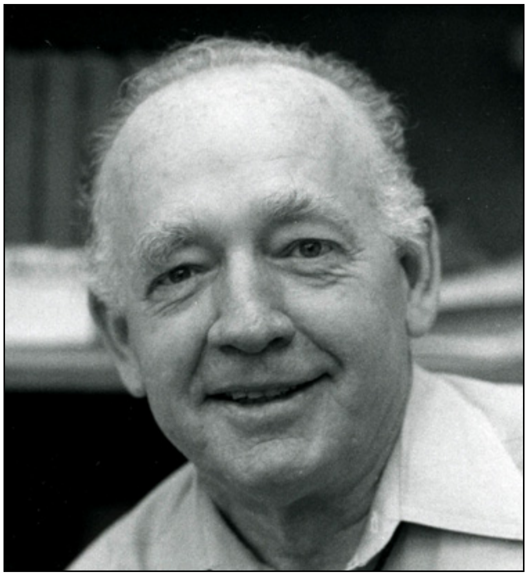

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LEON T. SILVER
(1925–)

INTERVIEWED BY
SHIRLEY K. COHEN

December 1994-January 1995, February 2000

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Geology

Abstract
Interview in six sessions, in December 1994-January 1995 and February 2000, conducted by Shirley K. Cohen with Leon T. Silver, W. M. Keck Foundation Professor for Resource Geology, emeritus, at Caltech. This lengthy interview begins with discussion of family background in Russia and Poland, youth in New York and Connecticut, and beginning of higher education at Colorado School of Mines, 1942. Recalls participation in navy V-12 program at U. of Colorado; degree in civil engineering and entry into Naval Civil Engineer Corps; decision to resume work in geology at U. of New Mexico after discharge in 1946; master's degree 1948; entry into PhD program in geology at Caltech, fall 1948. Recalls Caltech geology division in 1948; its history and personnel to that time. Changes following WWII: Robert P. Sharp becomes chairman; recruitment of Harrison Brown to Caltech; beginning of geochemistry studies; Silver assigned to Brown's group; group members Samuel Epstein, Clair Patterson and Charles McKinney; Patterson's work on age of the Earth. Funding for new state-of-the-art geochemistry labs; McKinney builds new mass spectrometers. Interlude concerning personal history. Continues on work with Harrison Brown group, 1950s; recruitment of Gerald Wasserburg, 1955; appointments of Clarence Allen

and Frank Press. Silver's work on radiogenic lead systems. Beginnings of planetary science at Caltech; Brown's study of meteorites; appointment of Bruce Murray. Decision to choose planetary science over marine sciences; subsequent loss of Frank Press. US decision to put a man on the moon; proposals to NASA to do lunar-sample analysis; Caltech dominates first lunar science conference (Houston, January 1970); Silver's training of Apollo astronauts in geology, especially Harrison "Jack" Schmitt (Caltech BS, 1957). Recalls 1971 San Fernando earthquake's impact on geology. Describes Grand Canyon trips for Caltech Associates and trustees involving Eugene Shoemaker and Silver; other alumni trips; establishing the R. P. Sharp divisional chair. Notes work outside of Caltech: USGS, NASA, Geological Society of America, National Research Council. Election to National Academy of Sciences, 1974; other awards. Comments on Caltech presidents DuBridge, Brown, Goldberger, Everhart; provosts Bacher, Christy, Vogt. Remarks on LIGO [Laser Interferometer Gravitational-Wave Observatory] and projects of special status. Comments on geology division chairmen. In final session, Silver describes expedition with Gene Shoemaker to find Indian Sipapu [hole where the Hopi people emerged onto Earth] on the Little Colorado River; description of geological feature housing Sipapu; obtaining and bringing sample of Sipapu water back to Caltech's geology division.

Administrative information

Access

The interview is unrestricted.

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Geologist Leon Silver logged innumerable hours in the field with everyone from Apollo astronauts to Caltech students, trustees, and interested faculty from other disciplines. Here he is seen (at left) in 1976 at Caltech's Freshman Camp with some tired-looking students and physicist Richard Feynman (at right).
Photo by Floyd Clark. Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH LEON T. SILVER

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

Caltech Archives, 2001

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COHEN: Good afternoon. Why don't we start with your giving us some idea of your family background.

SILVER: Well, I was the seventh child in a family of seven. The two oldest children didn't make it to adulthood. I was nine years younger than the next older so I was, by all odds, the baby of the family.

COHEN: In every way.

SILVER: In every way. My family came to this country. My father came from Russia, and belonged to a family of horse traders who traded horses between Sevastopol and Transylvania. My father was a harness maker and leather man, and that was the business he brought to this country. My mother came from Poland. I don't know the exact town. I think it was somewhere near Lvov, but I don't know exactly. Of course that's a part of Poland that migrated back and forth.

COHEN: They came as adults?

SILVER: They came as very young adults, and met and married at an early age in this country—in Brooklyn, New York. But my father didn't want to stay in the big city. He was a country boy. He and my mother had a series of farms, and I was born on a small farm in the Catskills, in Monticello, New York. I'm told I was delivered on the kitchen table. My family moved, within a few months of my birth, to northwestern Connecticut. We lived in and around Waterbury, Connecticut. I went to the schools in Waterbury.

COHEN: The public schools?

SILVER: Public schools in Waterbury. During the latter part of elementary school and high school, we were living on a farm about eleven miles from Waterbury, and I commuted back and forth from it. I had three brothers and a sister.

COHEN: Older than you?

SILVER: Older than me—considerably older. My oldest brother never finished elementary school. He went into my father's business, which by that time had moved from harness, to upholstery, into automobile repair, to automobile dealership. And he ran that business for my dad in later years. But they sent all the rest of us off to college, as best they could. I was born in 1925. By the time my older brothers were ready to go to college, my sister was in college in 1931 and '32. It was the Depression years and the Depression times were very rough times, but everything was done to make sure that we had some kind of education.

COHEN: They placed a real value on education.

SILVER: There was a real value on education, although my father had never had that education. And my oldest brother had never finished his. I have one surviving brother and my sister. My surviving brother lives close by here, and he sent one of his sons to Caltech. So I have a nephew, Richard Silver, [BS physics '66, PhD physics '71] who is an alumnus and a PhD in physics. He was a student of George Zweig, and he's now at Los Alamos. My sister still resides in New York. I had two children.

COHEN: Let's finish with you first. You grew up and went to school in Waterbury.

SILVER: I finished high school in Waterbury in 1942, which was an interesting time to finish high school. I had to decide whether I was going to be in political science and history, or geology. And I had scholarships in both areas, which was very important to the family. I had an older brother, Caswell, who was a geologist, and his influence led me into geology.

COHEN: Where did he go to school?

SILVER: He originally started at Connecticut State Agricultural College in Storrs, which is now the University of Connecticut. Then he moved west and finished his undergraduate work as a geologist at the University of New Mexico. He took a master's degree there, and then went into the oil business as a consultant, and then later as an independent oil-company owner.

COHEN: Your father's sense of the outdoors really carried through.

SILVER: Very much. We were all Boy Scouts, and very active in it. We were all outdoor people. I initially went to the Colorado School of Mines when I had just turned seventeen. I had two brothers and a brother-in-law in the service. I joined the Navy Reserve and I spent a little over a year at the Colorado School of Mines, which is in Golden, Colorado, where Coors beer is made.

COHEN: As part of the navy?

SILVER: No, not yet. The navy called me up in July, 1943, when I was a little over eighteen. They sent me off into a Navy V-12 program, which was very similar to the programs that were here at Caltech at the time. But I went to the University of Colorado, which is in Boulder, and into an accelerated program. I got a degree in civil engineering from the University of Colorado. I got a line officer's training subsequently, and then went to the Navy Civil Engineer Corps Officers Training School at Camp Endicott, Rhode Island. I missed most of the activity of the war. I then got a Naval Civil Engineer Corps commission on top of my line commission, and was assigned to build the San Francisco Naval Shipyard at Hunter's Point. And I was given a very interesting experimental engineering task—we were building the regunning pier. Regunning mole is what it was called.

COHEN: Regunning mole?

SILVER: Yes. It was a large pier, built in such a way that a bridge crane on it—which was the largest crane in the world then, and still is, to my knowledge—would be able to lift a complete sixteen-inch gun turret off a battleship, or the huge elevators off an aircraft carrier, to repair them and then put them back. The regunning was for war-damaged vessels, but by the time I went to work there, the war was over. But the navy followed through, and I became assistant project manager for waterfront construction. And in the end, as the war contracts wound down, I wound up at the age of a little over twenty as the sole project manager, settling about $150 million worth of contracts.

COHEN: You were still part of the navy.

SILVER: I was still in the navy at the time. I got some education in the ways of the working world there.

COHEN: For some people, the war was not a bad experience.

SILVER: Well, it wasn't. I've always felt that someone was looking after me, because I did have two brothers and a brother-in-law on active duty. My brother, the geologist, was in photo intelligence in the western Pacific, from Guadalcanal all the way to the end of the war. My brother-in-law was a doctor with Patton in North Africa, and then with the Fifth Army in Italy. And my oldest brother, the one who did not get a commission, was a master sergeant for an automotive maintenance battalion with Patton all the way through Europe. My brother Alex—Richard's father—was an aircraft engineer in Connecticut. But my father was very well known in Waterbury, and at the Selective Service office there, and I think that since I was so much younger, they were probably recognizing …

COHEN: That was enough for one family.

SILVER: That was enough for one family at the time.

COHEN: I'm impressed with your father. He came as an immigrant, probably not a lot of English.

SILVER: Not a lot of English. He always spoke somewhat broken English.

COHEN: But he evidently got to a position of respect and prestige in the community.

SILVER: Oh, yes indeed. He knew everyone and he was known everywhere. In fact, surely that influenced my growing up, because as a youngster, I hitchhiked all over Connecticut. Every state trooper in Western Connecticut knew my father was Jake Silver. They knew all of us and they stopped us, picked us up and brought us home. In fact, they stopped all of us because we were roaming boys.

COHEN: In those days, it was something one did.

SILVER: One could do it and not face anything like the hazards one has to now. It was a wonderful time. Connecticut is a beautiful state. It was a great place to grow up.

COHEN: So through your navy experience you found your way west?

SILVER: Well, actually, when I decided to make this decision, I had scholarships to both Oberlin and Reed. I made the decision to become a geologist. I asked my brother at the time what the best school was. He said the Colorado School of Mines. He was wrong. He was wrong.

