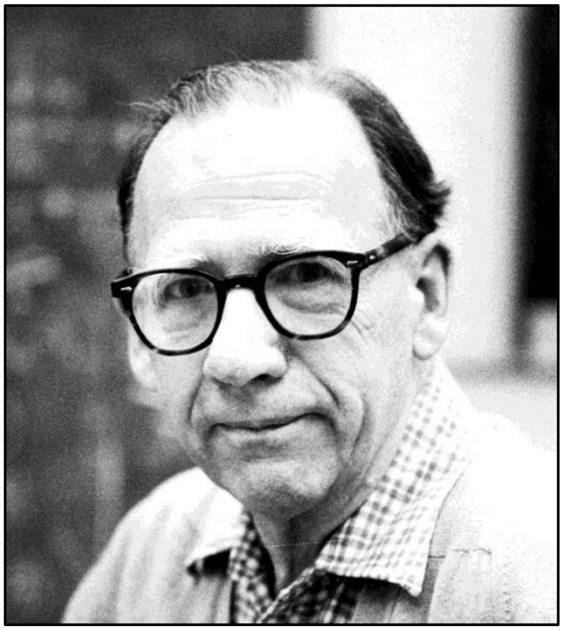

Edward W. Hughes, 1979. Photo by Floyd Clark

EDWARD W. HUGHES
(1904-1987)

INTERVIEWED BY
GRAHAM BERRY

November 20, 1979

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Chemistry

Abstract

An interview in November 1979, with Edward W. Hughes, senior research associate in the Division of Chemistry and Chemical Engineering. BS, Cornell, 1924; PhD, 1935. 1938, becomes research fellow at Caltech, working with Linus Pauling; teaches war-training courses. Postwar work for Shell Development Company; returns to Caltech as research associate in 1946.

He recalls the early days of crystallography in the U.S.; his good fortune to work with Sir Lawrence Bragg while still at Cornell; later work at Caltech with Pauling; defense of alpha helix before the Royal Society. Leeds lectureship. Discusses Pauling's part in the eventual discovery of DNA structure; Pauling's sponsorship of 1957 U.N. petition against nuclear testing. Recalls arrival of women as graduate students at Caltech. He concludes with remarks on his current

writing, on his wife's secretarial work for Pauling and as head of Chem Wives; and his participation on the chemistry division's safety committee.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Hughes, Edward W. Interview by Graham Berry. Pasadena, California, November 20, 1979. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Hughes_E

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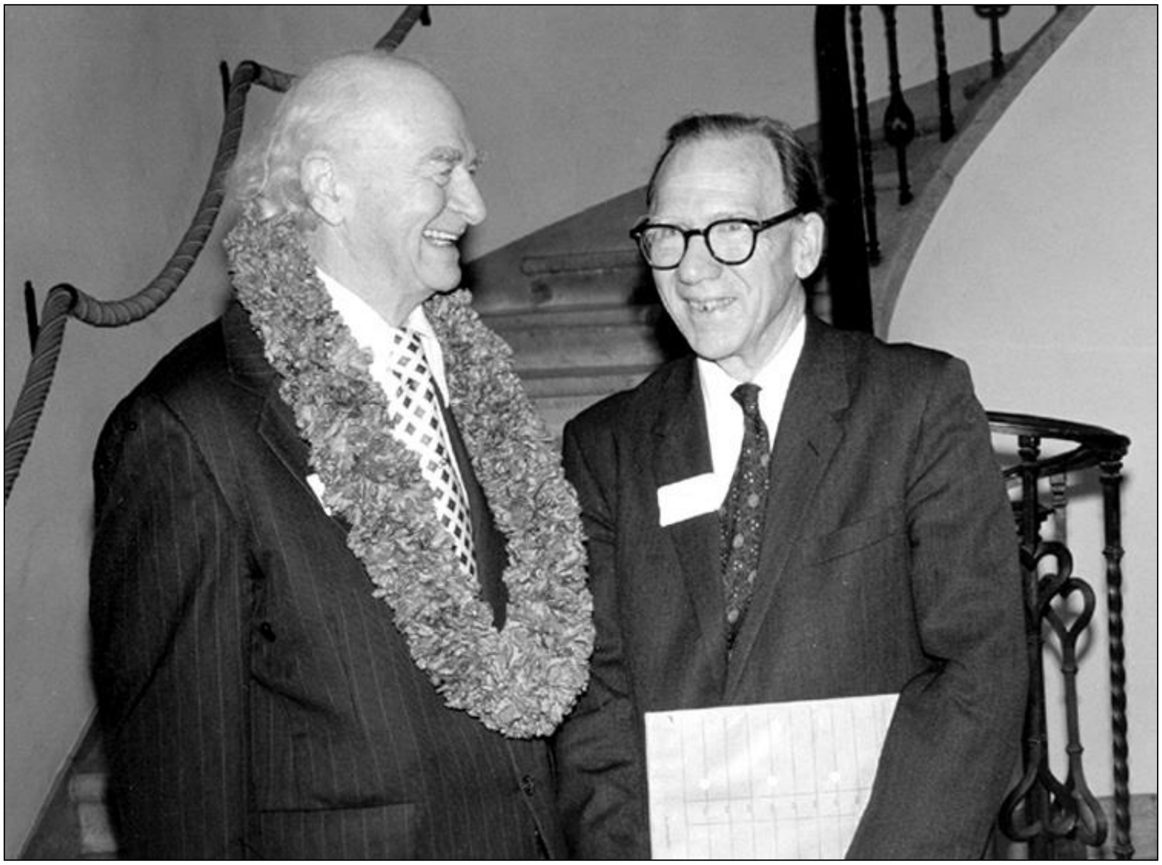

Linus Pauling (left) and Edward Hughes at Pauling's 75th birthday party, February 28, 1976, Caltech Athenaeum. Photo by Floyd Clark.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH EDWARD W. HUGHES

BY GRAHAM BERRY

PASADENA, CALIFORNIA

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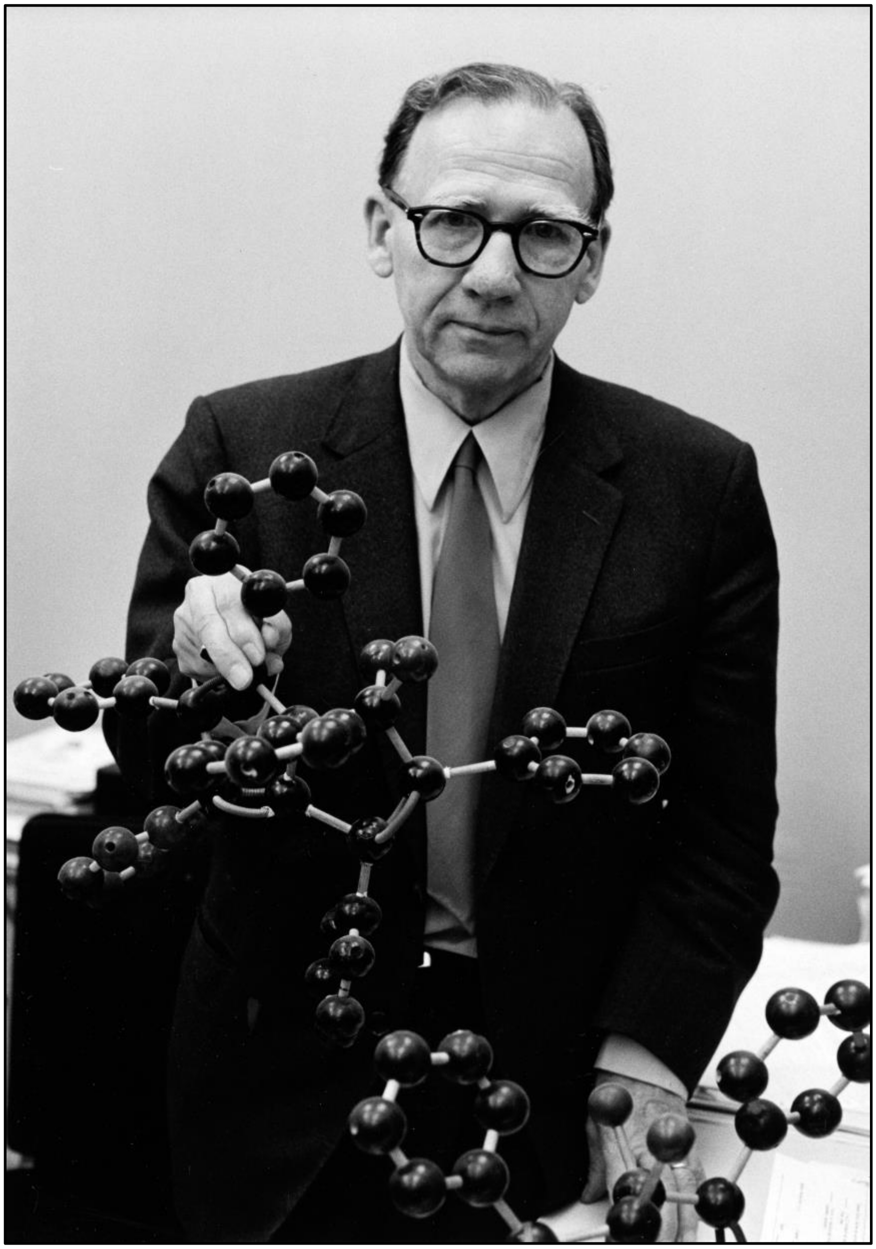

