That was nice. A couple of times, perhaps only once, I went with Paul and his mother and I don't know who else, on an automobile ride about fifty miles or more down towards Orange County through the orange groves.

I went on a hiking trip with another graduate student, Prescott, in the Mount Wilson area and we got lost in the wilderness there, and wandered around with no trails, finally ending up at Owens Camp after crawling through brush. This taught me a lesson about just following along with someone else. In fact, he crawled out on a cliff, and I crawled out on this cliff too and then I realized this was a foolhardy thing to do. It was loose rock, not hard rock climbing, and here he came from the east, from Yale. He didn't know anything about the mountains. He might well have killed both of us, so I stopped and sort of froze and said we'd have to go back, and with his encouragement I gradually worked my way back. There was a big drop of five hundred feet perhaps from this mountain, but he might have killed himself. In fact, I guess he did kill himself some years later in an explosion in his laboratory. I can still remember how frightened I was when I realized what was going on. So we were stuck up there hiking our way out and nightfall was approaching. I remember that a fraternity brother of mine lived up the canyon and we managed to find my fraternity brother's mother in the Arroyo Seco, who put us up for the night and sent us on our way the next morning. There was a bus. We had gotten into a canyon, the Arroyo Seco, where there was a bus line.

So that was about the extent of my social contact during my freshman year. I wrote a letter to my future wife every night and got a letter from her every day and I worked in the laboratory until about eleven o'clock perhaps, and then came home and went to bed. Paul delivered the morning paper in Portland, and he was in the habit of getting up at four o'clock. We had contact at all three meals that we had in his mother's home there, but didn't have very much other contact.

After I got married, of course, then we had a lot of contact with faculty members and other students. Bob [Howard Percy] Robertson had come down my second year. I think he had arrived from Washington where he had been studying mathematics with E. T. Bell. E. T. Bell arrived to be professor of mathematics. And Bob Robertson and I and his wife and my wife were together quite a lot then. He's the Robertson who had a student named [A. G.] Walker so that there's a Robertson-Walker universe. He became a cosmologist.

[END OF TAPE, SIDE 2]

PAULING: There's others. I studied mathematics also with a graduate student who became a professor of mathematics at CIT.

STURCHIO: Who was that?

PAULING: Professor Morgan Ward. He was an algebraist. His wife and I got along well. We got well acquainted, as well acquainted as one could get, I think, with Harry Bateman, mainly with his wife. He was always thinking about mathematics. We went to see them once at their home in La Cañada. And Mrs. Bateman called to Harry and he came out and shook hands with me, and shook hands with my wife, and then shook hands with Mrs. Bateman. Still thinking about some differential equation that he was working on.

STURCHIO: That reminds me of a story about Herman Schlesinger that I heard a couple of years ago at an ACS meeting. He used to be in the habit of taking his kids out for a walk and bringing along a couple of issues of JACS and getting lost thinking about chemistry. One day he came back from the walk and his wife said, "Where are the kids?" He had left them in the park.

PAULING: I'll tell you a story about [Norbert] Wiener at MIT who invented cybernetics. They moved and he couldn't remember his new house so he stopped a little kid on the street and said, "Do you know where Professor Wiener lives?" And the kid said, "Yes, that house, Papa." [laughter]

STURCHIO: Well, what you have been telling me is very interesting because it sounds as though you had a lot of contact with the mathematicians at Caltech. You were obviously very comfortable with the sorts of things they were doing and you were doing. Who were the people you would talk chemistry with all the time? Was it mainly Dickinson or were there fellow students in addition to Paul Emmett?

PAULING: Well, there was Oliver Wulf, who just died a month or two ago. His wife was secretary to Dr. Noyes. Later, after he was posted to CIT by the Weather Bureau for a good number of years, she became my secretary. We saw a lot of the Wulfs, and then in later years too in Washington, DC, of them and Paul Emmett and his wife. Tolman was twenty years older than I. G. N. Lewis was twenty-five years older I think. Roscoe was seven years older.

Chemistry at CIT was mainly physical chemistry. Howard Lucas was there doing good work in organic chemistry. He didn't have a Ph.D. A. A. Noyes didn't think much of him, but here he became a member of the National Academy of Sciences and had outstanding students; he wrote a very good textbook.

Noyes asked me, perhaps in 1932 or 1933, if I would be willing to be professor of organic chemistry and build up the organic chemistry program. Well, that shows he didn't know much about organic chemistry. I just rejected that idea. But that was because I was making contributions to the understanding of organic chemistry without knowing the art at all, the practical side.