COHEN: But would either of the other two have been?

SILVER: Well, the other two were great liberal arts schools, and if I had wanted to go into political science or history, they would have been great places. Another one of my nephews did go to Reed and did very well there. And my colleague, Clarence Allen, was a Reed product. I have great respect for Reed College. But I bent the way I bent and I don't regret it. I got out of the service in July 1946, almost a year after the war was over, and I decided to go back to school. So I went back to the Colorado School of Mines. And the Colorado School of Mines would not recognize the bachelor's degree in civil engineering from the University of Colorado, or my experience in civil engineering. They wanted me to start again, as almost a beginning sophomore. The reason they gave was very simple—they were just overwhelmed with returning vets, and they couldn't put many irregular students in. It's not a big school. Now that was their decision. It was okay with me. So I called my brother, who suggested I go down to the University of New Mexico, in Albuquerque, where he was finishing his master's degree. He'd gotten out of the service in the fall of '45, because he'd been in much earlier. So I went down to New Mexico and made up a lot of the geology that I'd not been able to take while I was at the University of Colorado.

COHEN: Was that a better school for you?

SILVER: It was. As it turned out, there was a small but very good faculty. By the time I'd finished the first year's work, I took the national examination for a professional position on the US Geological Survey. I came in first in the country. And it was kind of embarrassing, because one of the faculty members, who was my best friend, was a brand new PhD out of Harvard—Carl Beck, who was a distinguished mineralogist. Carl took the same exam and I waxed him. So at the end of my first year there, I was given a summertime appointment as a junior field assistant to the US Geological Survey. And that began an interesting relationship, in that I worked a total of eight summers for the US Geological Survey. That was a wonderful education, and a great opportunity. I worked on a great range of projects. The first summer, I worked on volcanic rocks in the San Juan Mountains of Colorado, to which I returned and researched many, many times. The second summer, I worked on the brand-new Colorado Plateau Uranium Project, which was the beginning of the great search for uranium to fuel our military and civilian aspirations for nuclear energy. And then I worked in Arizona. And by the time I started working in Arizona, I'd already come to Caltech.

COHEN: Now when did you get your degree?

SILVER: Well, in New Mexico, I didn't get a bachelor's degree in geology—I went right on to a master's degree. At the end of my second year, I had a master's degree, and my supervisor was a very distinguished product of our geology division here at Caltech by the name of Vincent C. Kelley. Vincent C. Kelley, incidentally, was a good friend of Bob Sharp's. The Survey offered me a fulltime job, but Kelley thought I ought to go into graduate school and he recommended Caltech. And he wrote letters for me, which I'm sure were good, and got me here. So I came here in the fall of 1948.

COHEN: Now the Caltech geology department could not have been very big at that time.

SILVER: It was not big by other departmental standards here, but it was a significant-sized department by national standards. At that time, there were probably ten or eleven faculty members, some of whom were great names like Beno Gutenberg. Charles Richter was here, and Hugo Benioff was here. In geology, we had Robert P. Sharp. There had been considerable change just about that time. John Peter Buwalda was still teaching, but he had stepped down as chairman. And the new chairman was a vertebrate paleontologist by the name of Chester Stock. John Peter Buwalda was the founding chairman of the geology division.

COHEN: When was the geology division founded?

SILVER: It was founded about 1926. It was quite early on. And there had been a seismographic station here that was run by Harry O. Wood. But Wood wasn't there any more. The first great seismologist to come here was Beno Gutenberg. He built things in geophysics. There was Richard Henry Jahns—a Caltech PhD that stayed on, and a very good geologist. In mineralogy, we had a man by the name of Rene Engel who taught sometimes, but wasn't a full-fledged member. And the same year I came, a new mineralogist/petrologist by the name of Albert E. J. Engel came to the department. I was his teaching assistant for part of that time. Who else did we have? We had C. Hewitt Dix, a great crystal geophysicist, and another geophysicist, a Russian name that I cannot recall [Gennady Potapenko]. This was a very distinguished faculty.

And while I was a graduate student, other important faculty members were added, and one or two left. Rene, I know, is no longer associated with Caltech. In the fall of 1951, Harrison Brown was appointed to the faculty, but he could not come immediately.

COHEN: Now, did you have your doctorate degree already by this time?

SILVER: No. I didn't get my degree until 1955, and I had a master's degree when I got here. It took me seven years of residency.

COHEN: There must be a story to that.

SILVER: There is a story to that. The new chairman, Chester Stock, died of a heart attack in the fall of 1950. I know this well because I was president of our geology club at the time. I was involved in organizing some of the memorial services and programs. Then we had a period of a year when Ian Campbell, whom I should have mentioned from the beginning, was acting division chairman. And then [Caltech President] Lee DuBridge asked Bob Sharp to become chairman. That would have been in '51. When Harrison came, he recognized that the group he was bringing with him didn't have any geologists.

COHEN: They were chemists?

SILVER: They were all chemists or physicists.

COHEN: Do you know anything about how Harrison Brown came to be convinced to come?

SILVER: Oh yes, I know something about Harrison Brown's coming. I can do better if I could go back and look at some old notes that I have, but let me just say that Harrison was suggested perhaps by Linus Pauling.

COHEN: Who would know him as a chemist.

SILVER: Who would have known him as a chemist, and knew his wartime work. Linus had discussed this with both Jesse Greenstein and with Beno Gutenberg, and they were seeing the opportunity to bring enormous advances in analytical chemistry to bear on some of the fundamental problems of the age and origin of the Earth and the origin of the solar system. Harrison had been a very brilliant young chemist, whose PhD thesis was on the gaseous diffusion separation of uranium-235 from uranium-238.

COHEN: Had he done this before the war?

SILVER: Well, actually I think he probably did it right into the beginning of the war—perhaps before the Manhattan Project had really been established. But he very quickly rose to become, I believe, Assistant Director of the Clinton Laboratories in Tennessee, where he was working on uranium isotope separation problems. But Harrison was very thoughtful; extremely broad. If you look at Harrison's career, you can see how broad he was, all the way around. And Harrison needed a geologist, so Bob introduced me to him. I was then a beginning third-year graduate student here.

COHEN: Was it unique to bring chemists in this way? Was this just Linus and Jesse's idea?

SILVER: I cannot give you full details—I wasn't party to that. But they talked to Bob, and to some of the other activist members of the faculty, including Al Engel. There was great enthusiasm. So they brought Harrison out, and Harrison gave a series of lectures. Harrison had been working with a couple of very fine grad students on the chemical characteristics of meteorites. The two grad students were Ed Goldberg, who is now a professor emeritus at Scripps Oceanographic Institute, and Clair Patterson, our own professor emeritus. Have you had an oral history from Sam Epstein? Has Sam told you how he happened to come?

COHEN: Well, yes. It's sort of a plum. "I met him one day and I needed a job."

SILVER: That's just the way it happened. That's what Sam has told me many times. Sam had been doing all of his work with Harold Urey. Before that, he'd worked in Canadian nuclear-energy projects.

COHEN: How did you convince all these talented people to come here?

SILVER: Not me! Chicago was an immense center of talent. And, if you will, these people were entrepreneurs. They were striking out into a new scientific world. They really had much courage, and I think they had confidence in their leader.

COHEN: That was Harrison Brown. So they started in a new place where they could do these new things.

SILVER: And when they got here, the most critical thing yet to find was a mass spectroscopist—a man who could not only operate mass spectrometers, but could build modern machines of the highest quality. I'll come back to that in a bit. But we started this by saying, how did I get involved?

COHEN: And why did you take so long to get a degree?

SILVER: Why did it take so long to get a degree? Well, in December 1951, we had to start making room in which to construct our first mass spectrometer. It was a senior student's room in the basement of North Mudd. We didn't have a South Mudd at the time. We just had a big parking lot out there, right next to Culbertson Hall. And my first job was cleaning things out. But it was understood that when the chemists and the physicists arrived, I would be working with them and for them. I was only a graduate student.

COHEN: So you were the geology major.

SILVER: That's what Bob had in mind. He felt that I had a more appropriate background for working with them. I guess that's what he thought—I'm assuming that. He never has told me that. And it was very exciting.

COHEN: So you were really their central work person. I mean you would be the boy that got sent on errands.

SILVER: I got sent on some horrendous errands. Wonderful errands, some of which were great adventures. I enjoyed it immensely, but I also did a great deal of just plain old physical work. I broke rocks. I prepared samples. I did all kinds of things.

COHEN: And had you met them on their visits out here?

SILVER: I heard them give seminars when they came out preliminarily. Harrison was a full professor; the others came as postdocs. I heard them when they came out and gave their talks. In fact, I was sent down to meet both Sam and Pat [Clair Cameron Patterson]—one at the train station, and one at the airport. That was fun, and we've been solid friends ever since. The three key people who came initially were Sam Epstein, Clair Patterson, and a man by the name of Charles McKinney. Charles McKinney deserves a very large amount of credit for producing a working mass-spectrometric laboratory, with a variety of state-of-the-art machines, quicker than anywhere else in the country.

COHEN: Where did the money come from?

SILVER: We had to spend a lot of money on instruments. Harrison came with an AEC [Atomic Energy Commission] contract to study the fundamental geochemistry of the "critical" elements—uranium and thorium. But Bob had to come up with some money to help us, money to pay for this mass-spectrometer laboratory. And there was a marvelous collection of vertebrates, which Chester and his preparator and his artist had prepared. They lined the upper floors of Mudd, and they were in the central museum in Arms—dinosaurs, mastodons, pygmy mastodons, mammoths.

COHEN: Where did he get this stuff?

SILVER: Oh, he found them.

COHEN: Out in the field?

SILVER: Out in the field. Most of them were from Southern California. He was the first man to do essential descriptions on many of these things. He was a specialist on Californian vertebrates, and he trained many distinguished students. He was quite a character. But we never had a zoology department here, to speak of. If you look at our history, we've had at most one or two zoologists. We've had lots of plant physiologists. Of course, plant genetics was one of our great things. And if you talk about insects, like fruit flies and what have you, yes. But that was insect genetics; it was not zoology so much. That's not completely true and I'm not an expert. So when Stock died, we began to lose momentum in paleontology. We had a very distinguished invertebrate paleontologist, Charles Merriam, who left. He had a joint appointment, I believe, with the US Geological Survey. So Bob took over about a year after Stock died. Bob made a decision—he approached the LA County Museum and said, would you like to acquire all of these wonderful vertebrate specimens? Dinosaurs and mastodons, all from Southern California. And they said they would. And he said, well I'd like to give them to you, but I can't afford to. Can we work out a number? So they got an evaluation.