Edward Hughes with model, 1974. Photo by Floyd Clark.

TABLE OF CONTENTS

INTERVIEW WITH EDWARD W. HUGHES

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Edward W. Hughes **by Graham Berry**
Pasadena, California **November 20, 1979**

Begin Tape 1, Side 1

BERRY: You were born March 22, 1904.

HUGHES: Wilkes-Barre, Pennsylvania.

BERRY: Your parents owned what?

HUGHES: Father was a mining engineer in the anthracite coal region. My mother was from northern Pennsylvania, too. And I was in public schools in Wilkes-Barre—Coughlin High School. Incidentally, Daniel Flood was my class president. That's the congressman who has just been kicked out.

BERRY: Do you have any brothers or sisters?

HUGHES: No, no siblings.

BERRY: You graduated in chemistry from Cornell in '24?

HUGHES: Yes.

BERRY: When did you decide to go into chemistry?

HUGHES: I had to take chemistry as an overload in high school in my senior year. The reason I got into chemistry rather than physics was because I took chemistry after physics, rather than the other way around. And in those days, it was easy to get into school. I remember I didn't sign up at Cornell until August and entered in September. You can't do that today, anywhere.

BERRY: Was math easy for you, and science?

HUGHES: Yes. I did very well in high school in math. Of course, at Cornell I met a very enthusiastic [chemistry] professor—Arthur Wesley Browne. So, between having had chemistry as a senior in high school and meeting him, I stayed in it.

BERRY: Was it general chemistry at first?

HUGHES: No, I was with Browne as a junior and senior in inorganic chemistry and was elected to Sigma Xi as a senior. Then I became Browne's lecture assistant and graduate student.

BERRY: Did you stay at Cornell immediately after graduation?

HUGHES: Yes. And I got rather fed up with it, because, really, they hadn't kept up with what was going on in other places in chemistry. I didn't realize that as an undergraduate, but as a graduate student I began to realize that they were sort of out-of-date. But, at the last minute, I happened to attend some lectures in crystallography given by Professor [Carleton Chase] Murdock in physics, and I asked him if I could work over there. So, I finally finished my degree doing my research in physics, in the physics department, on problems associated with chemistry.

BERRY: There were eleven years between your bachelor's and PhD degrees.

HUGHES: I was there teaching, and partly doing just research, and some editorial work for the Cornell Press. The last four years of graduate work, I was doing crystallography. And I was very, very fortunate. In the spring term of 1934, the Baker Non-Resident Lecturer was W. Lawrence Bragg, the Nobel laureate who invented X-ray crystallography—he later became Sir Lawrence Bragg. I knew he was coming, and I was very interested. I was already engaged in that line of research. But I knew there were some people in physics who wanted to work for him, and it never occurred to me that I could. I was amazed one day when the acting chairman of the department called me in and asked if I would like to be a teaching assistant to Professor Bragg. And it turned out that he [Bragg] didn't know I was doing X-ray crystallography. So, he had called Professor Murdock and said, "Look, I want somebody that knows X-ray crystallography to be this assistant. And I'm willing to appoint a physicist to the job. Do you have any graduate students you want to nominate?" And Murdock said, "Well, you don't have to go to the extreme of appointing a physicist to a job in chemistry. You have a chemist who knows all about this." So, I got that job. And so, for a whole term, I had Bragg to myself, practically. We had a suite of offices together. And for a person just starting out in X-ray crystallography, to have Bragg at his elbow for five months was just unbelievable!

Then, when he left, his book [*Atomic Structure of Minerals*] was not finished, so he asked the department and the Cornell Press to make me the technical editor of his book. This involved superintending the drawing of about 143 figures for the book, which became a classic. It was the first full-scale book on the crystal structure of minerals. You see, he and his students—and [Linus] Pauling and his students—had done maybe 50 percent of the mineral structures that had been done up to that time. So, I was engaged in that for the next couple of years and even had to go to England and work with Bragg there on those drawings—for a couple of months at his home in England. Then, as a result of that— In '37, I should have been out getting a job, but I knew that Pauling was coming in the fall of '37 as Baker Lecturer. And I decided I'd take the money I'd save and live there in a penurious fashion, just to listen to Pauling's lectures. And to my amazement, the day after he arrived, I was called into the office, and they said, "Would you like to be teaching assistant to Dr. Pauling?" It turned out that Pauling had read the published part of my thesis, which was generally chemical physics. And so, after he'd been in the office a few minutes, he said to the chairman, "Is Hughes still here?" And the chairman said, "Oh, do you know him?" And Pauling said, "Well, judging from his thesis, he's the only one at Cornell who's doing anything I'm interested in." [Laughter]

So, they appointed me assistant to Pauling, and before he left he offered me a two-year appointment at Caltech. And that's how I got here. But I had to stay at Cornell for another six months to get his book through the Cornell Press—*The Nature of the Chemical Bond*.

BERRY: You were the technical editor of his book also?

HUGHES: Yes. And, also of Farrington Daniels' book [*Physical Chemistry*], which was not a classic, but a very good book. It came in between the other two, and this is why I had enough money saved up to stay to listen to Pauling. [Laughter]

But instead of living poorly, I was doing very well, because the assistants to the Baker Lecturer were paid about twice as much as the regular assistants were.

BERRY: What were the figures? Do you remember?

HUGHES: The regular teaching assistants got $500 for nine months, and the Baker assistant, I think, got nearly that much for a semester. But out here, I remember one year I was a Hale Fellow—$1,000 a year.

BERRY: In physics, was that?

HUGHES: No, in chemistry. After the two years as Hale Fellow were up, suddenly the war came on, and this upset everything. I had to stay here. But before that third year was up, the government asked us to start teaching training courses for technicians involved in preparation for war. They were called the Defense Training Courses, and after Pearl Harbor, War Training Courses.

BERRY: What years?

HUGHES: '40-'42. I remember one of the courses was in explosives. Pauling and [J. Holmes] Sturdivant went back to the explosives research lab of the Bureau of Mines in Pittsburgh and learned about this. And they came back and started the course, training technicians in handling and testing explosives. Professor [Royal W.] Sorensen was asked about training technicians to do X-radiography of metal castings, and the only place where X-ray work was done then was in chemistry. So, he came to Sturdivant and me, and we looked around and found that there was an excellent lab run by a company called Triplett & Barton that was doing the testing at Lockheed. So, we made an agreement with them, and the Institute taught a course, of which I was the instructor for over a year, training people how to radiograph metal castings. The Army required that every stressed part that went into an airplane had to be radiographed, sometimes in two or three different directions.

