SAMUEL EPSTEIN
(1919 – 2001)

INTERVIEWED BY
CAROL BUGÉ

December 19 and 26, 1985, and January 10, 1986

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Subject area

Geology, geochemistry

Abstract

An interview in three sessions, December 1985 and January 1986, with Samuel Epstein, William E. Leonhard Professor of Geochemistry in the Division of Geological and Planetary Sciences. Dr. Epstein received his BS (1941) and MS (1942) degrees from the University of Manitoba and his PhD (1944, with Carl Winkler) from McGill University. He became a research fellow at Caltech in 1952 and two years later joined the faculty as an associate professor. He received the Leonhard chair in 1984, retired in 1990, and died on September 17, 2001.

In this interview, he discusses growing up in Poland between the two World Wars and immigration to Winnipeg in 1927. Recalls his first interest in science and influence of Alan Newton Campbell at University of Manitoba; graduate work at McGill; meeting European scientists during war work on Canadian Atomic Energy Project. Moves to McMaster University to work with Henry G. Thode on isotopes, using mass spectrometry; thence to the University of

Chicago, 1947, to work with Harold Urey on paleotemperatures and Heinz Lowenstam on marine shells.

He discusses the advent of geochemistry at Caltech in the early 1950s, with the hiring of Harrison Brown, Clair Patterson, Charles McKinney, Gerald Wasserburg, and himself from the University of Chicago. He describes his isotopic work and the evolution of the geology division, especially under Robert Sharp (1952-1968). Comments on Linus Pauling case and Pauling's departure from Caltech. The interview concludes with comments on current state of Caltech and reminiscences of his career and colleagues.

Administrative information

Access

The interview is unrestricted.

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ORAL HISTORY PROJECT

INTERVIEW WITH SAMUEL EPSTEIN

BY CAROL BUGÉ

PASADENA, CALIFORNIA

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ORAL HISTORY PROJECT

Interview with Samuel Epstein **by Carol Bugé**

Pasadena, California

Session 1	December 19, 1985
Session 2	December 26, 1985
Session 3	January 10, 1986

Begin Tape 1, Side 1

BUGÉ: How long did you live in Poland and where did you live?

EPSTEIN: It's a complicated thing as to where you're born in Poland, because they have cities which are of considerable size and then there are villages that surround them. I was born in one of these small villages, near Kobryn. Then my family moved to Kobryn, where I went to a *cheder*—a one-teacher, one-room school. Most of my subject matter dealt with the Bible and Hebrew. The teacher would hire a young schoolteacher to teach us Polish and mathematics.

BUGÉ: What language did you speak at home?

EPSTEIN: Well, actually I spoke two languages. Yiddish was my main language, but I also spoke a White Russian language, which is a mixture of Russian and Ukrainian. Mostly with my young friends in the village, who were White Russian peasants, I spoke both languages fluently. When I immigrated to Canada, I still spoke Yiddish. I like Yiddish, but I no longer have the opportunity to speak White Russian.

BUGÉ: Have you ever gone back there?

EPSTEIN: No, I didn't. It's Russia right now. I was in Russia twice—four years ago and this past summer—and I tried to go to Kobryn, but I couldn't get a visa very easily. Maybe it's because those cities are still depressed. Actually, my wife's parents came from Pinsk, a city not too far from Kobryn. So we both wanted to go to that part of Russia.

BUGÉ: Did your family move to Kobryn for your education?

EPSTEIN: It was probably one of the factors.

BUGÉ: Were you an only child?

EPSTEIN: No, I have two older sisters who came with me from Poland and a younger half-brother and half-sister. My mother passed away one year after we came to live in Canada. She's been dead for many years. My father remarried in Canada and lived to a ripe old age. He died not too long ago, at the age of ninety-five. He was a very nice man.

BUGÉ: Did he stay in Canada?

EPSTEIN: Yes, after emigrating from Poland, he lived in Winnipeg the rest of his life. That's where we settled, and that's where I got my education. I went to the University of Manitoba and got my master's degree [1942].

BUGÉ: Why did your family leave Poland?

EPSTEIN: My father didn't appreciate the situation as it existed in Poland at that time. As a teenager, he spent four years in New York. Then he went back to Russia in response to a call-up for military service. Kobryn was part of Russia at that time. If you did not present yourself to the military service, you were exiled for life. He did not want to be separated from his family for the rest of his life. Then he met my mother. Shortly after

that, World War I broke out, and he couldn't get back to the USA. He tried to get to the states by crossing the Gobi Desert and was caught in Harbin, near the Chinese border.

BUGÉ: That must have been quite an adventure story.

EPSTEIN: Oh, yes. He wrote it up but never published the stuff. It would probably be a best-seller now! As a result, after the World War I experiences, my mother got very ill.

BUGÉ: Was it harder for Jews at that time?

EPSTEIN: I did not get that impression from the stories my parents told me, because everybody suffered.

BUGÉ: Weren't you aware of anti-Semitism then?

EPSTEIN: Yes, there was anti-Semitism. Once in a while, one would hear of a pogrom. I recall one such case just before we left Kobryn. The community I lived in was mostly Jewish. I do remember the police being disrespectful and rather arrogant to the Jewish people. But it didn't affect me very much, because my contacts were mostly with Jews, so that I had no occasion to feel anti-Semitism. I was too young and secure in my family circle.

BUGÉ: Was that a cause for leaving the country?

EPSTEIN: As far as my father was concerned, he had had a taste of the USA. He wasn't making a good living in Kobryn. He was anxious to get out, and I'm glad. I had very little reason to question him. His judgment was obviously sound!

BUGÉ: What did he do for a living?

EPSTEIN: The Jews, in the czarist time, were mostly minor brokers or small businessmen. Before I was born, my father used to buy things from the farmers and resell them at a

small profit. I remember his last enterprise in Kobryn, in which he rented an orchard from a Polish aristocrat lady. She owned a lot of land, including a small, beautiful orchard. His job was to take care of it and make sure that everything was sprayed. He was to then harvest it, sell the fruits, and have a share of the profits. Some years he would do fairly well and some years were very bad. So he decided to go to Canada. He had to go to Canada, because that was the only place that would accept immigrants from Poland on a non-quota basis. He went to Canada first, and the rest of the family came the following year, when I was nearly eight years old.

BUGÉ: Why did he choose Winnipeg?

EPSTEIN: Actually, he came to Canada as a potential farm worker. There was a shortage of farm labor. Winnipeg was in the Canadian farm belt.

BUGÉ: There was no minimum time that he had to do it?

EPSTEIN: Basically, they allowed a certain number of people to come in. And then there were those who worked on farms. But some chose to use their other talents. My father was a skilled man, a relatively well-educated person—well read and literate. He chose to work in a factory, because he got a job as soon as he arrived in Canada. And he made a half-decent living at it. After his official retirement, he embarked on a new career altogether. He was hired to work in a large Jewish temple, where he prepared youngsters for Bar Mitzvah, conducted morning services, read the Torah scrolls, and other duties. In other words, he was able to utilize his early education to earn his livelihood for the next thirty years. He was extremely successful. The congregation did not let him retire until he was in his nineties.

BUGÉ: How did you get into graduate school?

EPSTEIN: Actually, I decided to do a master's with Dr. [Alan Newton] Campbell at the University of Manitoba. He was a good physical chemist, and he accepted me to do a thesis with him. And that's when I really fell in love with scientific research; I was

absolutely fascinated. He thought I did a good job. As a matter of fact, I got the thesis work and my master's degree done in one year. I even had a paper published, based on this work.[1]

BUGÉ: And that was in chemistry?

EPSTEIN: That was in chemistry. I never really understood what science was until I started doing it, and then it turned out to be so much fun that I decided, if possible, research was to be my life's work. After my master's degree, I went and got my PhD in two years (1944).

BUGÉ: That's very rapid.

EPSTEIN: Yes. I worked very hard.

BUGÉ: And what were you working on at that time?

EPSTEIN: At that time, we were doing war work—working on an explosive called RDX. There was a great need to develop this explosive, which was very efficient—much more efficient than TNT. Also, it had some important useful properties; it could be incorporated into plastics and molded, so you could make shaped charges, and that made it much more effective. The trouble with that process was that it had by-products that were unstable. And then more efficient ways of making it were discovered. One of the things that chemists in Canada—and particularly at McGill, and other universities, too—were doing was to try to understand the fundamentals of the mechanisms involved in its formation. And actually I published two papers on RDX after the war.[2]

BUGÉ: So that was the subject of your thesis.

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[1] A. N. Campbell & S. Epstein, "The Density of Selenium," *J. Am. Chem. Soc* 64 (11), 2679–80 (1942).

[2] S. Epstein & C. A. Winkler, "Studies on RDX and Related Compounds: VI: The Homogeneous Hydrolysis of Cyclotrimethylenetrinitramine (RDX) and Cyclotetramethylenetetranitramine (HMX) in Aqueous Acetone, and its application to Analysis of HMX in RDX(B)," *Can. Jour. Chem.* 29:9, 731-33 (1951); Epstein & Winkler, "Studies on RDX and Related Compounds: VII: Relation Between RDX and HMX Production in the Bachmann Reaction," *Can. Jour. Chem.* 30:10, 734-42 (1952).

EPSTEIN: Yes. It was bona-fide thesis work.

BUGÉ: Did you go to McGill knowing that you would do that?

EPSTEIN: I was going to McGill; I'd do anything. [Laughter]

BUGÉ: But you knew that you wanted to go.

EPSTEIN: All I knew was that I wanted to do research in line with the needs of the war effort.

BUGÉ: When did you start being interested in science?

EPSTEIN: Actually, when I got into my master's program. Before that, I was good in mathematics. I didn't work very hard; I guess I had too good a time. But I enjoyed doing mathematics and physics—the quantitative things. I used to love that. But my environment was very different from the modern environment that exists for present-day youngsters. Now the children are exposed to every modern scientific thought that their minds want to accept. There are television programs, all sorts of books and comic books. We have an eight-year-old grandson who knows much more about space science than I ever knew after my PhD. [Laughter] My point is that at that time, scientists were considered to be a bunch of queers, you know—monsters, like Frankenstein.

BUGÉ: Did you know what you wanted to be before you did your graduate work?

EPSTEIN: The only thought I had before I did my graduate work was how to make a living.

BUGÉ: Did you think you'd go into business?

EPSTEIN: I didn't have the slightest idea. After my bachelor's, if somebody had given me a job I probably would have accepted it. Yes, I had all sorts of idealism. Those were the

Depression years, and I was a very pragmatic person. Most of my high school friends went to work after they graduated.

BUGÉ: It sounds like you needed some encouragement.

EPSTEIN: I didn't need encouragement. I didn't come from a family of academics. They were very poor and very bright people, but the motivation was all mine.

BUGÉ: It just didn't seem like something that was open to you?

EPSTEIN: That's true, but like most of my friends who went to college, my aims were rather limited. I was very motivated to get a good job and make a living, and to do something that I enjoyed and not to be a factory worker. But I'm sure that if I'd gotten a job in chemical analysis after my bachelor's degree, I probably would have been happy. I recognized that if I had an opportunity to go for a PhD, it would be something very special. I recall, as far as my admission to McGill was concerned, that I came rather late and the professor asked me if I wanted a job. Although I could have used the money very badly, I said, "No, I want to go to graduate school." That statement apparently impressed one of the other professors, who thought since I was so motivated I should get a chance. After that, that was it. I wanted to do research. I just loved it, and I did very well.

BUGÉ: Who was the professor who asked you if you wanted a job?

EPSTEIN: Carl Winkler. He later became my thesis adviser. When I came to Montreal, I got an appointment to see him. He looked at my grades and so forth, and he said, "You know, you're kind of late to be admitted. Well, do you want a job?"

BUGÉ: Instead of going to school?

EPSTEIN: Yes, instead of going to school. But I said, "No, I want to go to graduate school." And there was another professor there named Raymond Boyer. He was one of the people who was accused of being a spy and was actually put in jail. Actually, he was

an outstanding teacher and a very nice man. He was a professor of organic chemistry, and it was he who said, "Yes, let's let him in." As a matter of fact, I was his teaching assistant at the beginning; that's how I got some financial support. I accepted their offer to go to graduate school without knowing just how much financial support I was going to get. That's a habit I have. I don't inquire about the financial situation. [Laughter]

BUGÉ: Do you think you would have the same opportunity today if you'd come through college not really being very focused?

EPSTEIN: Today I would be focused. In fact, my teachers were very discouraging. They were the kind of people who never tried to light any fires.

BUGÉ: The teachers you had when you were an undergraduate?

EPSTEIN: Yes. Most of them were very old and were looking forward to retiring and not having to teach. They had big teaching loads and poor salaries. They were not as lucky as we are in these smaller schools, such as Caltech. In most good schools, if you're a good scientist, you're given lots of leeway, including opportunities to interact with students.

BUGÉ: Do you think that had to do with the war?