COHEN: And this was something Bob just did?

SILVER: Did.

COHEN: That's fantastic.

SILVER: He raised $100,000. Now Shirley, this was $100,000 in 1951 or '52. You'd have to ask, or you may have it in the records. But, being Bob, he may not have told you all this.

COHEN: I don't remember anything about a deal like this. I would have noticed it. I did not see it in his oral history.

SILVER: That money was used to build the mass spectrometers, a clean lab for Patterson to work on meteorite isotope systems—which gave us the age of Earth—and extraction lines for Sam [Epstein] to build with. He introduced stable isotope geochemistry to Caltech, and really to the world. So that was a marvelous thing.

COHEN: This was all on those bones?

SILVER: Well, it wasn't *all* on it, but it was the Caltech contribution on those bones. And I don't know anything about the full funding story. I do remember, because I had been a student who had always admired the bones. There was a beautiful little pygmy mastodon that had come off Santa Cruz Island. It stood that high. There was a saber-toothed tiger. It did go down to the museum, but the museum has given it back to Caltech. And it stands on the first floor of Arms. If you get over to Arms again, it's outside the big lecture hall. It's just down the hall from it. It's beautiful. There are many beautiful things. Up on the wall there's a giant mosesaur [word?], or something like that. So there were lovely things, and they served the museum well. And I think perhaps the museum's collection grew to the point where they weren't as essential, so we got some of the things back. Ed Stolper [current Geology Division chairman] has welcomed them back, because they are great for public displays. The only thing I want to say is that Bob Sharp, at a very crucial time—and this is something he did over and over again—leveraged the resources he had to do big new things. This makes a point which I think—well, maybe I've made this one before, too. Entrepreneurship in science is very, very important.

COHEN: So this was an entrepreneurship thing.

SILVER: The whole thing was entrepreneurship. That's true in the whole history of work at Caltech. The greatest entrepreneur of them all may have been Robert A. Millikan. Maybe the second greatest was George Ellery Hale. In any event, that's where that money came from. And it was matched against the AEC money.

COHEN: Now Harrison Brown brought in the AEC money.

SILVER: Brought in the AEC money. He had all the connections. He knew all the people who were now managing the various offices and were worried about the next phase of the development.

COHEN: It's amazing that Chicago let him go.

SILVER: It's not so amazing to me, in part because Chicago was focusing on other things, such as the Enrico Fermi Institute, where Harrison was a member. Not only did Harrison go, but

Harold Urey subsequently went. They worked in related fields, and to some extent, I believe there was a degree of professional competition. If you had Harold Urey and you had Harrison Brown, you had to make a decision. And Harold Urey was a Nobelist, you understand.

COHEN: So both needed a big world to move in.

SILVER: And they were good enough to work in the big world. Well, let's see. Let's try to come back and pick up the thread that we were talking about.

COHEN: You were still a graduate student.

SILVER: I was still a graduate student, but starting in January 1952, I went to work full-time as a research geologist for Harrison's project.

COHEN: Even though you were still a student.

SILVER: I was still a student. I wasn't taking courses. I was now working in the summers on my dissertational work, which I was doing with the support of the US Geological Survey.

COHEN: Now, was this in any way related to what you were doing for Harrison Brown?

SILVER: Not immediately or directly, but Harrison was broad enough in his thinking to see that my growth as a geologist was very good for the group. This had to do with the formation of mineral deposits, and the general geology of the great mining districts in southeastern Arizona. But it was all a wonderful education for me, and Harrison very quickly established a strong working relationship with the US Geological Survey, where I was the liaison. I knew the USGS people as a junior geologist, not as a senior person. And at that time, several other faculty members at Caltech were also part-time employees of the Survey. That included Engel and Jahns. So we had a history of association with the Survey. And I worked with a particularly intelligent geologist that Harrison had made the initial connection with—a man by the name of Ralph Cannon. And Ralph Cannon's job was to bring the power of isotopic studies to bear on problems of mineral deposits and other things. I just learned an enormous amount from Cannon.

He was a classmate of Al Engel's at Princeton, in graduate school. I had already worked with three other classmates in the USGS. The world wasn't so big back then. We all got along well. And I got along very well with Al Engel. Al Engel's wife, Celeste Engel, became an analytical chemist here, and did some very nice research, both in collaboration with her husband and in collaboration with Bob Sharp. But of all the people around, she and I were closer in what we were interested in—in what we talked about—than many of these others, so I got to know her work very well. It was a great time to be a graduate student, even though I was working full-time. And I did work full-time from then on, until I was appointed to the staff.

COHEN: It sounds like everybody was a colleague. It didn't matter if they were a professor, or a student, or what have you.

SILVER: By and large, that was true. Everybody was a colleague. But I didn't graduate until 1955 because I was working on all these other projects, and doing this other research.

COHEN: So it was seven years.

SILVER: It was seven years. I said I started working with Harrison in December '51. It was in January 1952, that I really went to work full-time. And I got my degree in June '55.

COHEN: And they weren't about to let you go.

SILVER: Well, I don't know about that. Because I did have some offers. I could have gone to work with the US Geological Survey at any time.

COHEN: Were any other institutions doing this chemistry that you were doing here?

SILVER: Yes, but by that time we were well started.

COHEN: And when are we talking about?

SILVER: We are talking about '52, '53.

COHEN: That's when all these people had come.

SILVER: Had come. Sam Epstein very quickly and efficiently set up a group of extraction lines. He had worked with Urey and Lowenstam in establishing the first technique for determining the temperatures of the ancient oceans, using the oxygen-isotope equilibration between carbonate shells and ocean water. That had been started in Chicago, and Sam came here to do some additional work. But Sam was interested, in a very broad way, in the whole cycle of oxygen and carbon. What made it really go for Sam was that Charles McKinney built a first-rate mass spectrometer that was quicker, faster, and more effective than anybody else's.

COHEN: Who blew all the glass?

SILVER: Sam blew some of the glass. Chuck [McKinney] blew some of the glass. There were good glassblowers on this campus. This was a time when the campus had a fair share of technical staff.

COHEN: Who could do these things.

SILVER: And who were appreciated. And who liked being in this environment. So things were at work by 1953. That machine, you know, is still running. It's no longer the world's most precise machine, but at the time, it was a stable, precise machine and Sam knew what it could do. At the same time that McKinney was building that machine, which was based on designs established by first-rate people at the University of Chicago where McKinney had come from, he also started building a solid-source thermal-ionization mass spectrometer—the first one here. That was the machine on which Pat and Harrison proposed to determine the ages of meteorites—a very important thing. There were many design problems. For the time, these were very sophisticated machines.

COHEN: And they were first. I mean, there was no model.

SILVER: No, no commercial models, although there were, if you will, earlier versions which had been built in Chicago and elsewhere. But Chicago had not gotten into the geochemical side of it, except for what Harold Urey was doing. And Harold Urey had a couple of outstanding students, one of whom was Harmon Craig. A very…

COHEN: Outspoken.

SILVER: Yeah, a very special character. How's that? Did I say that well? A very, very bright, very energetic, very provocative loudmouth. But he's a brilliant man. And he and Sam were, in effect, rivals. Harmon worked on water and Sam had worked on carbonates. That closes the loop, because it's carbonates in water, and you have to understand the whole hydrology of water from the atmosphere to the ground—the whole mixing of the oceans. That's what this stuff was going to help us understand, one way or another. Between them, they just pushed the science. And they were the leaders until their students started doing …

Begin Tape 1, Side 2

SILVER: [Continuing] So we now have working mass spectrometers. And we have Sam doing his thing, and Pat doing his thing.

COHEN: And where's Harrison? Is he just organizing things?

SILVER: I'm just about to enter into that. Now, Sam was working on stable isotopes. That's got nothing to do with the fundamental geochemistry of the critical elements, but Harrison could see that it was extraordinarily promising science. So he managed to sell it to the program managers in the San Francisco AEC operations office, which is where our contract came out of. The AEC was paying. The Survey never gave money. The Survey gave people and time.

COHEN: So the AEC is where the money came from.

SILVER: That's where, the SFO [San Francisco operations office]. Pat and Harrison were interested in meteorites and the age of the Earth. And on the one hand, Sam was working with the light elements—primarily carbon and oxygen, but also hydrogen, and, later on, nitrogen and silicon. But Pat was working on uranium and thorium-lead.

COHEN: Those are called the critical elements.

SILVER: Well, lead was the radiogenic daughter of the critical elements. And there are, as you know, several lead isotopes. At the same time, Harrison's research was directly related to the chemistry and mineralogy of meteorites, and he had a couple of research people working with him. He and Pat and another one of Harrison's students, George Tilton—another eventual member of the National Academy of Sciences from that generation who was, at that time, at the Carnegie Institution of Washington in the Geophysical Laboratory—had worked up a study of the distribution of uranium and thorium in granites. And they had done the very first isotopic analyses of common minerals in rocks. There'd been isotopic studies of radioactive minerals, which had been done by A. O. Nier, who was a great physicist and mass-spectroscopist at the University of Minnesota. But these were the first to look at U-Th-Pb in the minerals that ordinary granites were made of.

Now, one reason why Harrison realized he needed a geologist was because when he had started this work, he had written to a very distinguished Harvard professor of petrology, Esper Larson, Jr., and asked him for a sample on which to do this work. Did Professor Larson have a rock in which he had samples of minerals where they could study the minerals in the rock? And Professor Larson did, because he was taking those minerals apart and looking at the geochemistry of the crystals for the US Geological Survey. And then Harrison and Pat and George, having done all this very original, first-time-ever analytical work, decided to write a paper. So they wrote to Professor Larson and asked him about the geological background of the sample. And Larson, who was very straightforward on this, said, I didn't collect that sample. I don't know anything about it. He'd just provided the minerals, the way that Harrison and Pat and George had asked him. He said, I got that sample from Professor Ellsworth at the University of Toronto, and it came from somewhere in Canada. Well, that began to be a struggle. You said I did errands—I went all over a certain part of Canada, trying to locate the source of that sample, because Professor Ellsworth had passed away.

COHEN: So you went out into the field …

SILVER: Out into the field, using whatever clues there were in the notes that had been sent to Dr.

Larson by Dr. Ellsworth.

