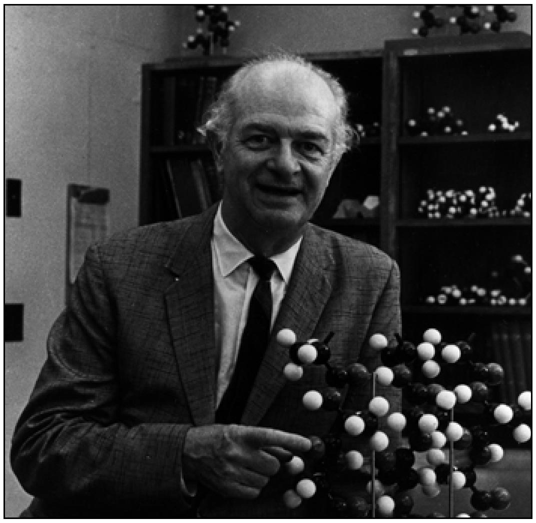

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LINUS PAULING
(1901-1994)

INTERVIEWED BY
JOHN L. GREENBERG

May 10, 1984

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Chemistry

Abstract
Interview in 1984 with Linus Pauling, professor of chemistry emeritus. He recalls his instructorship in quantitative analysis at Oregon Agricultural College at age 18. To Caltech for graduate study, 1922. As preparation, Arthur Amos Noyes sent him proof sheets of Noyes's new book, *Chemical Principles.* Studied X-ray crystallography with Roscoe G. Dickinson. Gave seminar on Debye-Hückel theory of electrolytic solutions for visiting P. J. W. Debye. Recollections of Noyes (then chairman of the Division of Chemistry and Chemical Engineering), Dickinson, and Ralph W. G. Wyckoff. Discusses X-ray crystallography and its history. Recollections of Gilbert N. Lewis, Caltech's rivalry with Berkeley. Paper with Richard C. Tolman on residual entropy of crystals; recalls courses with Tolman. Offer of professorship at Harvard in 1929 and MIT c. 1930. Death of Noyes (1936) and Pauling's appointment as chairman of chemistry division (1937). Remarks on Biology Division and advent of Thomas Hunt Morgan (1928). Work on hemoglobin in mid-1930s. Remarks on Karl Landsteiner and immunology. Lectures on "complementariness" as basis of biological specificity; paper with Max Delbrück. Projected book on the molecular basis of biological specificity, to be called *The Nature of Life*. Recollections of Albert Tyler and

George W. Beadle. Comments on relations with Warren Weaver and Rockefeller Foundation. Discusses work on protein structure and discovery of alpha helix. Discusses his reasons for leaving Caltech in 1963 and the attitude of Caltech president Lee DuBridge and John Roberts, then chair of the chemistry division. Recalls his resignation of division chairmanship in 1957; attitude of trustees toward his politics; his efforts to raise money to defend colleague Sidney Weinbaum. Recalls being badgered by Lawrence Spivak on *Meet the Press* in 1950s. Comments on quantum mechanical theory of resonance and the chemical bond. Comments on Center for Study of Democratic Institutions.

Administrative Information

Access

The interview is unrestricted.

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Preferred citation

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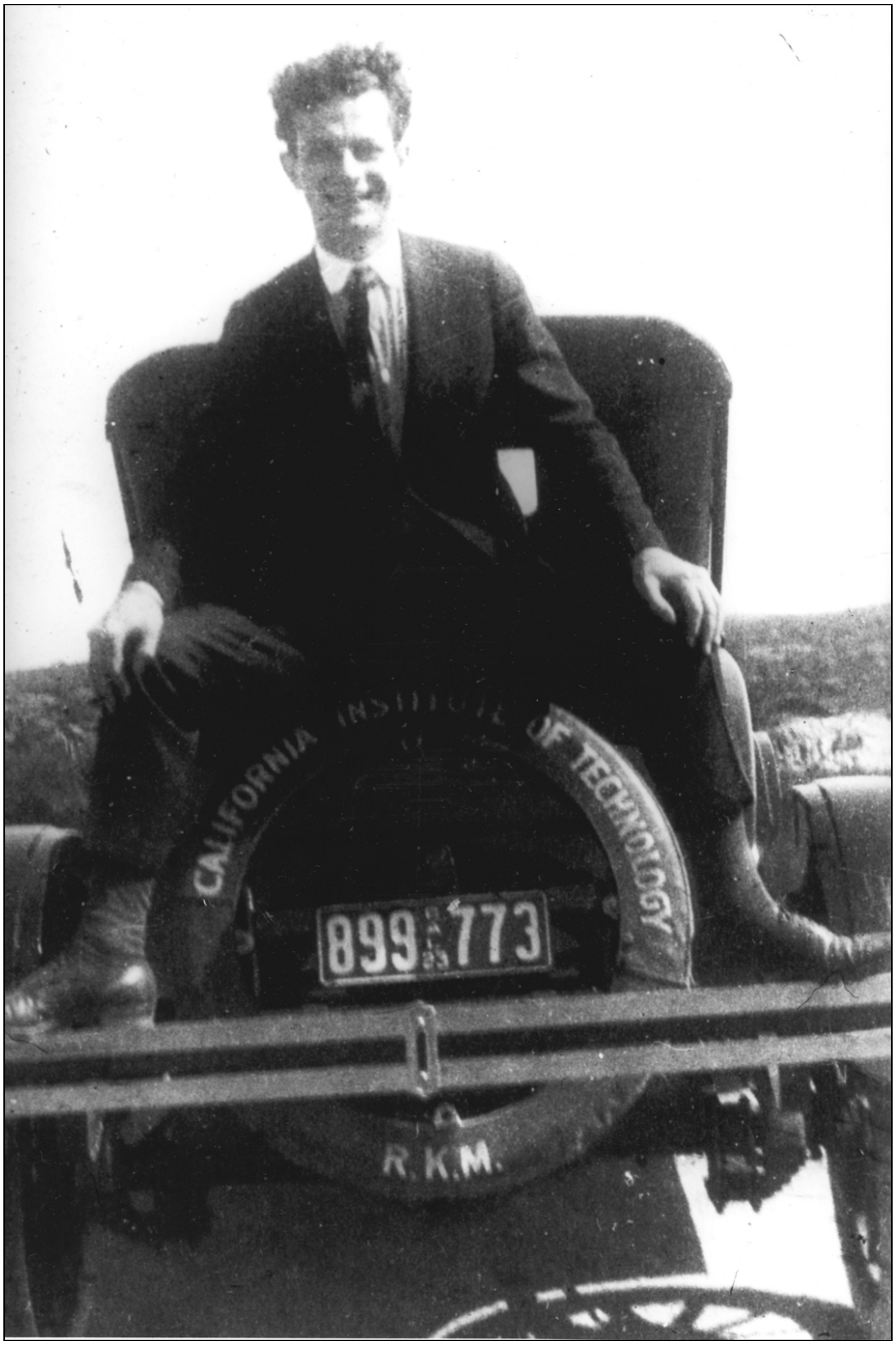

Linus Pauling astride car, 1923

California Institute of Technology
Oral History Project

Interview with Linus Pauling

by John L. Greenberg

Palo Alto, California

Caltech Archives, 1984

Errata:

p. 1: “I knew him as the author of the book *Qualitative Analysis*”—The full title of Noyes’s book is *A Course of Instruction in the Qualitative Chemical Analysis of Inorganic Substances.* (Many editions; published by Macmillan).

p. 3: “*Chemical Principles”*—Full title is *An Advanced Course of Instruction in Chemical Principles*, written with Miles S. Sherrill (Macmillan, 1922).

p. 5: “it may have been Leeds, where I think the elder Bragg was”—William H. Bragg left the University of Leeds in 1915, to become professor of physics at University College, London.

p. 7: “his book, the *Analytical Representation of the Results*….”—Correct title is *The Analytical Expression of the Results of the Theory of Space Groups,* by Ralph W. G. Wyckoff.

p. 17: “[Alfred A.] Sturtevant”—Correct name is Alfred H. (for Henry) Sturtevant.

p. 19: “my lectures, which both [E. C.] Watson and [Francis] Crick heard”—Correct name is [James D.] Watson.

p. 30: “Private Eddie Slovak”—Correct name is Eddie Slovik.