EPSTEIN: No, I think the younger generation doesn't quite understand what our standard of living was at that time. One of the remarkable things about our society in the United States and in Canada is the tremendous increase in our standard of living. In those days, very few people had cars; everybody took streetcars and buses. And you never went to restaurants. When you took a girl out, you went to the cafeteria and had pie and coffee. That was a pretty good thing. Not only that, but I never went beyond fifty miles from Winnipeg until I went to McGill. Once we came to Winnipeg, which was in 1927, I did not leave until I was twenty-two; no one did.

BUGÉ: Was it unusual for you to go away when you did your doctorate?

EPSTEIN: Yes. But only Toronto and McGill Universities gave doctorate degrees. As a matter of fact, my trip to McGill was very unusual. It all happened in a matter of a few hours. I just decided on the spur of the moment that it was time to go. McGill said I was eligible to come. I wanted to go without having to pay train fare, so I got a job as a newsie on a troop train, selling oranges. I got the job around noontime, packed a few things, told my parents, and left at six o'clock.

BUGÉ: You just told them, and then you did it? Just all in the same day?

EPSTEIN: Yes. They came down to the train station.

BUGÉ: Were they shocked?

EPSTEIN: No. Considering the experiences of my father, I guess they weren't. Maybe they were, but they knew I'd have to do something like that if I was to take advantage of the opportunity.

BUGÉ: How long a trip was it?

EPSTEIN: I think it was two nights and a day. I stopped off in Toronto for a day. I went to visit the university there and then came to McGill.

BUGÉ: Did you know anybody when you arrived?

EPSTEIN: In Montreal, it turned out that there were some very good friends of my sister's, and I stayed at their house for a couple of days.

BUGÉ: And then did you get your own place to live?

EPSTEIN: Yes, I rented a room in a family's house. It was very nice. I found it very comfortable. It was within walking distance to McGill.

BUGÉ: What was the student body like at McGill?

EPSTEIN: Mostly very dedicated people. There was a war on at that time, and the people who went to graduate school were those people who could do something special for the war effort. In Canada, there was a selective service rather than conscription. When the war broke out, I was nineteen and a sophomore and I was already taking chemistry. A colonel came by and told us to stay in school. He said, "We will need people like you." When radar came in, physicists were needed to train the radar people. They needed chemists for the development of explosives. I did teaching assistantships for four afternoons a week. I did most of my classes in the morning, so I did most of my research in the afternoons and evenings.

BUGÉ: Was that true for most of the students?

EPSTEIN: Yes. In the evening we'd come there at seven o'clock or so, and we'd work until three or four o'clock in the morning.

BUGÉ: Were they mostly Canadians?

EPSTEIN: They were mostly Canadian.

BUGÉ: Were the PhD programs big?

EPSTEIN: At that time, my thesis adviser, Winkler, had seventeen students.

BUGÉ: That's quite large.

EPSTEIN: Oh, that's very large. Winkler was very nice; he let you do your work. He was a Canadian. He got his PhD at McGill, but went on as a postdoctoral fellow or Rhodes Scholar at Oxford. Then he got a job at McGill as an assistant professor. Since he had seventeen students, he couldn't pay attention to too many of them. I think that environment was very good. We were doing war work. I guess we were underpaid, but nevertheless, we used to work awfully hard and long hours and didn't have much time for socializing. Well, I got my PhD in two years, which included taking courses and being a

teaching assistant and so forth. And some of the other students went for three years. In fact, one of the other students was Rudy [Rudolph A.] Marcus, who is now a professor at Caltech and a very distinguished chemist—in fact, he just won the Wolf Prize. [Marcus received the Nobel Prize in chemistry in 1992—ed.]. We got our PhDs with the same man.

BUGÉ: You worked in Dr. [Henry G.] Thode's laboratory. Was that at McGill?

EPSTEIN: No. After I graduated, I went to work for the Canadian Atomic Energy Project. That was a wonderful experience. The best European scientists came to Canada, because they couldn't do any work over there. As a matter of fact, two people on the project eventually won Nobel Prizes. [John D.] Cockcroft won one in physics [1951], and Geoffrey Wilkinson won one in chemistry [1973]. There was a brilliant bunch of people that came from overseas, some of the best physicists and chemists in the world. It was as good as any academic environment that existed at the time.

BUGÉ: Were there others whom you remember particularly? Did you ever meet Franco Rasetti while you were in Canada?

EPSTEIN: No, but I knew [Bruno] Pontecorvo; he was in Canada at the time. He was the youngest of the famous Italian group and a very bright man. He's the one who defected to Russia [1950]. I don't think it was a matter of an ideological defection; it was a matter of having the opportunity to do things in the USSR. And there were some other personal problems. But apparently that was quite a blow. There was also a man named [Alan Nunn] May, who was convicted as a spy for Russia, at this Canadian laboratory. It was an environment that was very pro-Russian at that time, because they were doing a hell of a lot of fighting.

BUGÉ: So there were a lot of scientists emigrating from Europe?

EPSTEIN: Well, they were not emigrating. They were sent here by their governments. They just could not do their job in Europe, because they were in danger. First of all, the

Germans occupied France and other countries. Britain was continually being bombarded. And if the Germans found out that there was some facility there where they were doing some special thing, they would make a point of getting it. So that whole activity was transferred to Canada.

BUGÉ: Did you all work together, or did you work separately?

EPSTEIN: No, each of us worked on different problems. I worked with two men named [K. F.] Chockitt and [W. J.] Arrol. They were both Englishmen, and they hired me as an additional man. We did the rare-gas work, the fission-product work. When uranium-235 is fissioned by bombarding it with slow neutrons, it breaks up into a whole series of lighter fragments. And there are two rare gases—xenon and krypton. Krypton isotopes have masses ranging around 80, and xenon isotopes range around 130. Because these are chemically very stable gases, there was the practical danger that they'd form bubbles and introduce imperfection into the uranium metals that would be used for whatever purpose. So there was a structural problem.

Presumably, the idea was that all of these problems were tiny gears or screws to add information so that they could do something about building the atom bomb. But the production of the atom bomb involved other processes. They had to purify or concentrate uranium-235 and understand the way bombs are formed, and so forth. But aside from that, it was always important to have this additional information to safeguard against any possible error. This research also turned out to be extremely important as a purely scientific problem, because the products that come from fission are radioactive, and they go through a whole series of radioactive decay to end up as the nonradioactive isotopes of krypton and xenon. So if you wait long enough, radioactive fission products end up as nonradioactive isotopes of the noble gases krypton and xenon. And by measuring the relative abundance of these isotopes, you can get what is known as the fission yields. There is little mass loss in the decay of the radioactive fission product elements to the xenon and krypton isotopes. So if a fission product starts as a radioactive element of mass 84, it ends up as a nonradioactive krypton stable isotope of mass 84.

BUGÉ: Does fission happen spontaneously?

EPSTEIN: Well, yes, it is spontaneous as soon as we put a slow neutron in it. The addition of a neutron makes it unstable and causes the uranium to break up into two. But at the same time, it also produces other neutrons, more fission, et cetera. It was very secret work all right; we never talked about it. But there was a surprisingly good academic environment introduced by the presence of the European scientists. For example, some of the people there offered some courses in certain math and physics for the other people who didn't know so much about it. And there were always conferences and seminars. That led, probably, to purely scientific results which were important. It was a very well-run laboratory and also very productive.

BUGÉ: So what you were basically working on was a better way to make a bomb.

EPSTEIN: No. At that time, I remember, we never even thought about a bomb. Basically, the project was—as far as I understood it—to see if it was a feasible thing, and, if it was, we should be first. I wish that it had not been feasible. We were afraid the Germans were also working on this. In Germany, there were a lot of good scientists. As a matter of fact, the person who first discovered fission, without knowing it, was German. Einstein wrote a letter to President Roosevelt about the potential of the creation of such a bomb. Heaven knows, when the bomb exploded, there was great surprise and apprehension. We hadn't even thought about that. It was just that we wanted to make sure that if such a thing were possible, we should be the ones to do it.

BUGÉ: So why was it such a surprise when it was actually done?

EPSTEIN: At that time, this was just a theory about the mass-energy equivalence. And, of course, not everybody knew what was going on in all the different laboratories. We were a small laboratory, and we didn't know what was happening elsewhere. As a matter of fact, I don't even remember that we knew that Los Alamos existed. We never knew what kind of effort was being put into this. Not that it made any difference; I would have worked on that project willingly. I think hindsight is just a terrible way to judge anything.

BUGÉ: I didn't mean to be judgmental.

EPSTEIN: No, I'm not talking about you. I'm talking about a general attitude. We have these people walking around, holier than thou, whose existence is due entirely to the fact that some people were sacrificing and trying to think of some way to prevent this crazy madman [Adolf Hitler] from taking over the world.

BUGÉ: What I'm curious about is how people work on these problems in a theoretical way, without thinking about the logical end result.

EPSTEIN: Because there was great scientific merit to understanding. This is something we had that nobody ever thought could happen. As a matter of fact, some of the great scientists were trying to—nobody knew that uranium was going to break up—build up bigger atoms and create new elements. So, these are things that no one knew. As a scientific problem, this was absolutely fabulous.

BUGÉ: So your orientation was scientific.

EPSTEIN: Yes, but my orientation was also to do the best I could to defeat Hitler. There was absolutely no question about the fact that I would have done anything to destroy that monster. So far, we have had no major wars, which I believe is due to the fear of the consequences of an atomic bomb war. I find this result to be consoling. But at the same time, the scientific aspect of fission was extremely important. I mean, we talk about the bomb, but people forget about all the different things that radioactive work has done. The leap in scientific knowledge was marvelous, and it benefited mankind.

BUGÉ: It must have been a very exciting time.

EPSTEIN: Yes, it was exciting. And of course, I was aware of what was going on in Europe, and I did not feel entirely secure about the situation of the world. And I got into the bad habits of working overtime and nights.

The political milieu was especially interesting. There were Communists and there were right-wingers. And there were discussions about it. Of course, there was a great pro-Russian environment, because they had taken the brunt of the war most of the time. And at that time, I don't think people were yet aware of what horrors Stalin had committed. It was sort of free spirits and lots of conversations and no reluctance to keep your opinions to yourself. [Laughter] I learned a lot during the two years or so that I was there. After Montreal, I went to McMaster University, because the Montreal lab was closing down.

BUGÉ: Why?

EPSTEIN: The Montreal labs were housed temporarily at the University of Montreal, because of the urgency of this research and because half of the university was unoccupied. The Chalk River facility was selected for the permanent home of the Atomic Energy Project. I moved on to McMaster University to work with Harry Thode, who was a very important influence in my career and a very nice man—very bright and a good scientist. I guess it was his way of doing science that broadened my own perspectives. His work was mainly in chemistry; but he had a broad-gauged view of his isotope-related research. Do you know what an isotope is?

BUGÉ: I have some idea; I've read a little bit.

EPSTEIN: Well, you know, oxygen has three isotopes that have essentially the same chemical properties but different masses. The lighter-massed oxygen tends to react more quickly. When you burn wood or anything like that, the lighter oxygen would preferentially be used. So you can study the consequences of what happens to this oxygen. You can compare how wood burns, how you breathe, how plants respire—all from an isotopic point of view. Eventually it comes in handy. But basically you're really not doing anything that's particularly different in terms of practical aspects, in terms of experiments or interpretation of your data or what fundamental chemistry or physics is going on. They usually have a common theme and a common base; and if you want to do some biology, you just have to learn a little bit of biology to make it useful. So that's

what makes it possible to do isotopes in a very broad way. You can work on ice and water, glaciology, meteorology—all these different fields which have been classified by some scientists on some basis of interest of the different expertise required to study glaciers or rains. I guess glaciology has something to do with ice. As far as I'm concerned, ice and water are the same thing. They contain the same isotopes, and this holds true for oceanography. You can use these isotopes as tracers to follow this history and geographic distribution, and so on.

BUGÉ: It opens up all kinds of fields to you.

EPSTEIN: All kinds of things. And when you're fortunate enough to be on the ground floor in the field, you have a heyday.

BUGÉ: Earlier, we mentioned anti-Semitism in the thirties in Canada. How did that affect you, or what were you aware of in this period?

EPSTEIN: I'm not quite sure what kind of anti-Semitism it was. I don't think there was deep hatred associated with it. It was more like looking down on people—like anti-black feelings; I don't think people want to go kill blacks.

BUGÉ: Well, in some parts of the country they evidently do.

EPSTEIN: They do, but even a lot of people who are anti-black just feel that blacks are inferior. It makes them feel superior. Also, in the thirties and early forties, Jews had a hard time getting university jobs, because they [universities] preferred Christians, period. This is the sort of anti-Semitism that I met and had to deal with. I had no trouble with having Christian friends; nobody who was Christian put me down in school. There was no vandalism; there was no fighting. Youngsters who came from recently arrived immigrants, who brought their kinky stuff from back home, tended to be a little more pugnacious and attacked Jewish kids. I guess at our time this was true in attacking Irish kids. So I guess I would say that if I were in a position where I wasn't competing in a well-established Christian enterprise, such as the school system in Winnipeg, I probably

did not feel it that much. The first time I met anti-Semitism that really hurt me was in academia. Maybe I'm blaming it on the wrong thing, but I was an honor student in geology and I never got a summer job working for the Canadian Geological Survey, which hired geology majors—and I was a geology major—for summer fieldwork. They hired all the honor students and many of the pass students but always overlooked me. This job was very important to me, because I was very poor and desperately needed the money for school fees. And a summer job in which food and everything else was taken care of—and I'd bring home a couple of hundred bucks—would have been an enormous help. As it was, I used to have to work Saturdays and holidays as a salesman. So, it's that sort of thing—very unpleasant when you're trying to do something that you love, and you want to do it and survive.