STURCHIO: It's an interesting reflection of Noyes' attitude. Speaking of art, that was another thing I wanted to ask you about. I was intrigued in reading some of the papers you wrote about the early days of X-ray crystallography (1a, 1b) that C. Lalor Burdick had constructed the first two X-ray spectrometers in the U.S. after coming back from Bragg's laboratory.

PAULING: Yes, yes.

STURCHIO: That must really have been a craft.

PAULING: Yes, and Noyes was responsible. Burdick was working for his Ph.D. in Germany when Noyes wrote to him. He got his Ph.D. in Switzerland, while World War I was on. Noyes wrote to him and told he should go England to Bragg and learn X-Ray spectrometry. So he did, and he and E. A. Owen, published a paper on the structure of silicon carbide, carborundum (24). This is work done in England. Then he came to MIT and built an X-ray spectrometer, and then Noyes got him to come to Pasadena and he built another one. There were parts of it around when I was a graduate student although we no longer used that spectrometric technique.

STURCHIO: When you came down to Pasadena, did Dickinson put somebody in charge of teaching you the technique?

PAULING: No, he did it. He was a National Research Council Fellow. He got his Ph.D. two years before, and perhaps he had this National Research Council Fellowship for three years. I'm not sure. At any rate, I was his only graduate student. He told me how to set up a crystal in front of the X-ray beam and how to develop the plate and the Laue photograph, and make the spectral photograph, measure them up and then how to plot, make a gnomonic projection of the Laue photograph, just step by step. He taught me one after another the basic principles of space group theory and of crystallography, the planes and zones, the laws by means of which you can find what plane lies in two zones. I have a feeling that I was a good graduate student in that he probably never had to tell me anything twice.

STURCHIO: You were certainly very productive in those early years. You published a whole series of papers, first with him and then by yourself (25).

PAULING: Yes. And I read the literature, and I studied in my courses, which were mainly physics courses, except for these courses that Tolman gave that I mentioned, in chemistry, which was sort of mainly quantum chemistry, quantum physics. There is no doubt, I am sure, about the statement I made that there is no place in the world where I could have gone at that time to get a better education than I got there.

STURCHIO: It was quite a remarkable group of people that Noyes had brought together.

PAULING: Noyes probably deserves most of the credit for the nature of the Institute. [Robert A.] Millikan was sort of his front man, and Noyes determined academic policy. No women were admitted. That was a waste of time, in his opinion, a waste of effort. He set up a junior travel prize, so that the best junior student was given money for six months in Europe, and given academic credit for it, because he lost one term plus the summer. So one year, in 1926, there were two; they had trouble deciding between them. So they gave the junior travel prize to both of them. My wife and I met them in Rome. They were Carl Anderson and Fred Ewing. Fred Ewing didn't have much of a career. He developed Huntington's Disease in middle life, and Carl Anderson, of course, discovered the positron. The next year there was a class I'd taught of freshman where one of them published his first paper with me for research done as a freshman student the following summer. That was Edwin McMillan (26).

STURCHIO: Yes, he was promising.

PAULING: There were eight out of the dozen members of section A, the honors section, eight juniors who were selected for the junior travel prize. Dr. Noyes probably put up the money for it. So the eight of them showed up in Europe. We saw Carl Anderson and Fred Ewing in Rome on Easter Day 1926. And we saw the eight a year later when they came to Münich.

STURCHIO: I guess by having a small student body, Caltech could afford to really invest that kind of time and resources.

PAULING: For example, Ken [Kenneth S.] Pitzer published a couple of papers with A. A. Noyes when he was a junior (27). When he was a senior, he published a paper by himself for work done under my direction, a crystal structure job (28). Then he went to Berkeley and got his Ph.D. in two years and was made assistant professor. Caltech was an excellent place. It has gotten pretty big now. They still have 760 undergraduates but they have 760 graduate students now.

STURCHIO: We're up to about 1925, the year before you went over to Europe as a Guggenheim Fellow. In addition to the crystal structure studies that you were doing, I read a couple of the early papers you published on chemical structure and bonding while you were still at Caltech. One of the first was still using the old Bohr theory (29).

PAULING: Yes.

STURCHIO: When were you beginning to become familiar with the new quantum mechanics? Was that before you went over to Europe?

