CHEMICAL HERITAGE FOUNDATION

LEO BREWER

Transcript of an Interview
Conducted by

James J. Bohning

at

University of California, Berkeley

on

3 April 1992

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Chemical Heritage Foundation
Center for Oral History
315 Chestnut Street
Philadelphia, Pennsylvania 19106

LEO BREWER

1919	Born in St. Louis, Missouri, on 13 June

Education

1940	B.S., California Institute of Technology
1942	Ph.D., University of California, Berkeley

Professional Experience

	Lawrence Berkeley Laboratory [Radiation Laboratory], University of California, Berkeley
1943-1946	Research Associate, Manhattan District Project
	University of California, Berkeley, Department of Chemistry
1946-1950	Assistant Professor
1950-1955	Associate Professor
1955-1989	Professor
1989-present	Emeritus Professor

Honors

1942	Great Western Dow Fellow
1950	Guggenheim Fellow
1953	Leo Hendrick Baekeland Award, North Jersey Section, American Chemical Society
1961	E. O. Lawrence Award, Atomic Energy Commission
1971	Palladium Medalist, Electrochemical Society
1974	Distinguished Alumni Award, California Institute of Technology
1983	William Hume-Rothery Award, Metallurgical Society AIME
1988	Henry B. Linford Award for Distinguished Teaching, Electrochemical Society
1989	Berkeley Citation, University of California, Berkeley
1991	TMS Extractive Metallurgy Science Award
1993	Fifty-year citation, American Chemical Society
1998	Fifty-year citation, American Association of University Professors

ABSTRACT

Leo Brewer begins the interview with a description of his family and his early years growing up in Youngstown, Ohio. Brewer's father worked as a shoe repairman until the Depression hit in 1929. Brewer and his family then moved to Los Angeles. Brewer became interested in chemistry through the influence of a high-school chemistry teacher. After graduating from John Marshall High School, Brewer attended Caltech. After receiving his B.S. in 1940, Brewer was advised by Linus C. Pauling to begin his graduate work at the University of California at Berkeley, where he studied under Axel R. Olsen. Upon receiving his Ph.D., Brewer immediately joined the Manhattan Project as a research associate. Brewer's job was to use models in the periodic table to determine the worst properties of plutonium. Brewer tested refractory materials, such as nitrites, carbides, lanthanides, actinides, sulfites, sulfides, and phosphides, and determined that cerium sulfide would serve as the best model. Later, Brewer predicted the electronic configuration of all the actinides. Brewer's research for the Manhattan Project found direct application at the Los Alamos National Laboratory, and was later published as part of the Manhattan Project Technical Series. In 1946, Brewer joined the faculty of the University of California at Berkeley, where he continues as an Emeritus Professor to this day. During his career at Berkeley, Brewer worked in many fields, including organic chemistry, ceramics, astrochemistry, and even geology. Within these areas, he applied his thermodynamic research, including studying high-temperature molecules present in comets and stars, and the distribution of elements in the earth's gravitational field. As an educator, Brewer taught many courses on several levels, including freshman chemistry, inorganic chemistry, thermodynamics, and phase diagram equilibration. In more recent years, Brewer and his graduate students have branched their research into metallurgy. Brewer concludes the interview with a discussion of his published papers, the future of research support and application, and thoughts on the future of education.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and he has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

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INTERVIEWER: James J. Bohning

LOCATION: University of California, Berkeley

DATE: 3 April 1992

BOHNING: Dr. Brewer, I know you were born on June 13, 1919 in St. Louis. Could you tell me something about your parents and your family background?

BREWER: Well, my father was born in Krakow, which was part of Austria then. He was a rather energetic guy and was getting into all sorts of political activities, and his mother was very worried about him. I don't know if he even knew his father. I don't know what happened to his father, but his mother raised him. She was worried about him getting into problems, so she saved up some money, and she gave it to him and said, "You go to America." [laughter] So he arrived in New York not knowing much English and decided he really wanted to learn it. He got a job in some small town in Illinois where he had to use English. He really learned his English there in a short time and then went on to St. Louis.

BOHNING: Why did he go to St. Louis?

BREWER: I think he had a brother there who had left Austria earlier and gone to St. Louis.

He worked in a variety of jobs, shoe repairing and a number of other things. My mother came from Vilna, which was then part of Russia. It's now part of Poland or Lithuania; I've forgotten. It's really Lithuanian in its culture. Now my father must have come around 1910 or so, somewhere earlier than that even, but my mother came during the war, actually. She got out of there. She was able to get out; she and her father were able to get to the U.S. They came to St. Louis because she had a sister in St. Louis.

BOHNING: I see.

BREWER: Relatives. That was where she and my father met. I was born in St. Louis, but they moved—I think before I was one-year old—to Youngstown, Ohio, where he set up a shoe-repair shop.

BOHNING: How did they select Youngstown from St. Louis? At that time that was a big move to make.

BREWER: Yes. Apparently they heard there was an opportunity there or something.

BOHNING: So you grew up in Youngstown?

BREWER: That's right. Well, until I was about nine or ten. Let's see, I think they were in the shoe repair business while I was being raised. My mother never finished high school, so when I started in kindergarten, she enrolled in high school [laughter] and got her high-school degree. There was a lot of emphasis on education. My father had wanted to be a lawyer, to go to school, but never had the time or the money. Both of them thought education was very important. That was the message that really got across to me.

BOHNING: Do you have any brothers and sisters?

BREWER: I had a sister. She was seven years younger than I. I'd better go back; this was when we left Youngstown. In 1929, when the Depression really hit and business was so poor, my father gave up his business there and moved to Los Angeles—again because of relatives. So both I (about age nine) and my sister (about age two) were essentially raised in Youngstown.

BOHNING: You had your high school education in Los Angeles?

BREWER: Yes. I had one strange experience. We moved several times in Youngstown and then moved to Los Angeles, and every time I moved, I had to change schools. They would give me tests, and they would always move me up a grade. So by the time I got to Los Angeles—where they did the same thing as in Youngstown—I was two years ahead in school compared to normal age. I described in the Eyring lecture (1) about how I really tried to learn to communicate; I took all those extra courses in composition and writing and oral [communication] and so on.

BOHNING: What high school did you attend in Los Angeles?

BREWER: It's called John Marshall, up near Griffith Park. Even though we moved away from that neighborhood—oh, many miles away—I still took the train and the bus and walked to finish school there.

BOHNING: Did any of your teachers have a strong influence on you?

BREWER: Well, there was a chemistry teacher who convinced me that chemistry was interesting. I decided then that I was going to be a chemist.

BOHNING: I see. What kind of lab experiments did you have in high school?

BREWER: Well, I enjoyed the chemistry laboratory.

BOHNING: You took extra courses to improve your communication skills; you indicated that was partly at the urging of relatives who said that was important. How did that fit in with the regular high-school program?

BREWER: Oh, I was able to squeeze it in. I didn't have much trouble with any of the courses. Also, my handwriting was poor, and my relatives convinced me, "You'd better learn to typewrite." [laughter] So I was using typewriters from junior high on.

BOHNING: How did you select Caltech [California Institute of Technology]?

BREWER: Well, I had heard that it was an outstanding school in the sciences and I actually applied for a fellowship there, a scholarship. You had to take these exams. Now, one of the things about my schooling is that I didn't go around memorizing. I would try to get a model that would tie things together. One thing, I have a very poor memory, and I have had all my life. So I don't try to recall things from memory. I try to work out some way of tying facts together so I can predict them. Apparently, the way to pass those exams—some of the high-school teachers would have the students memorize all the answers. I didn't have that kind of education, that is, to memorize. I wasn't able to do as well, so I wasn't able to go there the year after I finished high school. Also, I did not have enough money to go to Caltech. I went to one of the junior colleges and took a number of courses, which I then retook at Caltech. Finally, my father saved enough money to send me there.

After I got there I did get a scholarship. I did very well. Very surprisingly, a lot of the students who had top grades in high school, largely due to memorizing, really found Caltech very difficult. Some of them just fell apart. But it really fit into my way of attacking things, so I did very well.

BOHNING: Your concept of model building sort of identifies your whole career. You've done that almost throughout. Did you retake the exams then, after you were in that year, to gain entrance into Caltech?

BREWER: I don't think I had to get entrance. The exams were to qualify for financial help.

BOHNING: I see. What year was it when you started Caltech?

BREWER: It was 1936.

BOHNING: What was Caltech like when you started there?

BREWER: Well, it had about the same number of undergraduates as now. That hasn't changed much. They've added lots more buildings since then. I did live in the dormitory there, and I had a single room. Part of the problem of being at a different age than the other students was that I didn't interact as much with the other students. I tend to do everything on my own, and I sort of completely did that at Caltech.

BOHNING: What courses did you start taking, and who were the instructors?

BREWER: Oh, everyone took the same courses. We had a freshman chemistry, and Linus [C.] Pauling would give some of the lectures. [Ernest H.] Swift in analytical chemistry I found wonderful. He really convinced me of the power of thermodynamics and physical chemistry. In the analytical courses, we had to calculate all the equilibria, kinetics, and so on. Then Don [M.] Yost played a very important role, and of course, [H. L.] Lucas, with whom I did my research. I have several published papers from the research I did with Lucas (2).

BOHNING: Yes. Can you tell me a little bit more about Pauling, Lucas, Yost, and Swift? Did you interact with them as an undergraduate?

BREWER: Well, not much with Pauling, except for one decisive factor. When it got around to my senior year, and it was time to consider graduate work, I didn't know much about other universities. Pauling was the chairman of the department then, so I went to see him. I said, "Can you give me some advice on where I should go for graduate work?" He said, "You should go to [University of California at] Berkeley," and I didn't even apply anywhere else. [laughter]

That was my interaction with Pauling, and he has sort of taken a paternal attitude to me ever since. He'll often write me, "I just saw your paper, and there's an error in this place." [laughter]

He got very angry with me once. I don't know if I described it in any of the stuff I sent to you from the Eyring lectures. Did I send you all the lectures, or just one?