But on the other hand, as in all things, when things don't go that easy, and you have various experiences, they benefit you and make you a better and more sensitive person. It turned out that the geology professor who was responsible for denying me the summer jobs may have nominated me for an honorary degree that I received in 1980. He must have learned something as well.

BUGÉ: So you felt anti-Semitism mostly as an economic hardship.

EPSTEIN: Primarily. And also, of course, the environment there was not very healthy in the thirties. I remember a very strange remark from one of my professors—I go to see him every time I go to the University of Manitoba. He's an old man of eighty, still spry, a wonderful man. He said something about his mother disliking Jews very much. She lived in Scotland and never saw a Jew in her whole life, but it was this religious thing. And he said that that's what he was told as a youngster.

BUGÉ: So it does sound like it was pretty pervasive.

EPSTEIN: Well, yes. They had a quota system for training doctors at the University of Manitoba. I was a teaching assistant for the pre-med chemistry; it was a class of two hundred and the class was divided between fifty percent Jews and fifty percent non-Jews. And then they admitted sixty freshmen—I remember that number—and two of them were

Jews. And that was the quota system. I guess they felt that the medical profession should not be flooded by Jews. I have a friend who got his doctor's degree at the University of Manitoba, and I always told him I really liked those doctors who graduated from the University of Manitoba, because they really picked the cream of the crop. They had to be very good. [Laughter]

BUGÉ: What about in the neighborhoods?

EPSTEIN: Well, we lived in sort of a ghetto. It was a very nice ghetto, and it wasn't imposed by anybody.

BUGÉ: You don't think there was any kind of redlining?

EPSTEIN: No. I guess most of the Jewish people acknowledged themselves to be Jewish. I don't remember anybody trying to pass themselves off as a gentile. There were no particular hardships being a Jew over there. Some people found it was a little difficult, but they managed.

BUGÉ: Well, it sounds like not getting summer jobs is a little bit of a hardship.

EPSTEIN: Yes. Or you'd get a summer job working at sixteen cents an hour, or something like that. But actually, the last couple of years I worked for the city. I think it was a graduate student who hired me.

BUGÉ: And the student population was mixed, aside from where the quota system was applied?

EPSTEIN: There were no restrictions, no quota system, for the arts or the sciences, just the medical school.

BUGÉ: What made you decide to go to graduate school?

EPSTEIN: After I got my bachelor's degree, I followed the path of least resistance. I wish I could say that I was really very clever.

BUGÉ: In your research, have you ever moved over into applications?

EPSTEIN: Not really. After we did the rare-gas project, Harry Thode did the mass spectrometry. He had the only mass spectrometer in the country; he measured the different isotopes of these rare gases and published a paper that was a beautiful classic.[3]

BUGÉ: Is there any way of explaining, simply and briefly, how a mass spectrometer works?

EPSTEIN: Let's take an atom or a molecule and hit it with an energetic electron. You knock off an electron, and instead of being a neutral molecule it becomes a plus-charged particle. Now, once you have a charged particle, then it responds to any electrical field. Now, if you have a particle that's positively charged, it will be repelled by a positive-charge electrostatic field and attracted by a negative field. In a mass spectrometer, to create the charged particles, you can accelerate a beam of the charged particles by passage through an electrical field to a specified velocity and direct them into a magnetic field. It's basically like shooting bullets into a V-shaped magnetic field. The magnetic field deflects the charged ion beam, bends its path about 60 degrees. For the same energy, a charged ion that consists of the lighter molecules H_2—mass 2, as compared to HD, mass 3—will be bent to a greater degree than something that is heavier. Let's say you start off with hydrogen gas. Hydrogen has two isotopes. For all intents and purposes, they are chemically identical and not readily separable by chemical means. The mass spectrometer separates these different beams almost perfectly, and all you have to do is collect these ions into separate cups, where they will lose their charge—become neutralized—and create an electrical current that can be measured very accurately. And this current depends on the amount of these different isotopes. We have mass spectrometers downstairs that can measure the relative amount of isotopes of oxygen,

[3] H. G. Thode & R. L. Graham, "A Mass Spectrometer Investigation of the Isotopes of Xenon and Krypton Resulting from the Fission of U-235 by Thermal Neutrons," *Can. Jour. Res. Section A* 25:1, 1-14 (1947).

hydrogen, nitrogen, and carbon. Thode measured the isotopic abundance of krypton and xenon and found these nice yields—fissions.

BUGÉ: Had you worked with him the whole time?

EPSTEIN: No, just a year and a half—many years ago, at McMaster University. He invited me to come to his laboratory, which was in Hamilton, Ontario, when the Montreal labs were dismantled and the Atomic Energy Project moved to Chalk River.

BUGÉ: So Chalk River was sort of like Los Alamos was to us.

EPSTEIN: Right. But instead of my moving to Chalk River, Harry Thode invited me to come to McMaster, because we were going to do some work together. He had the mass spectrometer and I had the techniques to extract gases and so on. He was a very good scientist.

BUGÉ: So when you went to work with him, did you then leave the project?

EPSTEIN: No, I came to work with him as an employee of the Atomic Energy Project. They were paying my salary. But at that time there was this whole business about spying. We needed some irradiated uranium in order to measure these rare gases, and the United States wouldn't give us any.

BUGÉ: That was the only uranium source?

EPSTEIN: Yes. So I was sort of puttering around learning about mass spectrometers. Then Harold Urey phoned up Harry Thode, who was once a postdoctoral fellow of Urey's, and asked him if he knew somebody who'd like to come and work in his laboratory at the University of Chicago.

BUGÉ: That is serendipity.

EPSTEIN: Absolutely!

Begin Tape 1, Side 2

EPSTEIN: Harold Urey had this crazy problem that nobody believed could be done. I guess he couldn't get too many people to work for him, so he asked Harry Thode to help him. Harry asked if I would be interested, because I guess he realized that this would be an opportunity for me. And being a naïve Canadian, going to work with Harold Urey was like going to work with God. [Laughter] I didn't inquire about salary—I knew I would get some salary—and I also made no inquiries about housing.

BUGÉ: And your wife [Diane] was no more practical than you.

EPSTEIN: No, we both didn't have very much sense. I mean, she showed it by marrying me. [Laughter]

When I arrived in Chicago, my wife was pregnant. I had met Diane in Montreal after I got my PhD. We got married after I came to McMaster. I married her in Montreal and brought her to McMaster. We didn't even have an apartment.

BUGÉ: How long did you stay at McMaster?

EPSTEIN: Over a year.

BUGÉ: So you took your pregnant wife and left everything you knew. Did you go home to visit your folks before you left?

EPSTEIN: It was easier to visit Winnipeg from Chicago. You see, at that time we just didn't travel back and forth; it was expensive to do a thing like that. I think I was getting something like $3,300 a year.

BUGÉ: Was that competitive?

EPSTEIN: It was pretty terrible. And there were no apartments in Chicago. We lived in a garage apartment. We had to heat up the car to heat the garage. It was a mess! But I stayed there four and a half years. And I imagine that's when my career really was established, because we solved a very, very difficult and important problem—and the more difficult a problem you have to work with, the more you learn. And I learned a lot. The work established a new field.

The University of Chicago was an awesome place for somebody who had never been exposed to all these famous and excellent people. It's hard to explain how things were in Canada—there's a certain formalism. Just to give you an example, I was a junior and a teaching assistant to the sophomore class, and everybody called me "Sir." Well, if you get used to calling everybody "Sir" [laughter], what would you call Harold Urey or [Enrico] Fermi?

BUGÉ: Your Highness! [Laughter]

EPSTEIN: Yes, right. [Laugher] And so, in that sense, it was pretty awesome to be there, and there were a lot of pressures. Harold was a very impatient man, very proud. All the same, it was an exhilarating experience and a fountain of scientific wisdom.

BUGÉ: How old were you when you went there?

EPSTEIN: Let's see. I went there in October '47, so I must have been twenty-seven years old.

BUGÉ: And you had your first child shortly after you arrived?

EPSTEIN: Yes. Our oldest son was born in February '48.

BUGÉ: What did you start out doing when you got to Chicago?

EPSTEIN: Well, Harold Urey had the idea that you could measure temperatures of the ancient oceans by measuring these tiny isotope effects that would be recorded in

carbonaceous-shell skeletons, such as oysters and clams. But this problem was considered to be a wild [goose] chase; and if I'd had any sense, I would have gone back home.

BUGÉ: Why?

EPSTEIN: Oh, because the mass spectrometry had to be improved by a factor of 10 in order to make this a feasible problem. And the greatest mass spectroscopist in the world had just built a new spectrometer and it was not good enough.

BUGÉ: Who was that?

EPSTEIN: That was Alfred O. C. Nier, a very famous mass spectroscopist and a physicist—a good man and still active. Another problem was that when you are dealing with biological systems, the effects of biology on the nature of the minerals themselves, as well as on the isotopic components, can be enormous. And then there were other problems that we weren't even aware of.

BUGÉ: Was this a new problem to Urey, or had he been working on this for a long time?

EPSTEIN: Urey won a Nobel Prize [in chemistry—1934] for discovering deuterium in nature. And then he did lots of chemistry, calculating isotopic effects—extremely good and important stuff. As far as I'm concerned, he made the field of isotopic effects his. By "isotopic effects," I mean, for example, when you are breathing oxygen you use oxygen in your respiration process, and the O-16 is preferentially used to O-18. So if you inhale a sample of air, and exhale, and then measure the isotopic composition of oxygen that you exhale, it's been isotopically altered—to a very tiny extent. These are isotopic effects. And by measuring what effect a certain process has on the isotopic composition, you are basically getting information about the process. So when animals lay down their shells and record a certain isotopic composition, which is temperature-dependent, by measuring these shells you can determine what temperatures there were when they laid down their shells.

BUGÉ: But you were indicating that there might also be biological processes that would interfere.

EPSTEIN: Right, but a solution to most problems is considered as possibility and not as certainty, and then you do experiments and find out whether it works or not. There was this interesting problem as to why the dinosaurs disappeared. And there were these well-preserved calcium carbonate shells that go back 200 million years that you can use to measure the temperature of the ocean at the time the dinosaurs disappeared. You see, temperature was a factor. I don't think any of us ever thought that this would develop into the field that it eventually became. Later on, it became quite clear that you can measure isotopic effects for anything. You can reexamine all of chemistry.

BUGÉ: Did you have misgivings at the time, or is it only hindsight?

EPSTEIN: Probably both hindsight and some misgiving when things didn't go too well. At that time, I was twenty-seven years old; I didn't care if I lived in a garage apartment. There were a bunch of young, bright people in the laboratory, and it was fun to be with them. There were some wonderful people at Chicago—real scholars; particularly one man there, among many. He sort of took me under his wing.

BUGÉ: Who was that?

EPSTEIN: A man named Clyde Hutchison. He was just a marvelous teacher. I guess the important thing about him was that he would convince you that it's all right not to know something. There are people around here like that, too. And you don't feel bad if you don't understand something. If you don't understand something, then you try to understand it. And you'd be surprised how, when you go through your college years and so forth, you get this notion that if you don't know the answer, you must be dumb or stupid.

BUGÉ: You really have to prove yourself all the time.

EPSTEIN: Yes, all the time, which is kind of a silly thing to do, but nevertheless, this is the way it is.

BUGÉ: Counterproductive, too.

EPSTEIN: Counterproductive. And sometimes you use excuses; you stop doing something because you think you can't do it, or you don't know. And when you come from an environment that is very low-key to something that was of such high pressure and really very energetic, it takes some guts and a certain amount of idealism.

BUGÉ: Was it difficult to adjust to the lack of formality?

EPSTEIN: Well, to some degree, but it was not a problem. But it was more the lack of knowledge about things that were difficult. I was really behind the times scientifically. I came from a place that was well behind on many subjects to a place that was the leading light in the world.

BUGÉ: Urey didn't ask twice when Thode said, "Yes, there's somebody here whom you can use"?

EPSTEIN: Well, he must have questioned Harry Thode. Actually, at the Atomic Energy Project we did a very interesting and important problem—and also a very difficult one. We were the first ones to extract and purify tiny amounts of gases and measure their concentrations. This required some very fancy techniques.

BUGÉ: So your work had the same potential application as Los Alamos but was in some ways approaching it more theoretically. Is that right?

EPSTEIN: No, we just did a certain problem and did it very well. As a matter of fact, when I got to Chicago there were some people who were participating in this sort of research. When they read this paper, they thought it was great. We got all these fission product yields in one sweep.

BUGÉ: So you really were in some ways ahead of even what was going on in Chicago.

EPSTEIN: Yes, in this special aspect. But that's not the impression you have of yourself.

BUGÉ: Was that because of your social skills?