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Linus Pauling — by John L. Greenberg
Palo Alto, California — May 10, 1984

Begin Tape 1, Side 1

Pauling: I arrived in Pasadena September 1922.

Greenberg: Was [A. A.] Noyes directly involved in your choice of Caltech?

Pauling: Yes, I suppose he was. I knew him as the author of the book Qualitative Analysis, which came out in several editions. He had written that around 1900, and then revised it; it was a very popular book in the 1920s. I remember that I was told by someone that after twenty years he was still receiving $2000 a year in royalties, which was quite a sum of money in those days. There were some editions in which "qualitative" was spelled without the "e" on the end, and other rather common words were spelled in simplified spelling. Noyes went through a period, I judge, when he felt that it was sensible to introduce simplified spelling, but then he reverted to conventional spelling after a few years. So I knew about A. A. Noyes. In 1919, during the summer, when I was eighteen years old, I had been in southern Oregon as a paving engineer, a paving plant inspector, working for a contractor for the state of Oregon. And at the end of the summer, I did not have money enough to return for my junior year at Oregon Agricultural College. So I didn't return. I'd been sending my money to my mother, who was a widow and was having a hard time. I thought I was sending it to her to put in the bank account, but she told me that she was not able to give it back. I received an offer from Oregon Agricultural College as an assistant instructor, full time, in quantitative analysis, which was taught to sophomore students. I had had the course the year before when I was a sophomore. So I taught full time sophomore chemistry--

quantitative analysis at OAC. During that year, and this probably was about January 1920, a poster arrived--I remember it clearly--and this poster was advertising teaching fellowships, or graduate assistantships for students in chemistry at CIT. And A. A. Noyes was mentioned there. The head of the chemistry department at Oregon Agricultural College said, "Perhaps that's the place that you should go to for graduate work." I'm not sure that I had been thinking of graduate work at that time. There was, however, at about that time, either 1919 or 1920, a new man in Oregon Agricultural College named Floyd E. Rowland, who had just got his doctor's degree at the University of Illinois. He was a middle-aged man, not outstanding in either his intellectual ability or his knowledge of chemistry and chemical engineering, but an enthusiast. The result was that of the twelve graduates in chemical engineering in 1922, of which I was one, seven later got their doctor's degrees in chemistry, which was really remarkable.

A year ago last January, I gave the Hitchcock Lectures at Berkeley, University of California, and three separate people told me a story that I had not heard before, surprisingly enough because I've been around Berkeley for sixty-one years, from time to time--my first visit was in the fall of '22. In the spring of '22, I applied to Harvard, University of Illinois, Berkeley, and CIT, and I think one or two other places, for an appointment, permitting me to go on for graduate work. I had refused to be a candidate for the Rhodes scholarship; I had been a candidate the year before when I was a junior, but as a senior I decided that it was not for me. I didn't have money enough to back up the Rhodes Scholarship stipend, for one thing, and it needed some backing up if you were to go to England to study. So I applied to these several places. Harvard offered me a half-time instructorship, and the letter said that it would take six years to get a doctorate on a half-time instructorship. So I turned that down. And I received a letter from CIT, offering me an appointment at $350 a year, I think with tuition not charged, and with a letter from A. A. Noyes containing, I think, the statement that I should reply at once. I hadn't heard from Berkeley, which was, in a sense, my first choice because of my interest in what G. N. Lewis had written about the chemical bond. But I hadn't heard, so I wrote accepting the appointment at CIT. A few years later, of course, the universities entered into an agreement that they would not put this in the letters of appointment, but would say "You have until the first of May to decide." So the story that I heard in Berkeley in January of 1983 is that G. N. Lewis was looking over the applications that had come in to Berkeley's chemistry department--perhaps twenty or twenty-five, you know, it wasn't like now, graduate schools weren't so big. He came to one application and said, "Linus Pauling, Oregon Agricultural College? I've never heard of that place." And down it went into the reject pile.

Well, I had written to Berkeley, withdrawing my application when I accepted the job at CIT. A. A. Noyes then wrote to me, and also to Paul Emmett, who is a distinguished chemist who got his Ph.D. in '25, too, saying that the course that we had had in physical chemistry was a pretty poor one, that he was sending us the proof sheets of the first nine chapters of his new book, called Chemical Principles, which was elementary physical chemistry. He asked us to work the problems in the first nine chapters, and then we would have a course with him in the fall of 1922 on the last three chapters, which were thermodynamic chemistry. This book was admirably suited to self instruction. It led the student up to the derivation of formulas, and then there was a problem: Derive, by the following steps, the Clausius-Clapeyron Equation, things like that. So I worked all of these problems, and Paul Emmett did, too, separately. I had a job, again, as paving plant inspector in on the coast of Oregon that summer, and I didn't have anything to do in this small town. Every evening after I got home from work I would work for three hours or so on these problems--a large number, several hundred. I have the book of my exercises, with a few comments on them by Dick Badger, who became Professor Badger at the Institute. He was a good man. There were few places where I had misunderstood or made errors, as I recall. I think that I'm right on that, perhaps not. At any rate, I have also the problems that I worked when we took the course from A. A. Noyes in the fall, Paul Emmett and I, and others. Dick Badger corrected the papers in that course, and made some comments.

That was the last course that Noyes ever gave, the fall of 1922. From then on, he served as chairman of the Division of Chemistry and Chemical Engineering, and supervised research, helped conduct, or perhaps conducted, the chemistry seminar, and was a member of the executive committee of the Institute.

Greenberg: How would you characterize Noyes's influence on you?

Pauling: Well, I had much respect for A. A. Noyes. And of course, I didn't care much for qualitative analysis. I liked quantitative analysis much more. I didn't work directly with A. A. Noyes, because I was carrying on x-ray crystallography with Roscoe G. Dickinson. But after my first year there, Dr. Noyes had written two articles about the Debye-Hückel theory of electrolytic solutions. He asked me to look them over. I did, and I made a number of suggestions about them. As I recall, I may have made some minor suggestions that were incorporated in the paper, but I got interested enough in the subject to formulate what I considered an improvement on the theory, an extension of the theory. So, in 1924, Noyes invited Debye to come for a week or two to the Institute. I presented my ideas in a seminar, which was a special seminar in that the only people there were Noyes and [Richard C.] Tolman and [P. J. W.] Debye. Debye just smoked a cigar and didn't say anything at the end. Instead, he suggested that I work on a problem that he was interested in, which was the effect of variation in the dielectric constant. I did, and completed it, and we wrote it up and published it in a paper in 1925. He also suggested that I work on another problem, which was the electropheresis--motion in an electric field--of liquid particles in a liquid medium. This was a problem that hadn't been treated before. I did a lot of work on it, but nothing was published--I worked for two weeks, part of the time on that. So the history of my effort to become involved in the field that Dr. Noyes was much interested in, the properties of ionic solutions, is that I couldn't convince Noyes and Tolman that what I had done was worthwhile. I presented it at the 1925 meeting of the American Chemical Society in Los Angeles, and an abstract was published. I also presented Dr. Noyes's paper, which he was planning to submit. He didn't want to go to the meeting, and I went over and presented his paper.

There was no time when I felt that Dr. Noyes was interested in having me as a student working with him, probably because my method of attack on problems was quite different from his.

Greenberg: I gather that he didn't have that many students anyway.

Pauling: Well, he had a good number of students, and up to just before his death. These were mainly undergraduates doing research, mainly on problems in inorganic chemistry that he was interested in. His work in chemical analysis was done mainly with [Ernest] Swift. And Swift, of course, got his Ph.D. degree--he was the second or third person at CIT. [Roscoe G.] Dickinson was the first person to get a Ph.D. degree from CIT. Dickinson had started as a graduate student at MIT.