BERRY: That's the test for translocation?

HUGHES: No, to look for any kind of a flaw that might cause it to fail. And then the Navy had a different system. Each time you melted up a bunch of stuff in the crucible, it was given a number. And from all the castings from that melt number, you would select 10 percent at random to radiograph. If they all passed, the whole batch passed, but if two or three of them failed, then the whole batch had to be radiographed. Over there at Lockheed, they were doing tens of thousands of castings a day, sometimes. And the techniques there were definitely better than anywhere else in the industry; they could detect many more minor flaws and do it more cheaply.

Incidentally, I remember that the second class I took over there— We did all the lectures here and the basic X-ray physics lab was in the basement of Crellin [Laboratory of Chemistry], but when we started doing the practical work we would go over to the Triplett & Barton lab at Lockheed. And the second group that I took over, the first night at Lockheed, was the Monday after Pearl Harbor. I didn't know whether we could get in or not, so I parked in one of the parking lots and walked over towards the main gate. Soldiers were patrolling the wire fence around Lockheed. And the first Jeep that came along that I saw had three Japanese in it, with helmets on and rifles. [Laughter] And I thought, "Oh, my God!" As it turned out, they were members of the California National Guard. But we got in, all right.

When that course ended, Pauling called me in and said that he had a war research project that he wanted me to do. This was a subcontract from Berkeley, with Melvin Calvin—who later became a Nobel laureate—on the production of oxygen by chemical means. The oxygen problem was a serious one in the war, because before the war all oxygen was delivered as gas in these big steel tanks, stored at 2,000 pounds per square inch. And to deliver to the armed services all the oxygen they needed would have taken all the steel in the United States, practically, to make the tanks. So, we had to have a method to prepare oxygen on the battlefield. The Berkeley people in chemistry did this by improving and making lighter the ordinary apparatus for liquifying air and distilling it. Calvin had a different process. We had chemicals that would absorb oxygen from the air and then give it off again, pure, when you heated it or pumped it off. Calvin wanted the X-ray crystallography done on these compounds, and he had a subcontract with Pauling, who turned it over to me. I would have to go up to Berkeley every few months and work up there with those people. We would write reports every month, and so on and so forth. And then a funny thing happened in July of '43. The subcontract expired, and Calvin wanted us to renew it. Pauling and I knew very well that the basic work we were doing would never get to the battlefront for two years. And for some reason or other, we decided the war was going to be over in two years [laughter], and we refused to renew the contract. We thought it would be just a waste of taxpayers' money. How we decided that the war was going to be over in two years I can't remember—but we were almost exactly right about it. So, then I went up to Emeryville to the Shell Development Company for two-and-a-half years.

BERRY: Is that Shell Oil?

HUGHES: The Shell Oil research lab. I didn't like it there very much. In December of '45, after the war was over, Pauling came up to a meeting of the Northern California Chemistry section of the ACS [American Chemical Society], and it was quite clear afterwards that what he came for was to hear Professor [George W.] Beadle—who was at Stanford then—give a lecture. He was trying to get him to come here as chairman of the Biology Division. I remember that he asked me, "Did you hear Beadle's lecture?" And I said, "Yes." He said, "What would you think of him as chairman of the Biology Division at Caltech?" Later when I told him how unhappy I was, he said, "Why don't you come back to Caltech and finish the work that was interrupted by the war?" So, the first of April in '46, I came back as a senior research fellow. And after I'd finished this, in June, I went to see him. I said, "I'll be through with this in a month or two. I should be looking for a job." Linus looked at me and he said, "Why don't you just sit around and see what happens?" And in a day or two, I had a letter from President [Lee A.] DuBridge offering me an appointment as a research associate. And I've been here ever since.

BERRY: To go back just a little bit: When you first came to Caltech in 1938, what were your impressions of the school?

HUGHES: I remember that everywhere I went I was impressed by how well the buildings were kept up, as compared with Cornell, where there were lots of buildings that I thought were a bit run down. But here, every building I'd visit, somebody was looking after it. Another thing that I remember was thinking how small the library was for such a well-known place. It was just nothing, compared to the Cornell library. Of course, you'd expect that in general—but in *science* even, it seemed to me that the library was not as good as the Cornell library.

But the people were wonderful. I was just looking at the list of people who were here. In chemistry, there was Stuart Bates, Jimmy [James E.] Bell, Roscoe Dickinson, Bill [William N.] Lacey, Linus Pauling, Richard Tolman, [Richard M.] Badger, [Howard J.] Lucas, [Ernest H.] Swift, [Don M.] Yost, [Edwin R.] Buchman, [Joseph B.] Koepfli, [Arnold O.] Beckman, [Carl G.] Niemann, [Bruce H.] Sage, Sturdivant, [Robert B.] Corey—these were all the upper people in chemistry when I arrived. Incidentally, Beckman amazed me. After I was here a year as a research fellow, he resigned his professorship to run his business. And when he came back as chairman of the Board of Trustees—I've forgotten exactly when, in the sixties [1964—ed.]—I met him on the campus one day, and he came over and called me by name! You must be a good businessman to be able to do this. It had been some twenty-five years.

BERRY: Well, he had a good memory. And, of course, he was a chemist also. Do you remember any particular stories about any of the early people?

HUGHES: Oh, there are lots and lots of stories. Of course, of all those people, I think Pauling and Swift are the only ones left—and Joe Koepfli, and Beckman, and Sage. Even Sturdivant is gone.

BERRY: In addition to the war classes, did you do any teaching?

HUGHES: Not until after the war. I had to give seminars every once in a while; research people had to do that. But in those days—well, even today—the research associates and research fellows and senior research fellows are not supposed to be asked to teach, except graduate courses. But in ’46, there was this tremendous in-rush of students because of the GI Bill. And the class in physical chemistry went from about twenty or thirty students up to around sixty—mostly people coming back from the Army. So, Pauling asked me to take a section of that. And the next year we modified the course for geologists; I taught that right down to the time I retired [1974].

BERRY: Was that geochemistry?

HUGHES: No, this was the regular physical chemistry, modified some to meet the requirements of geologists. The last couple of years, we enlarged it and took in biologists, too. It went to three quarters.

BERRY: Was this a graduate course?

HUGHES: No, juniors. But there were mostly graduates in it from other fields. In geology, juniors were required to take it, unless they were in geophysics. And all the graduate students that came into geology and had never had P-Chem—and most of them never had—were required to take it as a makeup. And later on, when Biology adopted it, it was chiefly as a makeup for their graduate students.

BERRY: Was there crystallography in it?

HUGHES: Yes.

BERRY: Weren't you in sort of at the beginning of crystallography?

HUGHES: It started in 1912, but the war in Europe stopped it dead for four years. So that by the time Pauling came here as a graduate student, it was just getting going again. He came in 1922. The first PhD granted by the Institute was to [Roscoe] Dickinson in 1920, and it was in crystallography. And he was Pauling's teacher. He died during the war, of cancer. It was a great loss to the Institute.

BERRY: What was the social life here in those early days?

HUGHES: Well, I had the impression that there was more work done at night and on weekends than there is now.

BERRY: By both faculty and students?

HUGHES: Of course. At that time, the graduate students, most of them, were paid by giving them room and board in the Athenaeum. So, there were a lot of graduate students over there. There would usually be two or three of those round tables at dinnertime, filled with graduate students. Also, the Athenaeum operated on weekends, and dinner on Sunday was a big affair at noon. And usually the [Robert A.] Millikans would be there, and the Paulings, and a lot of other people. That, of course, has gone by the board.

BERRY: You mentioned that Pauling talked to you about Beadle. Was Pauling concerned with getting Beadle here?

HUGHES: Yes. I think, at the time [1945], he was one of the members of the Institute's Executive Committee. You see, Millikan had never taken the title of president; he [was chairman of] the Executive Council. Certainly, when DuBridge came in as president

[1946], he kept this council, too. Pauling was on that committee. It's still going on [as the Institute Administrative Council—ed.]; it still meets. Pauling was on that, and I'm pretty sure he must have been delegated to go and talk to Beadle. Otherwise, I can't understand why he would have gone all the way to Berkeley just to listen to the lectures at the section meeting. It wasn't a national meeting of the American Chemical Society; it was just a local meeting.