EPSTEIN: Oh no, my social skills were actually OK. I could communicate well with my peers, and there was no problem getting friends. I had many friends all the time. No, that wasn't the issue—just your image of yourself.

BUGÉ: Did you take classes at Chicago?

EPSTEIN: Oh, yes. I attended seminars all the time and some night classes. And I even did a teaching assistantship for one of the courses.

BUGÉ: Who were some of the people whose seminars you attended?

EPSTEIN: Well, the University of Chicago had a Thursday afternoon seminar. Fermi was sort of the pope. Well, he was just a great scientist and human being. And then people would get up and say something informally. Almost every physics discovery that was made in those days was somehow reported in this seminar. It was not a formal seminar. We just sat around with people like [Edward] Teller, Fermi, and Urey.

BUGÉ: And did the other people there feel free to say, "What are you talking about?"

EPSTEIN: Right. Not only that, but people would go there and say, "Gee, what the hell is that crap about? It's a bunch of nonsense." [Laughter] It was just marvelous!

BUGÉ: Did you get up eventually and say, "This is what I'm doing?"

EPSTEIN: I don't remember if I ever did or not. It was very dynamic. The young people were there. [Gerald J.] Wasserburg was there, and Harmon Craig. As a matter of fact,

from our laboratory alone, there were three or four who won pretty good awards in geological sciences.

BUGÉ: It sounds like it was a real hot little laboratory.

EPSTEIN: Yes! When I came to Chicago, I came there on some funny visa. I don't know exactly whether I was allowed to work or not, but anyway I did. Then I had to go back to Canada, because it [the visa] was only for one year. And it took me six terrible months to finally get a permanent visa to come back to Chicago.

BUGÉ: Six months that you spent in Canada?

EPSTEIN: In Montreal. My poor in-laws; we stayed with them. [Laughter] And we had a little kid, too. Anyhow, when I came back, Urey got interested in some other things, and so he sort of put me in charge of the lab. Not only did I have that opportunity but I also had the chance to communicate very well with the young people and affect their way of doing things. It was a very good experience.

BUGÉ: So you began by working on the shells. And how long did you stick with that project?

EPSTEIN: The real productive year in Chicago was about the last year I was there [1951-52], because all that time we were doing testing and developing. And finally we got this problem solved.

BUGÉ: Solving it depended on the apparatus in part, didn't it?

EPSTEIN: And also tests, and so forth. We had to grow animals under controlled conditions.

BUGÉ: Mollusks?

EPSTEIN: Yes. Well, not the total shell but just enough carbonate. There was a rather interesting trick that a biologist figured out. He would take a live abalone, grind off a bit of its shell, then put it into a tank with known temperatures and isotopic composition, and so forth, and then the animal would regenerate the missing piece. As a matter of fact, Heinz Lowenstam, a professor here, was the paleontologist who cooperated with us and [Ralph] Buchsbaum was the biologist.

BUGÉ: Lowenstam was a peer of yours at Chicago?

EPSTEIN: Yes. He also was a wonderful influence there—he was great. He's a very nice man and a very interesting fellow.

BUGÉ: What other kinds of problems did you work on? Or was all the work that you did at Chicago having to do with shells?

EPSTEIN: Well, I worked on a number of problems that were auxiliary to the project. We had to know something about the isotopic composition of ocean water, because the isotopic composition of the ocean is related to the isotopic composition of the shells. So that was a project that was really the first piece of work. And we showed that the isotopic composition of ocean water varied because as the water evaporates, the lighter isotopes are preferentially removed. And of course, the isotopic distribution of ocean water is connected with the isotopic distribution of meteoric water—rain and precipitation. It turned out that the isotopic composition of this water varies in the world in a systematic way. The heavier-isotope water falls initially in the warm areas, but as the air masses lose this heavy water, it becomes lighter and lighter and lighter, resulting in rains in the northern latitudes consisting of isotopically lighter waters. And the consequence of that turns out to be enormous.

BUGÉ: Does the oyster produce shell differently when it's had some of its shell cut off, or when it has been transported?

EPSTEIN: No, when we cut it, it behaved well. We had to know the exact temperature for our calibration. In some cases, collecting samples in their natural environment provided material for calibrating our temperature scale. And we found that all these came out perfect. We did, for example, a conch shell. If you take a piece of the lip of that shell, you can shave off layers. And when you do that, you get a beautiful seasonal variation indicating that the animal grew shell both during colder winter and warmer summer.

BUGÉ: I've seen some dendrochronology in Arizona similar to that.

EPSTEIN: As a matter of fact, one of the first things we did was analyze a shell that was 120 million years old, called a belemnite. If you take a section of it, you see it's like a tree-ring pattern.

BUGÉ: One shell that's 120 million years old?

EPSTEIN: Yes, there are lots of them. And then we sampled successive layers, and sure enough, we found beautiful seasonal variation.

BUGÉ: I can see why you get enthusiastic.

EPSTEIN: It's very easy to get enthusiastic. Science can be beautiful. People don't realize this, but there is beauty in uncovering the unknown. But it's the idea that you can do practically anything you want that excites me.

BUGÉ: Yes. If you can think about it, you can do it.

EPSTEIN: No, if you can do it, you can think about it. [Laughter] No, it's that simple. I've never had any problem thinking of projects. There are just hundreds of them—just analyzing humans.

BUGÉ: This wasn't your own project as much as Urey's project; and you just worked on getting everything together.

EPSTEIN: Well, it was his idea. But, you know, when you put in as much effort in developing all this and working on it, it becomes partly yours.

BUGÉ: Were there other people working on different aspects of it?

EPSTEIN: No. As far as this paleotemperature stuff is concerned, Urey, myself, and Lowenstam were involved. Buchsbaum was also involved. There's a young man named [J. M.] McCrea who did some inorganic things. He never published very much after that.

BUGÉ: Didn't he publish with you at least once?

EPSTEIN: No. He did part of his thesis, and in Chicago a thesis can have only one author.

Yes, this was Urey's project, but I learned. When I left Chicago [1952], I felt that I wanted to do something different, so I started doing things on silicates.

BUGÉ: That was while you were still at Chicago or here at Caltech?

EPSTEIN: Here.

BUGÉ: It wasn't geochemistry, then, at Chicago.

EPSTEIN: No, that was at the Institute for Nuclear Studies. No, it was chemistry. Urey was professor of chemistry; formally he had nothing to do with geology.

BUGÉ: Did you meet Harrison Brown while you were at Chicago?

EPSTEIN: Yes, I met him, but I hardly knew him. Harrison Brown was offered a job here. He was an assistant professor of chemistry at Chicago, but he was interested in meteorites and stuff like that. He had nothing to do with me. I decided that it was time for me to be independent; I had been at Chicago long enough. I was thirty-two years old; I could go out on my own. Although if I'd stayed in Chicago, I would have written dozens of papers, because we'd just started and I knew of many projects I would like to do. But I

figured I just had to go. I went to see Tony Turkevich. He was one of the professors at Chicago whom I knew. He said, "Well, you know, Harrison Brown is doing something in geochemistry; so he may know something about jobs." So I met him in the hall and I said, "Harrison, I'm ready to leave and want a job." And he said, "Why don't you come with me?" Just like that.

BUGÉ: Serendipity again.

EPSTEIN: Right!

SAMUEL EPSTEIN

SESSION 2

December 26, 1985

Begin Tape 2, Side 1

BUGÉ: Returning to your days at the University of Chicago, you talked about the seminars attended by Fermi and Teller. Were there other notable people who came to those seminars?

EPSTEIN: So many of them. They were a very interesting bunch of people. Almost everybody who made a major discovery would go through one of these seminars, though they were from different parts of the world. I can't think of all the names. Urey gave some talks there. I think [Wolfgang] Pauli was there.

BUGÉ: When the people came, did they socialize?

EPSTEIN: Yes. People would have tea and socialize. But I was a very junior member. But there were many great scientists there.

BUGÉ: In Urey's lab, was there anybody you remember particularly who was working in the lab with you or when you were put in charge of it?

EPSTEIN: From what time? The personnel changed. Harmon Craig was a student of Harold Urey at that time, and he was doing some isotope work. And Gerry [Gerald J.] Wasserburg worked there to earn some money and also became a student. Cesare Emiliani did some very interesting work on isotopes. There weren't that many people, at least when I was there. But every one of them did some very nice work and became very well known. It's hard to describe in detail any kind of environment. Bill [Willard F.] Libby was there, too, in these seminars; he won the Nobel Prize [1960] for carbon-14. They would just stop and talk with you—the people in the geology department who came over—Heinz Lowenstam, for example, with whom I interacted very much. And Julian Goldsmith. You learned a lot when interacting with all of them; you couldn't help it.

BUGÉ: Did that have anything to do with your interest in interdisciplinary studies?

EPSTEIN: Yes, actually, Chicago did a lot, as far as I'm concerned. Once I got my own lab, I could do whatever I wanted. You just think of doing all sorts of things, and I guess the key situation is that you think of doing something and then you are able to do it because of your past training. I was experienced in high-vacuum work and did glassblowing and mixing chemistry, and the questions I asked were pertinent to my past knowledge. And they happened to be problems that had not been answered or previously done. This is the way it works with most scientists.

BUGÉ: You mentioned people from Chicago who had won awards. What were these awards?

EPSTEIN: One is the [Arthur L.] Day Medal from the Geological Society of America, and the other one is the [V. M.] Goldschmidt Medal from the Geochemical Society. The Goldschmidt Medal is something relatively new; and the Day Medal is for somebody who has done something for geology but used chemical and geophysical knowledge and techniques. Well, Urey got it. So did Wasserburg, Harmon Craig, and myself. They give one a year. Since that time, there have been so many Nobel laureates from Chicago, it's unbelievable. The only reason I mentioned it is because in one laboratory there were four people who were able to be acknowledged for their contribution to what I would call "modern geology," which reflects an interesting thing about that laboratory.

BUGÉ: Well, this campus is star-studded, too.

EPSTEIN: Yes. In our geology division, there was a revolution. We were all acquired at the same time.

BUGÉ: Now we come to Caltech.

EPSTEIN: Well, I had decided that I wanted to leave Chicago. As a matter of fact, I may have stayed there another six months or a year. After I went to visit the University of

Arkansas and gave a seminar, they offered me a job as an associate professor and I turned it down, for a variety of reasons. Anyway, my visit to Caltech changed all that. First of all, I came in February, and it was miserably cold in Chicago, and I brought a big winter coat with me to Pasadena. I went by train. So I hung up the coat on the train, and just never put it on again until I came back to Chicago. When I came to Pasadena, it had rained for the previous fifteen days, and the weather was just like this—even cleaner—and the place was full of cottages with gardenias. It was just marvelous.

BUGÉ: Had you ever been to a climate like this before?

EPSTEIN: Never—my first time. And the people here, too, were also warm—a marvelous bunch of people. I gave a seminar, and it was an exciting seminar. It was very novel work, and they all seemed so enthusiastic.

BUGÉ: What was your seminar about?

EPSTEIN: On paleotemperatures—measuring the temperatures of the ancient ocean. It was a rather interesting topic.

BUGÉ: And nobody here was doing anything like that?

EPSTEIN: No. We developed it in Chicago; we were the first ones. Bob [Robert P.] Sharp [chair of the Division of Geological Sciences] and [George] Beadle and [Beno] Gutenberg all came and talked to me. It made me feel as though they wanted me; they were interested in what I was doing. Actually, they weren't sure what to expect, because I was a strange animal; I was a chemist. So I spent my first two years here as a postdoctoral research fellow. Nevertheless, in two years I became an associate professor with tenure. The major disadvantage of being a chemist in the geology division was the student body. The students that come here are not inclined to do chemistry. Indeed, most of my students were kind of unusual people. Actually, my first student was Bob [Robert N.] Clayton, from the chemistry division. He's now a very famous chemist, a professor of chemistry at the University of Chicago. And my second student is a professor here,

Hugh P. Taylor, a very brilliant man, who went away for a year and decided he wanted to come back and work with me. And he's certainly not sorry about that. He's now a member of the National Academy; he's done a lot of very exciting work. What he did, basically, was use the isotope technique and apply it almost exclusively to certain types of geology problems. We sort of crashed the barrier into geology thinking. All told, I think I must have something like ten graduate students in all. But this is still a very, very great place to work.

Begin Tape 2, Side 2

EPSTEIN: Bob Sharp is a very unusual person. As far as I'm concerned, he could be president. [Laughter]

BUGÉ: Of the university or of the country?

EPSTEIN: Well, why not? He was primarily responsible for my staying here.

BUGÉ: Was he head of the division?

EPSTEIN: Two years before I came, the head of the division [Chester Stock] died. Then there was a temporary acting chairman.

BUGÉ: Ian Campbell.

EPSTEIN: Right. And then Bob became the real head. It's just the way he ran this place. It was very democratic. I had the feeling that he had a certain philosophy that we were going to try something new. We were going to be innovative, and we were going to do things that would give us some real information about geological processes. He wasn't at all reluctant to talk to people and ask their opinion. It was just wonderful. We discussed things, and we'd have these evening sessions. I think we became a great geology division primarily because we put a lot of work into hiring people—mostly young people. We didn't just hire people because we liked them; they had to have a great deal of promise. Every appointment was thoroughly thrashed out.