Noyes began coming to CIT in 1913 for brief periods. He quit his job at MIT in 1916, and probably gradually between '16 and '17 shifted, bringing some people with him to Pasadena. One of the people that he brought was Lalor Burdick. Lalor Burdick had taken undergraduate work at MIT, gone to Europe and was studying organic chemistry in Germany. At the outbreak of the First World War, he moved to Switzerland, and got his Ph.D. degree. Noyes suggested that he go to London to the Royal Institution and learn about the new technique--or it may have been Leeds where I think the elder Bragg was--learn about the technique of x-ray crystallography. He published a paper with a man named Owen on the structure of carborundum, silicon carbide, for the work that he did in England. He came to MIT and began to build an x-ray diffraction apparatus, and then moved to Pasadena. He and [James H.] Ellis, who had taken his Ph.D. at MIT but had come to Pasadena as a research associate--unpaid, I think he had private income--in chemistry, then carried out the first determination of the structure of a crystal by x-ray diffraction that had been done in the United States. That was done at CIT--or it was not yet CIT--and published in 1917. In the same year, the powder method was used by Hull at General Electric Company. So there were two contributions from the United States during that year, one of which was a single-crystal study of chalcopyrite. I reinvestigated chalcopyrite with one of my students--it was with Brockway--in 1932, to determine the sulfur parameter more accurately. It turned out that the structure Burdick and Ellis had reported wasn't quite right, the metal atoms, iron and copper, had not been distributed properly among the metal atom positions. The difference in the x-ray patterns was pretty small, so the earlier technique just wasn't good enough for them to have got exactly the right structure. Burdick is still alive. He runs the Lalor Foundation, set up by his uncle. Perhaps his mother's maiden name was Lalor. One of the things the foundation did when it first began to operate was to give money to the Institute for the Arthur Amos Noyes Lalor Fellow; that was just for one year I think. A man was appointed in chemistry, a postdoctoral man, as Lalor Fellow. Well, you ought to check with Burdick about what he remembers about the period around 1917 in Pasadena.

Noyes wrote to me, saying that he thought I should work with Dickinson when I arrived, and that I should read the book, X-rays and Crystal Structure, by W. H. and W. L. Bragg. I was in a small town on the coast of Oregon. I wrote to the state library in Salem, and they sent me a copy of the book, the first edition. So I read that book and mailed it back. Then, when I arrived in Pasadena, I immediately started to work with Dickinson. I was put in a room--208, I think, on the second floor of Gates Laboratory--a small room, which had chemical benches on each side, and a desk. It was for two graduate students. It was very satisfactory; it even had a hood. [Charles H.] Prescott was the other graduate student. He got his Ph.D. degree probably in '25, too, or '26--I'm not sure. Ultimately, he was killed in an explosion in his laboratory here in the Stanford area. I don't have details about him. Prescott was from Yale, an easterner. I remember his expression of astonishment at my not knowing something about the geography of Europe that he thought everyone ought to know.

Dickinson was the major influence on me, I would say, in regard to scientific research, with Tolman in an almost equal status, with respect to theory. Dickinson taught me the meaning of rigor and proof in scientific work. Up to that time, I suppose, my thinking had been sort of fuzzy. But Dickinson showed me how to attack a problem, and know what you were doing at each stage. The state of x-ray analysis at that time, as developed by Nishikawa and [R. W. G.] Wyckoff, and to some extent Dickinson, was extremely interesting. Every crystal that was studied represented a problem. There were no really powerful means of determining structures. The symmetry of a crystal was of great help. Wyckoff had got his Ph.D. at the age of twenty-two at Cornell. There was a man, Nishikawa, a Japanese who was perhaps on his way from Europe to Japan, and had stopped in Cornell for a while, who had published some work on the application of the theory of space groups to the determination of the structures of crystals and who taught Wyckoff. Wyckoff carried out his first structure determination on cesium dichloroiodide, around 1918, 1919, or '20, and then went ahead to do a great number of structure determinations. In 1922, I think, he brought out his book, Analytical Representation of the Results of the Theory of Space Groups, published by the Carnegie Institution of Washington. So this book was available, and when I had just started doing x-ray crystallography, a little later, it was very useful.

Dickinson said, the first thing to do is to determine the size of the unit cell by taking x-ray photographs and analyzing them. It is never possible to be absolutely sure that you know what the size of the unit cell is. You take, let's say, a cubic crystal of some substance. From reflection from a cube face, you find dimensions of possible unit cells. For example, the unit cell might be three angstroms on edge, a cube three by three by three, or six angstroms on edge, or nine angstroms on edge, or twelve angstroms on edge. All of these would explain the observed reflections. So you look carefully. If you get no x-ray diffraction maxima that require a unit larger than three angstroms on edge, you assume that the unit is three anstroms on edge. There is still a possibility, however, that the actual unit is twice as large linearly--that's eight times as large in volume--or even larger. And that possibility ought to be kept in mind. Then, he said, you look for the reflections, absences of reflection, that are characteristic of a cubic cell based on a body-centered lattice or a face-centered lattice. If you have some reflections of the sort that are not permitted by those, then you assume that it's a simple cubic lattice. If you observe absences of the reflections of the sort required by a body-centered unit, then you assume that the unit is body-centered. But there's the possibility that some of these reflections are really there, but are too weak to have been observed. So you have to be careful there. You have to recognize that you are making an assumption. So, then, there are also absences characteristic of different space groups. So you check again, to see what the space groups are that are compatible with the x-ray observations that you have made. And here, again, it may be that your decision about the space group is not the right one because the reflections may be occurring but be too weak to have been observed. And then you apply the theory of space groups and calculate the number of atoms in the unit from the density and size of the unit, and look to see where the atoms are located. Then you can calculate the intensities of the reflections and throw out some possible ways of introducing the atoms. If there are parameters, you vary the parameters, too. And so you eliminate as many structures as possible. Then, you may be left with one structure, and you say that this is probably the right structure, with these various reservations that I've mentioned before.

The Laue technique, in particular, was used by Wyckoff and also by Dickinson. Wyckoff spent a year in Pasadena. He was working then for the Carnegie Institution of Washington, the geophysical laboratory supported by the Carnegie Institution. I didn't see Wyckoff for some years, I think not until 1930. He and Dickinson, I judged, didn't get along very well with one another, perhaps not on scientific matters. My understanding is that Dickinson and Mrs. Dickinson were upset about the way Wyckoff treated his first wife. The Wyckoffs were divorced afterwards and she married another scientist. Wyckoff is still alive. He spent a year, '21 to '22, in Pasadena. So he is a person who would have something to say, I'm sure, about the Institute in those days.

The Institute was, for a number of years, essentially the only place in the United States where x-ray crystallography was being pursued. And pretty soon, I think we could say, it had to be ranked with the leading departments in the world in which x-ray crystallography was pursued. Almost the only chemistry department--most of the others were physics departments. Odd Hassel in Norway carried on x-ray crystallography in the chemistry department in Oslo. V. M. Goldschmidt in Norway, and then later in Göttingen, was a geologist. He was using x-ray crystallography for studying geochemistry. He trained

Zachariasen, who then came to the United States and was professor of physics in Chicago. About 1930, after the geology department had been set up at CIT in 1928, I suggested to Noyes and Buwalda, the head of geology, that the Institute offer Zachariasen an appointment, half time in chemistry and half time in geology. Buwalda would have none of it. Buwalda was a pretty old-fashioned geologist. Chester Stock felt that he held the department back. Chester, I think, was more open-minded when he was the chairman, for a short while, of the Division of the Geological Sciences. But, of course, in this last survey of graduate schools, 1983, Caltech came out well in every department, essentially--tied for first place with three others in chemistry, tied for first place with three others in physics, and it was in first place by itself in geology, with Harvard and Berkeley and Princeton, perhaps, tying for second place. So geology at CIT has changed a lot since the time of Buwalda.