PAULING: Well, the new quantum chemistry didn't exist yet. But before I went to Europe in March I had heard lectures by [Max] Born on quantum mechanics, in 1925. In 1925 [Werner] Heisenberg discovered quantum mechanics, and published his first paper (30). Born and [P.] Jordan noticed that his theory involved matrix mathematics, which was a well developed field of mathematics that Heisenberg didn't know about. They published papers on matrix mechanics (31). Born, in December of 1925, gave some lectures on matrix mechanics in Pasadena which I attended and took notes on and tried to use. But I gave up, because it was just too difficult. It was very hard to apply to the hydrogen atom. [Wolfgang] Pauli succeeded in treating the hydrogen atom with matrix mechanics. So, it was still with the old quantum theory still that I was trying to understand chemistry, and not very successfully. Other people were trying to develop the theory of the hydrogen molecule, or even the hydrogen molecule ion. Pauli didn't succeed, nor did Heisenberg, when they tried to treat that problem. It took the Schrödinger wave equation to permit quantum chemistry to be born. My early effort, where I assigned structures to benzene and naphthalene, was rather amateurish I would say, though it perhaps showed some imagination. And this idea, I think, wasn't any worse than some of the others that were being proposed.

STURCHIO: That showed you had continued to think about those questions that you mentioned earlier today, that had interested you when you first came across Lewis and Langmuir's work in the journals. Your first European sojourn from 1926 to 1927 I know you have discussed at some length in other interviews (32). You mentioned a little while ago that Caltech was a very stimulating environment when you got there. How did you find things when you left for Europe?

PAULING: Well, I had provided my own stimulus pretty much. Sommerfeld suggested a problem to me when I arrived in Münich, that I work on the anomalous G factor of the electron and check up on what Max Abraham had written around 1900. Which I did. But it didn't really interest me, and it never came to anything. I had applied the old quantum theory to the problem of the dielectric constant of a dipole gas in a magnetic field, and made some predictions, which were checked by experiment and were found to be wrong about the time I went to Europe. The physics department carried out the experiment. So I attacked the problem as soon as Schrödinger published his papers. I attacked the problem of the dielectric constant, the motion of a dipole molecule in crossed electrostatic and magnetic fields, and solved it (33). And I was trying to develop the quantum theory of electric and magnetic susceptibilities at the same time as [J. H.] Van Vleck was, and I had some results, but Van Vleck made a very thorough study of those problems.

So I attacked problems that Sommerfeld didn't know about or wasn't interested in. They were essentially chemical problems. I found a paper by Gregor Wentzel, which was just published in _Zeitschrift für Physik_ (34). He was Privatdozent to Sommerfeld. The paper was on the theoretical calculation of the X-ray doublet screening constants, by quantum mechanics, for atoms with many electrons. He had used an idea of Schrödinger's of idealizing inner shells as a spherical shell of negative electric charge with penetrating orbits. Schrödinger had published this about 1920 (35). And Wentzel had put in quantum corrections, had changed the equations of old quantum theory to the corresponding equations in quantum mechanics, and published the results, which were that the experimental values, the observed values, didn't agree with the theory. Well, this is just what I was hoping to get involved in. He treated atoms containing many electrons. And even though his theory didn't agree with experiment, I thought maybe I could use it, with my interest in atoms that have many electrons and in molecules. So, I read the paper. Instead of just reading the paper I went from one equation to the next equation. Pretty soon I found that my equations were not the same as his equations. In fact, he had just made a fundamental mistake. So I took this to Sommerfeld, who showed it to Wentzel. My screening constants for these relativistic or magnetic X-ray doublets agreed with the experiment. So I wrote a paper and published it (36). And then I went ahead to use the same method, a correction and an expansion of what Wentzel had done, to calculate many properties of atoms and ions. I published several papers on that basis (37).

Well, you see, I had plenty of problems and things I was interested in. One of them was sizes of ions, and these screening constants permitted me to derive my ionic radii. That led to principles determining the structure of complex ionic compounds, the silicates and many others, practically all oxygen compounds or compounds of the halogens. I had mentioned that when I went to Zürich after being for a month in Copenhagen, I was supposed to be working with Schrödinger, but I didn't bother him with my presence. I just worked on my own problem, the interaction of two helium atoms. A little later I published a paper on He_2^+, the three-electron bond (38). Later I developed the theory of the three-electron bond, as well as the one-electron bond. There were all sorts of problems in chemistry that could be attacked once the Schrödinger wave equation was formulated, and the general principles about perturbation theory especially had been discovered.

STURCHIO: When you learned X-ray techniques from Dickinson, you worked directly with him, and he sort of showed you the technique step by step. Was there a similar kind of interaction when you were over in Europe working on quantum mechanics, or was that pretty much a question of working through the material yourself?