COHEN: Fun. Hard. Hard.

SILVER: Well, it was fun. It was frustrating. Oh, it was frustrating. And it taught me a lesson that I have rubbed every geochemist's nose in, ever since. If you're going to work in the Earth sciences, you'd better know the context of the materials with which you work. And that was one of my major contributions—to provide the context for all these families of rocks and minerals. And I've had to practice what I preach, because I've been working on samples for forty-three years, since I first went to work.

COHEN: So how long were you up there?

SILVER: I was up there for about ten days. I visited the University of Toronto. I visited the Geological Survey of Canada. I got the best maps they had. I found a note or two, but it ultimately was not quite definitive. It was in a geological province where I subsequently did a great deal of work, in that very special part of Canada called the Halliburton-Bancroft Highlands, which was where Canada was producing some of its uranium and thorium as well.

COHEN: So that's why people were up there to start with. That's why the stuff came.

SILVER: That's why the stuff came. But eventually, with my help, they produced—I wasn't a co-author on the work. I was just—

COHEN: The errand boy.

SILVER: I was the geological errand boy. And they produced a classic paper that has been extremely important to the Earth sciences, but nowhere near as powerful as it could have been. The next time such a study was done, I collected the samples. And I did the study.

COHEN: You had the contacts.

SILVER: I had the contacts. But it was never as important because that was the pioneer paper. They developed chemically clean analytical techniques for attacking a variety of minerals, some of which were extremely chemically resistant. They were the first people outside of the actual people working on the Manhattan Project to do clean chemistry.

COHEN: By clean chemistry, what do you mean?

SILVER: That the products that came out of their chemical extractions were dominated by the characteristics of the minerals and the rocks as they were in nature, and not dominated by background contamination. What they were seeing were, in fact, the intrinsic qualities. They learned how to do microchemistry on lead, microchemistry on uranium and thorium. Pat was the lead man. George Tilton was the uranium and thorium man. But to understand the system, you had to bring both together, and they developed techniques which are now used around the world.

Sam got his first graduate student—a guy by the name of Robert Clayton, who was a chemist in the chemistry division. Not the Clayton who was a physicist here [Donald D. Clayton]. And Sam continued the work on the equilibration of carbonates and water. He worked with Bob Clayton to develop a variety of techniques for extracting oxygen, again without fractionating the sample—without changing the fundamental natural signatures. That was the challenge. A whole series of papers started coming out by Sam and Clayton. Al Engel was a co-author on one. Bob Sharp was a co-author on another. And we quickly established the promise of stable isotopes to tell us not only about temperatures, but how geological processes work chemically. The physical fractionation of the isotopes leaves a signature of the history of the process that is indelible, unless you screw it up. This is extremely important, so Sam's work quickly grabbed on. Sam was made a member of the faculty in 1954, I believe.

COHEN: I think he was here for two years as a postdoc, and then became an associate professor.

SILVER: Yeah. He stepped right in. Sam had come here with a lot of experience. He was older than postdocs would be. He'd gotten his PhD with Professor Thode at McMaster University. And he worked with Rudy Marcus. You know they were classmates together. Rudy Marcus and Jack Halpern were the president and vice-president of the student body.

COHEN: Yes, I know they're all Canadians.

SILVER: They're all Canadians. But Pat in 1953 extracted cleanly the lead from crystals of troilite which is an iron sulfide, in the iron meteorite at Canyon Diablo. He also extracted leads from ordinary basalts.

COHEN: Did people go out and get this stuff by themselves?

SILVER: Some of these things were sent to them. Some of these things I got. They came from several different sources, but I helped Pat with all the preparations. I was the errand boy.

COHEN: But you were learning all the time.

SILVER: I was learning all the time. In 1953, Pat analyzed the lead in the iron sulfide from this iron meteorite which was found in Canyon Diablo, right on the south side of the town of Flagstaff. However, probably the meteorite had nothing to do with forming Canyon Diablo. It probably was a piece that had bounced from somewhere else. However, the lead that he extracted was unlike any lead ever seen on Earth. Leads were probably formed in nucleosynthesis, and then they were constantly modified by the addition of leads which are generated from the decay of uranium to thorium through geologic time. And the sample that Pat analyzed had the least radiogenic lead. The least ratio of lead-206, -207, and -208 to lead-204—non-radiogenic—which are the radiogenic decay products of uranium-238, -235, and thorium-232.

COHEN: But does that make it newer then?

SILVER: No, it makes them more primitive; they were isolated in the earliest history of the solar system. And that lead is central to our understanding the age of the Earth and the solar system. That number and similar samples have been reanalyzed by other people. But Pat's number—

COHEN: They come back to that number.

SILVER: They come back to the same number. If there is a Nobel Prize-winning number in the Earth, that's it. Pat has not been properly recognized. He's by far the most distinguished producer of critical numbers. He and Sam have been our two giants in geochemistry.

COHEN: How do you account for that? Is it because they are quieter?

SILVER: Personal style. People are recognized who go out and sell themselves.

COHEN: That's the way of the world.

SILVER: That's it. Okay, so by 1954, there were people coming here to use the numbers, to talk.

COHEN: So you really had your choice of students, I would imagine.

SILVER: Oh, we were getting students. The excitement was growing. So from the fall of '51 to by 1954, we were established, because we had the men who could build the machines. We built the labs, and we had the people who could do the science.

COHEN: And also the people who knew what they were doing, who recognized what they had got.

SILVER: And behind that, we had a man who had organized, promoted, and defended the projects—Harrison Brown. And Bob Sharp. I mean Bob behind Harrison behind Sam and Pat. But that ultimately led to some problems, which we'll talk about next time.

LEON T. SILVER

SESSION 2

December 20, 1994

Begin Tape 2, Side 1

COHEN: I think we will backtrack a little bit, and I'm going to ask you a little more about your own family life from the time you came here. And then I think we should get on with talking about your own work.

SILVER: Well, I was married when I came here. I was married about a year and a half after I got out of the navy. I was at the University of New Mexico at that time.

COHEN: What year would that have been in?

SILVER: That would have been in 1947. April 1947, and then my wife and I moved here. My wife, Betty Silver, went to work as a secretary.

COHEN: You met her in New Mexico?

SILVER: No, I met her when I was in the navy—in San Francisco. And she was working in San Francisco for *Sunset* magazine group. And when she came here, she looked for a job. She went to work in the old Hydrodynamics Lab, working with, let's see, Robert Knapp, director of the lab at that time, and also working with Vito Vanoni. This is 1948, it would have been. She did a lot of work with Vito, and they were very good friends. That helped us save up a little money and then when our first child came in September of 1951, we were in somewhat better shape. I was renting a house from another old Caltech technician, Ed Hoge, on South Marengo. Don't ask me for the street number. It was a long time ago. That name may not mean much to you, but Ed Hoge was an outstanding photographer. And it was his super photography that was making a lot of the observational work on turbulence and laminar flow in the Hydrodynamics Lab possible. And he was a Caltech alumnus and a close friend of Frank Capra's. That was an interesting connection in its own right—he had done a lot of photography with Frank Capra back in the old days. Well, when our first child came, we were renting a little house behind a house that Ed Hoge owned. And shortly thereafter, I used a VA loan to buy my first house, which was in northwest Altadena. And I was there for four years and then I had a second child.

COHEN: This was a girl?

SILVER: The first one was a boy. That was Stuart. Stuart has passed away now. And the second child was a girl, Victoria. Vicky is now teaching at Columbia and has just come home for Christmas, bringing my only grandson home. So then we moved to a somewhat larger house up on Crestford Drive, because of the need with two small youngsters for a better yard and what have you. And the kids were enrolled in Audubon School. And that was quite interesting because when Frank and Billie Press came here, Billie taught at Audubon. And Billie was, for one year, a teacher of Vicky's—a great, enthusiastic teacher—so it was one of those things which helped us get to know each other when Frank came.

We lived right on the edge of the Arroyo. My backyard looked directly over the Jet Propulsion Laboratory. That was during the McCarthy hearings, when McCarthy was throwing mud, as you know, at a lot of people. There was sort of a general unrest in the country, with fears being built about communists, and all the other stuff. And Caltech was getting a lot of mud. At that time, they were still firing rockets down there in the Arroyo.   And the people who didn't like it were happy to join in on the knocking. And you would read letters to editors calling Caltech "Comtech" back in those days. That was an easy way of saying you didn't like Caltech firing rockets. And we were starting to get into the period of time when Linus's [Pauling's] activities on behalf of a moratorium on surface testing of nuclear weapons, or nuclear devices, was getting a lot of controversy. So it was an interesting time to be there. But it was a lovely place to raise my children, because we used the Arroyo Seco as our backyard. That's a beautiful place. It was great for geologists to have that as a backyard. And my kids went on through Audubon and then Elliott Junior High and then John Muir. Even though we subsequently moved and were in the PHS [Pasadena High School] district, my kids insisted on finishing at John Muir, which had been a great high school. And then they went off on their own collegiate careers, with mixed success.

COHEN: That's how it goes.

SILVER: In the end, it all worked out very well. Stuart went to Grinnell College and got kicked out in 1969, because he'd been involved in those student strikes against the Vietnam War. And they wouldn't give him a chance to get back in.

COHEN: Those were bad years.

SILVER: Those were bad years, and I don't know, maybe he did other things. My daughter says to me, "Papa, you don't know what Stuart did."

COHEN: Well, one doesn't have to know everything.

SILVER: One doesn't have to know everything. But in any event, Stuart came back here, and went to PCC [Pasadena City College]. He picked up his grades and became student body president. He worked a couple of terms and then went on to USC [University of Southern California] and got his degree there. And Vicky was always a good student, so there was never a problem. So that's my personal life.

What did it mean to me and my science to get this full-time job as a grad student? I was catching the first wave in a major new area, not just of Earth science, but of general science. And I was catching it in an environment which was perhaps best epitomized by Harrison Brown. People were open to new ideas. They saw so many possibilities that they couldn't do all the things they wanted to do. Harrison himself was full of ideas, but the doers under Harrison were not anxious to have Harrison tell them what to do. And Harrison learned that. Harrison did a remarkable job of standing back and just creating an environment in which things went forward. He had his own interests—he was particularly interested in meteorites, and what meteorites can tell us about the early history of the solar system. He had some technical people working with him—analytical work. They had a happy program. For me, I had been trained first as a civil engineer and then as a geologist. My geological training was entirely classical, and then I was encouraged to take chemistry while I was here. I did have a minor, an option which really hadn't been created then—geochemistry, which was mostly chemistry courses. I was just completely charged up about learning more. It had always been my understanding that I would provide geological assistance—showing people what to do with samples, how to use a microscope, and going out and doing these long, sometimes frustrating treasure hunts that I have described already. I would do mineral separations, which were very, very critical. And I did this with different people.