I learned a great deal from Dickinson. I can remember, along about 1925, perhaps even a little later, '27, riding in an automobile with Dickinson, and he was talking about what I was doing in x-ray crystallography and perhaps other fields. He said he thought it was worthwhile for me to be doing the things I was doing, but that he could never do them. Of course, his nature was such, I think, that he liked to have everything proved as rigorously as possible, and to know just where he was. Here I was guessing crystal structures, complicated ones, that the available techniques didn't permit determination of; I would guess a structure, and then I would check it by calculating the intensities, and if they agreed, I said, "Well, I found this structure." I built up a body of knowledge, starting right in 1922, about atomic radii and other properties of crystals, as determined by x-ray crystallography, or even a background of chemical information, that permitted me to start formulating structural chemistry into a coherent whole. As I said, this wasn't the sort of attack on science that Dickinson felt he was capable of or was interested in. He was a very able, clever man. So I owe a great deal to him.

Greenberg: There's a wonderful story about your not being permitted to set foot on the Berkeley campus.

Pauling: Yes. I don't know the whole story. I've heard only parts of it. G. N. Lewis turned up about 1925, perhaps when I was finishing my doctoral work. He came to a seminar that someone gave. I may have talked with him a little bit; I had seen him in Berkeley briefly. And then he went away. That's the only time he was in Pasadena while I was there. He had been there earlier because I have a photograph from the Archives of him sitting on the running board of Noyes's car--I reproduced that in my paper on Noyes. Years later, only a few years ago, I heard that G. N. Lewis had come down to offer me an assistant professorship, and Noyes had told him that he shouldn't do it. Since Lewis had been a pupil of Noyes's in a sense, and was assistant director of the research laboratory in physical chemistry that Noyes had founded at MIT and was director of, I think Noyes probably could exert enough moral pressure on Lewis, perhaps by just saying that he wanted me to be at CIT.

Greenberg: Well, in fact, beginning in 1929, you did for the next five years lecture for a term each year at Berkeley.

Pauling: Yes, that's right. There was something else that happened that you probably know about. I applied for a National Research Council Fellowship in the spring of 1925. It was required that you move to a different school, and I put down that I would go to Berkeley. In the fall, after I had begun my term on the fellowship, Noyes said, "Here you are. You have a lot of material that you haven't written up for publication. It would be better if you were here where you have your x-ray apparatus than up in Berkeley where they don't have x-ray apparatus. So why don't you stay here long enough to get these papers written for publication?" Well, Noyes had been involved in setting up the National Research Council Fellowships, so I just assumed that this was all right, so I did. So then he introduced me to [Frank] Aydelotte; he and Millikan took me to lunch with Aydelotte in 1925 or early '26, at the old faculty club which was in the orange grove, which is where the student houses and the present faculty club are now--it was an old farmhouse right in the middle of this orange grove. I think that's the only time I ever ate in that faculty club, that time with Aydelotte. I think, now that Aydelotte was considering whether I might be appointed a Guggenheim Fellow in the first batch, before they had announced them--when the committee just selected a few people, perhaps ten, I'm not sure. I learned about this only later. There was discussion about my applying in the spring for a Guggenheim Fellowship for the following year. Along about January, 1926, Noyes said I should apply for a Guggenheim Fellowship. He believed in the importance of going to Europe, shown by his support of the junior travel prizes at CIT for many years. So, then, I applied to the Guggenheim Foundation, to go to Europe. I wrote to Bohr and to Sommerfeld for permission to come, which was required by the Guggenheim Foundation. Sommerfeld answered my letter, but Bohr didn't. So that's why I went to Munich with Sommerfeld, which was a fortunate thing. I went later, for a month, to Copenhagen, and recognized how valuable it was for me to have been in Munich. At any rate, Noyes then said, "You should go to Munich in February, resign your National Research Council Fellowship at the end of February, and go over to Munich. You can go up to Oregon, see your folks, and then travel across the country." He had worked out just what we would do--arrive in Naples about the first of April, and spend the month of April traveling up through Italy. And arrive in Munich at the end of April. And I said, "But the fellowships won't be decided until April, announced the first of May." And he said, "Well, I'm sure you'll get the fellowship; and the Institute will give you fifteen hundred dollars, so you can buy the tickets and be supported." So, that's what happened. I wrote the man in charge of the National Research Council Fellowship, resigning my fellowship after six months. He was very angry, as he wrote me a very strong letter, saying this was unethical, improper of me. Here I had kept someone else from having a full year of the National Research Council Fellowship. Only later did I realize that Noyes was determined that I wouldn't go to Berkeley at that time. So I didn't. I'm sure that he had schemed this out, to keep me from showing up at Berkeley.

Greenberg: During those five years, when you were moving back and forth, did you sense any animosity, or did you have a feeling that the two institutions were competitors, or anything like that?

Pauling: No, I don't think so. I have a feeling that Lewis was resigned to my staying at CIT by that time. And I'm sure he was fond of A. A. Noyes. Something Tolman said about Noyes suggested that Tolman wasn't so fond of A. A. Noyes. Of course, Tolman was a professor when Noyes was running the department. I was a professor, too, but never got into any arguments with Noyes. I just accepted anything that he did. It didn't occur to me to come into conflict with him. I have a feeling that the relations between Berkeley and CIT were good ones. Of course, Berkeley and CIT competed for [J. Robert] Oppenheimer. Oppenheimer at this time was spending two-thirds of his time at Berkeley, and one-third at CIT. He came down in the spring.

Greenberg: In fact, the competition in nuclear physics, between the nuclear physics groups, was rather keen in the thirties.

Pauling: Yes, I can believe that. But so far as I was concerned, there was no structural chemistry at Berkeley, because there was no x-ray crystallography; little molecular spectroscopy being done, and of course general physical chemistry, and thermodynamics, but practically no structural chemistry.

One of the first theoretical papers that I wrote was with Tolman. I gave a seminar talk at CIT on a paper by Erman Eastman, who was in Berkeley. In his paper on the entropy of crystals and super-cooled liquids, he suggested that a complicated crystal would have residual entropy at the absolute zero.

Begin Tape 1, Side 2

Pauling: So I gave a seminar on Eastman's paper about the entropy of crystals and super-cooled liquids at absolute zero, and presented a statistical mechanical argument to show that Eastman was wrong, that a complicated crystal would have no more residual entropy than a crystal with a small unit of structure--that is, Eastman had said the larger the unit of structure, the greater the residual entropy. It was just erroneous. And Tolman said that I should attack this problem by the method used by [Paul] Ehrenfest and [Victor] Trkal. So I looked up

Ehrenfest's and Trkal's paper, and wrote out a discussion and gave it to Tolman. And Tolman rewrote it; and this was published as a paper by Tolman and me. My name was put first on the paper. Later on, of course, I discovered the theory of the residual entropy of ice, a very interesting problem, after [William F.] Giauque in 1935 had determined that ice had a residual entropy at the lowest temperatures that they could measure. I worked out the theory of a sort of partial randomness in positions of the hydrogen atoms; and later, I worked on other somewhat similar cases of residual entropy.

So that's the only paper I published with Tolman. I got a great deal out of Tolman's courses, starting in the first year, when he gave a course in 1922-'23, I think, a course on the general principles of science--a very interesting, sort of philosophical and practical course, too; practical in the way of how you think about theoretical problems in chemistry and physics. And then, of course, I learned a tremendous amount from the seminar in the chemistry department in quantum mechanics--or, while I was a graduate student, quantum theory. So I really had mastered that field and was ready to learn quantum mechanics when it came out.

Greenberg: This would have been [Paul S.] Epstein who taught you the quantum mechanics before you went to Europe?