BUGÉ: Several other people came here at about the same time as you did. How did they find the division?

EPSTEIN: They found that the geology division here, exclusive of the geophysics, was a little bit conservative. I think [Linus] Pauling may have had something to do with offering a job to Harrison Brown. I'm not sure.

BUGÉ: I know Pauling wrote to Harrison Brown encouraging him to accept a job in the division.

EPSTEIN: Yes. Well, Harrison Brown was asked to come here because he was a very imaginative guy, and he was extremely broad-gauged and very popular, in the sense that he communicated well with the general public. He gave lectures and was on television. He was involved in the problems associated with population growth.

BUGÉ: Even before, in Chicago?

EPSTEIN: Probably. I'm not so sure about that. I rarely spoke to him. He started to work on meteorites, and that became a very hot subject. Anyway, he was invited to come to Caltech, and he was permitted to bring people with him. So he brought me, even though I never really met him until he met me—in the hall that time—and [Clair C.] Patterson and [Charles R.] McKinney.

BUGÉ: Did you know Patterson before?

EPSTEIN: Oh, yes. I also knew McKinney very well. He was an excellent engineer; he was responsible for the building of many of these instruments here. Eventually Lee [Leon T.] Silver worked with Harrison. Also, Heinz Lowenstam was offered a job to come here. He's a very unusual man, an excellent paleontologist. We were friends in Chicago, and both of us decided we would come here. So a whole group of us came here.

BUGÉ: You mentioned that you interacted with some of the people in Chicago. Were they all working on similar problems? Did you come as a unified group?

EPSTEIN: Oh, no. Patterson was working with Harrison Brown; he was trying to measure lead isotopes and concentrations. I was working with Harold Urey on this paleotemperature problem, stable isotopes. McKinney started working with Urey when I first came there, but he quit after a year. Actually, the main thrust of the work planned here was the work on lead. That was Harrison's interest. I came here as sort of a throw-in—at least, that was my impression. Most of the big mass spectrometers for lead work were built before mine. But I had somewhat of an advantage, because I was the only stable-isotope man, so this became sort of my domain. And I was greatly encouraged. Bob Sharp and I cooperated on a number of papers. He got me interested in glaciology, and we published a number of papers together. We were of different interests, but we were all in geochemistry. It was just like any department. It took us a while to evolve and begin to teach.

BUGÉ: In Bob Sharp's interview, he says that people used to make fun of the geochemistry department at Caltech, because it [geochemistry] wasn't recognized in most other places.

EPSTEIN: Right. And, you know, we hired strays—people who were not really geologists. When I was in Chicago, Gerry Wasserburg and I were real good friends. He worked there with me, and many times we used to discuss the problems we should do. He was a bright young man, so I brought up his name to Sharp, and Bob Sharp went to see him, and we hired him as an assistant professor [1955], in spite of the fact that he was doing geochronology and other people here were also doing geochronology. And I think he had only one offer of a job when he was a postdoc in Chicago. But he's turned out to be very good and a credit to our division. We were all people who sort of didn't fit anywhere.

BUGÉ: Why did you come here, even though they gave you a position that you thought was below what you deserved for your credentials?

EPSTEIN: Actually, I never thought at that time that I deserved a better position—not that I was so modest. I knew I had to start at the bottom somewhere. I came to Caltech because it was a place where I could get some equipment built, where there was some space to work in, and where the people were supportive. I just felt good about this place.

BUGÉ: So you had no doubts about coming here?

EPSTEIN: I didn't think too much about that. I try to make the best of things—and if things don't work out, then I'll have my doubts. [Laughter]

BUGÉ: So even in the beginning, it was what you had hoped for?

EPSTEIN: You have to understand, I had no idea what it was that I hoped for, because those were uncertain times. I could not gauge the American scientific scene. I mean, I had a model: My father was an honest man, and I hoped to be an honest man. But that's being simplistic. I was never privy to the life of an academic. I know what I hoped for—to do interesting work and have financial security.

BUGÉ: By the time you came here, you had observed a lot of academics.

EPSTEIN: The University of Chicago did not have a typical lot of academics. If I'd come to a place and I had to teach twenty hours a week, which was not done at the University of Chicago, I probably would have said OK and I'd end up doing the best I could.

BUGÉ: It wasn't that aspect of Arkansas that didn't appeal to you.

EPSTEIN: No. First of all, there was a man there who thought I should be doing a certain type of research. I just was not secure that I would have independence there. Independence was very important; I had some ideas that I wanted to carry out. And I knew I enjoyed working on things that I wanted to do. So that was the main thing. And I was told that I'd have complete freedom to do what I wanted here. What else could I want?

BUGÉ: Once you came here, you really didn't have very much to do with Harrison Brown?

EPSTEIN: Oh, no, we were colleagues; we had lots to do with each other. As a matter of fact, we did a project together dealing with the concentration of atmospheric CO_2, but we never completed the project. That was a pity, because Harrison was interested in the CO_2 problem in the atmosphere. The idea was to see if the change in the CO_2 concentration would be reflected in the isotopic record of tree rings. We had a visiting professor named Edmund Schulman who was a dendrochronologist who analyzed tree rings. Well, there were some problems. Ed Schulman was here for only one year. And there were problems, as we discovered later. I'm doing tree-ring analyses right now and I've found that the isotopic record as a whole is complicated. The isotopic record would be muddled up depending on how much cellulose, lignin, or lipids are present in the wood, and not on the isotopic composition of atmospheric CO_2. Harrison Brown finally published a little paper—sort of as a preliminary thing.

There was a lot of interaction between members in our geology division here; not only was there interaction between various students but we were all friends. The whole geology division had an excellent *esprit de corps*. We also interacted with geologists and geochemists from outside Caltech. We used to have informal one-day meetings, twice or three times a year, at the Scripps Institute, UCLA, and the Chevron Laboratory, which was an excellent laboratory. People came from San Diego, some even came from Chicago, and there were some who came from Berkeley. And we'd get together for one day, and each one would get up and talk about their most recent work. It was very informal, and we had much fun. And there were lots of discussions and fights; we were all very close. Right now, we interact a lot. I know what Patterson's doing, for example.

BUGÉ: Does that continuously lead to new ideas for projects that you might collaborate on?

EPSTEIN: We don't necessarily collaborate scientifically very much, because each one is interested in different things. Patterson's interested in lead and the concentration of lead

in the environment. I'm interested in it, too, but in order to measure lead properly, you have to have clean labs and very precise techniques, which he has developed over the years. We all like to sort of be our own independent researchers, because we have enough ideas. I don't have any problems with ideas; it's carrying them out that's the problem.

BUGÉ: Does it take a different kind of spectrometer to do your work than to do the lead work?

EPSTEIN: They're all based on the same principles. But in the case of lead, you only have a solid compound, lead oxide or lead nitrate, or whatever it is. It has to be vaporized at very high temperatures, so they put it on a hot filament. They heat the filament, and that vaporizes the lead, and then it goes through the mass spectrometer, separates out the mass, and then it's exactly the same thing. Also, the details of the equipment depend on how precisely you want to measure these ratios and what the abundance of these ratios are. For example, in my case, the abundance of oxygen-18 is 0.2 percent of the total oxygen. So, we can't work like the lead people; we have to focus on getting this little bit of 0.2 percent measured very precisely. And also, using gas, we don't have to heat anything; we have oxygen from the CO_2. Basically they're all the same type of thing. A mass spectrometer is a mass spectrometer, but its design and the emphasis of the various constructions depend on what your problem is and what it is you want to measure precisely.

BUGÉ: What were you working on when you first came to Caltech?

EPSTEIN: Actually, the first thing I started working on was trying to measure the temperatures at which high-temperature rocks—igneous rocks, like granites—were formed.

BUGÉ: The temperature at which they were formed?

EPSTEIN: Right. At which they crystallized. But we found some interesting things. We can measure high temperatures using the isotopic method. We also found that there is a complicated temperature record in igneous rocks as well as in metamorphic rocks. The important lesson is that the isotopic composition of oxygen in the crystals that formed the rocks provides information about their origin and subsequent history. You can find ored bodies and hot spots. The Italians, who were using natural steam as an energy source, were able to locate these hot spots and drill into them and tap the steam as an energy source.

Bob Sharp and I started working on glaciers. For example, I found that if you sample an ice sequence in a core from an ice cap, you find that there's a nice seasonal variation corresponding to $^{18}O/^{16}O$ ratio in the ice, with the summer snow being different from the winter snow. And you can then do a lot of stratigraphy and determine how rapidly the snow accumulates. The geologists used to spend years trying to figure out how rapidly snow accumulates. Actually, isotopic values profoundly affected the whole field of glaciology. The amount of work that's being done now in isotopes of the Greenland and Antarctic ice caps is enormous. We started this whole work.

BUGÉ: It reminds me of archeology and how important stratigraphy is in identifying pottery.

EPSTEIN: Yes. But you know, even with pottery, you can use isotopic value analysis to learn things. You can analyze the oxygen of pottery and see at what temperatures these things were heated, whether they reacted with outside oxygen in the air.

BUGÉ: In your first project, when you came to Caltech, who else worked with you?

EPSTEIN: My first graduate student, Bob Clayton.

BUGÉ: And did you teach?

EPSTEIN: Yes, I taught—mostly geochemistry classes.

BUGÉ: Who took these geochemistry classes?

EPSTEIN: Oh, a lot of people took them—excuse me, "a lot of people" at Caltech may mean, you know, five or six. [Laughter] It's fun teaching here.

BUGÉ: Were they mostly in chemistry or mostly in geology?

EPSTEIN: Well, there were some who came from chemistry; I had people from environmental engineering. I think I even had somebody from biology. Two from biology came and took my course, and they stayed to do PhDs. My last one, the one who's interested in anthropology, got his PhD, actually, in biochemistry with a thesis he did with me.

BUGÉ: That's fascinating. Do you think you've done more—or this department has done more—to encourage interdisciplinary programs like that than other departments?

EPSTEIN: I don't know what is meant by "encouraging." Every fall, chemistry has introductory speeches, each about fifteen minutes, by the various faculty, and they tell the people what they're doing. For years, I got up and told them what I did, and nobody paid attention. You see, this is a very good school, and the people who come here are bright kids, and they come here already with their minds made up about whom they're going to work with and what they're going to do. And they go through the catalog and see: "Max Delbrück is here, and I want to work with Max Delbrück"—the late Max Delbrück.

BUGÉ: Are you talking about undergraduate students?

EPSTEIN: I'm talking about graduate students. Now, there were a few souls who *hadn't* made up their mind. Bob Clayton came over here, and the whole thing turned out very well. As a matter of fact, Rod [Roderic] Park, who is a vice chancellor in Berkeley and who was a biology major, did his thesis work with me. He did a good job on his thesis and did good research, but they made an administrator out of him.

BUGÉ: I've read that the students at Caltech don't get much background in geology, or they're not attracted to geology as early as they may be attracted to other fields.

EPSTEIN: In the past, a large majority of the people who came here were all going to be physicists. They took physics and chemistry, and some biology in their high school years. There used to be required freshman courses here in geology and astronomy, but the geology undergraduate student body has always been rather small. However, we have a good graduate student body.

BUGÉ: Is this institute-wide?

EPSTEIN: I'm not sure—probably. I just know from my own experience. I benefited greatly by my interaction with scientists in several different locations. It's very important for students to do their undergraduate work at one place, their PhD somewhere else, and then do postdoctoral work in a third place.

BUGÉ: I think it would be harder here than in many other places, because Caltech is *the* place to be, in so many fields.

EPSTEIN: A lot of fields, yes. But, you know, we don't accept everybody. A lot of people apply, but we accept only those we think will do well here.

BUGÉ: It must be frustrating for the students to not be able to continue on here if they want to.

EPSTEIN: I don't think so. They seem to move on willingly. There are other good schools, too. You know, when you look at the faculty here, you might be surprised as to the schools where they got their PhDs. Many of these schools do not have the students of Caltech.

BUGÉ: You were here a couple of years before you got on the faculty. In 1954, you were made associate professor.

EPSTEIN: Yes, and I came here in ’52. Yes, I was a research fellow from ’52 to ’53; and a senior research fellow from ’53 to ’54. So that was only two years. I did better than I thought.

BUGÉ: Did you have any doubts during that time?

EPSTEIN: Of course. You always have doubts; but you don’t think about it. You just do your work.

BUGÉ: Well, you had just made a major move, and I wondered if you thought, “Well, this isn’t what I thought. I wanted to be on the faculty. Maybe I should go someplace else.”

EPSTEIN: [Laughter] If you stay young, then you always think, “Gee, how would I do as an actor? Or as a musician? Or an author? Will I make a million bucks?” You always have these thoughts. But I’ve never been unhappy with my work; this has been a wonderful experience.

BUGÉ: But you didn’t know that in a couple of years they’d put you on the faculty?

EPSTEIN: No. What I was told when I came here was that despite the fact that I was a research fellow, I was on a tenure track, whatever that meant.

BUGÉ: Well, that would help.