BOHNING: Just the one.

BREWER: Oh, I spent a whole week there. On the Manhattan Project, when we were using these models to predict things, we decided that there were two serious errors in the values that everybody uses. One was the enthalpy and sublimation of graphite. The other was the wrong dissociation for nitrogen being used. So after the war, my first graduate student, Paul [W.] Gilles, set about to check the enthalpy and sublimation of graphite. The problem was that there were previous experiments on the rate of vaporization from the surface, which would correspond to a very low vapor pressure if you assume the accommodation coefficient is large. But there was spectroscopic data that [Gerhard] Herzberg had worked on. They interpreted that the enthalpy of vaporization would be very much smaller than what you would conclude from the sublimation dynamics, and that there was a real barrier for the vaporization. They were taking 125 kilocalories, whereas we wanted something like 170—an enormous difference.

What Gilles did was a Knudsen-cell experiment where he would reduce the size of the orifice, and if it were true that the vaporization coefficient were low, the pressure coming out of the orifices would go way, way up, which it did not. We published this paper (3) and, oh, Pauling was so angry. He said, "You've destroyed organic chemistry! You're changing all the organic bonding energies." Well, of course, all I did was move them up and down, relative to the base. I mean, it didn't change—but he had to rewrite his book where he had used the old value. [laughter] Herzberg, too. In fact, it took ten years before they finally could see this. There were enough other confirmations; they finally had to give in and came up to 170. [laughter] I have found that on a number of times when I would present new results, my colleagues rejected this information, until I got an award about ten years later after the results were confirmed.

BOHNING: You've commented that when you got to Berkeley you were tired of taking courses because you had taken so many at Caltech. What other courses did you have at Caltech?

BREWER: Well, first there was the physical chemistry, and Yost's inorganic courses, which I found very excellent.

BOHNING: Who taught the physical chemistry?

BREWER: Oh, I've forgotten. That's one of my problems; my memory's so poor, and particularly for names. [laughter] I've forgotten who it was, but I found it useful. Then, of course, organic—I tell people that it was organic chemistry that made a physical chemist of me. They say, "Oh! You were turned off by the organic chemistry?" It was just the opposite, because Lucas really emphasized the value of bonding energies, physical chemistry, and understanding what happened in organic chemistry. I realized how powerful thermodynamics and physical chemistry would be.

BOHNING: That was a rather unusual approach to organic at that time, wasn't it? To emphasize physical chemistry?

BREWER: Well, Lucas played a very important role in organic chemistry by introducing G. [Gilbert] N. Lewis', then later Pauling's, bonding energy concepts into organic chemistry. It played a very important role, so he was really up on it. He never had a Ph.D., incidentally, but he had really learned a lot of what went on with quantum mechanics, physical chemistry, and thermodynamics and applied it to organic chemistry.

BOHNING: As you indicated, you did some research with Lucas and in fact published two papers (2). Was that expected? Did everyone do undergraduate research?

BREWER: That was encouraged, to have people do some undergraduate research, and we do that here at Berkeley, too. It was a good experience for me because he just told me what he wanted done and I just did it all on my own and really learned how to tackle a research problem.

BOHNING: So he really treated you like a graduate student, didn't he?

BREWER: Yes.

BOHNING: Who were some of your peers, your classmates at that time?

BREWER: Well, let's see if I can remember their names. There weren't many chemists. There were only about four or five chemists, actually, in the 1940 Caltech class. Let's see, one of them, a chem engineer, [Harold] Mickley—he went on to industry. There was another fellow named [Alexander] Brewer, as far as I know not a relative of mine. [laughter] Then there was another guy, [Donald] Loeffler, in chemistry, and Frederick C. Brunner. I can't remember the names of others.

BOHNING: Oh, yes. He was at Florida State [University], I believe, or somewhere in Florida.

BREWER: Really?

BOHNING: I remember the name.

So at Pauling's recommendation, you went to Berkeley, came up here. You commented about something that happened after you received your degree. You went camping with some friends.

BREWER: Yes, out to the Sierra. We were shocked when we returned from the Sierra and learned that Paris had fallen. When we realized that the war in Europe was going to expand and we'd get into it, we figured we'd better get our Ph.D.s quickly, so it took me just a little over two years.

BOHNING: You worked for [Axel R.] Olsen?

BREWER: Olsen, right.

BOHNING: How did you select Olsen?

BREWER: Well, I had done this kinetics work with Lucas, and that was the sort of work Olsen was doing. In fact, I essentially continued working on what I had been doing at Caltech.

BOHNING: Did you select that?

BREWER: Yes. That was something, I guess, going back to my parents. My parents were always so busy earning a living that I was on my own and I had to learn how to do everything myself, so that was sort of the pattern I developed. As I indicated, I decided I didn't want courses, and I convinced my advisor not to take any, and instead went studying on my own.

BOHNING: What kinds of things were you studying on your own?

BOHNING: Well, in Berkeley we had a seminar then for graduate students and postdocs where the people who attended were expected to give one of the seminars. So I gave a seminar every semester I was here as a graduate student. For example, the Debye-Hückel theory, I gave a seminar on that. Things outside my own research, but trying to get a broader education. That was something that G. N. Lewis emphasized very strongly: the need for breadth, to cover the whole field. For him, physical chemistry was—I don't know if you've ever heard of his definition: "anything that's interesting." [laughter] For him that included meteorology, anthropology; these are all part of his physical chemistry. [laughter]

Bill Giauque was a graduate student here before he became a faculty member. When he retired he told us an interesting story about the effect of Lewis' emphasis on breadth. When he was a graduate student he went around the department and interviewed every graduate student once a week on their research, so he kept up on all the research within the department.

BOHNING: That work was never published, with Olsen, was it? At least I couldn't find it.

BREWER: That's right. You see, I went right into the Manhattan Project, and I've never had the time. That's one of those dozen papers that I'm hoping to publish. It turns out no one has ever done anything like it and it would still be worth publishing.

BOHNING: Really? That's what, almost fifty years after the fact, I guess. Yes.

BREWER: That's right.

BOHNING: That's amazing.

BREWER: In fact, that's part of the problem; I've tackled too much work all along and I haven't had a chance to really write up all of it. Although I've written over a hundred seventy papers, I have at least a dozen important papers that should have been written up that I haven't gotten to yet. I have changed careers every decade of my professional life: astrochemistry, geochemistry, materials science, thermodynamics, and all areas of chemistry.

BOHNING: Who were some of your student colleagues here at Berkeley in that time period before you got into the Manhattan Project?

BREWER: Well, there was Dan [D.] Cubicciotti in SRI down there, and Don [L.] Hildenbrand; Cubicciotti's now with the electrical energy outfit down there and Hildenbrand is in SRI; LeRoy Eyring—some of the students here.

BOHNING: What was Berkeley like at that time?

BREWER: Well, it was very challenging, a lot of interesting work going on. Also, being a smaller university, it had the advantage that you had time to interact with other departments—geology, materials science, and so on. That helped you get a broader preparation.

BOHNING: You completed your Ph.D. in October of 1942, and that's when [Wendell] Latimer asked you to work on the Manhattan Project.

BREWER: Right.

BOHNING: What kind of a person was Latimer?

BREWER: Well, he had broad interests in thermodynamics and physical chemistry generally, and the use of predictive models for predicting the data that were lacking. That was what he emphasized to me: "You should predict the properties of plutonium and all these other things that are needed for the Manhattan Project."

BOHNING: Now, at first when he asked you to work on it, he couldn't tell you what it was.

BREWER: No. But when I agreed to work on it, then he was able to tell. That was one of the benefits of working on the Manhattan Project at Berkeley compared to other places—we got the whole picture here, whereas at other places, they were just told about the particular thing they were supposed to work on and didn't understand the whole picture.

BOHNING: Before Latimer talked to you, were you aware of any of the war work that was going on?

BREWER: No.

BOHNING: Where was it done?

BREWER: Well, it was done, in fact, in the same building I was working in.

BOHNING: I noticed a plaque out here on this building. I didn't realize that this was the site of the original cyclotron.

BREWER: Yes. Well, actually just beyond it, we built just north of it.

BOHNING: So Latimer gave you a project in which he essentially asked you to do several things, as I recall. One was to predict the worst properties of plutonium.

BREWER: Well, he always emphasized the fact that when you're using these models, there's an uncertainty range. In view of that uncertainty, you have to take the edge of what might be a possible value, so that's what he meant by the worst.

BOHNING: One of the things that you had to do was to find something that would contain the plutonium.

BREWER: Yes, so they could cast it. We really assumed it was much worse than it turned out to be because we thought the melting point would be closer to that of uranium. It turned out to be much lower. I don't know; did I have in the Eyring paper (1) the report from Los Alamos about the effect of our containers?

BOHNING: Oh, yes. What kind of models were you using to make these predictions?

BREWER: Well, we were using some of the models that Latimer had used, of how things varied across the periodic table, using the lanthanides to try to predict the behavior of the actinides.

BOHNING: You were really working in a totally new field. There must have been very little information that was available.

BREWER: That’s true; we had to really predict a lot.

BOHNING: How did you come up with the cerium sulfide?

BREWER: Well, we looked over all the refractories. In fact, let’s see [retrieves a book; returns]. After the war, some of our work was published, and this was a volume entitled “The Chemistry and Metallurgy of Miscellaneous Materials Thermodynamics”(4). That was all work from Berkeley and included Marjorie Evans, B. J. Fontana, Dan Cubicciotti, C. D. Thurmond, and E. D. Eastman. But here was all the other work we had done.

BOHNING: All right.