COHEN: This was your job?

SILVER: This was my job. That's why I was being paid full-time. But everything I did brought me into contact with other people. I remember taking off a gold wedding ring so that I could show Pat Patterson a sample of the Nuevo Laredo meteorite, which turned out to be a key sample because it was the first meteorite which gave radiometric ages—lead-isotopic data was coupled with the isotopic data from the Canyon Diablo meteorite I mentioned earlier. I never did find that wedding ring again. But Pat and I remembered how exciting it was to take that thing apart. It was a remarkably fresh, unaltered basaltic achondrite. Turns out to be almost an end member chemical and geologic differentiate of its kind. It was, in terms of what we knew about meteorites at that time. I had a wedding ring on my finger. But you understand that when you process materials and you want to see their original indigenous properties, you must add nothing to the sample. I had to take the ring off, even though I was using gloves. I set it down in the lab, and I completely forgot about it until days later. And then I went looking for it, but I never found it. It probably got thrown into the trash.

We had to make up the rules for preparing critical samples, with very low levels of lead, uranium, and thorium, in which we could use the micro-analytical techniques developed by Patterson to get some of the key numbers that we wanted. And there were simultaneously programs going with Sam Epstein. Sam got his first student, a man by the name of Robert Clayton, who's now a distinguished professor at Chicago. And for what Rob had to do, he needed help. He knew nothing about rocks and minerals. He did some collaborative work with Al Engel, who was a professor at the time, but I was doing follow-up sample preparation work for him. And after I was appointed to the faculty, I still had to finish my degree, which was something which was handled with a lot of support and understanding. When I wasn't immediately needed, I would sneak down to the subbasement, to a little cubbyhole I had down there, and write my thesis. And it was a very long thesis. It was sponsored, as I mentioned, by the US Geological Survey, but it gave me an opportunity to continue to grow as a geologist.

COHEN: So your thesis work was just straight, traditional geology.

SILVER: Yeah, except I never saw things the same. Every year I learned so much that my perspectives were changing continuously. And ultimately I drew on the combination of my new, much broader background, which was bringing me in contact with some of the most interesting people in the Earth sciences on a global basis.

COHEN: Well, you certainly had a unique student time.

SILVER: Oh, I did. Absolutely unique! It was so enriched, in part because Harrison was bringing the finest minds in the business to Caltech to give lectures. He organized a course, called Ge 150, with a different distinguished speaker every week. We had Harold Urey. We had Fritz Laves from the ETH, Zurich [Eidgenössische Technische Hochschule]. We just had so many distinguished people come through, and it was just rich. Whether I was taking the courses or auditing the courses—sitting in on the lectures—the place was incredible. It was a very rich environment for me. So I was simultaneously doing classical geological work and learning about all these technologies. And I was being helped by the three postdocs Harrison had brought, two of whom became members of the faculty. Sam was the first guy to become a member of the faculty, and then ultimately Patterson became a member of the faculty. The third guy, Charles McKinney, was the man who built the mass spectrometers, and solved so many of the technical problems. He was the engineer and physicist who could build mass spectrometers, and clean labs, and do all the other things. Of my colleagues, he got the least recognition. Harrison was a professor. Sam became a professor. Pat did this wonderful work. Chuck was the doer. Chuck hired the key people—the machinists, the electronics people, and the others—and ran the thing. We had mass spectrometers up and running so fast. While everyone else was still talking about getting into the business, we were cranking out important numbers. We could get to the problem. If the Keck [Telescope] is the new eye to the universe, then these mass spectrometers were the tools for doing isotope geochemistry.

COHEN: First with the best.

SILVER: We were first with the best, and I can't overstate how much I think of McKinney's ability to make things work.

COHEN: What ultimately happened to him?

SILVER: McKinney did not get into a paper-producing [i.e., writing] mode. He was a support man. And as a support man, he became increasingly frustrated. And he ultimately went to work for a major electronics firm over in Monrovia.

COHEN: So he left Caltech.

SILVER: He left. I think we failed to help him make the transition, but maybe, considering what he subsequently did, it might have been the best thing for him. I know it was an agonizing time and decision, for both Harrison and Bob Sharp. But nevertheless, every one of these guys, and Harrison, were my mentors.

The first paper I ever co-authored was in 1953. It was presented at the Geological Society of America's annual meeting, which was held in Toronto, and it was the initial thrust of our AEC contract—extracting energy from ordinary granite. We had been studying the abundance of uranium and thorium in ordinary granites, and with Harrison's knowledge of breeder reactors and everything, and with my information on what the concentrations of uranium and thorium were, we had a paper that talked about providing an endless resource. Harrison and I figured that we could, in fact, develop a sustainable, energy-profitable system based on the uranium and thorium contained in granite. It was a very good idea, but it did not foresee all the concerns about how one uses these reactor processes, and what you do with the waste products and all. But it was interesting, because it bears on the future of the research I was going to do. I was learning about the mineral sites for—and the non-mineral sites for—uranium and thorium in granites.

COHEN: So you were actually out in the field?

SILVER: Well, yes and no. I was finding the granites, but I was taking granites apart in the lab. I was crushing them and separating the radioactive minerals, and learning that uranium and thorium are distributed in ways that people never anticipated. I also learned that individual mineral species—a mineral species is a particular natural compound—in granites and in the volcanic equivalents of granites, were remarkably heterogeneous, even within a small specimen. And understanding that heterogeneity which I approached systematically, helped me develop the technique which is now very widely used in geochronology—that is, the science of determining the ages of ancient rocks. I focused particularly on the distribution of radioactivity in zircon, and I found many interesting things I started talking to Pat about. He was doing some isotopic analyses, because I wasn't in the isotope-analysis business at the time, and he was chasing the wonderful world of lead in meteorites and in major rocks of the Earth and the oceans.

COHEN: He was not to be diverted, I would imagine.

SILVER: What he said was, Lee, why don't you analyze this? Come into the lab and I'll show you how. Actually, he didn't show me how. He had a marvelous, beautiful young woman by the name of Judy Kawafuchi who was his technician. And the top technicians showed the newcomers what to do. So that's where I started to learn, and I started getting into the business. And the initial results were so interesting that Harrison encouraged me to do a postdoctoral project on them—to get grad students right away. My first grad students were all in classical geology, because I was teaching a number of the classical geology courses.

COHEN: So you actually didn't go away between getting your degree and doing your postdoc.

SILVER: No, but from the time I was a grad student until after I got an appointment on the faculty here, I still was working every summer for the US Geological Survey. And when it came time, when I was finishing my degree—you remember, I'd done well on my exams—the Survey wanted me to work for them. They wanted me to bring some of this stuff to them. I had to make a decision, and I made the right decision. But the Survey let me retain an appointment—what's called a WAE [When Actually Employed] status—so when the Survey needed me and I wanted to work with them, I could work with them. They gave me status comparable to what other PhDs got.

COHEN: Where was the Survey?

SILVER: The Survey wasn't here. The Survey had its offices in Washington at that time. They subsequently built major offices in Denver, and in Menlo Park, and subsidiary offices in various places around the country. Basically, while I was doing work for the Survey in Arizona and Colorado, I was working for people who eventually became Acting Chief Geologist or Chief Geologist of the USGS. And they all knew my work and they liked me.

COHEN: So you had a foot in both camps.

SILVER: I had a foot in both camps. I've always had a foot in both camps. You'll hear more about that when we talk about Apollo and other things. So my education continued. I built contacts with the Survey—I mentioned Ralph Cannon earlier. I should mention a man by the name of George Neuerburg, who was sent here by the Survey to be my opposite number, so that there would be some feedback to the Survey. George was a UCLA PhD and had grown up in North Hollywood, so coming back here was good with him. He and I did a number of things together. My first postdoc came in about 1957. His name was Marc Grunenfelder. He had a PhD from the ETH in Zurich, and he worked with me on the business of taking granites apart. I got a student field assistant shortly after that, by the name of Henry P. Schwarcz. He was my first PhD. He was jointly supervised by myself and Professor A. E. J. Engel.

COHEN: So you actually had postdocs before you had students.

SILVER: Yes. That's because the research was moving—when the word came out about Patterson's numbers, it shook the world. You have to appreciate how much our understanding of the age of the Earth changed when Pat's numbers came out. We had some of the most distinguished astrophysicists and physicists in Europe come to visit us.

COHEN: This was in the middle fifties?

SILVER: This was in the middle fifties. Patterson's first publication on those numbers was in '53, and he kept adding important numbers in '54 and '55. It was a sensitive time. Harrison had tilled the field. He built the clean labs so Patterson could do just exactly the things he did. Harrison didn't do the analytical work himself. Patterson, being a rugged individualist, did it his way, but he did it right. We can never overestimate Harrison's contribution, because this was the prime reason for building a solid-source thermal-ionization mass spectrometer. The prime reason for developing Pat's clean labs was to understand isotopes in meteorites. And then these numbers began to fall out. I told you about the Canyon Diablo lead. That number just opened everything up. So the ferment in this environment was fantastic.

COHEN: Was Gerry Wasserburg involved in this?

SILVER: Gerry Wasserburg was appointed to the faculty in 1955—the same year I was appointed. The same year that Clarence Allen was appointed. The same year that Frank Press was appointed.

COHEN: Sounds like a vintage year.

SILVER: Guess who did the appointing? Bob Sharp. Gerry came from the University of Chicago, where he'd done his PhD thesis with Harold Clayton Urey and Mark G. Inghram. Let me speak frankly, because it is for the record. Gerry had difficulty with our entire group when he first came here. He's had difficulty with us ever since. And the origins of that are human—they reflect both Gerry's personality and personalities of other people. I was astonished when, in the late '50s, I began to see that Gerry was very unhappy. We had made all the equipment here available to him, but he didn't like working in the shared mode, where we had to schedule machines like you schedule telescopes. He wanted his own clean room. He was a very ambitious man. He was also very disrespectful of my colleagues, and of me. And that too has persisted over the years. Here's a guy with great intelligence, great charm, fine singing voice. He had a setup where he could do wonderful things. And somehow, in my view, he's not been happy.