EPSTEIN: Well, yes, that’s why I came here. If they’d told me I could only be here for two years I wouldn’t have come, but I was assured that advancing quickly could happen. Well, sometimes you get assured about something and people forget. I don’t remember what my actual contract was. Then after I was here a year, I participated in teaching a course. Then they promoted me to tenure after two years, which is better than what they do to an assistant professor. Apparently there was no difficulty. I didn’t get much accomplished here in the first two years, because I had to build instruments and so forth. But I was writing papers from what I did in Chicago.

BUGÉ: Was that partly to reassure them that you were productive? Or was this just the way you would have worked anyway?

EPSTEIN: To answer your first question, no, I never thought of that. You know, you do some work, and you're supposed to write it up and tell the people what you did. It doesn't matter how smart you are, or how important the work you do. If it doesn't see the light of day, then it's as though you hadn't done it. And it's very difficult to write up things. I find it painful, but I love to see the product.

BUGÉ: Does it take you a long time?

EPSTEIN: Yes. Well, it doesn't take a long time, but I do it sort of in steps. Of course, what I like best is when a student writes something, and this is good, because that teaches them as well. This is part of the teaching process here—writing a manuscript, discussing everything. It's a very nice way to teach someone.

BUGÉ: Do you generally publish the research you work on now with students?

EPSTEIN: Always, and also with postdocs.

BUGÉ: Who has funded your research since you've been here?

EPSTEIN: Mostly AEC [Atomic Energy Commission], NASA, and NSF [National Science Foundation]. NSF has funded me for years. NASA has been funding me since 1968 or so, since the lunar project.

SAMUEL EPSTEIN

SESSION 3

January 10, 1986

Begin Tape 3, Side 1

BUGÉ: We were talking about the funding for your research that had come from AEC, NSF, and NASA. And you were just starting to tell me about your relationship with them.

EPSTEIN: Yes. The funding has some bearing on the history of the geology division. Do you know the history of the geology division here?

BUGÉ: Tell me, briefly.

EPSTEIN: Some very bright professors in several divisions decided that the original plans drawn in the thirties for the geology division weren't going as fast as they should, and they tried to bring in geochemistry and people who make measurements.

BUGÉ: You're talking about the late forties?

EPSTEIN: No, this was the very early fifties. So, to start the ball rolling, they hired Harrison Brown, who was an assistant professor of chemistry at the University of Chicago. He did some work on meteorites and things of that type, although he himself was not a geologist.

BUGÉ: So why did they choose to hire a chemist?

EPSTEIN: Because he was doing interesting things that had relevance to geological processes, such as the formation of the Earth. He's also a very articulate man and quite visible in the scientific community.

BUGÉ: Why do you think he chose to accept an appointment in a geology division?

EPSTEIN: I believe the University of Chicago was not the most suitable place for his interests. And this was an opportunity to get some money from the AEC to initiate work in his interest; he was interested in the origins of the Earth and various processes involved in the distribution of uranium and thorium geochemicals. He was also able to bring several of his students with him.

BUGÉ: That's exactly what I wanted to ask you about yourself: Why you chose—with a background in chemistry—to come into a geology division?

EPSTEIN: Actually, my undergraduate degree was in geology and chemistry. But I left geology because I didn't see any future in it at the time and took my master's and PhD in chemistry. As I told you, I took the path of least resistance. Also, I was pretty good in chemistry; I enjoyed those subjects.

BUGÉ: But at the time you came here with Harrison Brown, you weren't doing anything with geology.

EPSTEIN: Well, not exactly. But I had been reintroduced to geology when I worked with Urey in Chicago. The problem we were doing was of great interest to geologists.

BUGÉ: I thought you were doing mollusks.

EPSTEIN: We were measuring the past temperatures of the Earth, the history of the Earth millions of years ago—that's geology. And then I was much influenced by a young colleague at that time, Heinz Lowenstam. He was a research associate in the geology department then, and he was doing some great work. And he also got an offer to come to Caltech.

BUGÉ: Was that also through Harrison Brown's contact?

EPSTEIN: Probably not. Although I'm sure Harrison knew about it and was very supportive of the appointment.

When I came here on my first visit [1952], I gave a seminar. It was well attended. And after I finished my talk, people crowded around and asked me questions, because apparently they recognized I was doing something new and this work had relevance to their research.

BUGÉ: It was a revolution, even for here.

EPSTEIN: Well, I guess to some degree, yes. They were enthusiastic and supportive, and I felt great. Here you had these sunny people in a sunny climate. [Laughter] And the opportunity to do work. I wasn't loaded with courses, and nobody pushed me; I had enough time to get the instruments built. So it was a great place. And it's the AEC money that initiated all of this possibility.

Harrison got a lot of money. He had Clair Patterson, who was a student of his, who was associated with me at Chicago. He was an engineer who directed the building of the instruments at Chicago. He was an excellent engineer. Lee Silver, who was a graduate student, was here. Wasserburg came a few years later. So there was a nucleus here.

BUGÉ: But you were really breaking new ground. There might have been isolated geochemists, but nobody else was doing a kind of concentrated work like this.

EPSTEIN: Not only was no one else doing it but no one else seemed to *want* to do it. After the papers came out, I was not showered with a bunch of offers to go places. Neither were any of our other young people.

BUGÉ: Why do you think Caltech was so receptive?

EPSTEIN: Because Caltech had bright, imaginative people. We had Bob Sharp.

BUGÉ: Who could see the value.

EPSTEIN: Of course. And he didn't feel threatened by new ideas, a different kind of geology. He was not only a good scientist but a good person, too; yes, he's still loved by everybody around here. So that was the key thing. And we came here and we started doing some work. There were lots of discussions and lots of activity, mostly about good problems with critical people. It was just a wonderful environment.

BUGÉ: Did you find that the reaction you got from other schools and other people gradually became more positive as they saw how successful this work was?

EPSTEIN: Oh, yes, sure. I think there were a lot of people who recognized that this was a very important activity, but I guess they just weren't ready to invest the money or something to bring it into their geology departments. It was sort of, "This is wonderful stuff, but it's kind of not geological."

BUGÉ: It doesn't fit.

EPSTEIN: Yes, it's not geological work—which, of course, is kind of silly.

BUGÉ: It sounds silly; but hindsight is wonderful.

EPSTEIN: Well, with many people it wasn't necessarily a lack of foresight. In most cases, geology departments couldn't provide the environment Caltech was able to provide. I mean, this building was a geology building. There was no real plumbing. They had to go through revolutions to get labs and equipment set up.

BUGÉ: So, after the AEC money—

EPSTEIN: Well, the AEC recognized that they were supporting a group effort. They got to know us young fellows and recognized that it became just sort of an extended group activity. But after a while, I guess, the AEC had to go in different directions. I think the initial reason for their support was to find out how you can extract uranium from common rocks and things of this nature. Then NSF was created [1950], and they were

willing to support the work. But of course, it's important for them to support productive people, because when they get a product, they can say, "Look, we got this product, and therefore you should give us money to support some more things." So they're very happy with people who are productive. And apparently I was sufficiently productive for them; so they supported me—and still do.

BUGÉ: So they started [supporting you] sometime in the fifties?

EPSTEIN: Well, it was probably closer to the sixties. As a matter of fact, I have a contract with them that's going to begin when I retire. It's not a great deal of money, but enough—just about as much as I asked for.

BUGÉ: How about NASA?

EPSTEIN: NASA came into the picture with the lunar program. This was in the late sixties. I was one of the original PIs—principal investigators. They were very generous. I've put a certain amount of effort in this program, and we did very well. And they still support us.

BUGÉ: Do you still work with the lunar program?

EPSTEIN: No. Well, the whole focus has changed in recent years. We found that meteorites give you a lot of information about the solar system, more interesting information than the lunar samples. But it's the same aerospace, or extraterrestrial, materials that we're analyzing.

BUGÉ: That evidently caused some anxiety on the part of alumni of the division. When the space program became part of geological interests, people wrote to Lee DuBridge and said, "What's going on? I'm an alumnus, and I don't think this is really geology." The advent of geochemistry didn't seem to elicit that kind of response.

EPSTEIN: That's not true. There were plenty of negative responses when our geochemistry department was introduced; but that was years ago. I guess Bob Sharp told you about that. No, I think this was perfect geology.

BUGÉ: But it was nontraditional.

EPSTEIN: Well, of course! The experiments that were done under these contracts stimulated advancements in instruments as well as in ways of thinking about geological processes. People can now make measurements on a variety of samples to do geochronology, using other systems that require greater accuracy and more elegant techniques. And this was all developed during the lunar program by people who participated in the program and probably supported it, which I think is a great product. I think the return from geochemistry of the NASA sample program was enormous compared to the cost. The amount of money that's involved is kind of small. I mean, when they lose a satellite, that could probably support this program for ten years, and I'm talking about the whole program.

BUGÉ: Well, that's essentially how Lee DuBridge answered the people, in a very nice way.

EPSTEIN: DuBridge was a great president. I once spoke at a luncheon meeting of the Caltech geology alumni at a national geological meeting. Our chairman was asked to send somebody to tell them what was happening at Caltech. So I told them all about the new things that were being done over here and their relevance to the "new" geology. There was quite a reaction—that we were not producing enough people for industry. And there happened to be another man there, a professor from another university, who was proud of the fact that he had thirty-five graduate students going into oil companies. Well, I tried to point out to them that people who teach these students who go into oil companies come from places like Caltech. But that didn't go over. Let me put it this way: I think what our geology division has done justifies all that Caltech put into it. We're certainly considered to be one of the best departments in the world.

BUGÉ: That must feel good.

EPSTEIN: Of course!

BUGÉ: Now that Sharp is no longer division chairman, does the division continue that philosophy of stepping into new fields and taking risks?

EPSTEIN: I'm not sure. Yes, we hire good young people. There's always a tendency to hire people you're comfortable with, but they're not always the most adventuresome or creative. So you have to work at hiring good young people. Also, there are some universities that hire, quote, "famous" scientists who have already done the work that made them famous elsewhere. I think it is important that we do not do that.

BUGÉ: It's harder to try to predict who's going to do the famous research, though, isn't it?

EPSTEIN: Not really. There are certain feelings that you have about people: They're bright, they have good records, they produced good theses, and they are knowledgeable. And they usually do a good piece of work. What's more important, their training is usually ahead of the rest of the crowd. They maybe know more physics or math, or they may know more chemistry. They have certain knowledge that will allow them to do things that other people have not yet done. One of the things in this division, for example, is the knowledge that was brought here about mass spectrometry and the implications of what mass spectrometry can do. That's very important.

BUGÉ: Do you tend to look for students who have been trained by your students?

EPSTEIN: No, not necessarily. A student has to be well motivated; that's an important characteristic. Sometimes a person who's less bright but loves his work and loves his science grows faster than somebody who's very bright. You're never a hundred percent right, but our division is remarkable in that respect now. I would say that we actually

have no dead wood, and I'll compare our young new appointments to any appointments at any university. We have wonderful young people.

BUGÉ: If the division chairmen have all maintained that feeling, what differences have there been between, let's say, when Sharp was chair [1952-1968] and when [Eugene] Shoemaker took over [1969-1972]?

EPSTEIN: Not really very much. Except for certain logistical things, I think the philosophy has been intact. We're all concerned about our division. This is like our scientific family. And our philosophy is, "What's good for my colleague is good for me, too."

BUGÉ: So the differences in style between [Barclay] Kamb [division chairman 1972-1983] and Shoemaker, or Shoemaker and Sharp, had a minimal effect?

EPSTEIN: Well, the thing is—I'm not putting down Shoemaker and Kamb—but the point is, they're carrying on a tradition, and they have enough sense to realize they have a good thing. But Sharp had to start that tradition, so that's why we appreciate him, besides other things. He went against the stream, and I give him all the credit. But there were other faculty people here at that time who were very supportive—Al [Abert E. J.] Engel was very much supportive of this. I think Pauling was very much interested as to what was happening. We have a very, very good division. I hate to mention this, but we have so many people here who are in the NAS [National Academy of Sciences]. They represent a significant fraction of a relatively small membership of the geological division in the National Academy of Sciences. Percentage-wise, I think, there's no other division that has had so many people elected to the academy. And even some of those who have not been elected should and will be elected. This is perhaps not necessarily the most important criterion, because there are a lot of people who should be in the academy and are not. But, nevertheless, it indicates that we are now being recognized by the outside world and that what we've done is significant.

BUGÉ: With regard to your own scientific research, the areas of application have changed. How has that developed from your point of view? How have you gone from one subject to another?

EPSTEIN: Well, in the beginning, when I came here, I had some plans. My philosophy about my research is that I can do different kinds of research—some dealing with biological material, some with rocks, with atmosphere, with meteorites. No one has done very much on those, and every time I examine something, I get new information. Usually it was obvious to me that there's some important information in the isotopic composition of rocks. So I recommended to my first four students that they do problems that pertain to these different rock types and see whether there is some systematic record and whether you can interpret this record in terms of geological processes. Also, Bob Sharp, who was a glaciologist, and I cooperated on some work, because we thought there'd be some important information in the isotopic composition of glaciers. In all cases, it was true, it had an enormous impact in these fields. The isotopic measurements are now almost the dominant research in glaciology. The isotopic composition of ice tells you something about the conditions under which the ice was formed and what kind of climate record there was on Earth's surface.