BREWER: None of this had to do with the actinides, but just the variation of properties across the periodic tables. We looked at all the refractory materials—nitrites, carbides, sulfites, sulfides, phosphides—and we indicated here that we considered their thermodynamic properties, and we even tabulated the values of them, so we could calculate what would happen if we have something as reactive as we thought plutonium would be, and we found the oxides would not work, and there our calculations were this type. With the oxides, with the small anion, you get the higher oxidation state, so they’re stable. But when you have a reactive metal, they will often reduce it to a lower oxidation state, which vaporizes as a gas. For example, we considered thorium dioxide and hafnium dioxide as containers. But you’ll get reduction by a reactive metal, going to hafnium monoxide or thorium monoxide. For sulfides, which have bigger anions, the lower oxidation states are more stable compared to the higher ones. Instead of just existing as gases, you can have solids.

[END OF TAPE, SIDE 1]

BREWER: So we felt that the divalent sulfides would probably be much more effective. In fact, ThS [thorium sulfide] had been known and was very refractory, but we took cerium because it was the most readily available and the cheapest of the lanthanides. We studied it and we discovered that it did have a divalent sulfide as we expected and was very refractory, and we worked out techniques for making it. We were very fortunate that in the Manhattan Project there were people from lots of fields and they tried to help one another. Claire C. Blake at Los Alamos had worked for one of these metallurgical companies in Chicago where they did powder metallurgical-separation materials. We would call him up and he would give us advice, which he wouldn’t have given us if he were working for a commercial company. So we got really top-notch advice for powder metallurgical preparations.

BOHNING: Well, it was only, what, less than a year before you were sending them to Los Alamos?

BREWER: Oh, yes. Well, we worked twenty-four hours a day. There were four of us. The three students—they had just gotten their bachelor's degrees and wanted to come to Berkeley for graduate work and instead were enticed into the Manhattan Project. We were very fortunate to have three such talented people.

BOHNING: Who were they?

BREWER: Well, [L. A.] Bromley, [N. L.] Lofgren, and Gilles.

BOHNING: Gilles. All right.

BREWER: They were top notch, and we kept things going twenty-four hours a day.

BREWER: The results of our crucibles at Los Alamos were published in 1961 (5). The interesting thing about that, which I've emphasized in my paper, he makes it sound as though it was an accident that Los Alamos happened to have the CeS [cerium sulfide] crucibles there and didn't give Latimer credit for anticipating that they would have this problem when they first started.

BOHNING: Yes, this sounds that way. It says, "Fortunately, an exotic one, cerium sulfide, had been studied by the chemistry group at Berkeley for use in vacuum at high temperatures, and they willingly gave us a supply of small crucibles." But you were purposely doing this.

BREWER: Oh, yes! It was designed to meet that problem.

BOHNING: It says they were made first here at Berkeley and then later at MIT [Massachusetts Institute of Technology].

BREWER: Yes. They set up a factory at MIT then, using our techniques.

BOHNING: I guess I'm looking for some indication of what it must have been like to start from scratch, to produce; you had to build equipment to make these, work at high temperatures and everything else. You had no background—<u>real background</u>—in that area.

BREWER: No background at all, but the group had a lot of talent, and even though they had no experience, they really wanted to try. For example, we had a glove box that had to be free of oxygen. In order to make sure, we placed a beaker of liquefied sodium in the box. When a film developed on the sodium, we knew that oxygen was in the box. We had to check the seals of the glove box until the molten sodium stayed shiny, which was when there was no oxygen film. During the Manhattan Project, we built a unique, high-temperature vacuum system and it worked beautifully when we were done. We used high-frequency electric fields to generate heating. We wrote up some papers describing this and they were to be published in volumes of this type, but they didn't sell enough of them, so they didn't publish any of the others. So the equipment that we used was never replaced. In the 1990s, when John Kouvetakis was looking through some of our old reports when he visited Arizona State to report our work, he was told that they wouldn't believe that you could do what we had done. [laughter] So maybe we ought to publish that even fifty years later.

Some of our crucibles were heated above two thousand degrees in a pyrex container, which was water cooled, and then the coil around it was generating electricity. The reason we could do that was that we went through very elaborate calculations: how much heat it would take if we had proper radiation shields around it. We used a metallic radiation shield, which people don't believe because they think that would generate heat, but we had it arranged so that there wasn't a completed circle. They acted as very effective radiation shields but didn't absorb much of the electric field, so we were able to get a very high temperature with a relatively small input of energy. We calculated that the interior walls of the pyrex wouldn't get over 150° C. That really worked beautifully. People these days don't believe that could work. [laughter]

BOHNING: Something else to put on your list of papers to be published.

BREWER: Yes. [laughter]

BOHNING: Who coined the name "impervium" that you used?

BREWER: Oh, I don't know who in the group eventually, but we were so impressed we could cast uranium in it, and titanium, and all sorts of things, and nothing would touch it—until that catastrophe with platinum, [laughter] when we found that molten, it destroyed the CeS crucible.

BOHNING: Yes. We'll get to that a little bit later. That must have been quite a surprise when that happened.

BREWER: Yes. But that, as I mentioned I think in one of the Eyring lectures, led to the change in our analysis procedure. One of things we were supposed to do was to develop a method for analyzing plutonium for oxygen content.

Anyhow, at the time we started it was important to have a method for analyzing the oxygen content of plutonium. We started out using a molten bath of iron in the graphite crucible, and we would drop uranium in and the oxygen would react with the graphite and would come off with CO. We could then determine the amount of it-except it didn't work because the uranium was vaporizing out of the iron and reacting with the CO. We had found that platinum reacted so strongly with cerium to pull the cerium out of these very stable crucibles. In fact, we had worked out the thermodynamics and we calculated that the cerium-platinum compound must very stable. We thought, "Well, maybe uranium will do the same," so we used the platinum bath and it worked beautifully. The uranium wouldn't vaporize out of that because it reacted so strongly with platinum just the way cerium did, so we worked out a very nice technique that analyzed the nitrogen, oxygen, and hydrogen.

BOHNING: The amount of work that came out of that early Manhattan Project is really quite impressive, the work that you did that you just showed in that volume there.

BREWER: Well, that was true of the Manhattan Project as a whole. If you go through what was achieved in such small time, one couldn't imagine doing that these days. But one of the factors was that we really had outstanding people. We had all these people who left Europe because of [Adolf] Hitler, people wanting to escape there, really top-notch people. It's amazing what they achieved in such a short time.

BOHNING: Of course, financial support was no problem either.

BREWER: No. We were able to get platinum fast. [laughter] No problem at all!

BOHNING: That probably makes a big difference, considering today's world and what it's like to get support.

BREWER: Now platinum is commonly used. Although we never published that, people heard about it and used it.

BOHNING: Many of these papers from the Manhattan Project were published right after the war (6). Were they declassified that quickly?

BREWER: Well, not quite after the War. For example, it took a while to get them declassified, and this finally came out in 1950.

BOHNING: All right.

BREWER: It took a while.

BOHNING: You mentioned earlier the graphite controversy and Pauling's dismay at your results. [laughter] You've also mentioned Herzberg's refusal to accept that. I remember a paper from years ago titled something like "Resistance by Scientists to Scientific Discovery" (7).

BREWER: Well, that's very common.

BOHNING: Of course, the nitrogen work that came shortly thereafter was another example of that (8). Here you have two people who are acknowledged experts in the field discounting your results. You said it took ten years before they finally believed.

BREWER: Pauling can be very rigid as far as changing his views. It's very hard to budge him. [laughter]

BOHNING: Did you have any personal contact with Herzberg on this matter?

BREWER: A little, yes.

BOHNING: What was it that finally brought them around?

BREWER: Well, after we published that (3), some people were saying, "That's crazy. Let's check it out." There were many confirmations, no question about that, but the general attitude of most people was negative. In fact, Gilles wanted to present a paper at a special conference on this, and they said, "Oh, that's crazy. We don't want to spend any time on that." Something like that! [laughter] So that's very common. People get into a rigid framework and even though they run into a lot of contradictions and so on, you finally have to get a drastic collapse of the system to get them to take a new approach.

BOHNING: Well, it's interesting, because so often one thinks of scientists as being open-minded and more altruistic about their attitude towards new data, new information, but here we have a case where that was not so. Yet the truth will come out in the end, I guess.

BREWER: Well, it's very common, as pointed out by Thomas Kahn, who wrote the book (7) about the philosophy of science and so on. He goes through this in quite some detail, how it's hard to advance change. In fact, G. N. Lewis had this problem. I don't know if you've ever heard the story about him. He always had all sorts of new ideas when he was at Harvard [University] and MIT, and oh, they just squashed him and said, "That doesn't fit in with the accepted view." He was so upset that he quit and took a job—a federal job in the Philippines—while he collected his thoughts. Then he heard about this position at Berkeley and decided, "Well, maybe I could start a different atmosphere here." He came to Berkeley, and one of the conditions for him coming here was that the university would provide some financial support for research. He came here and he saw the budget that was published, and oh, he was so angry. He went down to the president: "You didn't keep your promise!" The president said, "Calm down, calm down. Here it is, 'Miscellaneous.'" [laughter]

He set up a different atmosphere here, and Joel [H.] Hildebrand used to tell the story of his first experience here. He came from the University of Pennsylvania, which was very Germanic; they would never question a professor's opinion. He came to the first seminar and the procedure was that after the speaker had finished, G. N. Lewis would make some remarks, which he did, and some graduate student in the back stood up and said, "Oh, you don't know what you're talking about. That's all wrong; it should be so and so." Hildebrand says he was terrified. What was going to happen to this graduate student? G. N. Lewis put his cigar down and turned and said, "Very impertinent remark, young man, but very pertinent." [laughter] And then Hildebrand knew that this was going to be a different atmosphere.

BOHNING: Do you think that attitude still prevails today?