COHEN: He's ambitious. He's driven.

SILVER: He's driven, but he's no more driven than Clair Patterson was, or Sam Epstein was, or Lee Silver was. And we didn't wear boots with hobnails. He really disliked Harrison Brown. Now I hope you get his version of this. But this tension appeared within a year or two. Everybody recognized how talented Gerry was, and everybody was willing to accommodate—first of all by sharing, and then by letting him go his own way. But the problems he provided to every division chair, and to some of us as individuals, were so profound that they left just an amazing imprint.

COHEN: But everybody stayed.

SILVER: Oh yes, because Gerry didn't dominate our department. We were all doing our own research. Gerry thought he was a geologist and a chemist and a physicist, and, to some degree, he was each of those things. But each of us, in our own fields, were also competent and capable of doing the work. I don't think Gerry ever realized that. Over and over again, he bruised and hurt, and in some cases, very seriously damaged, the careers of a lot of young people. And the amazing thing is, we all respected his science. We didn't respect his judgment. We didn't share quite the same values. But we were willing to put up with it to a degree. I'll tell you, there were times when I disliked that man so much.

COHEN: And here you all are.

SILVER: Here we all are. We're still here. Gerry has had a distinguished career. We invited him to become the chairman. He couldn't let his desire to dominate go. He had to tell us how it should be. He had to tell the whole faculty we didn't know what was best. In the end, he was doing a number of things which were just like the things he had done when he was much younger. He was starting to destroy young people again. And it isn't generally known, but the faculty just totally rebelled. So let's say Gerry has his own internal devils. There is one slightly humorous aspect: Gerry, Frank, Clarence and I all had birthdays within about six months of each other. We were all the same age. And then, I guess about twenty years ago, Gerry suddenly announced that he was two years younger than us. I believe Gerry had volunteered to go into the army in World War II, and I think he was underage. He lied about his age, and that carried over in all of his documents. He saw real military service in Europe, but he had to live with the documents, so finally he undeclared them. Now he has a very good friend, who's also a very good friend of mine—George Wetherill, a distinguished physicist and an expert on asteroids, and cometary orbits, and things like that. George was the director of the Department of Terrestrial Magnetism [at the Carnegie Institution of Washington] for a while. They had been officemates at the University of Chicago, and they had graduated together. George happens to be the same age as the rest of the group. And it was George who announced to me, because he was the first to learn, that Gerry had renounced his age.

COHEN: So how old is he?

SILVER: Well, I think he is the age he likes to be, which is sixty-seven or something like that. Which is why he's not coming up for retirement as fast as some of the rest of us.

COHEN: Okay, so we've got the department up and running. You've done this first big piece of work. You've set the record straight on how you feel about Patterson.

SILVER: I love all of my colleagues. I even have a warm spot for Wasserburg when he doesn't act like a pompous ass. I wish him well.

But let me come back to my own work. Under the tutelage of this woman, Judy Kawafuchi, I started analyzing minerals. I learned how to run a mass spectrometer from Chuck McKinney. I learned how to deal with something that Patterson worked with only once, in a landmark paper. Then I took it from there. I started working with what I'll call radiogenic lead systems. Radiogenic leads are leads which have been significantly modified from their initial isotopic composition by the in-situ decay of uranium and thorium. And I began to learn the natural behavior of radioactive minerals—radiogenic leads and their relationship to uranium and thorium. One of the important things that we learned from an initial study by George Tilton, whom I mentioned earlier, Patterson, Brown, and Esper Larson Jr., was that different minerals in the same rocks gave different ages. They were sufficiently different that nobody in the world of chemists and physicists could understand why. And when new labs were being built, both at the Department of Terrestrial Magnetism and the Geophysical Laboratory—both run by the

Carnegie Institution of Washington—they tended to steer away from uranium- and thorium-lead systems, even though they had George Tilton, a superb analyst. George had accumulated data, and the ages were so screwy, that he came out with a theory of something called "continuous diffusion"—loss of lead. That is, the radiogenic lead just didn't want to stay. Actually, he didn't originate that concept. It was originated by a chap by the name of Louis Nicholayson, a South African who had been a visitor at the Department of Terrestrial Magnetism for a while. But I began to note that the mobility of lead, particularly in zircon, was related to the observed concentrations of uranium and thorium, and, plotted on the right kind of diagram, produced remarkably linear arrays. These linear arrays could make use of a hypothetical relation for daughter-parent isotope systems—lead-206 and lead-207, uranium-238 and uranium-235—which George Wetherill had calculated in such a way as to give an apparent age that was very precise despite chemically disturbed systems. And about that time, I had my second postdoc, who was a wonderful woman by the name of Sarah Deutsch Schnek from the University of Brussels. She was not only a fine scientist, but a great friend. So she and I did a systematic study, which we reported for the first time in 1960, and we showed there was order where everybody else thought there was chaos. That was probably the most influential thing in my career. It was a landmark paper. We actually put out a series of papers. Sarah returned to Brussels. I continued to work on this. I got another postdoc, Robert Pidgeon, from the Australian National University in Canberra. And we continued to find this marvelous order. I was working day and night—I wasn't yet a tenured professor. Part of my problem was that I was competing with Gerry [Wasserburg], because what I was doing in geochronology was giving far better and more consistent results than what Gerry was doing in geochronology. That's not because of my superior intellect or anything else—it's that I'd hit the right sample. It's ironic, but the rock we used came from my thesis area in southeastern Arizona. What I had in my thesis area was a geological setting where I could establish the relative ages of things. Now a measured isotope ratio can be calculated into an age, if everything is favorable, but you don't know what it means. It's an apparent age. So you have to go to a place where you can establish a sequence of events. For example, you can model the evolution of stars. Then you start using other parameters, like redshift, in order to get some idea of whether your model fits the sequence. That's what I did. And that's what caught the attention of geologists, because the geologists could see that it could fit. But I've always used the term "apparent age" because it's a consistent, but not necessarily absolute, age. The term “absolute age” is widely used, but we don’t have absolute ages. We have ages derived from models based on certain assumptions, and testing those assumptions was one of the things I did for many years.

There were a whole series of interesting times. I remember rooming with Gerry at a conference organized by the New York Academy of Sciences. Gerry was kind of despondent, because he had to give a key paper the next day. We talked all night, and he gave a very good paper—but it wasn’t just his paper. It was built around the order we had found, and I was reporting in the same conference. And you won’t hear that from anyone else.

LEON T. SILVER

SESSION 3

January 9, 1995

Begin Tape 3, Side 1

COHEN: Let's talk about the different ways in which one went about planetary science here, and how that started.

SILVER: Well, planetary science here really started with Harrison Brown. Harrison was deeply involved in trying to understand the limited samples that we had from bodies outside the terrestrial system. He'd long been interested in meteorites. He'd done work on this at the University of Chicago, including some work with Clair Patterson and some work with other grad students. And, as I mentioned earlier, one of the main objectives in building the first clean labs for lead microchemistry was, in fact, to explore these isotope systems in meteorites from the chronological point of view. He brought Patterson as a postdoc. Patterson's work is *the* reference work for the age of the Earth.

COHEN: Did Harrison Brown work at all with Clair Patterson in Chicago?

SILVER: Oh, yes. He was his dissertation supervisor. Clair had already done work with him and knew him. And so Harrison wanted to bring Clair with him. So there had been a history of studying meteorites, and Harrison had produced a number of students. And he had a couple of staff people—Walter Nichaporuk; [Eleanor] "Glo" Helin, who is now famous for her subsequent work in observing comets and asteroids.

COHEN: And naming them.

SILVER: And naming them, yeah. Well, some things get a little cutesy, but that's all right. She's a serious scientist doing serious work. But in the course of time, I too became interested in meteorites, in part because Harrison put me to work trying to find the sources of certain different classes of meteorites. When Pat started analyzing meteorites, he used meteorites which were what we call "finds"—that is, they were found at some unknown time interval after they had fallen. And because there usually is a significant lapse of time, the meteorites were of course subjected to terrestrial weathering, and possible terrestrial contamination. At one point, though, Harrison had been given a very spectacular little meteorite called a Nuevo Laredo meteorite. It came from near the town of Nuevo Laredo, in the state of Nuevo Laredo, Mexico, just across the border from Laredo, Texas. It was an interesting meteorite, because it was by far the freshest meteorite that I had ever seen. I suggested to Pat that this was one meteorite that he ought to analyze. He had already found this marvelous number in the iron sulfide phase, troilite in the Canyon Diablo meteorite. And that was where I lost my wedding ring. I took my wedding ring off because we did not want to contaminate the meteorite when we broke it up. It wasn't a very big meteorite. It weighed less than a pound—a few hundred grams. But from that, Patterson got something he had not succeeded in getting from any other meteor—a sample which yielded comparatively very radiogenic leads. Those radiogenic leads represented the long-term accumulation product of the decay of uranium and thorium to isotopes of lead. And when you used the lead-207 to lead-206 ratio that Patterson measured on that sample, you got the same age that Patterson had gotten when he had applied the Canyon Diablo meteorite lead to modern, or contemporary, terrestrial leads—this 4.5-billion-year age. And so here we took an initial lead from one meteorite, and a radiogenic lead from another meteorite, put them together, and got exactly the same number that we had gotten by putting the initial lead from that meteorite against terrestrial leads. And so this independent indication of an age of 4.5 billion years, which has subsequently been confirmed in many different ways with different isotope systems, really backed up the relationship that these meteorites, wherever they came from, had a common point of origin in time. The Canyon Diablo was an iron meteorite, representing presumably the core of a differentiated planetary body which had been broken up somewhere in space sometime. This other rock, Nuevo Laredo, was a basalt equivalent to the terrestrial rocks which make up the oceanic plates and the island of Hawaii, which are very new and which are coming from the deep interior, the mantle, of the Earth. This meteorite had developed from the interior of some unknown planetary body, which had subsequently been fragmented, and a small fragment of which had come to us. They had started evolving at the same time, and they had fragmented very close to that time. That was another inference you could draw from these things. And they had been floating around in the best, cleanest, most secure sample containers one could have;

namely, in the vacuum of space. Patterson did a basalt from the Columbia plateau, and a sample from Hawaii, and he applied the same initial lead to those, and the numbers kept coming out, 4.5 plus; 4.5 plus; 4.5 plus. So that was where we really got into planetary science. But as time went on, we came up with an interesting organizational problem.

COHEN: What year are you talking about?