Pauling: No, it was in the chemistry department. It was Tolman's seminar in chemistry. And then, I also studied thermodynamics for a year, using Lewis and Randall's book as a textbook. I think Tolman may have been running that seminar, with other people contributing. But it was the course in statistical mechanics that Tolman taught, that I found most interesting and valuable, and I still make use of statistical mechanical ideas. I've just written some papers on metals, in which I make use of these ideas, relating in this case not to a temperature equilibrium but rather to resonance structures, covalent bonds distributed over a large number of positions in the crystal.

Greenberg: How early did you know that you would succeed Noyes?

Pauling: Well, first, I think in the late twenties and in the thirties, it didn't occur to me that Dr. Noyes would ever die. Actually, about 1922, there was a report published in the Pasadena paper that Noyes had died. Did you know that? I have a letter from my wife; I know it was the year '22 to '23, because we were married in June of '23, and this is a letter I had written her saying that Noyes had died. But it turned out that this was a false statement in the newspaper.

Greenberg: He was very ill at one point, I think.

Pauling: Well, he had caught a bone in his throat in 1922. And a year later, when I was on the desert trip with him, and perhaps one other graduate student, and Ellis, I think, in his old car, that same old car, we went out to Palm Springs and camped on the sands, he got sick in the early evening and was taken into the hospital, I believe, in Palm Springs. So he apparently did have some sort of trouble with his throat that I didn't know much about.

In 1929, when I was twenty-eight, I was offered a professorship of chemistry at Harvard. When the first Nobel Laureate in chemistry in the United States, Theodore William Richards had died, Harvard started looking for a replacement. If my memory is right, they offered the job first to G. N. Lewis, and he turned it down. Then they offered it to Tolman; he turned it down. And then they offered it to me; I turned it down. Actually they first offered an associate professorship to me. I went back and stayed for a week with Conant, who was head of the chemistry department. Conant said that they would give me a full professorship, but I turned it down. And then they offered an assistant professorship to Badger, and he turned it down; and then they got [G. B.] Kistiakowsky to come. And it may be that about that time, I got to thinking, well, in the course of time, I'll be the chairman of the Division of Chemistry and Chemical Engineering at CIT. But I didn't think very much about it.

Along about that time, 1930 or '31 perhaps, Noyes announced that I was executive secretary of the Division of Chemistry and Chemical Engineering. I didn't ever do anything, because he didn't turn anything over to me, and I didn't take any action, and this position just died out. I'm not sure it was ever mentioned in the catalogue. But he, I judge, was thinking of preparing me to be the chairman of the division by giving me some experience.

Greenberg: We've come across references to the effect that Noyes, perhaps because he was one of the founding fathers of the Institute, sometimes put the good of the Institute above the development of chemistry at the Institute.

Pauling: Well, it may be. I'm sure that Noyes felt strongly about the Institute as a whole. I would say he determined the nature of the Institute. Millikan was brought in later. Hale, of course, was the one who had the idea and got Noyes to come out in 1913. Hale was a trustee of Throop College. So Hale had the idea. And Noyes determined what the Institute would be. And Millikan was the front man, who I think came in '21. Millikan did a job that Noyes couldn't have done; it wasn't in his nature to do it.

Greenberg: By the mid-thirties, it must have become pretty clear that somebody else would be taking over, probably you. Did it become more difficult to work with him, or was it harmonious right up until the end?

Pauling: Well, I never had any troubles with Noyes at all. In 1929, I think, my wife said that we ought to have more money. And I asked Noyes for an increase in salary--I think the only time I have ever done that. So my salary was increased from $3000 to $3500. [CIT records indicate $4500, 1929-30; $5000, 1930-31; 5/7/30 salary advanced to $7000 per annum.] In 1930, I got notice that my salary was increased to $7000 a year. That was astonishing. Well, at about that time MIT wanted me to come as head of the department of chemistry. I turned them down. There was only one time, later on, when I used an offer of a job as a bargaining chip. I never bargained; I just turned down jobs. So I got $7000. And I've decided that what happened was that Bill Houston and perhaps someone else had been offered jobs elsewhere, and that Millikan had arranged that they got $7000. But I didn't know that at the time. I just wasn't interested in these things. I didn't ask myself, then, how does it happen that I've got an increase from $3500 to $7000--these numbers may not be exactly right, but I think they're approximately right. So I think that's what happened, that Noyes probably stood up to Millikan and said, "If you give them $7000, you'd better give Pauling $7000, too."

Greenberg: Noyes died, and you weren't pall bearer at his funeral. Is there anything there?

Pauling: Well, I know people thought it was odd that I wasn't a pall bearer. The feeling was that there was a bit of jealousy about it, and that I was pretty young and that the older people should be pall bearers. I don't remember who the pall bearers were. It wasn't something that bothered me. I don't think it was in my nature--I'm not an achiever in that sense, you know, in this personal sense. Well, I'm an achiever in the sense that I like to do things, to get things done, but advancing my own interests isn't something that concerned me. I think my wife was somewhat upset by my not being a pall bearer; it didn't bother me. Of course, there was about a year before I was made the chairman of the Division of Chemistry and Chemical Engineering, and director of the Gates and Crellin chemical laboratories. There was one point when things just went on; the division ran itself--no trouble about not having a head. There was one point when I was told that I could be appointed chairman of the Division of Chemistry and Chemical Engineering, but not director of the Gates and Crellin laboratories. I said I would not accept that job. This was at a point when I decided that I would say what I believed. And I suppose there was some question: should Tolman have these jobs? Tolman was made dean of the graduate school. I think we hadn't had a dean of the graduate school before, so he was made dean of the graduate school; and I think that that was a sensible solution of this problem. After all, I was a very active person in the division, teaching and carrying on research and directing research, and I had built up structural chemistry to a place where the Institute was the leading institution in the world in the field of structural chemistry.

Greenberg: Let's turn to T. H. Morgan, who arrived in the late twenties.

Pauling: '29. [Hired in '27, arrived in '28.]

Greenberg: Did the merger between biology and chemistry at Caltech begin with T. H. Morgan?

Pauling: You see, I wasn't involved in decisions. T. H. Morgan arrived with [Alfred A.] Sturtevant and Bridges, and [Albert] Tyler, who was a graduate student, and Sterling Emerson, postdoctoral I guess, and other people. I soon became friends with them and began talking with them and doing things, to some extent, together with them. He also brought in [Henry] Borsook as professor of biochemistry. I had nothing to do with this. Whether there was any discussion as to whether biochemistry should be in biology or in chemistry, I just don't know. And [Arie J.] Haagen-Smit, I'm not sure what his professorship was, but he was a bio-organic chemist. And [C. A. G.] Wiersma and [Anthonie] Van Harreveld were physiologists, no question about that. I don't remember discussions about the relation between biology and chemistry. By 1935, I was working on biological, or biochemical problems. Well, in 1929, shortly after Morgan and his crew had come, I began thinking about the problems they were working on. I gave a seminar in genetics on crossing over. Someone, perhaps one of them had asked me to read some papers by a Hungarian on a theory of crossing over, and so I developed my own theory of crossing over and gave a seminar on what this fellow had written and what my own ideas were. I didn't publish this. I developed a theory of vision, scotopic vision, which I think had something new in it. I wrote about it to a man in the field of vision in New York, and he published it later on without mentioning me [laughter]. So it had a really good idea in it, that would have occurred to a physicist and not to a biologist probably.

The question of biological specificity began to interest me in the early thirties. Tyler was working on substances present in sperm and eggs that seemed to react with one another. And Morgan was working on self-sterility in the sea urchin, I believe. I kept asking, "How can these things occur; what is the possible molecular mechanism?" But then I got to work on hemoglobin and published my first papers on hemoglobin. I went to New York and gave a seminar at the Rockefeller Institute for Medical Research, in 1936. And at that time, I asked the director, Simon Flexner, to send Alfred Mirsky and his family to Pasadena to be with us for a year, because of my interest in hemoglobin. So Mirsky came. Mirsky was astonished that I would have the temerity to approach Flexner--I was a brash young man, I think--and then astonished that it worked out!