BREWER: It is hard sometimes to change people's minds. For example, on this business of this model of metallurgy, oh, there was a lot of resistance to that. I don't know; did you read any of my accounts of Neils Engel?

BOHNING: Yes.

BREWER: Yes. His difficulty in getting it even published, let alone accepted. In fact, I just had a paper reviewed in which I pointed out the need for expanding our predictive powers and so on. One of the reviewers said, "Well, I guess we have to publish it, but it only works 95 percent of the time." [laughter] He was raising objections to something like that.

BOHNING: One of the names that shows up in some of that early work is F. [Francis] A. Jenkins. Who was Jenkins?

BREWER: He was a professor in the physics department.

BOHNING: All right.

BREWER: Again, as I was mentioning before, we had much more interaction with people in other departments in those days. Actually, Gilles used several techniques for studying this, and one was a spectroscopic technique. Jenkins was a spectroscopist and we built him a special piece of equipment for studying the spectrum of carbon species. Jenkins was a co-author with Paul Gilles. We had a very effective collaboration of the techniques. In fact, the physics department gave us a laboratory in which we could work. I retained that research space for about forty years until finally they were getting a little pressed for space, and they said, "Well, we have to ask you to move." [laughing] So I had a lot of graduate students do their research work using that equipment in physics. I've had very successful collaborations with other departments-in geology, in the metallurgy, in the materials science department. I've often used their facilities, set up equipment there.

BOHNING: Let me get my list out. You had an early paper in the *Journal of Chemical Education*, "Utilization of Equilibrium Vapor Pressure Data" (9). You were imploring physical chemists, and you said the following: "It is a simple matter to develop equations which will permit accurate predictions of delta-H that will yield data outside the experimental temperature range. Most thermodynamics texts don't tell you really much about it or how to do it."

BREWER: Well, [K. S.] Pitzer and I updated the Lewis and [M.] Randall book (10) and there were books that don't go through that with ample good data.

BOHNING: While you're mentioning the updated Lewis and Randall, how did that come about? I mean, Lewis and Randall is still talked about as being the epitome of a thermodynamics text.

BREWER: Yes, that played a very important role. Lewis really had a tremendous effect—not only here, but in other countries and around the world—and one of the ways was in thermodynamics and to solve problems.

BOHNING: Well, how did you and Pitzer come about to revise it some, what, almost forty years later?

BREWER: Well, we were teaching thermodynamics; we needed a more up-to-date text. Both of us had been teaching thermodynamics for years and we decided to update it. But we tried to keep the spirit that Lewis had presented. McGraw-Hill has been after us for years for another edition and I was always counting; the first edition was in 1923, the second edition 1962. "Come around and see us in the nineties!" They laid off us for a while, but they've been around recently. [laughter] So we've agreed to do it, but boy, I haven't had any time. Pitzer's actually did the work on it, but I've been so tied up I haven't been able to do my part for the third edition.

BOHNING: In 1950 you had a sabbatical at the Imperial College, and that's when you did the nitrogen work (11).

BREWER: Yes.

BOHNING: How did you select the Imperial College?

BREWER: [A. G.] Gaydon was the man I was interested in. He had done some very interesting spectroscopy. Again, he was a person who didn't stick to the established format and came out with a lot of new ideas and so on. I worked with one of his graduate students to get some results that would indicate that N_2 should be more stable than the accepted view.

BOHNING: You had one paper with [Lieselotte] Templeton and Jenkins in *JACS [Journal of the American Chemical Society]* on the dissociation energies of N_2 (8); then the paper with Gaydon (11) came later, and I wasn't sure what the sequence was.

BREWER: Oh, that paper (8) was on CN, yes. Lieselotte Templeton was one of my graduate students and, again, we used that equipment that we had in Paris. We were trying to get some idea, although it wasn't really definitive. It really wasn't convincing.

BOHNING: So this preceded your going to England.

BREWER: That's right, but it didn't really give us the answer, the definitive answer. The subsequent work, I think, did.

BOHNING: There again, the values you came up with were not the accepted values. Did you have the same problem as you did with the graphite?

BREWER: Not as strong, but yes, people were reluctant to accept it.

BOHNING: It wasn't a ten-year wait, though, [laughter] as it was with graphite controversy.

BREWER: That's right.

BOHNING: Now, at the same time, what about your second graduate student, Lofgren: cuprous chloride polymers?

BREWER: Now, that was very important. We were using the bonding model, and in fact we predicted the vapor pressures of all the halides, and so on. There were some problems—cuprous chloride was one of them—where our model didn't work. We weren't considering these very complex polymers at high temperatures. So Lofgren and I tackled that to find out what was really going on with this cuprous chloride vapor (12). According to the literature it was a dimer, Cu_2Cl_2. Well, we found out that it wasn't. It was a trimer, but it turned out the trimer was very stable. From his results we realized our model was incomplete and we'd have to modify it. People still are upset by what we say. We say that if you have an equilibrium between a condensed phase and a gaseous phase, as you go up in temperature the vapor gets more complex. There will be more different species and their size will increase.

People keep saying, "Oh, we know from thermodynamics that as you go up in temperature, the big molecules will break down." But they're thinking of a constant pressure situation, and of course as you raise the temperature of saturated vapor, the vapor pressure goes up. Wherever you work out the model, it shows clearly that complex gaseous molecules will become more stable in saturated vapor as temperature is increased. Now people are finding all these complex species. This causes a lot of trouble in technology, because they try to predict what will happen at high temperatures from what they've experienced at low temperatures and they get all these surprises.

BOHNING: Well, this becomes very important in today's material processes.

BREWER: Oh, yes. People now are recognizing these complex gas species.

BOHNING: So that work was going on before you went to England or thereabout the same time?

BREWER: Oh, yes. Well, before. Lofgren started on that right after the end of the war.

BOHNING: All right. How did you get into that area? That's still a follow up from your original model, then.

BREWER: That's right, to correct it. We find two situations with these predictive models. We find contradictions all the time with stuff in the literature, and the question is, are the literature results wrong—experimental errors? Or is our model wrong? Well, sometimes it's one; sometimes it's the other. [laughter]

BOHNING: What's the percentage? Is your model better, more than 50 percent correct?

BREWER: Oh, yes. In fact, let's see if I have this computer disk. I don't know if I covered that in any of this stuff you read. Here it is. Did I describe that in any of the stuff you've read? I predicted the electronic configurations of the actinides, all the way to lawrencium, and I sent these results to Los Alamos. Oh, they were very upset; it didn't fit in with their ideas, as you can see from that. [Indicates results.]

BOHNING: That's interesting.

BREWER: But they finally had to come along. [laughter] It was a very simple thing. They assumed that lawrencium would be like lutetium, which is a corresponding lanthanide. In the case of lutetium the ground state is ds_2. But as you increase your nuclear charges—that is, as you go through the actinides, the s and p electrons are more strongly affected than the d, because the s goes right through the nucleus, and the p goes in front of it, whereas the d, I like to describe as just going around the outside. [laughter] According to my model, the p would be stabilized over the d, and you'd get s^2p.

The Los Alamos people had been assuming all along that lutetium would be the same as lawrencium.

BOHNING: That's interesting.

[END OF TAPE, SIDE 2]

BREWER: At the fiftieth anniversary of the discovery of plutonium, we had a special symposium, and they asked me to give a special talk on what I had done on the Manhattan Project and on the actinides afterwards. One of the things I presented there was the predictions I made in 1971 for the lowest levels of each of the configurations. There were no experimental data then. In the next twenty years experimental data were found, and there was a sort of comparison.

BOHNING: That's amazing. [laughter]

BREWER: They found the same levels that I predicted. Well, here there are a couple small deviations, but I had indicated that before; otherwise the states all corresponded.

BOHNING: That's amazing. Do you have a copy of that talk?

BREWER: Well, I guess she didn't say, but they did record it and they did do something with it. I think Darleane [C.] Hoffman should be able to provide a copy.

BOHNING: All right. Where is she?

BREWER: At the LBL [Lawrence Berkeley National Laboratory] and in the chemistry department.

BOHNING: All right.

BREWER: She's an emeritus professor.

BOHNING: I'm going to sidetrack for a moment. You said she's an emeritus now?

BREWER: Yes.

BOHNING: Because we're constantly looking for women to interview on their experiences, and I'm not aware of her at all.

BREWER: She's actually taking a job now at the Livermore [Lawrence Livermore National Laboratory], at the Seaborg Institute [Glenn T. Seaborg Institute for Transactinium Science].

BREWER: All right. She would be someone, then, who we should probably talk to.

BREWER: She worked at Los Alamos for many years and would talk to you.

BOHNING: You indicated in your Eyring talk that after the Manhattan Project you became a ceramist for a while. You looked for other refractory materials, primarily silicides and borides and transition metals. You had a number of papers on this.

BREWER: Yes. We thought those would be useful refractories, and they have turned out to be very useful. We've been pioneers in developing that. [laughter]

BOHNING: Did you use similar techniques to what you did with the cerium in terms of preparing them?

BREWER: Yes.

BOHNING: Were many of them unknown when you started?

BREWER: Oh, yes. We discovered a number of them.

BOHNING: Which were the best sulfides?

BREWER: Well, the most stable is the thorium sulfide. For really extreme conditions, high temperatures, or very reactive metals, that would be the best. We did send Los Alamos some of the thorium sulfide crucibles, too.

BOHNING: Again, in the early 1950s you had a number of different papers. You looked at the stability of gaseous diatomic oxides (13).

BREWER: Yes. People often asked me what field I worked in. Well, I was in organics and ceramics, and then I became an astrochemist. I was interested in high-temperature molecules, which would be in stars or comets. I did a lot of work trying to characterize and be able to predict. We did a lot of predictions on stability in gases.

BOHNING: You also had a paper with John [G.] Phillips, who was an astronomer (14).