BUGÉ: Once it's been laid down, doesn't it change through time?

EPSTEIN: Well, it's like piling on something; it gets a little squeezed, and then the bottom part gets melted away. But the record spans 150,000 years; it may be more, depending on the nature of the ice cap. I'm just giving this as an example. You know, we tried something, and there were these nice systematic records, and then word expanded to many other labs.

BUGÉ: So nobody knew about this before?

EPSTEIN: No.

BUGÉ: Were you just working with oxygen isotopes at that time?

EPSTEIN: We also did carbon isotope work. We also did some work on petroleum. I had a friend in an oil company—he was a student in Chicago—and he decided to do some work on petroleum. We found certain systematics in the petroleum that tells you something about the origin of this stuff. And petroleum people are using this technique.

BUGÉ: But in the beginning, it was oxygen and carbon, because of the spectrometers?

EPSTEIN: Yes, we had the mass spectrometer. You see, you couldn't buy these things; everything had to be built over here. I would have liked to have built several mass spectrometers, but I could only build one.

BUGÉ: And each mass spectrometer tells you only about one element?

EPSTEIN: Well, it was primarily oxygen and carbon—that was the way we set up the mass spectrometer—and then we built another one for hydrogen and another one for something else later on.

BUGÉ: I didn't understand that they were so specific.

EPSTEIN: Well, you can change them and analyze different elements, but that requires a lot of work. So what you do is to start doing a research project, and you find very exciting things. Then you just keep doing it, because it takes time to modify instruments.

BUGÉ: So that's why you worked mainly with oxygen. But you could change the materials.

EPSTEIN: But just working with compounds, ice and shells and rocks—and then there's oxygen in everything.

BUGÉ: And all the information fed back into the common history of the Earth.

EPSTEIN: Yes, then they could expand it. One of my former students was Hugh P. Taylor, a professor here who's very well known. He did a thesis with me on igneous rocks.[4] And then as things developed, there were some unusual things about some of the samples that were done, and he looked into what happened to these rocks in their subsequent history: whether they react with water—things of that type. But he's been able to examine many, many geological situations. He was in a better position to examine these than I was, because he was also a good field geologist with lots of field experience. So he started running with the ball on some other aspect of this. Taylor represents many excellent examples of combining different skills, plus lots of talent, to create a very important field.

BUGÉ: You stick with one material until another material catches your interest, or until you've exhausted the possibilities?

EPSTEIN: Well, I never exhaust the possibilities. I mean, I could have worked on shells for the rest of my life. But then you read something, or you attend a seminar, or you discuss something, and you say, "You know, it would be interesting to analyze that. It might answer an interesting scientific question." Fortunately, because of my chemical experience and my history as a chemist, I can then proceed to develop techniques to treat the materials in the desired way. For example, I developed the techniques to analyze the oxygen and hydrogen in plants, and this requires dealing with living materials. Of course, once you start to do living stuff, you find something else of interest. "Gee," you say, "this tells me something about climate. There are some plants that grew during the glacial period; let's see what kind of climate existed then." And there are even some plants that have survived in some coal beds for millions of years, and you say, "Gee, let's try that to see what the climate was like on Earth millions of years ago."

BUGÉ: But you didn't do plants until fairly recently, is that right?

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[4] Hugh P. Taylor, "O18/O16 ratios in coexisting minerals of igneous and metamorphic rocks." Dissertation (Ph.D.) 1959, California Institute of Technology. http://resolver.caltech.edu/CaltechETD:etd-03032006-133345

EPSTEIN: Yes, because the amount of work that this laboratory has produced depends not only on me but on the number of postdocs. You know, if somebody gave me the required amount of money, then everything I did in this one career I could have done in five years. [Laughter] But I like to keep my fingers in the work. I do not just manage an operation; I'm deeply involved in all the work. It's not a very efficient way to do things, but the important point is to do things well, so that it stands up over the years.

BUGÉ: So if I tried to trace the different kinds of research you've done, it's not so much a chronology as an assortment?

EPSTEIN: There's no logical basis, except that there are some interesting problems that I thought we should do. Now I'm doing something that's related to medicine. I thought of doing it a number of years ago, but because there are some opportunities now that are arising, I will pursue it.

BUGÉ: And what's that?

EPSTEIN: Oh, this has got to do with respiration.

BUGÉ: Can you tell me how it applies to medicine?

EPSTEIN: I won't go into that, not yet. First, about this tree business. Initially it was to see whether the isotopic composition in trees would give you any information. We found that if you analyze a total wood, it won't give you any information; so you have to separate parts, and finally we found that cellulose may give information. Then you think, "Gee, I could measure continental climate, if these problems interest me." Or the last thing we did—we published a paper about a tree in Africa, where we found the temperature record in Kenya.[5] It's very interesting. It is a temperature record of this

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[5] R. V. Krishnamurthy & S. Epstein, "Tree ring D/H ratio from Kenya, East Africa and its palaeoclimatic significance," *Nature* 317: 160-62 (1985).

place in Kenya, starting from about 1820 to the present. You see, this heavy hydrogen—D—goes up in the cellulose, which means things have gotten warmer and dryer. Then that big drop was in 1960 and there was lots of rain and cooler climate, and then it went up again. The bottom record is the record of the water level in Lake Victoria. It's very interesting, because this is a tree-measured record of what things were like in these dry areas.

BUGÉ: This was 1820?

EPSTEIN: Yes. So things are getting worse in Africa to some degree—due to dryer climate, not only due to how people behave.

Begin Tape 3, Side 2

BUGÉ: So this is another thing you're working on right now?

EPSTEIN: Yes. This fellow from India and I discussed some data on trees from different parts of the world—Tasmania, the Sinai, and other places. He's analyzing these data to find out whether there are cycles.

BUGÉ: So at the moment you're working on more recent history, to see if it correlates with the known?

EPSTEIN: Yes. We're doing other things as well there. This is sort of his problem. He's a postdoctoral fellow. Just to give you an example, you were asking about a real problem. Quite a few years ago, I thought it would be interesting to do a tree from Africa, because of the famine that exists there now. A young man from the University of Utah [T. E. Cerling] brought us some samples. He was working in Africa, and I talked with him about this possibility. He was just a marvelous guy, and he got me a nice slice of a big tree.

BUGÉ: Was he working in the same field?

EPSTEIN: No. He was working in some other field; he was analyzing some sediments.

BUGÉ: Have you actually gone to the Arctic for glaciation?

EPSTEIN: No.

BUGÉ: So how do you get your samples from there?

EPSTEIN: Well, there was the [International] Geophysical Year [1957-58], and we applied for some funding to do this oxygen isotope work. And these people sent us the water we requested. I was busy analyzing it.

BUGÉ: Did they send it frozen?

EPSTEIN: No, you don't have to have the samples frozen. You just put each one separately in a bottle. But they must not evaporate.

BUGÉ: Do you have any idea what you'll work on next?

EPSTEIN: I'm supposed to be working on a book, and I might. I have a number of projects now that I think I'll probably work on until I retire. I've got only four years to go [Professor Epstein retired in 1990—ed.]. This June will be four years, maybe five. Maybe I'll write some articles; I have some unfinished things. Maybe I'll try to have a good time. [Laughter] I think I've earned a certain amount by the time I hit seventy.

BUGÉ: Do you see a trend within geochemistry, aside from your own areas of interest? Has the field evolved in any particular direction?

EPSTEIN: Oh, enormously, in many different directions. Most of the people here are doing new things in petrology. Of course, in seismology there have been some interesting things going on. In earthquake prediction, we have this young guy, Kerry Sieh, who's dating the times of past earthquakes from records in pits. And at other

universities they've now gotten some equipment to do the same. Some universities are more advanced than others. Geology has blossomed enormously. This whole new concept of continental drift—plate tectonics—has many implications. And there's a lot of work in the chemistry of the cosmos—meteorites, radio astronomy.

BUGÉ: When you first came here, it was a couple of years before Millikan died. Did you ever meet him when you first came to the campus?

EPSTEIN: He was around, but I think he was already quite sick. I used to see him once in a while, kind of shuffling.

BUGÉ: Did people talk about him or tell stories about him at that time?

EPSTEIN: Yes, the stories about how he used to go around with a bucket and collect money from the neighbors whenever he needed something. [Laughter] He was a very vital person.

But I met [theoretical physicist] Paul Epstein; we had a nice conversation. I remember being invited to participate in the weekly sessions in which different people from math and physics used to discuss their research. I think this had to do with mathematics. [Professor of mathematics E. T.] Bell and Harrison Brown were there.

BUGÉ: Do they still have weekly meetings?

EPSTEIN: Probably. Of course, there are many more professors now. The Athenaeum is a very good place to meet people. You get to know a lot of faculty.

BUGÉ: Do you think that that atmosphere prevails now?

EPSTEIN: Sure.

BUGÉ: Because some people have said that it's different, now that it's so big and there are so many people.

EPSTEIN: What you do is, you interact with a smaller fraction of different divisions. But I interact with physics people and chemistry and biology.

BUGÉ: Is going to the Athenaeum one way of maintaining that interaction?

EPSTEIN: Yes, or through personal contacts. It's just that there's an environment here where people are interested in what is going on in the institute. People from different divisions are interested in what I am doing, and I'm interested in what they're doing.

BUGÉ: So can you learn from these people about things that you might never otherwise be exposed to?

EPSTEIN: Sure. Not only do you learn but it's like nourishment. Anybody who does any research never really knows how significant it is or whether they're going in the right direction, and it's important to get support from various people and to bounce off ideas and data on various people. Also, the people here are not afraid to give you an honest opinion. The attitudes are different here. I had a very strange interaction with [theoretical physicist Richard] Feynman. We were discussing something, and he said to me, "If I don't absolutely know something about it, I don't say I know something about it." And this was meant in a very friendly way. You don't get offended by that. And people are critical, and they say, "Well, I don't know if this is right." At least it clarifies things in your own mind. This place does not consist of a bunch of yes men.

BUGÉ: So you have to be strong yourself to deal with them.

EPSTEIN: Of course, but it's usually done in a constructive way. And when you realize how much benefit you get from that, you begin to like it and to appreciate it.

BUGÉ: Did you know [astronomer Fritz] Zwicky?

EPSTEIN: I knew Zwicky. He was wonderful. He was an Athenaeum luncheon eater. Whenever you sat at his table, there'd always be these fantastic stories. Most of them

were about the various things he did—his experiments, observing the stars, and the conclusions he came to. He used to claim that he was the first one to see this. And I think he was right—I'm not sure that he really followed it up. You know, I think if you make twenty suggestions and two are correct and eighteen are not correct, you kind of remember the two. But this is OK; this is the way science should be done. But it was fun, you know, to be with him and hear him discuss all these things. He was always such a healthy-looking man; and then he just died [1968]. What a loss. He had a fairly young family.

BUGÉ: Paul Epstein. Tell me a bit about him.

EPSTEIN: Well, when I met Epstein, he was retired and quite subdued, and he spoke very softly and slowly. But I guess it was natural for us to get together. As a matter of fact, because of some mix-up in the institute office, he was paying my insurance for about six months. It wasn't deducted from my paycheck, and suddenly they found the mistake.

BUGÉ: Is that how you met?

EPSTEIN: No. Actually, there was a Russian here, and he decided to introduce me to him. So Paul Epstein and I got together and we checked our lineage. We found out we were not related. Mrs. [Alice] Epstein is still a member of the Athenaeum, and we sometimes get her dinner bill; I wouldn't be surprised if she gets one of our bills.

BUGÉ: Do you feel the campus has changed in the years since you arrived?

EPSTEIN: Oh, yes, the campus has changed enormously since I arrived. They have built much.

BUGÉ: I meant the atmosphere.

EPSTEIN: Oh, the atmosphere. I think the important things have not changed. Since I've been here, the faculty has always had a very important say in matters, surprisingly so.

And they don't mind expressing their opinions about something. The list of priorities, I think, has not changed. The young faculty has always been treated well, particularly in providing opportunities to do something new, and this is a very important philosophy. Doing good science is of primary importance here, and this is the main thing, to this day. There is a surprising lack of dead wood at Caltech.

BUGÉ: So even though there's more bureaucracy because of the size of things and the government—

EPSTEIN: And overhead, which has always been my pet peeve. I understand the increase in costs and so forth. I don't know the details, and I don't want to know, because I think that the administration has to have its own prerogatives and its own problems. So I really can't say what's happening. But in spite of the fact that Caltech is considered to be rich and getting a lot of money, we're still getting money from the agencies. Sometimes we have a bit of a hard time, because everybody thinks of Caltech as being very rich. I'm not exactly sure what they mean by that—whether they include JPL [Jet Propulsion Laboratory]. But in terms of what the agencies get in return for their money, I think Caltech does very well.

Bugé: It was said to be a strategy of Caltech's early years to pretend to be wealthy, on the theory that wealth attracts money. And if they looked like they were prestigious and had money, people with money would be more forthcoming.