I was working on hemoglobin, gave this seminar, and was asked by Karl Landsteiner, who had discovered the blood groups in 1900, if I would come to his laboratory the next day and talk with him about something. I went, and he told me what experiments he had been doing and asked how I would explain them. And these involved biological specificity, the interaction of antigens and antibodies, or haptens and antibodies, and antigens. I was much taken by this; I thought, "This is a fine example of biological specificity and I ought to understand it." For four years, I strove to understand it. Actually, in the fall of 1937, Landsteiner came up to Cornell, when I was Baker Lecturer there, and spent several days with me. We were talking about immunology. He was giving me a course, essentially, in immunology--better than any course, because he would tell me about the experiments and contradictory experiments, and which ones were more credible. I thought about this question for four years, and in 1940 published my paper on the structure of antibodies and the nature of serological reactions. And, of course, that same year Delbruck came to me with a short paper that Pascual Jordan had published in Germany, in which he said that the quantum mechanical phenomen of resonance, stabilizing certain systems, would permit a molecule to catalyze the formation of a replica of itself because of the special interaction between two identical structures. We [Delbrück and I] published a note in <u>Nature</u> saying that this can't be true because this quantum mechanical effect is too small; this isn't the explanation. Instead, molecules would tend to catalyze the formation of other molecules that are complementary to them in a detailed way. And the gene consists of two mutually complementary strands, each of which can act as the template for the synthesis of the other. I incorporated this in my lectures, which both [E. C.] Watson and [Francis] Crick heard, about complementariness as the basis of biological specificity. And for eight years, my students and I carried out experiments on interaction of antibodies with antigens and haptens to prove that this was right, so far as these immune systems are concerned. We proved every point, one after another, as to how the various interactions operate, how close together the molecules have to be in and are, in fact; how great the degree of complementariness is.

Greenberg: In his oral history, Delbrück in fact gives you credit entirely for that paper. It was your paper.

Pauling: It was my paper. But Delbrück had brought it to my attention. I thought it was proper to have him as a coauthor. So by 1948, I thought I must write a book about the nature of biological specificity, the molecular basis of biological specificity. And I've collected a pile of materials this high, and now I'm planning to write it. But I've changed my concept of the book. Instead of going through carefully all of the papers, all of the work that we did, describing it all, I thought I would make it as a sort of partial biography--what I was thinking at different times along here, how we had the ideas, and carried out the crucial experiments. And so now we have proved, I think, that molecular complementariness is the basis of life. So I think I shall write my book and call it, The Nature of Life.

In the books on biology, genetics, they mention complimentariness, but very little. They don't point out that this is the basis of life. All specificity in biological systems results from complimentariness in structure.

Greenberg: Were you closer to Beadle than you were to Morgan?

Pauling: I didn't publish a paper with either one of them. I didn't discuss things with Morgan very much. He was of the older school. I discussed them with Albert Tyler more than with anyone else in biology. He was Albert Titelbaum when he arrived, you know. And after a few months, I think he and his brothers decided to change their name.

Albert was my closest contact in biology. Beadle, well, I talked with Beadle quite a bit. And, of course, I suggested that he be brought back from Stanford as the chairman of the Division of Biology. I think that's known, isn't it? I remember when I talked with them and said that I thought he would be a good man to have as chairman of biology.

Greenberg: So, in fact, you had gotten to know him when he was a research fellow in the mid-thirties at CIT?

Pauling: Yes, that's right. And I'd even seen him here at Stanford, the times when I was up at Stanford on short visits. So Beadle and I didn't discuss theoretical biology very much. My close affinity is mainly with theoretical physicists, to some extent theoretical chemists. There aren't very many theoretical biologists. So Beadle and I talked about developing biology and chemistry, and we made our joint application to the Rockefeller Foundation. In general, we got along well. In the Institute, we didn't struggle against one another to increase our budgets, one at the expense of the other. In fact, in the Institute the handling of the budget was really remarkable. We didn't strive to spend the budgeted amounts. One year we would allow them to be held over, and they weren't even held over formally; we just allowed them to lapse. We asked the Executive Committee to allocate funds to us that we could operate on conveniently. And we didn't strive the way that people often do to get the most out of the administration. And, of course, we were members of the administration in that we were members of the Executive Committee. But, of course, when Millikan retired, there was the need to get a front man. You have to have a president or the equivalent. Millikan was willing to do it as chairman of the Executive Committee. I think Noyes probably put this over on Millikan originally.

Greenberg: I want to come back to Millikan in a minute. But first, I'd like to ask you what Warren Weaver was like, and could you have done all that you did without the help of the Rockefeller Foundation?

Pauling: Well, Warren Weaver was really a fine man. He, of course, was at the Institute before my time.

Greenberg: When it was Throop College?

Pauling: Yes. He was there, in 1920 or something like that. He had written a book with Max Mason on electromagnetic theory when he was at Wisconsin. I think Warren Weaver was responsible for a lot of the development of science in the United States. I don't remember when I first met him, probably not until '33 or '34. About 1931, I applied to the American Geological Society, the Roebling Fund, for a grant of $3000 perhaps, to permit me to have an assistant and additional apparatus for determining the structure of the sulfide minerals. They turned me down and said that they didn't give money for apparatus. So I removed that and applied again, and again was turned down--actually, just ignored. So I made out an application for I think $5000, perhaps $10,000, to the Carnegie Institution of Washington and took it Dr. Noyes. He said there's no use applying to them, but he would talk with Millikan about applying to the Rockefeller Foundation. So I got the $5000 from the Rockefeller Foundation, and studied sulfide minerals, and the next year got $10,000. And something else happened. Millikan had put in an application along with my application for money to support [Alexander] Goetz in the physics department. And they got the money. Warren Weaver told me afterwards that this money had been misused, and that the Rockefeller Foundation would not give any more money in physics to the Institute.

Greenberg: And [Fritz] Zwicky's money was also terminated.

Pauling: Well, it may have been Zwicky and Goetz that were in solid state physics. I didn't know what was going on until later, when Warren Weaver told me this. Warren, I guess, then said the Rockefeller Foundation wasn't interested in the sulfide minerals. But they were interested in biology. So I put in an application for some money to study the magnetic properties of hemoglobin, and got a good amount. And every year, they kept increasing the amount of money that they gave to me.

Greenberg: So you did write grants, to some degree, in function of what the Rockefeller wanted to sponsor?

Pauling: Yes, well, you see, I had been developing an interest in biology anyway. I wrote a paper about the oxygen equilibrium curve of hemoglobin. I'd been wondering about how the oxygen molecule is attached. I knew that oxygen is in a triplet state, has two unpaired electrons--dioxygen. And I thought, if it forms chemical bonds with the iron atom, presumably, in the hemoglobin, it would lose these odd electrons; they'd become paired. But if it's just held by physical forces, which was one idea current at that time, then it would presumably retain its magnetic moment. So I'll measure the magnetic susceptibility and find out. And that was what I applied for. And, of course, I got a surprise--one of the few times when I've made a discovery that I hadn't anticipated. The iron atom changed from a high-spin state in hemoglobin to a low-spin state in oxyhemoglobin. This was a surprise to me, but it was a great discovery. We were able to publish about twenty papers describing our researches on a large number of hemoglobin derivatives, using the magnetic technique. And, of course, [A. H. T.] Theorell came about 1939 to work in our laboratory for a month, learn the technique. He discusses this in his Nobel lecture. When he got the Nobel Prize [1955] later on, he describes this part of the work for which he got the Nobel Prize. So that was a real contribution to protein structure. Then, Dorothy Wrinch had an idea about protein structure. The Rockefeller Foundation asked her to come to Cornell in 1937 and talk with me. I had her give a seminar; I talked with her. I wrote a rather critical report to the Rockefeller Foundation about her. And when, a year or two later, she published a paper with Irving Langmuir, I decided the time had come when we should speak up in print. So Carl Niemann and I published a paper on the structure of proteins, in which we presented arguments as to why proteins consist of polypeptide chains, as had been thought from the time of Fischer on, and did not have these special cydol structures that Dorothy Wrinch had proposed.