BREWER: Right. That was on C_3. In fact, we did a lot of work continuing on carbon vapor, C_2 and C_3, really characterizing what went on there.

BOHNING: Did you follow your predictions up pretty well with experimental verification?

BREWER: Yes. They checked out pretty good.

BOHNING: You did that too?

BREWER: Yes.

BOHNING: All right. Were there other people working in this area?

BREWER: Oh, yes. Of course, right now they're up into their sixties. [laughter]

BOHNING: But you were really the pioneer in the high temperature work, weren't you?

BREWER: A lot I guess, yes. Here's a real pioneer [Henri Noissan]. [laughter]

BOHNING: Oh, yes. That's a good picture.

BREWER: In fact, we've just carried out some of his work on molybdenum carbide [MoC] (15). He found this MoC phase and later it was repudiated. They said it was unstable and only produced when you had nitrogen or other impurities around, but my student, John Kouvetakis, was able to show that it really is stable, at least up to about, what was it, 1,100 Kelvin? [laughter] Then it decomposes and forms certain phases.

BOHNING: You've commented about the breadth of your work and we just mentioned the work with John Phillips. You also did a paper on the equilibrium distribution of elements and the earth's gravitational field, which is geology (16).

BREWER: Yes. The geologists asked me for help there. There were some geologists who claimed that uranium and thorium and these heavy elements would all be concentrated in the core, because in the gravitational field the more dense ones go down. But, as my paper showed, it's the other way around, because what happens is the major elements like oxygen and magnesium and so on concentrate near the crust, as you would expect from the effect of gravity. But uranium and thorium react very strongly with oxygen, so they're going to follow the oxygen gradient. If you go through the thermodynamics, you find that the gravitational term is wiped out by the bonding. So the predictions are that they're more concentrated in the crust than in the core of the earth's center.

BOHNING: How was that accepted by the geologists?

BREWER: I think they generally accept that point.

BOHNING: I'm just looking at your publication list now. You did some more work with James Kane, red phosphorous and arsenic, again looking at complex molecules at high temperatures (17).

BREWER: Yes. That goes back to the graphite problem again. I think it was one of my graduate students who worked out the kinetics of sublimation surfaces. Are there materials where the rate of vaporization in a vacuum is very much slower than you'd expect from their equilibrium? The red phosphorous and the arsenic are examples there. You have in the gas a P_4 and an As_4 molecule. In the solid, they're in a more planar structure, and you have to distort quite a bit to get As_4. So it turns out that's a real barrier. He was able to show in some cases the rate of sublimation in the vacuum is a factor of ten to the seventh power slower than you would predict from the equilibrium pressure.

He did some interesting work, and he was able to show that you can catalyze it. He was able to add, for example, some liquid thallium in which the arsenic would dissolve as atoms and then on the surface be able to recombine to form As_4. It wouldn't be restricted to the solid rigidity.

BOHNING: The work on gaseous molybdenum oxychloride (18): how does that fit in with your ideas?

BREWER: Well, that goes back to Lofgren's work, actually. We had done some work—one of the chapters in here was predicting the behavior of the halides of molybdenum and tungsten at high temperatures (19). There wasn't much data on that, and after Lofgren did the cuprous chloride work (12), he thought he would check the molybdenum work, and he did as he had done with the copper. In copper, he passed HCl-hydrogen over copper, and varied the HCl-hydrogen ratio, and that was able to fix the formula of the cuprous chloride. He did the same for molybdenum and he found as he varied the HCl-hydrogen ratio that there were four chlorines, the gaseous molecule, which seemed reasonable-$MoCl_4$. But his results didn't check with our thermodynamic predictions and in particular didn't check with the entropy. We knew the entropy couldn't be off as far as what he had indicated. He concluded that he wasn't getting $MoCl_4$; he must have been getting an oxychloride. He was doing it in a quartz tube, and somehow oxygen reacted with the gas. So I later had an undergraduate check that out, and he was able to show very convincingly that it was the oxychloride that was being produced there.

Now, there was another interesting aspect with this. We did this with molybdenum, where I predicted, where it wasn't known, all the properties of molybdenum halides. There were three papers in the literature in which they passed iodine over molybdenum in a quartz tube, and they found vaporization, as they varied their iodine pressure, and it was proportional. They said they were getting MoI_2, but the amount of MoI_2 they were getting was very much higher than what we had predicted in our previous work. We felt sure that they were getting $MoOI_2$. And Schaeffer in Germany checked it out by putting in some graphite to keep the oxygen from reacting. Then you can't get any significant MoI_2. That's an example of where the experimental data were way off.

In fact, I wrote a paper on that; the data and the literature are very far off (20). I don't know if you ran into that one on how important it is to check—first to have good models to give an idea of whether they're right or not, and secondly to check for systematic errors.

BOHNING: Was this one of the *Journal of Chemical Education* papers?

BREWER: No. I do cover that in one of them, but I have a more recent one in the metallurgical journal. Let's see; here it is, "The Critical Evaluation of Typically Unreliable High Temperature Data" (20).

BOHNING: All right.

BREWER: I go through a number of examples, and what happens? In fact, there was a fellow: his name is given in this paper. What this fellow said is, someone will do an experiment, get a result, and publish it; someone else will repeat it, and as he varies the conditions and checks the parameters, he finds them to vary. Finally they get a value corresponding to the initial publication and they'll publish it again. You'll get more and more confirmations as you go, until finally you get someone who is very thorough in checking all the systematic errors, and he finally finds he gets a number much different. Then he'll publish it; all the old data are wrong.

So there are these systematic errors. We've been doing some work recently with high temperature solid-state EMF [electro-magnetic field] readings. They're very easy measures to take, but we've found the modern data literature way off, and particularly in the MoO_2 review I made there. I found thirteen papers in the literature where they used that technique, and only three of them were usable. The others clearly had very serious systematic errors.

BOHNING: Well, this presents an interesting problem, doesn't it, when one is using the literature?

BREWER: That's right.

BOHNING: You're looking at all these numbers. How do you determine—

BREWER: That's why you have to have a model. I never go reviewing the literature without having some way of checking out: does this fit into a reasonable model?

BOHNING: In one of your *Journal of Chem. Ed.* papers you talked about a meaningful inorganic chemistry course (21). In that paper you emphasized using an inductive process and going from data to a model and then using a deductive process to go back to the data.

BREWER: That's very important.

BOHNING: Is that the way you were teaching inorganic chemistry?

BREWER: Yes.

BOHNING: Because in 1959 that's probably not the way most inorganic chemistry was taught.

BREWER: No. And in fact, I published a paper somewhere or other. I had a paper in the *Journal of Chemical Education* just before that, "Undiscovered Compounds" (22).

BOHNING: Yes.

BREWER: Okay. I had another one; I forget the name. Well, I've emphasized that in "Principles of High Temperature Chemistry" (23).

BOHNING: Oh, yes. That's the Welch proceedings.

BREWER: Somewhere or other I think I have another paper. Also "Relevancy of Research," (23) covers that and the role of models. Too many papers here. [laughter] Well, there's "Principles of Critical Evaluation and Compilation of Phase Diagrams" (24), this business of using models. I've given my paper at some of the conferences of chemical teachers and we covered thermodynamics. I know it's somewhere around here, but I'm not sure which paper it is. "Methods of Obtaining Thermodynamic Data," (25) probably covers it to some extent. Anyhow, I've emphasized that in a number of papers.

BOHNING: How receptive was the chemical community to this concept? As you said, it plays such an important role throughout your entire career.

BREWER: Oh, I think generally that's well accepted. There certainly are lots of people who use the same approach very strongly.

BOHNING: I'm going to share with you my own experience in inorganic chemistry, because one of the things I didn't like about inorganic chemistry was memorization. It seemed to me there were all these disjointed facts. I'm not good at memorizing either.

BREWER: Yost played a very important role because he always had these models for trying to tie data together.

BOHNING: Even today inorganic chemistry texts don't seem to emphasize that as much as they should, from what I've seen.

BREWER: That's right.

BOHNING: Let's talk for a moment, since we're talking about your teaching this inorganic chemistry course. Was that an undergraduate course or a graduate course?

BREWER: I've done both.

BOHNING: What other courses did you teach here at Berkeley?

BREWER: Oh, I've done quite a few. Of course, Thermodynamics, and I have a course, which isn't taught now, Phase Diagram Equilibration. In fact, recently at a meeting a fellow came down and said, "I want to thank you for changing my whole career. I took your course." It wasn't recent, but he was referring to a course he took in the 1950s with me on phase diagrams. He said, "I was originally going to be a metallurgist and I took your class." Oh, yes, I have this phase diagram here.

BOHNING: Oh, yes.

BREWER: It's falling down. I covered how we could predict what would happen with these ternary oxide systems. He said, "Oh, I found that so exciting I became a ceramicist." He works for Dow Corning now. [laughter]

Then, of course, freshman chemistry.

BOHNING: You did teach freshman chemistry.

BREWER: I was very active in the laboratory. I did something that they've abandoned now. The TAs say they won't put up with that, but I insisted that the TAs do the experiments the week before the students do it, and I would even take their notebooks and go through and tell them about what they did incorrectly. Because I found the TAs didn't understand the experiments and were telling erroneous things to the students. They now have these graduate-student TAs who say, "Oh, that's too much work; that must do." [laughter] They claim I was too tough on them. [laughter]

BOHNING: But that's the only way you learn both chemistry and teaching. You have to do it yourself.

BREWER: That's right.

BOHNING: If you haven't been through that, how do you explain it to someone else?

Let me come back to your staying on here at Berkeley. You got your Ph.D. in 1942. Then you went into the Manhattan Project until 1946, I think.

BREWER: Yes.

BOHNING: Then you stayed on at Berkeley. Had you thought about looking elsewhere at that time?

BREWER: Well, actually I had offers, like John Chipman, who was familiar with my work on the Manhattan Project and invited me to take a position at MIT. But I had been told that even though I stayed on doing the research work, I had a teaching position available here, so I stayed here.