SILVER: I'm talking about 1957 through 1962. I was a young man on the faculty. I was going to very important meetings—one thing Bob Sharp did was incorporate the young faculty into discussions. The problem was, Harrison had been appointed chair of a National Academy of Sciences committee to look at the future of ocean sciences, and he turned out a singularly important report. He also said to the division, should we be going into oceanographic sciences? One man, who was deeply interested in doing so because of his own background in marine geophysics, was Frank Press. So Frank pushed this, and we all had to consider it. There was Scripps Institute down the coast. Caltech had a little tiny marine biological station near Corona del Mar. USC had a marine station out on Catalina [Island]. Up the coast, a number of places were doing marine work, and, of course, we had the great institutions on the East Coast: Lamont, Columbia University, Woods Hole, and a number of other schools as well. At that very same time, Harrison brought to Caltech a postdoc whose name was Bruce Murray. We had never used the term "planetary science" before Bruce came, but Bruce was a postdoc in planetary science.

COHEN: Where did the name come from?

SILVER: Well, it was probably coined by Harrison, or it might have been Bob. I don't know, but it became current with Bruce and the work he was doing with his students, which was remote sensing of planetary surfaces using infrared spectroscopy. And they discovered so many, many interesting things. It was very important work. To my mind, it's the most important science Bruce has done.

So we started expanding the grad student program, and then slowly we began to add other people. But there had to be a decision made first: The President of the United States had announced that we were going to the moon; JPL was starting its unmanned exploration of the solar system. So what was better for us, to go into the marine sciences or to go into the planetary sciences?

COHEN: Now the answer seems obvious, but it probably wasn't then.

SILVER: It wasn't at all obvious. To my mind, that decision ultimately led to Frank Press's going to MIT, where he was nominally chairman of the Department of Earth Sciences, but he also had Woods Hole under him. But that's the way it goes. You make a decision and they went off. And so we got into planetary science.

COHEN: And in retrospect, that was a good one.

SILVER: It was a great move. It was a great move because we had students and faculty who were interested in pursuing work in that field. I had a PhD student by the name of Michael Duke, who is now a distinguished member of the planetary exploration group down at Johnson Space Center, who I put on the study of a special class of meteorites called basaltic achondrites. And he did a superb thesis that stimulated Sam Epstein and Hugh Taylor and myself to work with him to study the oxygen-isotope composition of meteorites. We did a lot of important initial work on stable isotopes and meteorites, and we were able to establish that there was a commonality between those meteorites and the Earth. This was very important, because that commonality was going to be tested when we got to the moon.

COHEN: Already plans were being made to go to the moon?

SILVER: Oh yeah, plans were being made. And about that time, as the planetary science activities grew, Bob Sharp said to us, we're going to have to expand our planetary sciences. We're going to have to have a new building. So Caltech went to NASA [National Aeronautics and Space Administration], and Jim Webb gave a million dollars for the construction of another building. James Webb was the NASA administrator who, until the moment just before the moon landing in 1969, had been the man who pulled the US manned space effort together. He was the head of NASA for a number of years, and he was a great confidant of Kennedy's and Lyndon Johnson's. Well, we had to get the rest of the money. And, close to the same time, there was a gift to Caltech by Dr. Seeley G. Mudd of a large block of stock in the Cyprus Mining Company. Seeley told Caltech to hold on to it for a while, for the purposes of building Millikan Library. By the time they sold the stock, they had far more money than they needed for the library. That extra money was used to augment the NASA grant to start building what is now South Mudd. But South Mudd is really South S. Mudd—for Seeley G. Mudd—and North Mudd is North S. Mudd. But they're two different Seeleys—the original Seeley W. Mudd who built Cyprus Mines, and Seeley G. Mudd, who gave the block of stock.

COHEN: I see. But the money all came from the same place?

SILVER: It came from the same place. Principally it came out of the ground of an island called Cyprus, where the ancient Romans mined copper. The elder Seeley Mudd went back and said, if the Romans could do it at a profit then, why can't we do it now? But that's another story.

Anyway, so the plans were made for that building. And Bob said to us, you guys had better plan on preparing proposals to NASA to do some research on samples returned from the moon. And I drafted what I think was actually the first proposal—a collective proposal that involved Sam and Pat and Gerry Wasserburg and others.

But I was still carrying on several other major programs. Among other things, we built some new labs. Pat and I were going to do the uranium-thorium-lead studies together, because I was now doing this kind of work, too, and we built a small but elegant lab on the third floor of North Mudd. We were still about a year away from receiving the samples when Pat said to me, Lee, I don't really want to do this work. I'm really more interested in lead in the global environment. I said, well, if that's what you want to do, okay. And he said, I need our new lab to do it.

COHEN: Sneaky.

SILVER: He wasn't sneaky. Patterson is up front. Of course, this was his last major stage of work on lead. But what he did with it was more than worth the lab—it was outstanding work. But I suddenly found myself with a contract to do lunar-sample analysis, and no lab. The new building, South Mudd, was going up at about the same time, and Bob and I talked about opening a new lab for that work in there. And I said, Bob, it'll never be ready in time for the first samples. And boy, was I ever right! So we did as much of a refurbishment as we could on the original clean labs in which Patterson had done the extractions from meteorites, while we planned and built the new lab. That construction job cost the Institute immensely. The general contractor went bankrupt. The architect could not live up to his guarantees.

COHEN: That's the whole building?

SILVER: That was the whole building, and a very significant part of that included my clean labs which were down in the subbasement. So my clean labs were not available until three years after the first lunar sample came in. And they were not built in the best possible way. The materials weren't up to specs. There were many such things—nothing but headaches for Caltech. I think there still may be lawsuits going on.

Bob had an ulterior motive in building South Mudd. He not only wanted the building for planetary sciences, he wanted to bring the seismo lab back to campus. And there have continued to be differences of opinion about whether that was good or bad, but I think it was outstanding.

COHEN: Now, at this time, was Gerry Wasserburg also building his lab?

SILVER: Yes, he was. He insisted he did not want to use our mass spectrometers. He did not want to use our labs. Gerry had done some very fine work on rubidium-strontium systems in meteorites with Dimitri Papanastassiou as a postdoc. It was Dimitri's PhD thesis that got Gerry off the ground. As far as I'm concerned, until that work was done, there was nothing unusual about Gerry's work. Dimitri did spectacular work, using the same class of meteorites, basaltic chondrites, that we had discovered were so profitable for uranium, thorium, and lead. And he got 4.5 billion years again, over and over again, in a totally different isotopic system. So that was very happy and from then on, Gerry became an increasing force in the analysis of meteorites. He did very elegant work with the aid of Dimitri, Jack Huneke and Fouad Tera.

COHEN: But Dimitri is still here.

SILVER: Yes. With a large number of other people he brought in.

COHEN: Meanwhile, your lab was progressing.

SILVER: Yeah, my lab was progressing, but slowly. We got samples in early November, 1969, and we were told that we must report analytical results at a global conference—the first lunar-science conference—to be held about the first week in January, 1970. So there was this immense conference in Houston where we all reported our results. And Caltech came out looking golden in that particular effort, in part because the then-chairman probably knew as much or more about the processes going on on the moon than anyone else. And that chairman was Eugene Shoemaker. So we had papers from Shoemaker, Wasserburg, Taylor and Epstein, and Silver. We were in there big time, all doing different things. We dominated the scene, both in terms of actual research and in terms of personalities. Gerry played an extremely important role in the design of the lunar receiving lab, which was going to accept this unknown material from another planetary body to Earth. And it was a matter of great concern that everybody might catch the lunar itch or something. So biological security had to be maintained, and the physical security of the sample, and Gerry was deeply involved. He made many important contributions to that.

COHEN: It turned out to be mostly nonsense, didn't it?

SILVER: It turned out not to be necessary. There was no trace of biological activity, but it wasn't nonsense. And if we ever get to Mars, we're going to go through the same damned thing again. We'd better, because some things are irreversible, okay? So we'd better watch it!

I was involved in a different way. I was a lunar-sample investigator. I'll briefly say that I demonstrated that in the lunar vacuum, some metals—lead and other things—were volatile, or moving around. This had great bearing on how you would interpret the isotopic systems and the chemical variations in the lunar soils. That was my major contribution to that study—I could go on, but I won't.

Apollo 11 landed on July 20th and another mission, Apollo 12, was scheduled to go in October. They were going at four-month intervals. Despite the absolutely incredibly good job that Neil Armstrong had done, now we had to start thinking about what kind of science we wanted to do, rather than just analyze the rocks that they happened to bring back. The first samples were treated the way we treated meteorites. We knew nothing about their context, we just had this rock. We knew it came from out there, but we didn't know from where out there, or how long it had been out there, or anything. So we just started looking at the sample, trying to read backwards.

But Gene Shoemaker had established what was called the astrogeology branch at the US Geological Survey at Flagstaff, Arizona, and had been studying the surface of the moon for some years. In fact, Harrison "Jack" Schmitt had gone to work there. And it was from there that Jack Schmitt and a couple of our former students, three of our former students, tried to become astronauts. Only Jack succeeded, and just barely. Not because he wasn't completely qualified, but because of the way in which the selection process is worked. I didn't know anything about all that then, but in August, I received a call from Jack Schmitt. He said he'd been talking to Gene Shoemaker, and he asked if I would be interesting in training the Apollo 13 crew for their mission.

COHEN: And you knew Jack Schmitt from when he was a student?

SILVER: I had met Jack when he was about eleven years old. Remember, I came from New Mexico. I was a grad student field assistant working for a professor of geology—V. C. Kelley, a Caltech alumnus—at the University of New Mexico. One day we visited the home of the most famous mining geologist in the southwest—Harrison Schmitt, who was Jack's dad, which is why Jack is in the geology business. Jack's dad lived in this little mining town called Solar City, down in southern New Mexico. He was extremely well read, and a very powerful individual. My professor—Vince Kelly, the guy who sent me here to Caltech—was talking to Harrison, and this young boy came in. It was Jack. That was when I first met Jack. I think Vince Kelly also called Jack's dad's attention to Caltech, although I think he probably already knew about Caltech. So Jack decided to come here, and I met him again when he was a sophomore. He was an undergraduate here. He was in the geology program here. I had him in the field, and I had him in several classes, and I had followed his career. I was very interested in what he did for his PhD thesis at Harvard. He'd come by and we'd talk about it. So I got to know Jack really well. So Jack called and asked if I would be interested in working with the Apollo 13 crew. But Jim Lovell, who was the commander, was not certain that this would pay off. The crews had such immense burdens in preparing for missions—so much training to do. So I proposed to Jack that if these guys would take a week, or eight days or so, we'd go out in the desert and we'd train each other. I want to stress that—I was always being trained by the crews as much as they were training me, because I didn't know the constraints they were operating under. And anything I proposed had to be, obviously, in the context of what drove the whole mission. We had a group that included Commander Jim Lovell, pilot Fred Haise, a backup crew, John Young and Charlie Duke, a NASA observer, and Jack Schmitt. And I had a very good postdoc, Tom Anderson, as my assistant.