So then, in 1937, I decided that I should try to find how the polypeptide chains were folded in proteins. And that's, of course, a long story. I should have found the alpha helix in 1937. The fact that it took eleven years is, I think, pretty significant. Why didn't someone else discover the alpha helix in those eleven years? Why didn't I discover it in 1937? I just didn't think hard enough about it. My wife said, about Watson and Crick--and the double helix of DNA--"If that was such an important problem, why didn't you work harder at it?" [Laughter]

Well, I could have discovered the double helix if I had spent more effort. There's also something else I say about this. Part of the reason for not making such a discovery that involves thinking, is that you don't know whether it's going to be possible to make it or not. It's just like the atomic bomb. We gave away to the Soviet Union, the secret, which is that you can make the atomic bomb.

Greenberg: I want to talk about Millikan and your relationship with him. I know you didn't like his courses.

Pauling: Well, I went to only one course of his, and it wasn't of the caliber of the others. But I can understand that, too. He had other things to do, and rushed in at the last minute to give his lectures.

Greenberg: When you became a member of the faculty, did you have a relationship with Millikan?

Pauling: No. Not until a year after Dr. Noyes's death, when I became a member of the Executive Committee; at that time I had a little interaction with him in regard to the budget. He was irritated when I said that in most universities, the chemistry department has a bigger budget than the physics department [laughter]. He didn't like that.

Greenberg: Can you compare the administrations of Millikan and [Lee A.] DuBridge?

Pauling: I don't think so.

Greenberg: Did you prefer one to the other?

Pauling: Well, I was already having trouble with the administration about the time that DuBridge came in, because of my political activities.

Greenberg: Was there a change in the ambiance, or the atmosphere, with the change of administrations? Did it become a different place?

Pauling: I don't think that I can say that it did.

Greenberg: Was it cozier in the time of Millikan than the time of DuBridge?

Pauling: I don't think there was a discontinuity, no. It became a little more formal, perhaps.

Greenberg: We'd like you to give us your view of the role of research laboratories on teaching campuses, organized like government or industrial laboratories.

Pauling: Well, I'm opposed to them, as I like the old-fashioned way, in which the professors teach courses and also take part in research and have graduate students working with them. At Berkeley, nearly every member of the department would have a freshman section to handle.

Greenberg: I've heard that at the end of your Caltech career, some of your colleagues felt that your laboratory was somehow an exception to the general rule on the campus. You'd brought in people from the outside.

Pauling: Well, I think that for my work in structural chemistry, we were operating in the old-fashioned, university way, where the people involved--there were a lot of postdoctoral people, of course--but the members of the department who were structural chemists also taught courses, and had contact with students. I had, however, another operation going. And the Institute tried to cause trouble. In fact, part of the reason for my leaving was that. This was my interest in chemistry in relation to medicine. One suggestion that I made was that there be a physician in a chemistry department to help in the application of chemistry to medicine. And this is aside from my own operations, which dealt with anesthesia and mental illness--not with vitamins; that came after I left the Institute. So I felt that the trouble that I was having with the Institute about my medical research may have been in part the result of the opposition to my political activities, and not so much objection to my doing medical research at the Institute.

Greenberg: Were your colleagues pleased when you won the Nobel Prize for Peace?

Pauling: I probably pretty much ignored them. [Tape ends]

Begin Tape 2, Side 1

Pauling: I perhaps ignored what my colleagues in the chemistry department said or decided. But I've heard recently that the chemistry department decided that they wouldn't have any celebration at my having got the Nobel Peace Prize. It was the biologists who arranged something. I don't even remember exactly what was done. But this is what I've heard. But, of course, it was DuBridge who caused me to decide to leave the Institute. I had pretty much decided already, because Jack Roberts as chairman of the division had told me that I should stop my work on the chemical basis of mental disease and liberate those laboratories by the fall of 1964--this is in the fall of '63 that he told me that. And I said, "Well, I'll give up my work on anesthesia," which was occupying a couple of rooms, "and I'll move out of my office into a small room next door so that this big room will be vacated," instead of stopping the work on mental illness. Well, that's what I said to him. What I thought is, "I think I'd better just leave the Institute if they're going to put pressure on me in this way." Moreover, I remember back at Chicago, the department had a lot of trouble with old [W. P.] Harkins. When he became emeritus, he wouldn't give up the large amount of research space that he had, so they had trouble with him. In general, you have trouble. [László] Zechmeister was angry with me because the division had decided that he should give up his big research laboratory when he became emeritus. So he was angry with me, as though I were the boss who determined what the division would decide. Of course, I wasn't. I let the division make its own decisions, in general.

So I was thinking about leaving. In fact, I was offered a job in anesthesiology at UC San Francisco. I think I had written that I was accepting it--anesthesiologists thought a lot of me at that time. Then I was notified that the UC administration hadn't approved the offer. I was in bad with the administration, with the Regents of the University of California at that time. So here I was. On the 10th of October 1963, I got word that the Norwegian Nobel Committee had awarded me the Nobel Peace Prize. The Los Angeles Times published DuBridge's statement, that "it's pretty remarkable for a person to get two Nobel Prizes, but there's much difference of opinion about the value of the work that Professor Pauling has been doing." So I decided that I would leave the Institute. Of course, I have such fondness for the Institute that I had a press conference and said that I was going to Santa Barbara because now that I had the Nobel Peace Prize, I felt an obligation to work for world peace. I didn't say that I had finally given up with DuBridge and the Institute. Some years earlier, he had called me in and said that the trustees didn't understand why they couldn't fire me.

It wasn't until later that I learned that there had been a committee set up by the Board of Trustees to investigate me. Beadle was the chairman of it. He told me about this committee, but not until quite some years later.

Greenberg: You were there for more than forty years; obviously you have feelings for the Institute.

Pauling: I was there forty-two years, I was away one year in Europe; but forty-two years, in that I quit in October of 1963, but I didn't really leave until June of '64. It took me eight months to wind things up and get out.

Greenberg: Did you have regrets, or were you philosophical about it all?

Pauling: Well, surely I felt it was too bad. My wife didn't want to leave our home there, or the children; but they'd been having trouble, too, of course, at Polytechnic School. And I didn't want to leave. I'd built up this great research organization in structural chemistry, and I had discovered molecular diseases there at the Institute. I was happy except for this continual pressure from the administration. I resigned as chairman of the division in 1957. Actually, a couple of years before, I had approached DuBridge and said that I thought I ought to resign as chairman of the division, and he asked me to stay on. But then he said that the Board of Trustees couldn't fire me as professor, but they could remove me as chairman of the division. So I said, "All right, I'll resign as chairman." That was '57, I think, that I resigned as chairman of the Division of Chemistry and Chemical Engineering. I didn't mind that; things were going well. I didn't feel that I had done any harm to the Institute by ceasing to be chairman.

Greenberg: Is there something about Caltech that caused this to happen, or could this have happened just as easily at that time on some other campus?

Pauling: Well, it might well have happened somewhere else. I think Tolman had lost his job at Berkeley in 1917 because of his pacifistic sentiments; and that's at Berkeley. Berkeley in '17 wasn't the way it is now! It was a smaller place. The Institute is essentially science and engineering, and that means, just by its nature, less liberal than a university that has the humanities and people who think about these broader questions more, even in a professional way, which nobody, or very few people at the Institute do. So you can understand this greater conservatism. Then, the Board of Trustees consists essentially of businessmen; they tend, of course, to be conservative. So this could have happened somewhere else, but there were also reasons for its being more likely to happen at CIT. And DuBridge wasn't the sort of president to educate the trustees. It might be different here at Stanford. When

Ken Pitzer--who you would think would be a pretty conservative fellow from a well-to-do family in southern Californa--was the president here, he was so thoughtful and rational that the trustees couldn't put up with him. He could see the side of the students as well as that of the trustees. DuBridge isn't like that. DuBridge would just accept what the trustees told him to do. He was employed by them to do this job. DuBridge seems to think that he's a good friend of mine. I don't think he realizes what he did when he was president. Have you talked with him?