BERRY: Were you involved in the DNA race?

HUGHES: No, I was hardly aware that it was going on. I was involved in the protein business. Pauling asked me to come to start crystal structure work on the peptides—in 1938, the year after Corey had been brought to start doing the amino acids of which proteins are made. When you join the acids together by peptide links, you get the peptides—and I was to start doing peptides. We did several different forms of glycylglycine and also triglycines. Corey's pupils were doing the amino acids themselves. I think it was the war that held things up. Because by '46 or '47, what we had found out about the amino acids and peptides and the dimensions of the molecules had merely confirmed what Pauling had in his book, *The Nature of the Chemical Bond*, in 1939. But it wasn't until '48 that he found the alpha helix. The only reason he didn't think of it earlier, I'm sure, is because for five years he was mostly doing war work. There was nothing that went into it in the end that we didn't know before; we just knew it a little bit more firmly. The dimensions were all substantially unchanged.

BERRY: He had three of the alpha-helix, didn't he, instead of two?

HUGHES: Well, it's a non-integral helix. People had tried helices before, but they would have them repeat after two turns, or four turns, or three turns, or something like that. And there was one chemist not at Caltech who was sort of bitter about this. He said, "Why, I thought of helical structures for proteins long before Pauling." It was the fact that Pauling made the turns come around where you could make hydrogen bonds—that made it come out non-integral. You had to go many, many turns before you got an exact repeat. And even then, you had to twist it a little bit. Whereas this other fellow had been forcing repeats after two turns, or three turns, or four turns, and this just doesn't work. He finally admitted that this was the fundamental difference.

Begin Tape 1, Side 2

HUGHES: In '51, I went away in June on a leave of absence and became the Brotherton Research Lecturer at the University of Leeds in England for a year. In May of '52, the Royal Society had a conference on proteins. Corey was supposed to give the first twenty-minute lecture on amino acids, and Pauling the second on the alpha helix. And then the rest of the day, the Englishmen would sit there, telling them what they thought was wrong about both. A week or two before, I got a letter from Pauling, with his manuscript, saying that it looked as if he couldn't come because he was being subpoenaed to appear before Congress, and that Corey didn't feel that he knew about the functions study of the helix scattering. So, would I please be there to do that for him? So, I went down and stayed with Dame Kathleen Lonsdale, who lived near the airport, and went over to meet Pauling, and he wasn't on the plane. So, then I went into London to the hotel where Corey was staying—this was the day before the lecture. And Corey said, "Pauling can't come— I had a cable—and he wants you to give his paper." And he handed me a stack of twenty slides. I'd already read the paper. It took an hour just to read it, and we were allowed twenty minutes. So, there I was, standing before the Royal Society with Charles II's portrait looking over my shoulder, wondering what I should leave out. Finally, some member of the Society got up and moved that I should have ten minutes longer. So, we had thirty minutes. And then at the end of the day, we were given five minutes of rebuttal, in which you couldn't do anything. I was very angry, and I wrote to Pauling and told him I thought we'd gotten a dirty deal.

He arranged a meeting in Pasadena in September of '53, with funds from ONR [Office of Naval Research] and one of the big foundations. We invited all these Englishmen to this meeting—and interested Americans, about fifty or sixty people in all. The meeting was held in the Hall of the Associates in the Athenaeum. It lasted all week. There were only two or three papers a day—it was unlimited discussion. And we were all set to really have a fight. It turned out that in the intervening year, the Englishmen had thought longer about this and had done some experiments. And they all agreed the alpha helix was right. So, it was just one big lovefest. [Laughter]

<u>Pauling's listing of those pictured is as follows</u>: "[Across the back] Wilson / Perutz / Schomaker / Watson / Dunitz / Huxley / Crick / Marsh / Pasternak / Schroeder / Lindley / Trueblood / Bruchner / Huggins / Pepinsky / Palmer / Rollett / Riley / Luzzate / Freeman / Beadle / Davies / Tyler / [Across the front] Wilkins / Kendrew / Rich / Magdoff / King / Pauling / Krimm / Corey / Harker / Sutherland / Astbury / Bear / Bragg / Patterson / Edsall / Schmitt / MacArthur / Furnas / Randall / Elliott / Low / Itano / Trotter / Hughes."

<u>Associated</u>: J. Norton Wilson, Max Perutz, Verner Schomaker, James D. Watson, Jack Dunitz, Julian Huxley, Francis Crick, Richard E. Marsh, Rafael Pasternak, Walter A. Schroeder, Hugh Lindley, Kenneth N. Trueblood, Hans Joachim Brüchner, Maurice Huggins, Ray Pepinsky, Kenneth J. Palmer, John S. Rollett, D.P. Riley, V. Luzzate, Hans Freeman, George Beadle, David R. Davies, Albert Tyler, Maurice Wilkins, John Kendrew, Alexander Rich, B. S. Magdoff, Murray Vernon King, Linus Pauling, Samuel Krimm, Robert Corey, David Harker, G. B. B. M. Sutherland, William Astbury, Richard Bear, W.L. Bragg, A. Lindo Patterson, John T. Edsall, Francis O. Schmitt, Ian MacArthur, C. C. Furnas, J.T. Randall, Norman Elliott, Barbara Low, Harvey Itano, I. F. Trotter, Edward W. Hughes.

I was looking the other day at the photograph of that group that was taken in September 1953, on the steps of the Athenaeum. And the only Nobel laureate in the picture at the time was Sir Lawrence Bragg. But looking around, there were at least seven or eight other people who have since become Nobel laureates. There was Beadle, Watson and Crick and Maurice Wilkins, Max Delbruck, Max Perutz, John Kendrew—all these people were there. And, of course, Pauling. This was in ’53, and he got his prize in ’54.

BERRY: Now was this before Watson and Crick had been announced?

HUGHES: It had been announced a few months earlier. [Before that] Pauling and Corey had published a structure of DNA that was wrong.

BERRY: Three-stranded, wasn’t it?

HUGHES: Yes. And it had the phosphates on the inside instead of the outside. I know someone else here had an idea about the structure, and Pauling said, “No, we can’t afford to have two wrong structures in a row.” But Pauling has told me that he was completely unaware of any race going on.

BERRY: Is that right? All the way through?

HUGHES: Yes. And also, it’s quite clear that the method Watson and Crick used was a straight imitation of Pauling’s methods of doing things. And they say that themselves.

BERRY: You’ve been in X-ray crystallography—

HUGHES: Since about ’31. I had an interesting experience last night, at the Swift dinner. Sunney Chan [Hoag Professor of Biophysical Chemistry, emeritus] brought a guy over, and he introduced him as Edward Hughes. And this Hughes was interested in me because he’d noticed that my wife’s name is Ruth, and his wife’s name is Ruth. My wife was sitting across the table from me, and his wife was over somewhere else. We were chatting about that, and suddenly he said, “Do you know, I got my PhD at Cornell in ’32, and I recall there was a fellow by the name of Edward Hughes there. Did you ever know him?” [Laughter] He was an organic chemist, and that’s why I didn’t remember him very well. I did remember—and he did, too—that we used to get our mail mixed up.

BERRY: Did you do any teaching after the war?

HUGHES: Well, that's when I started—in '46. And, incidentally, among the geologists that I had in my class, one was Lee [Leon] Silver, who is now professor of geology, and another was this guy [Harrison] Schmitt, who was the first trained geologist to step on the moon; he's now a senator from New Mexico. Also, I took over from Pauling the supervision of the thesis of Barclay Kamb [Rawn Professor of Geology and Geophysics, d. 2011] when he was a graduate student. He used to come and discuss doing his crystal structures. He was supposed to be working with Pauling, but Pauling was out of town much of the time and he turned him over to me. I'll never forget. This was in '56, I guess. We were gone for three months in Europe. We left around the end of March. And I said to Barclay, "You can't possibly get your degree by June. So, I'll be back the Fourth of July, and you go ahead and do your thesis this summer." When I came back after the Fourth of July, there was no Barclay. So, I called up the geology department and said, "Where's Barclay Kamb?" They said, "Professor Kamb is over in Arizona, conducting a summer class in geology—a field class." It turned out that they had told him, "If you get your degree by the first of June, we'll make you an assistant professor." So, he had sat down—and I think that's the fastest he ever wrote anything in his life. [Laughter]

BERRY: Was it on crystallography?