PAULING: I was fortunate during the year that I was there. Sommerfeld gave lectures on wave mechanics. Schrödinger's papers were just coming out and so Sommerfeld gave a course on wave mechanics while the papers were still being published. I attended these lectures and read the papers. Sommerfeld had a seminar, too. I probably reported on something in the seminar, although I don't remember doing so. I remember a seminar at which another American was supposed to present Schrödinger's paper on the Stark effect of the hydrogen atom and he started trying to talk in German. But he got mixed up and couldn't remember the German words, so Sommerfeld said he should present it in English. Unfortunately he didn't understand the material well enough to present it in English. The students at the seminar, twenty or twenty-five of them, were very impolite, of course, the way Germans could be. I think they started laughing and making fun of him. He left after a while. I don't know what happened to him.

STURCHIO: Who were some of the Americans you came across while you were in Europe?

PAULING: Our American friends in Münich were Ernst and Victor Guillemin. They both took their Ph.D. while we were there. We helped them celebrate, just as we helped Walter Heitler celebrate passing his doctoral examination. Ernst Guillemin and Victor were sons of an industrialist in Wisconsin, I believe. They were probably from Milwaukee, and they spoke German well. Probably the family spoke German at home. But people thought they were Germans. Ernst became professor of electrical engineering at MIT and wrote a very good textbook on electrical circuits or some such subject (39). I think his thesis was on electromagnetic waves, radio waves, transmission. Victor had a thesis in molecular structure, methane. I saw Ernst once or twice after then before his death. Victor I never saw after Germany. He was working as a physicist at the Wright Aeronautics Laboratory in Dayton, Ohio. I wrote a letter to him and got an answer back from his son saying that Victor had died. We had German friends. Especially Heitler and [Fritz] London. They were the two closest ones.

[END OF TAPE, SIDE 3]

PAULING: We met Americans from time to time who were passing through. [Robert] Havighurst was one of them. David Dennison had just succeeded in explaining the heat capacity of hydrogen gas while he was in Europe. There was Ed [Edward U.] Condon. We met [Robert] Oppenheimer in Göttingen. [James] Franck took us to dinner at his home in Göttingen. I had written a paper with Debye in the United States (40). I had worked out a theory of interaction of ions that I thought was a refinement of the Debye-Hückel theory of electrolytes. Noyes got Debye to come to Pasadena in 1924 so that I could present this to him. He didn't say anything. He just smoked his cigar. This was at a seminar attended only by Debye, Noyes and Tolman. I presented my arguments.

STURCHIO: Not a bad audience.

PAULING: No. Debye suggested that I work on a problem which I handled very quickly, as I think he knew I would. So we published a paper in 1925 on the Debye-Hückel theory and the influence of the dielectric constant on it. Debye had been in the United States and I knew him and his wife. When we were in Zürich we saw a good bit of Debye when I was supposed to be working with Schrödinger. By that time I had so many problems of my own I didn't need to work on something someone else suggested.

STURCHIO: This might be a good time to stop, since you are about to come back to Caltech.

PAULING: Well, this was a great experience, being in Germany and Copenhagen and Zürich that year and a half, meeting so many European scientists, mainly physicists, and some chemists. I had a good bit of contact with [Kasimir] Fajans in Münich. I just wrote a statement about our interaction with Fajans and his wife, for some Fajans symposium that's coming up. I am not going to repeat it, but I wrote a statement.

STURCHIO: I'd be interested in seeing that. Do you have plans to publish it?

PAULING: I don't know. I am not sure it has been sent in yet. I dictated it before I started on this trip.

STURCHIO: Perhaps I can write and ask for a copy when it is ready. He's someone I've always been interested in. When I took chemistry in college the Fajan rules were mentioned and I was intrigued by that. He had an interesting career too.

PAULING: It was on radioactive decomposition. He shares the credit with someone else on the rules of radioactive decomposition. [The English physicist Frederick Soddy.]

Well, it was really a great experience, and A. A. Noyes engineered the whole thing. When I'd got my Ph.D. I had decided I should go to Berkeley. I applied for a National Research Council Fellowship, which I got. Noyes said I shouldn't really go to Berkeley immediately. (Lloyd Jeffress was still a graduate student in Berkeley.) But I was still eager to be associated with G. N. Lewis, or to be in his environment and learn more about chemical bonding. So Noyes said, "They don't have any X-ray apparatus." And it's true what I said, that CIT was about the only place in the world where a chemistry department was strong in X-ray diffraction. Elsewhere it was mainly physics departments. So he said, "They don't have any X-ray apparatus, and you have a lot of work that hasn't been written up for publication. You may want to make more X-ray photographs, so it would be wise for you to stay here." So I just stayed on, despite the rule of the National Research Council that you had to move to another institution, and I'd said that I was going to Berkeley.