BOHNING: We talked about your revision of Lewis and Randall. We're up to about the 1960s. Instead of looking at specific papers at this point, maybe we could look at some broad categories. You did substantial spectroscopic study of gases and equilibrium of graphite.

BREWER: Yes. That was C_2 and C_3.

BOHNING: This is still going back to those early graphite days, isn't it?

BREWER: Yes. I think we've pretty much taken that as far as practical. Lucy Hagan's last paper really covered that adequately (26). That was done quite some time back, but we didn't get around to writing the document. It was a rather complicated document to explain all the possible systematic errors that we had to check out and compare. That was published in 1979, but she did that a decade in advance.

This is the origin of these models. As I mentioned before, I have a very poor memory. To give you an idea of how I use the model, a student came around recently and said he'd like to borrow that book he'd borrowed from me several years earlier. "Gosh, I can't remember having that book, but if I had a book of that type it would be stored over there—oh, there it is!" [laughter] So I have to have ways of predicting things. I can't remember them.

BOHNING: Well, I can identify with that. I get annoyed every time I read a textbook in which the author says, "You must memorize the following." But today's students are incredibly adept at memorization. I'm still amazed at what my students could memorize.

BREWER: Well, we've had that problem in preliminary exams. We have a student, the record shows an "A" in all his courses, but you get him up there and have him try to explain something, a situation a little different, and he's just a blank. [laughter]

BOHNING: Well, do your examinations then require the same kind of thinking process?

BREWER: Oh, yes, on our oral prelim exams, we check to see how they can tackle a new situation and think through it.

BOHNING: How do you find the students? Have they changed over the years?

BREWER: Well, the main change I've noticed is they're poor at writing. Their writing abilities have degenerated a bit. The oral is probably not that much different, although some of them are very reluctant to speak. [laughter]

BOHNING: Yes. Well, let's come back if I may for a moment to the spectroscopic study. You did a number of general things—we've just talked about that—and then it branched in two directions. There were studies designed to fix thermodynamic data of gases by way of radiative lifetime determinations. Is that still using the spectroscopic equipment? How did you do that?

BREWER: Well, actually we did this in the lab next door, which we had to move out of. [laughter] The idea was that if you knew the lifetime, the radiative lifetime, you could calculate the reverse process, the absorption coefficient, and you could use it as an analytical tool to get the equilibrium concentrations. So we were very interested in improving our ability to measure radiative lifetimes. In the equipment we used over in the physics building, we knew we had a way of predicting the absorption coefficient from the radiative lifetime. So with a number of high-temperature equilibrium, we were able to determine the vapor pressures by spectroscopic absorption measurements. Of course that's very important for astronomical purposes, to determine concentrations around the stars.

BOHNING: Have astronomers picked up on your work?

BREWER: Oh, hi! [Someone enters room.] I'd like to have you meet one of my other students, Karen Krushwitz.

BOHNING: How do you do?

KRUSHWITZ: How do you do? Whom am I talking to?

BOHNING: Jim Bohning.

KRUSHWITZ: How do you do, Jim Bohning.

BOHNING: I'm from The Center for the History of Chemistry [Chemical Heritage Foundation].

KRUSHWITZ: Ah! [laughter] I've heard about your center.

BREWER: You have?

KRUSHWITZ: Yes.

BOHNING: Well, that's good.

KRUSHWITZ: From my advisor. [laughter]

[END OF TAPE, SIDE 3]

BOHNING: Then you also used [George C.] Pimentel's isolation matrix method.

BREWER: Yes. That was also to characterize some of the high temperature molecules.

BOHNING: Did you collaborate with him at all?

BREWER: Well, a little—just on techniques—but mostly we were looking at molecules that weren't of interest to him.

BOHNING: Well, that brings us to the Engel's story, and I don't know how we do that, but that's a long and ongoing story.

BREWER: I don't know how much of that you've covered, but did I discuss his visit to Berkeley, his sabbatical here?

BOHNING: Yes.

BREWER: He had an inability to publish. I was asked to give a paper on metallurgy at the time when I was an astrochemist. [laughter] A fellow—[Paul] Beck was his name, of Illinois—I asked him later on: "Why'd you ever ask me?" He said, "Oh, I thought you were working with metals. I remembered your Manhattan Project work." [laughter]

BOHNING: Well, you were certainly instrumental in getting Engel's work (27) to see the light of day. That was another example of something not being accepted because it was different and controversial.

BREWER: That's right.

BOHNING: Why did you pick up on that?

BREWER: Well, it goes back to that platinum accident. [laughter] I didn't understand at the time why platinum was reacting with cerium, but from Engel's model it was immediately obvious—Engel's model plus G. N. Lewis' acid-base concept. That was very intriguing, but I still didn't go into it except in my undergraduate courses. I was teaching an inorganic lab where they had to do a little research problem, so I would give them these problems to test the strength of the acid-base reaction. I had one of the students put ZrO_2 in a furnace and calculate a reaction; when passing hydrogen over it, how much water would form by reduction to zirconium? They did the experiment with platinum present, and the amount of water was up a factor of ten to the twentieth power, or something like that, due to the lowering of the activity of the zirconium by the platinum.

The other student heated zirconium carbide with platinum, and I'd say the platinum oxidized it to liberate graphite. So I did publish that, the results of those experiments, and the results of John Margrave's student's detonation accident (27, 28). In our case, we started with the zirconium already tightly bound, so when it reacted with platinum there wouldn't be much heat evolution. But when the student mixed hafnium and platinum together, the temperature shot up to 6000°. [laughter]

There again the metallurgists were really upset because metals don't react strongly with one another. A lot of people still find it hard to accept that these inert metallics can be so stable when you have left hand plus right hand transition elements.

BOHNING: Well, you're still involved in that, aren't you?

BREWER: Yes. Well, I don't know if I mentioned in any of the papers the way I got my first graduate metallurgical student. I had published the results of these undergraduate students, and we had a paper reviewing Engel's work and so on (29). I got a letter from P. R. Wengert, who had just gotten his bachelor's degree in London. He said, "I've read some of your work and I'd like to do my Ph.D. on it. Would your department accept me even though I've never had organic chemistry and I don't want to take it?" [laughter] So that was my first graduate metallurgical student. It seemed more and more challenging and he gradually expanded it.

BOHNING: In the list of publications you sent me, you have a number of papers that you flagged as being important. Maybe we could just discuss those briefly and you could comment on them. We've talked about a number of them, but the first that I have on my list that we haven't talked about is "Predictions of High Temperature Metallic Phase Diagrams" (28).

BREWER: Oh, yes. Well, that was where I developed the Engel theory for the first time to really apply it very broadly and add some features to it. I presented that in a seminar or a symposium here at Berkeley. It was almost a one-hundred-page paper. I went through it in great detail to show how widely applicable his ideas would be and how you could predict all sorts of behaviors. Particularly, I think some people now are getting excited about—it's taken a long time for them to recognize it—the way you can predict multi-complement phase diagrams very effectively.

BOHNING: Well, that's one area though—I think that showed up in the questioning at the Eyring lectures—that still isn't covered very well in curricula, and that's metals.

BREWER: MIT has, in fact, a book out in which they cover it; they've used it in one of their undergraduate courses. This was in metallurgy. Not many chemists present it. In fact, that's one of the liabilities of most chemistry programs. There's not enough emphasis on the solid state, generally. There should be much more.

BOHNING: Well, lots of materials science departments have sort of taken that part of it away from chemistry, haven't they? A lot of people who are in that area move to materials-science groups.

BREWER: In fact, that's where the greatest contributions have come from: from chemists who have moved over to that area. A lot of the people who have just the typical engineering background don't have enough chemistry understanding, and they have a great difficulty following the chemical bonding problems.

BOHNING: The next paper is: "Viewpoints of Stability of Metallic Structures" (30).

BREWER: Okay. That was a continuation of this. This was a conference in which most of the people there objected to Engel's ideas, and you know Engel's model takes off from the [William] Hume-Rothery rules; that's a decisive part of it. Hume-Rothery was there and wanted to come around and talk to me privately; he's hard to talk to because he's very deaf. We got along very well. He said, "Yes, I think this model would be very good. I would like to push it if you don't associate Engel with it." [laughter] He for some reason didn't want Engel to get any credit. Well I told him, "Gosh, I can't do that. I can't say I made up these ideas." [laughter]

BOHNING: That's interesting.

BREWER: He then did write a little monograph on the subject, which mostly has been good. He did have some errors in it; I had to correct some of them.

BOHNING: Did he mention Engel at all?

BREWER: Oh, yes.

BOHNING: All right, so he did.

BREWER: But there was a lot of resistance, and a lot of people here at this conference didn't like it. So, in fact, all these different pages are places where they've raised questions saying, "This doesn't seem right," and I'd have to defend it, and so on. In fact, I presented a paper that was published (31). General Electric [GE] had a series of papers presented from which they published a book, and one of the chapters is mine. He said, "Well, so many people say it's objectionable. Can you defend it?" So I took the portions of the paper (31), in which they raise objections, and I inserted it as an appendix there, to point out that they don't understand it very well.

BOHNING: The next one is "Energies of the Electron Configurations of the Lanthanide and Actinide Neutral Atoms" (32).

BREWER: Now, one of the decisive points of the model I'm using is that I often like to use the Born-Haber cycle as a starting point. The idea that if you just look at the alkali metal formations of halides as you go down the periodic table, the heat of formation of fluorides decreases from UF to CsF. For the larger halogens, the stability increases from Li to Ca. But if you go through the Born-Haber cycle and separate the energies of formations of the ions and the halide lattice energies, you then do get a simple picture that gives you trends that you can represent.