HUGHES: Yes.

BERRY: Had Caltech pioneered in crystallography?

HUGHES: Oh, yes. Oh, in 1917—let's see, the Gates building was built in the winter of 1916-1917. But there was a man, C. L. Burdick, whom Noyes[1] had sent to Europe to study, and he was on the last boat that got through before the war broke out. He got his PhD in Germany. Then Noyes wrote and said, "Why don't you go to England and work

[1] Arthur Amos Noyes, then at the Massachusetts Institute of Technology]

with [William Henry] Bragg? I think this X-ray crystallography business is going to be important." So, this fellow had managed, going through Holland, to get to England and work with the elder Bragg for a few months. He came back to MIT and built an X-ray outfit but never used it. Burdick came with Noyes to Pasadena that winter, 1916-1917. Noyes suggested that he build an apparatus that had got all the latest improvements, based on his experience of building one at MIT. And so, he did. He and Professor [James H.] Ellis did the first American single-crystal X-ray structure here in Gates Laboratory, in the winter of 1916-17. And the first American paper was published in *JACS*[2] and in the National Academy's publication, the same paper published twice—I don't know how you do this nowadays. But in 1917, that was the first crystal-structure paper in the United States for a single crystal. There was a man by the name of [Albert W.] Hull, who independently invented the powder method at GE [General Electric Research Laboratory] the same year. So, in 1957, Pauling received a letter from one of the foundations, with a $1,000 check, and instructions: "Arrange a celebration for the fortieth anniversary of this first paper." The reason they did this was because one of the members of their board of directors was this fellow Burdick who had done this work. So, we had him here, and we tried to get Hull here, but Hull was too old to travel. But they both wrote up their experiences. We had a one-day meeting. We brought Dorothy Hodgkin from Oxford, who had just completed the crystal structure of vitamin B_{12}, which was the biggest molecule that had been done down to that time. And we had ten or twelve other papers from around this country. And we had Burdick here. We had a big dinner in the Athenaeum that night. This was to celebrate the fortieth anniversary of the first crystal structure paper in the United States.

BERRY: That's interesting. That was about 1957?

HUGHES: It was '57. When I was in England in '51-'52, I went to visit Dorothy Hodgkin at Oxford. In fact, I had been married the day before.

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2 Charles L. Burdick & James H. Ellis, "The Crystal Structure of Chalcopyrite Determined by X-rays, *Jour. Amer. Chem. Soc.*, 39:12, 2518-25 (1917).

BERRY: When were you married?

HUGHES: In October 1951, on Halloween Eve. So, the next morning, practically around ten o'clock, I walked into Dorothy's laboratory and introduced my new wife to her. We talked a while, and Dorothy said, "How long have you been married?" I looked at my watch and said, "Oh, about fifteen or twenty hours." She called a graduate student in, and she spoke to him. And we went on talking. About an hour later, the graduate student stuck his head in, and we went out into the lab. She had sent out and got a cake and a couple bottles of iced champagne. And this was the only reception we had.[3]

But I remember Dorothy showed me the first pictures of vitamin B_{12}, with various substituents. And the films superimposed on top of each other perfectly, but you could see slight differences in the intensities. She said, "Do you think these differences are enough to get started on determination of this structure?" I didn't have the slightest notion of whether you could or not. I said, "Well, Dorothy, knowing you, I would expect in about five years there will be a paper on this structure of vitamin B_{12}." And it was exactly five years later that she gave that paper here. I could see it was a tough job. There was no big computer at Oxford in those days; at first, everything had to be done by hand—all the calculations. In fact, a graduate student can do today one or two crystal structures for his PhD that you wouldn't have given to a couple of postdocs even fifteen years ago. You'd tell them to take a couple of years.

BERRY: Because of computers?

Hughes: Well, there are two things: the computer and the automatic diffractometer. We used to have to look at all of these spots and estimate visually 5,000 spots. And now the diffractometer sits down there night and day just measuring and counting photons from the crystal and will do hundreds of these reflections a day much more accurately than we could ever do before.

BERRY: It was by photograph before, wasn't it?

[3] See *Interview with Ruth J. Hughes.* [Oral History] https://resolver.caltech.edu/CaltechOH:OH_Hughes_R

HUGHES: Yes. And I feel that they should still use photographs to make sure that the computer hasn't gone astray, and that the diffractometer hasn't gone astray.

BERRY: The diffractometer does it by counting.

HUGHES: At first, it was a Geiger counter, and now it's a proportional counter. There's a computer that sits right there by the machine and keeps moving the crystal around so that the different reflections can be counted. And it counts for maybe ten seconds, and if it doesn't get enough, it says this is too weak and records that and moves on to the next one.

BERRY: Automatically it does that?

HUGHES: Yes. You go and watch there. You see this crystal moving around automatically and hear the counter clicking away. The numbers appear in red light on the machine. And every so often *bang!* it prints all this information onto a tape—or nowadays, it will print out onto a magnetic tape and you won't hear it—and goes on to the next. Of course, they cost $50,000 to $80,000. I introduced [the] least-squares [method] into refinement of crystal structure in 1940. And one round of refinement of a structure in which I had 105 observations, and 18 unknowns to fit to them, would take me about 24 man-hours. I figured out the other day that our computer today would take something less than a minute for the same job—things are done that much faster.

BERRY: Did you do a paper on that? I imagine you did.

HUGHES: Yes. And that was published in '41. Unfortunately, the title on the paper is the name of the crystal that I did, and the least-squares business is part of it. But about that paper—I got a letter from *Citation Index* the other day, saying that since 1961, when they started counting, this paper had been cited more than 600 times.[4] Between 1941 and '61, it must have been 1,000 times. And so, they asked me to write an account of how I did this and why I did it. This note was published recently, called "Citation Classics" in

[4] "The Crystal Structure of Melamine," *J. Am. Chem. Soc.*, 63:6, 1737-52 (1941).

Current Contents. I had to say that this 1941 paper has been cited not because of the crystal structure of melamine but because of least squares. If everybody who used this method cited that paper, today it would be about 2,000 or 3,000 times a year, because everybody does it. But now they always quote the person who wrote their computer program.

BERRY: I guess it was Oliver Wulf who said that your wife, Ruth, had been Noyes's secretary.

HUGHES: No, Oliver's wife, Beatrice Wulf, was Noyes's secretary. My wife worked with Pauling a few times. She began under Mrs. Wulf's tutelage. Mrs. Wulf has served under Noyes, Pauling, Swift, and Roberts [John D. "Jack" Roberts, chairman of the chemistry division 1963-68]. She was divisional secretary under all those people. And she retired only when Oliver did. She got my wife working part-time collecting the published papers of the division. And in the divisional office, you will see a great big long set of leather-bound books, of publications from number one down to around 7,000 or 8,000. Ruth collected those. She'd write to all the old authors and see if they had reprints. If she couldn't find reprints, they'd make a typed copy of the paper. I think she finished them, and would index them, and Mrs. Wulf would have them bound in leather. Ruth also did the library work for Linus on his book, *No More War!* And we have the second copy he received of that book, from Pauling to Ruth, for her help. She also managed the team that did the secretarial work on that petition to the United Nations that caused so much controversy. There were over 9,000 names. These petitions went out all over the world. They had to be translated into foreign languages, and then, when returned, translated back into English to make sure they hadn't changed the wording any. And all these signatures had to be deciphered and typed up, and there was no Xerox process, so it had to be done on stencils. There had to be copies for the press and copies for the United Nations, and for each UN delegation.