EPSTEIN: It seems to me that the thing that makes money come here is what it produces. When they built the Palomar telescope, Caltech was the world's greatest astronomy department. Everything they did was good science and therefore attracted attention and probably some funding. I think they didn't get as much money as they could have, had they made the effort to do so. Caltech had ideas of getting money which I think were not fully exploited. It's easy to say that, I guess, on the outside.

BUGÉ: Do you think that's true now?

EPSTEIN: I'm not sure; I can't really say. It seems to me that Caltech is a great place to put money into. Look at the impact it has on science, or even politics. DuBridge was a presidential advisor, and [Harold] Brown [Caltech president 1969-1977] was a cabinet member. And I think they have really contributed quite well to the politics of science, but mainly their scientific accomplishments are highly appreciated worldwide.

BUGÉ: Speaking of politics, I wanted to ask you about the McCarthy era, in the fifties, when you were fairly new here. How did you perceive that from this haven?

EPSTEIN: Well, we were not markedly affected. The McCarthy era exploited the fear of, quote, "Communists." The classification of "Communist" was, of course, horrendous. This fear of Communism was, as a threat to the USA, ridiculous. But as far as we're concerned, I don't think there was any real threat from American Communists. I think so many innocent people were hurt that this witch hunt was a bad thing for our country. What they were doing was concentrating on the wrong problem. I was much more concerned about our freedom to disagree.

BUGÉ: Well, the campus was not untouched. There were a couple of people who had a lot of pressure put on them. Sidney Weinbaum was here, and was sent to jail. He was a chemist—he'd worked with Pauling—and he was convicted of perjury. He claimed never to have been a Party member.

EPSTEIN: I don't remember that.

BUGÉ: And then a couple of trustees were furious with Pauling himself.

EPSTEIN: But that was a different thing.

BUGÉ: Well, it wasn't unrelated.

EPSTEIN: But, you know, Pauling was supported by the whole institute, including the president. I think Pauling was insulted. And I think he should have felt terrible about it, been angry about it. But I don't think there was any question about his involvement.

BUGÉ: Not in fact. But it's just that there was pressure.

EPSTEIN: I don't think his career was ruined. The people who were ruined were the ones who lost their jobs and couldn't do anything; those are the victims. I don't think Pauling was a victim. As a matter of fact, I think he gained a certain amount of notoriety, and a lot of support from people. I remember when he got the Nobel Peace Prize [1962]. I attended the celebration. You know, I know his family. And I don't agree with some of the things he said.

BUGÉ: Evidently the campus, as a whole, did not respond with as much enthusiasm as one might have thought when he got the Peace Prize, because there were a lot of people who felt that he'd been something of an embarrassment to the institute.

EPSTEIN: No, he wasn't any embarrassment. On the contrary, he was a great asset to the institute, and he was, to some degree, a beacon of light. I don't agree with some of the politics, but he wasn't an embarrassment at all. At that time, I didn't particularly care about his politics either, about his movement for peace. I think it's wonderful to have peace, but you know, there has to be a balanced evaluation of the responsibilities for the lack of peace. Look, anybody who's experienced the political situation in the 1930s is very rarely for unilateral disarmament or unilateral protests. There were lots of peace movements during those times, too, saying we shouldn't rock the boat, and look where that got us. As a matter of fact, the present situation, with this terrorist business and the lack of response from Europeans, reminds me a little bit of that. In the thirties, they also didn't want to respond too much. I worry about that.

BUGÉ: It's a horrible situation. And when [Muammar] Gaddafi says, "Next time on your shores," there's no reason not to believe him.

EPSTEIN: But I believe that Gaddafi is faking. You know, there were several coup attempts on him, so I don't think his people over there are crazy about him spending all that money. And this is something that we don't know too much about—how he distributes his money there. And actually, what he's aiming for is to be the big man in the Arab world, which wouldn't make any sense. Think of what a disaster that would be. I think most of the Arabs themselves think that way—that he would be a disaster.

BUGÉ: Going back to Pauling, why do you think he left Caltech [1963]? Was part of the reason because he felt unappreciated?

EPSTEIN: I'm not sure why he left. I think he just got an opportunity somewhere to expand in a certain direction, and took it. There was, I guess, a certain amount of resentment. But I don't think that governs his behavior. I really don't know very much about that. Pauling was a vital man with lots of ideas.

BUGÉ: He doesn't seem to have suffered any long-term—

EPSTEIN: No, he looks terrific. He's a wonderful man, who can take care of himself. And as far as I'm concerned, he deserves everything he got.

BUGÉ: Are you friends with him?

EPSTEIN: Well, we became friendly with Barclay Kamb, who is his son-in-law, and Linda, his [Barclay's] wife, and we went over to their place a number of times. He's [Pauling] a very straightforward man and kind of shoots from his hip a little bit, but he's bright. And if somebody wants to give him the Nobel Peace Prize, why not.

BUGÉ: What about vitamin C? Do you have any thoughts about it?

EPSTEIN: I have no feelings about that. I don't take vitamin C. I think I get enough of it from my regular diet.

BUGÉ: Are you active in division politics?

EPSTEIN: I'm on quite a few committees. I've been on the Academic Freedom and Tenure Committee; I was chairman of the committee that selected the latest provost. I don't know what you mean by politics. I don't go telling them my opinions on what I think should be done, but I'm interested and active.

BUGÉ: But you're active in the division affairs.

EPSTEIN: We all are. I'm interested in division affairs, and we're all interested in institute affairs, but it doesn't mean that we have to poke our nose into everything.

BUGÉ: Do you think that the trend has been toward more of a separation, because the administration has had to deal with more?

EPSTEIN: It's their job to do certain things. And so far, I've not seen any deviation from what I think has been the most important feature of the institute, what are the important issues here—science and teaching.

BUGÉ: What about the decision to admit women [1970]?

EPSTEIN: Oh, I was all for it. But there was some reason, at one time, for Caltech's policy. We put a lot of effort into our students, a lot of expense. You know, a good student who's graduated with his bachelor's requires a lot of money. And at one time the idea was that if a student graduates, he should go out and pursue and utilize what he's learned. And because in many cases women got married and didn't go on with careers during that time, there was a little reluctance to have a more inefficient output, so to speak. Also, there were the facilities. Right now, things are much more permissive, and women and men can live in the same buildings. In the good old days, it was much more difficult. I think the majority of the faculty was for admitting women.

BUGÉ: You think so—the majority?

EPSTEIN: Oh, of course. It was voted in.

BUGÉ: I thought that that was not without controversy, though, at the time.

EPSTEIN: I think most of it was a problem of logistics rather than philosophy.

BUGÉ: Have you had any particular feelings about the role of the humanities division?

EPSTEIN: Oh, yes. I thought that the role of the humanities was extremely important. As a matter of fact, the whole faculty seems to feel that way, because the only real requirements are for the humanities subjects. We, for example, taught oral presentation here, which was a combined effort, with a humanities man and one from here, so that students know how to present their papers. As a matter of fact, I'm very pleased when I go down and hear our students present their papers. It's usually well done, well organized. And I think part of this is due to the fact that they have to take this oral course. They go through some exercises, and then people comment on how they did and what's wrong. And I thought the humanities did very well in teaching the students, and we put a lot of importance on them taking these humanities courses.

As far as the humanities division is concerned, it's a question now as to whether we can get a large enough [program], or critical mass, so that they can also be very famous scholars and so forth. I guess we're trying to do this, but that's very difficult, you know.

BUGÉ: You don't think that that dilutes Caltech's importance as a scientific institution?

EPSTEIN: There's nothing that dilutes an institute. The scientific quality depends upon the people who compose the scientific community. It doesn't matter whether there are also lawyers or economists or anyone else. One thing can't undo the other, as far as I'm concerned. Now there are some financial issues, money issues. And also, we're not a complete scientific and engineering institution. I mean, we major in certain things. Biology is strong. Geology is strong, but we have a big division; we're over thirty people. Now, can you do history without having at least twenty people? That is the

question. There's been some effort, and I won't make too many comments on that. But it kind of disturbs me when we get some good young people in political science and they leave to go to Harvard. One of the important features in our division, for example, and most divisions, is that people don't leave. Feynman has been here since 1950 or something like that. All our people stay here. I've been here since '52. This was Wasserburg's first job.

BUGÉ: There was a period when a few people left here at the same time. It was about the same time that [Frank] Press [director of Caltech's Seismological Laboratory] left [1965].

EPSTEIN: Maybe we didn't give them tenure or something like that. I don't think Press took anybody with him.

BUGÉ: No, I don't think they went with him. They left within a couple of years before he left. And that's why it came up in the records.

EPSTEIN: There were some people leaving because they didn't like the things that were going on. But I think these people left because they didn't belong here.

BUGÉ: Was it a matter of getting rid of the dead wood?

EPSTEIN: It's not that they were dead wood; it's just that they felt that this was not their environment, which is OK.

BUGÉ: But you think that there's a different problem going on when they leave the humanities—that the division is not holding on to the good people.

EPSTEIN: Yes, certain good people. That is a different problem. And, as a rule, usually the people will leave when somebody else wants them. So you lose the better people.

BUGÉ: Beginning in the middle seventies or thereabouts, you began receiving many honors. I wondered if anyone was especially significant or meaningful to you?

EPSTEIN: I got the Day Medal [1978]. This is a medal that's given by the Geological Society of America for contributions in chemistry and physics as applied to geology. And it was given to me in Toronto. The people that organized the meeting were Canadians. That touched me—the fact that they invited me. You see, I was raised in Canada, and I was not very successful there. These awards and things like that depend on some people thinking you're doing all right. It's OK. But I had lots of support from very critical colleagues; they told me I was doing fine. That was more important.

BUGÉ: Was being made the William E. Leonhard Professor [of Geochemistry—1984] a significant event in your life?

EPSTEIN: Yes, that was nice. First of all, there's a little bit of an advantage here. And it's also good for the institute. Sometimes chairs are given in terms of practical things. Yes, I was glad to receive it.

BUGÉ: Did you know Leonhard?

EPSTEIN: Yes, I met him. He's one of the major contributors who is still living, so that is kind of nice. It's nice to have a chair. Let's put it this way, it's a pat on the back from the administration. And I always felt secure with NSF; they were always very nice. When you get an award, you kind of realize that it's not a big issue. I've not gotten many awards.

BUGÉ: There's quite a few on that list.

EPSTEIN: Yes. I don't know how to put it. It's OK. That's not the vital thing in my life. The vital thing is that I have this wonderful environment and I can do what I want.

BUGÉ: Is there any person whom you consider a major influence, or someone about whose influence you have a strong feeling in terms of their science?

EPSTEIN: Oh, yes. At first, I wasn't interested in science. I managed to get grades and so forth, but you know, like somebody gets a crush. That happened to me in science as well. There's a man—I saw him last year—a professor of chemistry at the University of Manitoba, a Scotsman, his name is [Alan Newton] Campbell. And for some reason or other, he accepted me as a graduate student. I guess he sort of put it, "Well, there's nobody else." [Laughter] And we were doing some stupid problem—the density of something—and it was the strangest feeling! It's like I had just discovered something very beautiful. And I got excited about doing science. I suddenly realized I was doing something unique. I think the big realization was that here I was, calibrating something, and if somebody were to watch me do this, they'd think I was a nut—you know, looking at something and measuring the height. It made, actually, no sense, except to me—and that's a wonderful feeling. It's like being an explorer. You go on a boat and discover a new lush island. You've experienced something that few others have. Just like an artist who sits down and paints something; through all the strokes, something comes out that is beautiful. Campbell opened that door for me.

After that, there was Harry Thode, who is still a dear friend of mine. I guess he's in his seventies now, but he looks like he's fifty. He's a good scientist, and he brought me into the world of mass spectrometry. And then at the University of Chicago there was a man named Clyde Hutchison—he was a professor of chemistry—for whom I have a special affection. He was friendly, honest, a great teacher, and generally a very good human being. And he was just marvelous. Of course, I learned a great deal from Harold Urey.

Begin Tape 4, Side 1

EPSTEIN: Harold Urey had a thousand ideas. He thought of doing a thousand important things. Maybe 998 were shooting from the hip; he hadn't really thought it out. But two of the ideas would be very unusual, and that's all you need, just two—even one good idea. With people like that, you don't concentrate on whether they were wrong or that they changed their mind. The important issue is to recognize their good ideas and take them seriously.

BUGÉ: He was quoted as saying, "I'd love to go to the moon; I think I'd go even if I knew I could never get back."

EPSTEIN: I'm not sure that I believe that. He was a pretty normal person. He liked life.

BUGÉ: Was there anybody else?

EPSTEIN: Well, all the young people here. There is this man [gesturing to Professor Heinz Lowenstam, who has just entered the room]. I used to argue a lot with Gerry Wasserburg, who was then an obnoxious graduate student. And there was a young fellow named Harmon Craig. He'd come and say some stupid thing on purpose, and I'd say, "That's not right." And by the time I got through with him, I was completely drained. But nevertheless, just talking with a guy like that makes you think. Clyde Hutchison was really special. The environment is extremely important. You're surrounded with a bunch of bright people; you learn by osmosis. It's very important to be in a place where lots and lots of radiation is coming at you. It's like sunshine—you absorb it, and you become a good scientist.