The point is that you want to represent the behavior of metals in terms of a gaseous atom of the same electronic configuration, so that means you have to know what are the energies in different configurations. What I did here, since the data were lacking for the lanthanides and actinides, was actually use some models. It was an Italian spectroscopist who started it; it looked very promising. Unfortunately, he died at a very young age from carbon monoxide in a motel he was staying at in Italy. Apparently their heating system caused it. I thought I'd carry out his model and develop it more and essentially predict it; in fact, as I was showing you and ended up doing, that was when I predicted the energies of electron configuration of lanthanides and actinides.

BOHNING: All right.

BREWER: Now, that has been very useful because it has allowed me to make predictions about many metals. In fact, John Kouvetakis and I are working on a paper now where we're predicting the stabilities of the hexagonal close-pack structures of all the elements, or almost all the elements. Do you know, in many cases they're quite metastable? By now people are making these metastable phases by getting a high-energy source that kinetically will produce this metastable phase. So we're predicting how stable they are, and we can do that if we have the spectroscopic data. We have almost all of it. We're missing data for some of the configurations with one s and two p's that we need for the heavy metals. Unfortunately, they have high energies and the spectroscopists have not done an adequate job of trying to get them. Whenever I run into them I needle them, "How about getting us some more data?" [laughter]

I have used various models for trying to predict them, but we don't have enough data to really make sure my model is adequate.

BOHNING: The next one was "Energies of the Electronic Configurations of the Singly, Doubly, and Triply Ionized Lanthanides and Actinides" (33).

BREWER: Well, that was a combination of the two. Here they cover all the ions. The ions of course are very useful for predicting stabilities of oxides and halides and so on. It was very important in a lot of respects. By knowing the way that the d, s, and f electrons vary in stability across the periodic table—the ions as well as the neutrons—you can do a better job on the neutrals because you get a better understanding of the interaction of the various kinds of electrons with the nucleus, the shielding of the inner shells and so on. By looking at the whole picture, you can then do a better job on the neutrals. That's true of a lot of these models. Some people sort of take a very narrow view of the given problem. Often if you try to take a broad view, way outside your immediate interest, you then will have a better understanding of your narrow problem.

BOHNING: The next one was "Transition Metal Alloys of Extraordinary Stability" (34).

BREWER: Okay. For that, [P. R.] Wengert was the student.

BOHNING: That was the student you mentioned earlier.

BREWER: That was his thesis, in which he did show a very clear confirmation that the Lewis acid-base reaction is going to be very strong and that it varies in the way you would expect. Yes, he did a nice job. He was a very good writer. It was a very great loss when he died of cancer when he was young. He could have really made many more contributions.

BOHNING: My goodness.

Well, the next one was "Thermochemical Properties of Molybdenum and Its Compounds and Alloys" (35), I believe.

BREWER: Oh, yes. Well, the International Atomic Energy Agency asked me to do a review of the thermodynamic data of the phase diagrams of molybdenum, which they had done on other elements, but the other reviews had only covered systems where data were available. I said, "Well, in most of the systems with molybdenum there aren't any data. Can I use predictive models to fill in?" They said, "Well, if your accuracy is within practical engineering accuracy." Well, I did, and an example I'd like to give is this one. Oh, yes. Here we are. No data were found, but there is the phase diagram of lawrencium. Obviously people aren't going to get that experimentally, but I'm quite sure it's quite accurate. It was not only lawrencium. In many of the elements no data were found, but I was able to predict what the phase diagram would be. This is an example of the power of these predictive models. You can get very valuable information that you may not know.

BOHNING: Let's see, the next one I have is "Thermodynamic Data for Flue-Gas Desulfurization Processes" (36).

BREWER: Yes. That was another example of the power of models in thermodynamics. The question was flue-gas desulfurization in aqueous systems at high temperatures and so on. Essentially what I did was extend the thermodynamic data up to about 500 Kelvin, in most cases for a lot of aqueous species, as well as a lot of gaseous species and solid species that are important for this process. I got together all the data one would need to really work it out.

BOHNING: The next one is "Systematics of the Properties of the Lanthanides" (37).

BREWER: Now, this was several things. One was an update of that paper from back in the 1970s on spectroscopic data, but carrying it a little further. From the spectroscopic data, which used the electronic configurations, I then predicted the thermodynamic properties of all sorts of compounds. Let's see. To give you an idea, first I have the spectroscopic data as I had in the previous paper. Here are the enthalpies of formation of the gaseous, the monoxide, the M_2O, the M_2O_2, and MO_2 species. Predicting all of those, we know what happens in high-temperature gaseous species, essentially summarizing other thermodynamic data for the lanthanides to show what you can do with these models. For example, here we're predicting solubilities in thorium, plutonium, and uranium of the different elements—and stability of diatomics and so on.

BOHNING: This one isn't on your list of significant publications, but the title is intriguing, "The Responsibility of High Temperature Scientists" (38). Oh, and there's "The Generalized Lewis Acid-Base Theory: Surprising Recent Developments" (39).

BREWER: Oh, yes. It goes back to the story on the cerium sulfide plus platinum and how it got us into developing the Engels theory.

BOHNING: The title on high temperature scientists is intriguing, but when I looked at the paper, it wasn't really what I thought it was going to be about.

BREWER: Well, again, it's the need to use models. This one was the actinines. I believe that was the one; let's see. Now this was a special symposium on my sixty-first birthday. What I wanted to do here is point out the need to critically evaluate data in the literature and to extend them using the models, and then as an example I went through the actinides. This case updated the spectroscopic data in sufficient detail so I could calculate the partition functions and work out the thermodynamic data of high temperatures. Now, essentially the responsibility I had in mind is of this type. If you work as a specialist in a given field that you really understand very well, then you have the responsibility of making the general models and concepts available to other scientists. That is, you should try to show how this could be used by people in other fields. Also, if you collect a lot of data that you're using for your work, then do a little more work to put it in a form where it's available generally.

Consider the problem of the engineer who needs some data for his process. It takes so much time and the expense of his time to go through the literature to try to get the data, and if he doesn't have the practical experience with those experiments, he won't know how to evaluate the discrepancies. You want a specialist who'll go through and know which things to throw away, which things to keep, and then publish it in a form that is generally available. That's a very important function I think people should do.

BOHNING: Do they do it?

BREWER: Well, not sufficiently. That is, they're limited, and particularly with the [National] Bureau of Standards being financially handicapped, they have not been able to do that. It's not adequate. People would be a lot more efficient if they had the money to review the data of the field. A lot of people don't realize how important that is. When the Bureau of Standards was being curtailed I was in Washington on some other business. I made an appointment with the budget director of the department of Congress. He talked to me about this. He didn't know anything about what they did, but he called one of his assistants over who did, and we got into a discussion about it. Their final statement was, "We don't think the work the Bureau does is of any use. We never hear from industry that it does any good." When these engineers use these tables, they don’t realize the work that had to be done. They continue to take advantage of it. But I couldn’t have done 75 percent of my research without Charlotte Morris’ papers (40).

BOHNING: Oh, yes.

BREWER: She did a tremendous job and has helped so many people to have all that data available. Without that we would have been handicapped in so many respects. We need more people like Charlotte. Well, she went way beyond—she spent her full time on that—but what I’m saying is that a specialist in a given field should take a fraction of time and devote it to a critical review of the data of their field.

BOHNING: Let’s see. I think the next one was "Generalized Lewis Acid-Base Titration of Palladium and Niobium” (41).

BREWER: Okay, that was work that was done by Michael Cima. He’s now a professor at MIT, and he was really a chemical engineer. He did some undergraduate research with me and then decided he wanted to do his graduate work with me, and the chemical engineering department agreed to that. He did a very thorough job of getting quantitative data on these acid-base reactions. When I agreed to do molybdenum, I wouldn’t have agreed to do zirconium, because I couldn’t quantitatively predict zirconium-platinum. I know the interaction is very strong, but I didn’t have the model.

But we’ve gotten enough data through other students and Michael Cima, so that now the next project is to develop a model that will cover quantitatively the acid-base interactions, so that we can cover any combination of metals. Not only just transition metals with one another, but transition metals with non-transition metals. One of the papers I have marked is the paper I wrote showing what aluminum does to transition metals.

BOHNING: Well, that’s “The Nature of Bonding in Transition Metal Aluminides” (42), I think.

BREWER: Yes. There are also very important ideas for this model that we hope to develop. It’s going to take a lot of work and I’m going to need a number of postdocs for it, so I’m planning to apply to the National Science Foundation [NSF] to get enough money to really do this job, which will be a culmination of all these other models. It should have a very broad application.

BOHNING: How has most of your work has been supported?

BREWER: The AEC [Atomic Energy Commission] or the DOE [Department of Energy]. Some of the matrix isolation work was NSF. The DOE doesn't have enough money, so I'm going to go back to NSF. [laughter]

BOHNING: The other paper I was interested in was "Nuclear Reactor Accidents" (43).

BREWER: Yes. That was an interesting session we had. That was in Florida.

[END OF TAPE, SIDE 4]

BREWER: We were asked to review this computer model that they had for predicting what would happen in case of an accident, and it was inadequate. To give you an example, they had trouble understanding why as much ruthenium vaporized from Chernobyl as they found. Their model was only considering binary gaseous species, and they weren't considering ternaries. In the case of ruthenium, the oxyhalides-oxyiodide for example-would be very stable, and their calculations weren't considering that. We had a number of suggestions on how they could improve their models. [laughter]

BOHNING: I think that pretty well covers the papers you had highlighted. Of course, we have the Eyring lectures (1). That's not out in a book form yet.

BREWER: Well, I don't know if they will be. They had trouble working out arrangements with a publisher. The publishers didn't think enough volumes would sell and so on. The last I heard they probably won't publish it, but people have heard about it, and I've been getting requests for it. [laughter] The trouble is, with the limited time and presentation, it doesn't give enough detail, and people would really have to go to the references to really understand it.