BERRY: Now, what was this particular document?

HUGHES: This was the petition against aerial testing of atom bombs.[5] There were over 9,000 names. And Fulton Lewis, Jr., inquired of a friend of his who collected signatures, and this guy told him it had probably cost $10 per signature. So, Fulton Lewis, Jr., wrote an article for his column, saying that the petition obviously cost almost $100,000, and no professor like Pauling could afford money like that. Who was supporting him? What Communist group was supporting him? Actually, it cost about $1,000. I know because, practically all the work passed through our house and Ruth was paying the typists and postage.

He did everything possible to keep the Institute's name out of it. He had letterheads printed with his home address on it. And everything that came to the Institute was immediately sent up there. The press mishandled this terribly. They kept saying that Pauling was trying to have the government of the United States give up unilaterally on atom bombs. Whereas the petition clearly said that the United Nations should endeavor to get the countries involved to *agree* to a *mutual* treaty to stop testing. Well, even before Eisenhower went out, he adopted this as the policy of the country. Kennedy put it into effect. It was as a result of that that Pauling got the Nobel Peace Prize.

BERRY: That was a major contribution he had made.

HUGHES: But just to show the way the press handled things, there was a long time before the treaty when there was no testing. And the press for some reason or other thought that the Americans and the Russians had agreed to this; they called it "The Gentlemen's Agreement." And then suddenly, we shot off about a hundred bombs in just about a month or two. And Pauling was aghast. He wrote a letter to Eisenhower protesting this. And this was splashed in the headlines of all the newspapers, and there were editorials about Pauling as a great Communist sympathizer trying to destroy our lead. Nothing happened for a while. And then, of course, the Russians came out with forty or fifty tests in a few months' time. So, Pauling sent off a hotly worded cable to Khrushchev. It wasn't as long as the one he sent to Eisenhower, because he had to pay money for a

[5] *An Appeal by Scientists to the Governments and People of the World, 1957.*

cable, but it was just as strongly worded, protesting this. And the newspapers paid no attention. He had released this to the press. The *New York Times* had about six inches, on page seven or something like that, and no editorial comments whatsoever. I don't think that the local papers even mentioned it, although I can't be sure.

BERRY: Did you feel that he was pretty good at using public relations and the press?

HUGHES: Well, no. In a way yes, but I wish he'd had a public-relations officer to advise him. Because he several times offended friends by the way he said things. But he certainly got the general attention of the American public. And I also know that he tried hard to protect the Institute. I've been in his office when people would call him up and ask him to give a lecture. And he would say, "Please, do not mention the Institute. Just list it as Pauling." Everybody knew, anyway.

BERRY: Do you think he would have followed the advice of a public-relations man?

HUGHES: I don't know. He knew what he wanted to do, I'll admit.

BERRY: Quite a humanitarian viewpoint.

HUGHES: Another interesting thing was—I asked him once about politics. He said, well, when he first registered as a young man, he was a Republican. And he voted for Hoover twice. [Laughter] And right after that, he became convinced that Roosevelt was doing the right thing, to prevent us from having a revolution. And from then on, he's always been a registered Democrat. And no newspaper I know of had ever looked that up. And yet he would speak at Democratic meetings for Democratic candidates. I remember one horrible thing once. One of the Hearst papers in LA, for no obvious reason whatsoever, published a little box, a biography of Pauling with his picture. And in there, it said that he was a member of the board of directors of the Hollywood Communist radio station. Pauling phoned the paper and told them it was the Hollywood *community* radio station and demanded a retraction. The paper at once published a retraction, claiming it was a typographic mistake.

Begin Tape 2, Side 1

HUGHES: At the International Union of Crystallography, for about four meetings, I was a delegate of the National Academy. These international meetings are not made up of private people but representatives from organizations. The United States National Academy of Sciences has five voting delegates. I was a delegate to three or four of these that resulted in the only grants that I've ever had. You see, when I came here, Pauling had gotten a big grant from the Rockefeller Foundation to run the X-ray crystallography unit, so we never had to ask the government for funds. If you wanted anything from the stockroom, you just signed "Rockefeller." Once, when I was elected as delegate to the International Union, I received a letter from the NSF [National Science Foundation] saying, "We understand that you have been appointed a delegate to this Congress. If you will fill out these forms, we will send you your travel expenses."

BERRY: You were also in Holland, Sweden, West Germany, Switzerland, Italy.

HUGHES: Yes. In fact, I've lived over fifteen months in England at one time or another—once, a solid twelve months.

BERRY: Doing research there?

HUGHES: I was the Brotherton Research Lecturer at the University of Leeds.

BERRY: Now, this lectureship—did you do research?

HUGHES: Yes. But I also had to give ten lectures. It was sort of a watered-down version of our Fairchild Scholar program.

BERRY: If I remember, Beadle went to England.

HUGHES: Also, Pauling. Pauling was the Eastman Professor at Oxford for a term.

BERRY: Now, about these trips to South Africa, Brazil, Peru, Panama?

HUGHES: That was all on the way home from England in 1952.

BERRY: Study and travel, Western Europe, Spain, to Sweden, ’56; Canada, 1957; Great Britain, 1960. Italy, Germany, England, Sweden, 1963.

HUGHES: Those were all trips to the International Union meetings.

BERRY: And you were president of—

HUGHES: The ACA—American Crystallographic Association. We had a meeting here in ’55, I guess, the year after I was president.

BERRY: What sort of molecules are they working on now? Are they pretty big ones?

HUGHES: You look in the journals and there are, worldwide, on the order of 1,000 a year. And they’re all kinds, ranging from little simple inorganic molecules up to protein molecules.

BERRY: Which are really big.

HUGHES: Yes, thousands of atoms.

BERRY: And the crystals are still grown here?

HUGHES: You mean here? Oh, no. Let’s see, [Richard E.] Dickerson is doing proteins. And [Richard E.] Marsh and [Sten O.] Samson are doing smaller molecules, both inorganic and organic. And also [William P.] Schaefer, who is a part-time research associate, I guess, and a part-time administrator.

BERRY: Are you writing now?

HUGHES: I should be. I could write eight or ten papers.

BERRY: Are you writing that biography of Corey?

HUGHES: Oh, yes. But I meant just scientific papers. There are completed structures.

BERRY: That you've completed?

HUGHES: Yes, with graduate students.

BERRY: Do you still have graduate students?

HUGHES: Oh, no.

BERRY: One of the standard questions in these interviews is, "How do you think the campus has changed over the time you've been here?"

HUGHES: Oh, it is unbelievable. There must be three times as many buildings, and nearly three times as big an area, and at least two or three times as many faculty. The student body—the undergraduates—hasn't increased all that much, maybe from 160 in the freshman class up to 210 now. But the number of graduate students has gone up tremendously. There may have been 20 or 30 in chemistry when I came. Now there are 150.

BERRY: Has the attitude of people changed?

HUGHES: Well, some of the faculty, of course, always have been sort of prima donnas, with very strong opinions. And as they leave and others come, why, the opinions change. But they are very strongly held. I remember in '57, when we had that meeting to commemorate the 40th anniversary of the first paper in crystallography. We'd been given the money by a foundation. Pauling decided the dinner that night should be complimentary. And it was. But I know one faculty member who refused to go because he thought that that was wrong—this money should have been used for science.

BERRY: That sounds somewhat typical, doesn't it—having certain people feeling very strongly?

HUGHES: For instance, last night we had this dinner for Ernest Swift. And that was complimentary. There were 100 people there. I don't know anyone who refused to go. So, this is one thing that has changed.

BERRY: Was much of the research privately supported when you came here?

HUGHES: Well, what we were doing in X-ray crystallography was for a long time supported by this Rockefeller grant. This not only bought the equipment but also material and salaries. There perhaps was some money from Institute funds, too; there always has been some of that. Certainly, most of it came from Rockefeller before NSF was created.

BERRY: Is chemistry oriented toward basic research?

HUGHES: Oh, sure.