After a while Noyes said that he wanted me to meet Frank Aydelotte of the Guggenheim Foundation, who was visiting Pasadena. They hadn't given any Guggenheim Fellowships out yet. I met Aydelotte. Noyes had said, earlier in the fall of 1925, that I should apply for a Guggenheim Fellowship. He said, "Your education requires that you study in Europe." So I made an application for a Guggenheim Fellowship, and the decision would be made about the first or fifteenth of April, as to which people would have Guggenheim Fellowships for the next year. Then Noyes came to me and said, "You should plan now to go to Europe in February or March so as to get an earlier start. You can write to the National Research Council and resign from your Fellowship." So I wrote, and I got back an angry letter from the director saying that here I was keeping somebody else from having a National Research Council Fellowship for the full year. I was, and perhaps still am, pretty unsophisticated. It took perhaps forty years before I understood what was going on.

That was when I learned about another event. G. N. Lewis showed up in Pasadena before my European trip. He attended a seminar that somebody gave and just hung around for a day and then went away. I didn't learn until many years later that he had come to offer me a job in Berkeley. Noyes was sort of his teacher. Noyes had argued him out of it and had arranged this business of my continuing to work in Pasadena instead of going to Berkeley, and then of having the Guggenheim fellowship and then of going six or eight months earlier than I could have gone. He said, "You're sure to get the Guggenheim fellowship and the Institute will give you $1,500 for the period up to that time." So I just accepted all this. Noyes was determined that I would stay at CIT and not go to Berkeley.

STURCHIO: That is interesting. How did you finally discover that, or did you figure it out yourself?

PAULING: Maybe Paradowski told me. I am not sure. I am not sure about G. N. Lewis coming down to offer me an assistant professorship at Berkeley, but I don't know what I might have done if he had offered it to me.

STURCHIO: Well, you might not have had the chance to go to Europe in that case.

PAULING: Well, all of the young people at Berkeley went to Europe on National Research Council, Rockefeller, or Guggenheim fellowships.

While I was in Münich, in the spring, I got a letter offering me an appointment as assistant professor of physical chemistry and mathematical physics at Caltech. So I wrote accepting it. When I got back to Pasadena in September of 1927, I found I was assistant professor of physical chemistry. The mathematical physics part had disappeared. I didn't ask about it. It just happened. But here I think Noyes decided that I should be a chemist and not spend half my time in the physics department, even though there was a very close relationship between the two. Tolman was on a joint appointment in physics and chemistry.

STURCHIO: But for a few years you did spend some part of the year at Berkeley.

PAULING: For five years I spent a month or two in Berkeley.

STURCHIO: Did it live up to expectations when you finally had a chance to spend some time there?

PAULING: It was fine. I enjoyed meeting all of the people, getting more intimately acquainted with them than I had been before. I had met some of them before, of course. I enjoyed that for five years, from the spring of 1929 to the spring of 1933.

Well, perhaps I should rest a little before going out for the evening. Although I notice that my instructions say that I am not say anything tonight.

STURCHIO: Well, thanks very much for your time.

[END OF TAPE, SIDE 4]

NOTES

1. See, for example
   a. Linus Pauling, "Early Work on X-ray Diffraction in the California Institute of Technology," in P. P. Ewald, ed., *Fifty Years of X-ray Diffraction* (Utrecht: International Union of Crystallography, 1962); 623-628.
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   c. Linus Pauling, "Fifty Years of Physical Chemistry in the California Institute of Technology," *Annual Review of Physical Chemistry*, 16 (1965): 347-360.
   d. Linus Pauling, "Fifty Years of Progress in Structural Chemistry and Molecular Biology," *Daedalus*, 50 (1970): 988-1014.
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15. Linus Pauling, "The Crystal Structure of Magnesium Stannide," Journal of the American Chemical Society, 45 (1923): 2777-2780.

16. Roscoe G. Dickinson and Linus Pauling, "The Crystal Structure of Molybdenite," Journal of the American Chemical Society, 45 (1923) 1466-1471.

17. Linus Pauling and James H. Sturdivant, "The Crystal Structure of Brookite," Zeitschrift für Kristallographie, Mineralogie, und Petrographie 68 (1928): 239-256.

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