BOHNING: Well, that's becoming more and more of a problem in chemistry; the cost of the book has gotten so high, the market is evaporating.

BREWER: It's unbelievable, you know. For some of them, the cost is sixty cents per page! [laughter] Students are photocopying them rather than buying them.

BOHNING: Well, I know when I was selecting textbooks for my students, if I ever asked a sales representative what the cost of the book was, I could never find out. They didn't have that information.

BREWER: They didn't want to have that information. [laughter]

BOHNING: I didn't know until it showed up in the bookstore what the cost was going to be. The students, they would let me know. [laughter] A specialized book now has just become very, very difficult. There are some moves to go to electronic publishing. Maybe we'll end up with that.

BREWER: Yes, they're talking of that. [laughter]

BOHNING: I wanted to ask you about a couple of other things. In your Palladium Medal address in 1971, you said the following: "It is my contention that the current political pressure to orient all research toward quickly obtainable practical results will be destructive of research progress and will seriously handicap the development of practical results." That was twenty-one years ago.

BREWER: You really see a lot of that; the industrial point of view is very short term, and they don't even think of the possibility—the Japanese are doing it—of setting aside a decade for R & D. In fact, John Kouvetakis has just had this experience working for industry, and they don't let you work on the problem long enough to really get a full development. Many of these developments have pitfalls, and you're not sure which way to go. It takes a while. This emphasis on the short term is destructive.

BOHNING: You feel that's still the case today; that hasn't changed in twenty-one years?

BREWER: Oh, yes, definitely. In fact, it's even worse. I remember I was consulting for Union Carbide and, at the time, they were interested in doing some fundamental research in directions that would be important. They were building a laboratory, I think in New York somewhere or other. Then there was some change in the board of directors who were all lawyers and things like that, and they decided, "Oh, that's a waste of money." Even though they had the building under construction, they stopped it, and they curtailed some of the long-term fundamental research that could be of interest.

BOHNING: Is it because the business people are taking over the management of the companies?

BREWER: Well, the same has happened to General Electric. General Electric, in the old days, did some beautiful work in developing new concepts, and they've now curtailed all of that. I don't know if you've ever heard of the diamond business at General Electric, how it started? Tracy Hall was working for General Electric, and he had the idea of producing batteries under high pressure. They said, "Oh, no. You're wasting time on that." But he did it anyhow. At that time, they didn't interfere as much as they do now, but he never got a salary increase. His salary was just frozen, but he kept working at it. One day he came in with a handful of diamonds. His boss says, "Oh, you're going to get a salary increase!" [laughter] He says, "I'm quitting." [laughter] He went back home to Utah, to Bringham Young [University]. At first GE had the patent on the measly device, but he invented an even better one and did some nice work.

But it's becoming very difficult in industry to really do a big project.

BOHNING: Years ago it wasn't uncommon to have a scientist as the head of the company or involved in the management, but that seems to have changed drastically.

BREWER: In many companies, yes. Fortunately there are a few of these semi-conductor companies that still have some scientists running them.

BOHNING: You were involved with the [Berkeley] Radiation Laboratory for a long time.

BREWER: Yes. From the Manhattan Project days on.

BOHNING: But then you were head of the Inorganic Materials Research Division [IMRD] at the Berkeley Radiation Laboratory. Did you start that group?

BREWER: Yes. I really didn't have the time for it, but it was a worthwhile effort because I was able to get a lot of young people started in solid-state work, particularly. Most of the people now in physics in their solid-state program—and they've been very productive—started through IMRD. We were able to get the building built, and we were able to have people use sophisticated equipment that they wouldn't have been able to afford on their own. It was really very productive.

BOHNING: Let's talk about Berkeley in general, and the colleagues you worked with over the years that you were here. Can you say something about some of those people?

BREWER: Well, of course in the G. N. Lewis days I certainly found it very stimulating, and we've had some very good deans trying to maintain that type of tradition. We still have some very good research and teaching going on.

BOHNING: Are there any specific faculty you interacted with who influenced your work or whom you collaborated with?

BREWER: Oh, yes. There were quite a number whom I worked with. Of course, there would be people like Pitzer, Hal Johnston, and Neil Bartlett. There were quite a few. Going back to the older days, Rollefson.

BOHNING: Oh, yes.

BREWER: Latimer and Eastman.

BOHNING: Let me close, unless there's anything that I haven't covered that you would like to talk about. I had one last question on "Chemical Thermodynamics in the Future Development of Chemistry Including Environmental Problems" (44). It's in *Pure and Applied Chemistry*.

BREWER: All right. That really has to do with the project we were finalizing then. In fact, that was the paper I presented in Moscow at a conference. The point was that we are approaching a real crisis as far as sufficient productivity to maintain the standard of living and meet the pollution demands and so on. We're going to have to revise a lot of our technological processes, which will need new materials. We won't be able to do it with the common, conventional materials we've used in the past. The difficulty is this. You consider all the elements that you could combine in various ways to maybe make new materials. It's such a tremendous number that you can't use the Edisonian method; you have to have some way of predicting which elements are the most likely candidates and then reduce, maybe to a dozen, the ones that you're going to work on. So that same idea again—we need to expand our predictive models so that we can predict undiscovered new materials that will meet our requirements.

BOHNING: As I recall, the introduction to that paper is very pessimistic. [laughter]

BREWER: Well, I think we're in for some tough times in the next couple of decades. The world's going to go downhill. I think that, at the rate we're going, we're going to have to adapt to a much lower standard of living.

BOHNING: Do you think people are ready to accept that?

BREWER: Well, if they're realistic they will. Part of the problem is an educational problem in our country. In the old days the workers really were accomplishing the real productivity—not just service industries. They were doing the farming, or building buildings, building automobiles and all the rest of it; we had a lot of people from Europe who came to the U.S. In Europe they were frozen by their status, whereas here they thought that if they worked hard enough and educated their children, they could advance socially. We've reached the point now where we're not giving people a bad time, except for some of the Orientals coming in. We do have a large Hispanic and Black population that ought to be put to work, but they're not being educated properly and they're always told, "Well, you're not very adept; you can't hope to do anything." They ought to have people in their group who have succeeded come back and visit.

I have a striking example. I have a graduate student who came from Jamaica, where the blacks are top notch, and he was top notch and had no problem doing things. He wanted to go back to Jamaica to help their country. I had him talk to some of the people who study such countries and they said, "No, you're wasting your education. Your education is too advanced to be utilized there. What you ought to do is stay in this country, use your education, and send money back to help develop things." Well, he did go back, and then he returned. He said, "He was right." He's now teaching at a university in this country. Now he would be a good role model for some people, and say, "I did it. You can do it too." We need more of that type.

But I talked to the white students in my neighborhood. "How are you doing in algebra?" "Oh, I don't want to bother with that. That's too much work." They want to be salesmen, business managers, politicians. We're losing out the people who are willing to put the effort in for real productivity—that's what we live on. We're going to have to make it possible for some of the low-income people who could take advantage of working at those hard jobs that the ordinary people don't want to bother with.

BOHNING: When you started in chemistry, it was one of the glory subjects. Chemistry and chemical engineering were like what computer science became later on. They were the top areas to go into, and that was an area of great promise. I don't think that's the case today. I was reading that the average salary for a major-league baseball player this year is over a million dollars. That's every player on every team. Why should people want to go into science? Your group worked twenty-four hours a day making those crucibles back in the Manhattan Project. People aren't going to do that type of thing and have very little to show for it.

BREWER: Well, I think we have to do a better job of educating the elementary school students on how challenging and exciting it can be. We were trying to do that in the elementary schools. We have a project where we were trying to help the teachers present science more effectively. When we went to the third grades it was really fun. Those kids were curious; they're eager to try things. But when we went to the tenth grade, unless they knew what answer you wanted, they would never answer your question. They had been told that they were expected to memorize this and then it would be coming back on an exam. They were ruined as scientists, because a scientist has to make guesses; he has to try out things whether he's sure they'll work or not, just to explore. That's been wiped out, and we somehow have to do a better job of maintaining their curiosity and eagerness to explore. We've really gone downhill.

BOHNING: It doesn't look like there's any immediate solution to that problem either. At least I don't see any.

BREWER: Well, there are solutions. It's a question of getting the financial support for them through the politicians, to recognize that. Now, when California was set up as a state, education was given the top priority, and for a century California really has led the other states and the rest of the world in education. It's going downhill now. The politicians are cutting off all the money for education.

BOHNING: The governor of Pennsylvania has just introduced massive cuts to education at all levels, from elementary schools to the major universities.

BREWER: They're cutting off the future of our society.

BOHNING: For example, the University of Pennsylvania has the only veterinary school in the state of Pennsylvania, and it might close because of these cuts. Those are the kinds of things that are going to be very, very difficult.

BREWER: In Congress, they just turned down the plan to divert the military funds to education. [laughter] Yes, that was a tremendous waste, all that money going to the military. What they've done is taken the money from our grandchildren. They will really be in a bad way. A lot of these politicians aren't conservative. Back in the seventies when we had the oil problem, I had a call from a fellow in Texas who wanted my support to get the government to greatly increase drilling and get more oil. So we were comparing how much oil we have, and we have a pretty good estimate. So I said, "Gosh, what's going to happen to our grandchildren?" His answer was, "That's their problem." [laughter] The politicians are just short-term sighted, and the country's going to suffer unless we can somehow get the message across that we'd better build for the future.

BOHNING: Well, I don’t mean to end on a pessimistic note, but is there anything else that I haven’t covered that you’d like to add at this point?

BREWER: Oh, we could go on for days, but there’s a limit. [laughter]

BOHNING: Well, I’d like to thank you for spending the time with me this morning. It’s been very enjoyable.

BREWER: Well, good.

[END OF TAPE, SIDE 5]

[END OF INTERVIEW]

NOTES

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