BERRY: Much more than applied, isn't it?

HUGHES: Oh, yes. Always has been, even to some extent in chemical engineering. Pauling always had an eye for what was becoming fashionable to do. If he was not ahead of the crowd, he soon was. That was one of his outstanding characteristics—that every so often he would take up something new and he would get someone, a graduate student or postdoc, to start it and work with him. And as soon as he was sure the guy was alright, he would turn it over fully to him. If the fellow didn't turn out to be much good, he got rid of him in a hurry. He started out doing ordinary inorganic structures, and then he started doing minerals. Then he started doing metals. And then he got over into biological work. Toward the end, before he left, he was doing chemical psychiatry. He had a group that worked with the people out at a neurological hospital east of here. One of his papers at that time was “A Molecular Theory of General Anesthesia.”

BERRY: I remember he called me into his office one day and said he wanted a release to go out on a new theory of anesthesia—actually having to do with things crystallizing in cells in the brain so that the electrical impulse was interrupted.

HUGHES: Yes, that was the general theory. And they were titrating goldfish—running the anesthetic into the goldfish’s water, until half the goldfish would turn on their side—to measure the effectiveness of various anesthetics. When they closed that program out, they had hundreds of goldfish that they gave away.

BERRY: Live goldfish?

HUGHES: Yes. What good is a dead goldfish? [Laughter] He and Harvey Itano, who was an MD trained by the Army during the war, came here as a postdoc and got a PhD. They discovered the cause of the first molecular disease—namely, sickle-cell anemia. And they had patients coming in giving blood. In return, they would give them oxygen to breathe, which made them feel better. Pauling’s institute now is geared for really all sorts of diseases. They have a big chromatograph and molecular mass spectrograph controlled with a computer in which they can analyze body fluids for hundreds of kinds of substances. They are all the time running normal fluids through and then sick people’s—looking for differences.

I can remember when Pauling was first interested in blood. I remember he was giving three lectures back East somewhere, about anaphylactic shock. When he came in, there was a big lectern on the table. He said, “I don’t need this,” and put it on the floor. He started to talk about how if one injected a small amount of a protein into a rabbit, waited a week or a few days, and then injected a small second dose, the rabbit would die almost instantly, of anaphylactic shock. The second day he came in, he was sort of annoyed; the lectern was there again, and he put it on the floor and continued his lecture. And the third day, when he came in and the lectern was there, he should have suspected something. He picked the lectern up to put it on the floor; and underneath it there was a live rabbit. [Laughter] And a big beaker of serum and a great big syringe. And a photographer standing there ready to get a snapshot of his face. [Laughter]

BERRY: Where did that happen?

HUGHES: Somewhere back East, I don't remember where. After the war, Verner Schomaker, who was a graduate student here and became a professor, his wife Judy had been a secretary. And she started an organization called Chem Wives. And when Verner left, she turned this over to my wife. This organization met once a month and consisted mostly of young faculty wives and wives of graduate students. Ruth's job was to make arrangements for the older faculty wives to use their homes for the meetings and to see that everything was done properly. This group got to be a very closely knit and important organization in the chemistry department, to the extent that both Pauling and Swift appropriated money to its use occasionally.

BERRY: What year was it started?

HUGHES: It started right after the war. For instance, if a girl got sick and couldn't operate her home, three or four of the other girls would go and help out. Well, when these people would go away, they would usually go to a better job. They didn't want to take all the old furniture and dishes and whatnot. And they would turn them over to Ruth and say, "Look, you can use these for somebody else." So, our garage got stacked up with dishes and knives and forks and baby pens and bassinets. But it would all go out in the fall when a new group would come in. Word got around the Institute about this wonderful business in the chemistry department. They would come around and try to borrow stuff; and she would say, "Oh, no. This is just for chemistry." But then, finally, the Women's Club took over the kitchen furniture, and the Service League had, for a long time before, taken over the baby furniture. And these are still running. And Ruth now runs the baby furniture for the Service League. And it's Institute-wide; any student—a graduate student, or a postdoc from overseas—can borrow all the furniture he needs to raise a baby. And sometimes sheets and blankets and dishes and things like that.

I was sitting in my office over in Crellin one day, ten or twelve years ago, and the phone rang. It was a character from USC [University of Southern California]. I identified myself and he identified himself. He said, "I'm not sure that you're the Dr. Hughes I want to get ahold of. Are you by any chance the husband of the fabulous Mrs. Hughes at Caltech?" And I knew right away what he wanted. [Laughter] One of his technicians was married to a graduate student at Caltech and they were about to have a baby and they needed help.

BERRY: Where are all the materials stored?

HUGHES: The southwest room in the basement of the Athenaeum for baby furniture. You know, you come down the spiral staircase and you go into the game room. The next room in there is the baby-furniture room; it has our telephone number on the door. If you go in there, there's just rows of potties and rows of bassinets and rows of baby carriages. The one that the Women's Club runs is called the Garage. At one of the apartments that the Institute owns, there's a garage that isn't used, and it's stuffed with dishes and knives and forks and things like that—and also, a certain number of sheets and pillowcases and blankets.

BERRY: Don't you have to buy some new ones once and a while?

HUGHES: Oh, yes. Ruth has a budget from the Service League to buy stuff. And I suppose the Women's Club does, too. She got out of that; she was doing too much. When it expanded to the whole Institute, she gave it up.

BERRY: Now, is this for graduate students and postdocs?

HUGHES: Or undergraduates if they are married. Well, also the postdoc people who come here from overseas get to use this. And, also for very short loans, faculty. It's well known that both Millikan and Noyes were not in favor of women students. I don't think there was anything in the charter that said that we couldn't have them, but as long as they were running things, there weren't any. But back before the war, there was a woman crystallographer by the name of Rose Camile Mooney who wrote and asked to come here on a Rockefeller Foundation Fellowship for the summer and work in Pauling's lab. And this came to Millikan, but she signed it R. C. L. M. And so, he sent it to Pauling and said, "Would you be willing to have Dr. Mooney in your lab next summer?" Pauling, who knew her very well, wrote back and said, "Yes, I know Dr. Mooney very well and I would be glad to have Dr. Mooney in my lab." They say that Millikan was very upset when Rose arrived on the campus.

BERRY: Was she, as far as you know, the first?

HUGHES: As far as I know, she was the first and only woman postdoctoral fellow, until after DuBridge arrived in 1946.

BERRY: What year did Dr. Mooney arrive?

HUGHES: It was in the thirties sometime, just before I arrived.

BERRY: And there were no women here on the faculty or as students?

HUGHES: There were some women technicians—a few—doing analytical work, and one or two in biology.

BERRY: Was Mooney a faculty member?

HUGHES: She was from the women's college [Sophie Newcomb College] of Tulane University. She probably was a faculty member there.

BERRY: And she was good?

HUGHES: Yes. Pauling wouldn't have let her come if she wasn't good.

BERRY: There was a woman involved with Watson and Crick's work, too.

HUGHES: Yes, Rosalind Franklin. Perhaps she should have had more credit. And in fact, in Watson's book [*The Double Helix*, Atheneum, 1968], he runs her down quite a lot. And then in an epilogue, he retracts all this; he said that she was OK. I don't know why he didn't correct this in the body of the book.

BERRY: Pauling apparently didn't have strong feelings against women in research.

HUGHES: He had no feeling at all against women. In fact, it was his motion that finally was carried to admit women graduates. The year before, Beadle had proposed it, and it was defeated by one vote.

BERRY: Was this a faculty vote?

HUGHES: Yes, to recommend to the Board of Trustees that we admit them as graduates.

BERRY: I think it was in 1952, as I remember.

HUGHES: Somewhere around there. And it was defeated by one vote. And a year later, Jack Roberts was brought here, and he had a woman graduate student from MIT that he wanted to bring with him. So, Pauling became even more active. He brought it up again, worded somewhat differently—mainly that the divisions could admit women if they wished to. But this was not a change. Because the divisions have always had the last say on who they admit as graduate students.