Greenberg: No. Your collaborator, Sidney Weinbaum, was imprisoned during the McCarthy era. Was he a scapegoat?

Pauling: Well, I really don't know.

Greenberg: We know that you tried to do something for him. Milton Plesset talks about this.

Pauling: Oh, yes. I raised a fund. I wrote to people and asked them to contribute to his defense fund, and gave the money to his lawyer--it probably went entirely for lawyer's fees. I think Sidney is still alive, I'm not sure. But Sidney was really a gifted man--one of these marvelous members of the intelligentsia. He was a fine piano player, and was chess champion of southern California; was interested in literature; rather impractical. I managed to get him his doctor's degree in physics. Here he was receiving $1800 a year, something like that. And when the depression came along and salaries were cut, he had his salary cut in half, whatever it was--it may have been $2400 cut to $1200--an impossible situation for him and his wife and daughter, who's a professor at Berkeley now. His wife lives over in Berkeley, too. Whatever Sidney was involved in--and he may have been guilty of perjury, I just don't know--I felt that he deserved my compassion; he deserved a proper defense. So I raised the money.

The only time that I received money from some source other than CIT over this long period was when I was a consultant for Lilly Labs, and got $5000 a year as a consultant, which was useful to me back in that period. I only had to go four times a year for a day to consult with them. When the paper mentioned Sidney being prosecuted, my job was terminated with Lilly Labs.

Greenberg: Because of your association with him?

Pauling: Yes. Well, I have a feeling that there were some people interested in what's called "left wing" ideas in the Institute during the Depression. And Sidney may well have been one of them. But I didn't have any idea. I didn't go to the trial, I never heard what the evidence was.

Sidney was in jail for four years. I think after he got out of jail, he got a job working as a bookkeeper for a man that I know in Hollywood. I didn't ever see him after he got out of jail.

Greenberg: Very recently, I saw the "Nova" program, and there's a clip in there from "Meet the Press" in the fifties. Watching Lawrence Spivak badgering you was very embarrassing. It was just an extraordinary thing.

Pauling: Yes, most people don't know that that sort of thing happened. When that program came to an end, Spivak took off down the hall, running as fast as he could go, with my wife after him, waving her fists. I guess she had a hard time restraining herself during the program. But he managed to escape [laughter].

Greenberg: But as usual, you handled yourself well in those kinds of conditions.

Pauling: I don't remember what they showed on the "Nova" program, but at one time, Spivak said, "Did you appeal to President Eisenhower for those convicted spies, Julius and Ethel Rosenberg, those people who were convicted of spying against the United States?" I said, "What did you say, that they were convicted of spying? The Rosenbergs were not charged with spying; they were not accused of spying; they were not convicted for spying; they were not hanged for spying." They were convicted for conspiracy, you know. It's like the trial of Private Eddie Slovak, or something like that, who was executed as a warning to other soldiers.

Greenberg: The quantum mechanical theory of resonance arose just about the time that you were beginning to work on the theory of chemical bonding, and the idea of quantum mechanical resonance enters into it. When you say the quantum mechanical theory of resonance, are you talking about the Gamow, Condon, and Gurney theory of resonance of 1928, the resonances that later on will enable one to understand that intensity of radioactivity, or probability of penetrating the nucleus of an atom, does not always increase with increasing energy of incoming particles, but sometimes rises, then suddenly falls, instead?

Pauling: Well, that's one application of it. The theory goes back a year or two earlier. It was first discussed by Heisenberg in relation to the spectrum of helium, a two-electron system. He introduced the word "resonance" and mentioned the similarity to classical resonance, when you have a resonant frequency--two oscillations with nearly the same frequency. In quantum mechanics, if you have two wave functions that might be assigned to the state of a system and they are equivalent, then in general, you get an interaction, which is a sort of resonance interaction. The phase relations are quantized so that you get one stabilized structure and one destabilized structure. And this shows up in chemistry, for example, in the Heitler-London theory, where there are two electrons with opposed spins and two nuclei, and the electron with the positive spin can be either here or here [gestures], so that you have the two structures, and they, we can say, resonate to give a more stable state. That's the chemical bond. The antisymmetric resonance destabilizes it--that's one of the excited states. So I just generalized that, and said that if you can assign two structures with the same energy--as in benzene, the two Kekulé structures--then the symmetric state is the normal state, stabilized by resonance. The resonance phenomenon shows up everywhere in quantum mechanics, including radioactive decomposition.

Greenberg: So you got your idea from Heisenberg?

Pauling: Yes. Everybody who talks about resonance got it from Heisenberg's 1926 paper.

Greenberg: You mentioned somewhere that the resonance theory of chemical bonding could have been arrived at earlier.

Pauling: Well, chemists, in particular Sir Christopher Ingold in England, had been developing ideas about chemical structure that were something like resonance theory. They're discussed briefly in my book The Nature of the Chemical Bond. I give several references to papers about these early ideas, where you had a superposition of structures. But the effect of the resonance phenomenon on stability, the resonance energy, that follows directly from the Schrodinger equation, so that I and others were led to the idea that when you have resonance, there is the possibility of stabilization. This may have some classical analogue, but it's hard to dig it out of classical theory, hard to understand it classically.

Greenberg: When Lauritsen and Crane discovered resonances in proton capture in 1934, did you follow any of that?

Pauling: Oh, yes. Well, when you have the spectroscopic energy levels, when two levels cross one another, there's the possibility of a resonance interaction so that the curve comes down like this and then instead of swinging on up, it goes down again; and the upper one goes up [gestures]. So that's another application of resonance theory. Or with carbon dioxide, where you can bend the molecule this way--well, it's in stretching--one molecule of carbon here, oxygens can move this way, one bond stretches and the other compresses. And you get a resonance phenomenon there, which is called Fermi resonance. So this idea of quantum mechanical resonance shows up over and over again. And, of course, it's the basis for Pascual Jordan's statement that two identical molecules are more stable than two nonidentical ones should be [laughter]. Well, that's a misapplication of resonance theory. The idea's right, but the energy quantities are wrong.

About the Institute, I have said that I have been fortunate in my life from time to time. And one time was when I sort of by chance showed up in Pasadena. I've said, I don't think there's a place in the world where I could have got better training for what I've done, than there in Pasadena. The caliber of the professors was so high, the place was small, the number of graduate students was small. There was freedom from bias determined by an old past history--all of this made it a great place to work. It's not surprising that it's become pretty close to the leading institution in the world in science.

Greenberg: I was interested to see that you took Bateman's courses.

Pauling: Oh, yes. Well, I had a nice surprise. I had signed up for chemistry as my major subject, and physics as a minor subject. When I got my diploma here it said major in chemistry, minors in physics and mathematics. That was a nice present for me [laughter]. But I had taken not only Bateman's but also E. T. Bell's courses. Bell tried to get me interested in number theory. I worked a little while on somebody's polynomials. But chemistry I liked--and physics, too--so much that there wasn't much chance of my getting involved in number theory. I'm not interested in rigor so much as in effectiveness.

Greenberg: H. P. Robertson was a near contemporary of yours.

Pauling: He came a year after me; and he may have got his Ph.D. the same time; perhaps he got his Ph.D. in two years. He had been at the University of Washington; I think he came with E. T. Bell in 1923. We were very close friends. We worked through Whittaker and Watson together, mathematical analysis. CIT surely was a great place in those early days. It still is a great place. I was sorry to have to leave, but I felt that I had to.

I was rather disappointed by the Center for the Study of Democratic Institutions. I felt that not only were they not interested in science, but also they operated at too superficial a level. I like to work on a problem, and work on it and work on it, and they were satisfied to work a little on it and get out a report.