BERRY: Each division independently?

HUGHES: Yes. The applications come to the graduate school. But as soon as the papers are processed, they're accepted or rejected by the division. And then it goes back to the Dean and he writes the letter and says yes or no, depending on what the division thinks. So, this really didn't change anything. And apparently enough of the people who were opposed to women felt, "Well, if the division wants to admit them, why not let them admit them." So, it passed by quite a large majority. And this girl that Jack Roberts brought with him [Dorothy Semenow] was the first woman graduate student. And we have more than any other division. Last year there were 30 women out of 150 grad students. Now, this year it may be down a little, because a number of them got degrees during the past year, and there were only two or three women in the new class. But there are a couple of research groups that are over half women in this division.

BERRY: Are you going to write up those papers?

HUGHES: Oh, I should, but it's awfully hard to get back to doing this after all these years.

BERRY: And you're working now on this biography of Corey.

HUGHES: A lot of it has been written by other people. But I have written to them and written in pieces.

BERRY: I guess there's no deadline on these National Academy biographies.

HUGHES: They ask a member of the Academy to do it. But they can ask somebody else. Pauling is the one who's supposed to be writing the Corey biography. And Roberts is supposed to be writing one on Dick Badger. He's turned it over to Oliver Wulf. But of course, Oliver is a member of the Academy, too. That's one of the reasons he takes it so seriously.

BERRY: Now, what work do you do now?

HUGHES: Well, I'm a member of the division's Safety Committee. We had a couple of accidents right after [John] Baldeschwieler came as chairman [1973]. And so, he sort of gave the Safety Committee a kick in the pants and said, "Do something about this." So, we were pretty busy for two years—it has sort of quieted down now. But I hope we're not just sitting around waiting for another accident to happen. For a while, each year we inspected every lab in the whole division, both chem engineering and chemistry, looking for accident-prone people and for places where accidents could occur—making sure that all the high voltage was labeled, and all the dangerous radiation was labeled, and all that sort of thing.

BERRY: Did you question people to see if they were accident-prone?

HUGHES: We established a corps of safety officers who were graduate students or postdocs. There is one in each research group. The professor appoints one of these people in his research group as his safety officer. And they had to attend a seminar once a month on safety. And they were responsible for their group's suite of laboratories. We would tell them when we were coming to inspect them. And they were supposed to hand us a list of the things that they thought were dangerous. We would look at those, and then we would try to find some more besides. We always had with us the administrator of the division who was authorized to spend money on safety. He would keep notes of everything we found that was wrong. And at the end of each session, the committee would decide, "This is urgent; this must be done right away. This is less urgent; this must be done within two weeks. And this is even less urgent; this must be done within a few months." And he would see to it that the shop would make things right.

We were inspected by OSHA just about the same time we first did this. And they fined the department—I've forgotten, $700 or $800. The chief thing they were worried about was that all these plug-in cables that were plugged into the wall for typewriters were not three-pronged. We hadn't worried about that at all. But what we worried about, which they had never paid any attention to, was the dangers that could result from an earthquake. So, if you go around in the labs, you'll find that all the shelves now have guards on them. We don't let them store solvents in glass bottles on the high shelves where they could fall off. And the first couple of years, we've been just getting that sort of thing done—making sure that anything that was dangerous is tied down so that an earthquake couldn't cause damage. These big gas cylinders that contain gas at 2,000 pounds per square inch—they are very dangerous. There have been accidents where they fall over. If the knob at the top hit something and broke off, they would take off like a rocket and go flying around. We're very strict about making sure that those are tied down to something solid—the wall or a big table—so that in an earthquake they can't fall over. OSHA didn't worry.

BERRY: What is OSHA exactly?

HUGHES: Occupational Safety and Health Administration. There's a federal one and a state one.

BERRY: And do you get both federal and state inspections?

HUGHES: We were inspected last summer by the state. We told them that there was one bottle of a carcinogen that we hadn't been able to track down in chemistry and get rid of yet. And they fined us $2,000. And they would never have known it if we hadn't told them. We have appealed this.

But there's a list of about twenty chemicals that we dare not keep on the shelves or we will be fined. The unopened, sealed bottles may be handled regularly before they've been unsealed. But after they've been opened, they can be stored only in a controlled area that does not connect in any way with the rest of the building, by means of ducts or anything. It has to be exhausted to the outside. We're fixing up the solvent house out in front of Crellin as a controlled-storage area for carcinogens. Anybody that uses them, when finished, then has to seal it and take it over there. The rules for disposing of it are that they must be burned in an incinerator that goes to 2,000°F. It turns out that, according to the estimates, one of those would cost about $1,000,000. The Institute has tried to get a consortium of Southern California universities to build one. But at the moment all we can do is put them in a controlled area. Even though we don't want them anymore, we can't dispose of them anywhere. And there's another list of 1,000 that are suspected carcinogens. Any one of those is liable to be moved over onto the other list any day. For instance, benzene.

BERRY: Well, that's just for cleaning, isn't it?

HUGHES: No. Benzene. That's a pure chemical, but it's decidedly a carcinogen. And it's been used by organic chemists and ordinary chemists for more than a hundred years, slopping all around the lab. It's been a common solvent in organic chemistry. Now we don't dare use it anymore. Well, that's one of the things the Safety Committee looks for when they inspect labs.

BERRY: I don't want to ask you why *you* have it *here* in your office.

HUGHES: Because I had it before the rule was made. [Laughter] I haven't had to get rid of it. There's just a little tiny bit there. I never open it except once a year. But in industry, this is very serious. Because the law says that in commercial and industrial plants the exposure to benzene must be limited to something like one part per billion. And companies where they have to use it have to spend an awful lot of money on ventilation and precaution to meet the requirements.

BERRY: It's apparently still useful. I suppose there are more safety problems in chemistry than in any other field.

HUGHES: We were the only division that had its own safety committee, as far as I know. There is an Institute Safety Committee—Dr. [Walter F.] Wegst is the head of that. Recently, [Caltech President Marvin L.] Goldberger has appointed an Institute-wide committee on chemistry safety, which is to look at the other divisions that are using chemicals and try and straighten them out. One of our chemistry faculty is chairman of that committee.

BERRY: I remember when they built that lab underground for radioactive studies. Is that thing used much?

HUGHES: I'm sure it's being used. And there's recently been a crisis in the medical business, where they're using radioactive tracers both in research and treatment in hospitals. There's only one place in the United States where they can send that work. It's very low-level waste. But the governor of Utah and the governor of Washington have forbidden any of it to be shipped into their state. The other's in South Carolina, and they are threatening. And this stuff is piling up in the hospitals and the research labs. They are worrying what they're going to do about it.

BERRY: Is there any research toward finding a way to use that low level?

HUGHES: It's been used, you see.

BERRY: Yes, I know. But there's some energy left.

HUGHES: It's almost innocuous, but nobody wants to have it disposed in their state.

At Cornell, the lab I worked in dealt with azides and other high-nitrogen compounds. And we would have explosions every so often. But we knew it was dangerous, and precautions were such that over fifteen years there were only two injuries that I know of. Well, Baldeschwieler got the same degree at Cornell that I have. And he found out about this, and that's how I got on our Safety Committee.

BERRY: Well, it sounds as if you've got things under control now.

HUGHES: Well, there haven't been any explosions lately. But there was a chemical that was released over in Jack Roberts' lab a year or so ago that sent six people to the hospital for observation. And most of those people didn't realize that there was a dangerous chemical of that kind in the lab. Wegst was out of town, and his assistant didn't really know what the stuff was. He had to go back and read the book. There have been only two fatal accidents that I know of since I've been at the Institute. One was in the rocket project during the war; a man burned to death. And the same year, here in chemistry, a girl got some war gas splashed on her face when a vial blew up. She died before the day was over. The first man who got to her was Bill Lipscomb, who was a graduate student then—now a Nobel laureate at Harvard. He was the first one there. Why he didn't get it, too, I don't know. Because he hauled her off and put her under a shower. He was my first graduate student, and I was very proud of him.