COHEN: Where did you go to?

SILVER: To a mountain range called the Orocopia Mountains, which is southeast of Indio, in the low desert. It's not a very high mountain range, but it's low, rugged, magnificent, beautiful—but drier than hell. And this was in mid-September, when it was still very hot down there. With Tom's help, I worked out the logistics. We took them down in one vehicle. There was a second vehicle that the NASA people brought down. I woke them up in the morning. I cooked their breakfasts. I took them out and exercised them all day. Then we got back at night and I cooked dinner, and I debriefed them on what they'd done all day. It wasn't random. There was a method. I took them to a number of different situations. The important thing was to teach them that they were, in fact, outstanding observers. They could see, interpret, make judgments, and use this to guide them in being the most efficient implementers of whatever sampling and photographs they were supposed to take.

COHEN: You were really teaching them to look at things in particular ways.

SILVER: Yes. Anyway, we had a mid-week break one day and went into Palm Springs for baths and things like that. We were kind of grungy.

COHEN: I can imagine.

SILVER: And when they finished their tour, they were enthusiastic, and they asked me to continue working with them. Now the US Geological Survey had been given the contract to run this lunar-surface experiment, so the Survey asked me if I'd develop the training program in conjunction with the experiment. Remember, I had worked for the USGS before, so all of a sudden I had the biggest single promotion the Survey's ever given me. I'd been a junior geologist, and they promoted me to their top rank. They paid me almost nothing, but that was because I never asked for pay.

COHEN: You still had your full-time job.

SILVER: Oh, I was still doing all my teaching. I was still doing the sample investigations, still running my other stuff. We didn't have a lot of time before Apollo 13 was to fly. At first they were going to fly four months after Apollo 12. Apollo 12 was a magnificent execution of a safe landing, exactly where they wanted to be. They wanted to land where Surveyor 3, an unmanned probe, had landed, and they landed about fifty meters from it. It was just absolutely incredible. It showed that the flight controllers, the flight director, and the pilots could do the job. But Apollo 13 was going to go to a very different place. The first two places [Apollo 11 and 12] were relatively low relief. These guys were going into much more rugged country, and we had to prepare for that. The photography [of the landing site] wasn't that good. Anyway, we did the best we could. We trained in the Verde River valley, and we set up mock impact-crater fields. We built a moonscape with explosives and things. We put up an elevated stand at the height that the crew would be when they landed in the landing module, to show them what they could see, and what they could do with their EVA [Extra-Vehicular Activity] planning. And we exercised them. It worked well. We were a happy crew.

So I was invited to my first launch on Apollo 13. And their launch went off magnificently. And then I headed for Houston, where I was going to monitor what they were doing. And when I got to Houston, I heard those famous words from Jim Lovell: "Houston, we have a problem here." That was the explosion that blew the side out of their service module. And then NASA went through this incredible effort to bring them back—the whole world was focused on it. They were totally disabled. They had to use their landing module as their lifeboat. They went around the moon, and in the time they went around the moon, all kinds of remedial solutions had to be found to keep them alive and bring them back. They had to make very critical adjustments to bring them back to Earth so that they could enter the atmosphere at exactly the right angle of inclination. They spent much of their time in the landing module, which had all of its life-support systems still going, but it was not intended to be used for such a long period of time, or by three men. And then they finally got back into the command module, which had the entry device. And they used the command module to come back into the Earth's orbit. There's a brand new book on it by Commander Jim Lovell. I just got a copy of it about two months ago, and Jim wrote that, "I'm sorry we never got to use your training."

But the idea was so good that Jack Schmitt recommended that they start something in parallel for the other crews. So he asked Richard Henry Jahns—Dick Jahns, who used to be a member of our faculty, but went to Penn State and then to Stanford as Dean of the College of Earth Sciences—if he would take on training the [Apollo] 14 crew. That was the crew with Al Shepard. And because of my 13 experience, I was brought into the science backroom to work on the 14 mission. Then, while Dick was working on 14, my 13 crew got wiped out. But the Apollo 13 commander, when he came back, recommended to the Apollo 15 commander—Colonel David Scott—that he try me as a trainer. And I took him and Jim Irwin out to the same place in the desert, and we did the same thing. They liked it.

Now, for the first time, we had time enough to do real training—we had eighteen months because the spacing between missions was being extended. Not only was I training them on observation and geology and other things, but other people—Farouk El-Baz, J. Head—were training the command-module pilot. But we were also training the team of scientists in the back room who were coming into the field with us. We were simulating more and more of the flight communications. Apollo 15, for the first time, had the lunar rover, which meant that they could travel. So we had a series of missions which got more and more sophisticated—more and more similar to the way we might operate with a science back room.

But there was a lot of nervousness about this. For example, Harold Urey was not keen in having geologists train the astronauts. He said he didn't want any geological biases developed in these men. He didn't want a big bias in sample collection. He wanted completely objective samples. Well, Harold had never been an observer. He did not know that in the field, or even sitting in this room and trying to tell you what's in this room, that I'm overwhelmed with all the things I can see. I have to be selective about what observations I would make. And anybody who goes to a brand new world is going to be so overwhelmed that no matter what you want, a certain bias is going to be introduced. So it's better that it be an informed and honest bias. Now the samples Neil Armstrong picked up, which were so valuable, even they included a certain bias. He had noticed as he jiggled into his final landing, with about anywhere from 10-20 seconds worth of fuel left, that he'd passed over a crater with a lot of rocks in it. He kept the landing module dancing around because all he could see was rocks, and he had to put that landing module down where there weren't any. But he decided that the crater was the best place to get good rock samples.

COHEN: Because there they were.

SILVER: There they were. So he broke one of the protocols. He departed from the field of view of the television camera and ran to the rim and filled one of the sample boxes. He shoveled in the fines up to the top of it, and gave us the most incredibly good collection he could possibly get in a few minutes. So we had to understand what the crater bias might have been. We didn't, at first—it was only subsequently that we understood that cratering bias. Anyway, these people had to be taught about all these things. I was the mouthpiece and, to some extent, I was the organizer and coordinator, and I worked very well with the Survey people. In the meantime, I was being taught by people like Gene Shoemaker about lunar mapping and lunar cratering. And I was being taught by each crew commander.

COHEN: It must have taken more than twenty-four hours a day to do all that.

SILVER: It did. I started in September '69—by '74, I was divorced. There were many other personal complications that go on all the way through this. But I had wonderful interactions with most of the NASA personnel. Some of my former students were now involved—Mike Duke, Tom McGetchin, and my fellow grad student here, Bill Muehlberger, who carried on the training after I backed out. In the latter part, the strain was getting to me. But intellectually, it was incredible fun. December '72 was the Apollo 17 mission [the last Apollo mission], so it was three years. During that time, I served on the surface geology experiment, the lunar sample preliminary examination team, the lunar surface traverse planning team, the lunar science working panel, the lunar sample analysis planning team, and the University Space Research Association Advisory Committee for the Lunar Science Institute. I was on all of that, and I was lecturing to the NASA scientist astronaut classes.

COHEN: Where was all this? Was this mostly in Texas or in Washington, DC?

SILVER: It was all over the country. The training extended as far west as Hawaii. We had to go see active volcanism, and learn about volcanoes. I spent a lot of time out there. We had outstanding Survey people—Dale Jackson, Dallas Peck, who were experts on the geology of Hawaii—but I was the guy who was given the responsibility [of training the astronauts]. This was a very critical thing. Instead of having a thousand voices shouting, one voice had to talk about things. And we'd bring these other people in as consultants, on a reasonable basis. Some of these people later carried on the training when we were through. It was a very exciting experience, and the Apollo 15 mission was considered by most people to be the highlight of all the Apollo missions, followed by the Apollo 17 mission.

Begin Tape 3, Side 2

SILVER: [Continuing] Apollo 15 was a great achievement. There was a group of doubters—Harold Urey, and people who were fundamentally chemists and physicists, including some people I admired very much, like George Wetherill—people who were given important monitoring positions. Finally, these guys got so nervous that they insisted on inserting themselves into the science back room. One of them sent George Wetherill to observe what we did—how we biased them.

COHEN: That sounds like they got very petty.

SILVER: It was so petty, it was incredible. This is another area where Gerry Wasserburg was a principal negative influence. It was reflecting differences in approach which Gerry could not accept. It got to the point where they set up a parallel science back room because they thought they could do it better than the US Geological Survey. And I was then a member of the lunar surface team, which was the US Geological Survey.

COHEN: They didn't trust their science?

SILVER: They didn't trust the geologists. But by the time the Apollo 15 mission was through, I got a direct, full-face apology for doubting us from Tony Calio, the guy who had been the head of the science office at JSC [Johnson Space Center]. And George Wetherill, a very distinguished scientist, the head of the Department of Terrestrial Magnetism, also said we did a great job. Everybody said we did. Those guys [on Apollo 15] up on the lunar surface had learned their techniques and had done their work so well that by far the richest trove of data, samples, observations, and photographs came back with them.

COHEN: You must have been tired by then.

SILVER: I was tired, but I was asked to continue and I continued the best I could. There was a critical point, down at Clear Lake City, which is where Johnson Space Center is located, and Jack and I went out, working and talking late. It was about two or three in the morning. And I said, Jack, I don't know how much longer I can keep it up—eventually I'm shipping home.

But Caltech was wonderful. Nobody penalized me in any way. Whether they criticized me, I never knew. They were pleased with what I was doing, that I was contributing in an interesting way. I was the only person who could interpret the geology to the geochemists, and the geochemistry to the geologists. I was the only person who could translate this science to both the crews and the flight directors who ran the missions. But it was a very difficult, trying time, and my contributions to science were stretched more and more. I wasn't able to finish my papers. I wasn't able to get a lot of other work done. I was just running ragged.

COHEN: And the tension.

SILVER: The tension was there. I was traveling like you wouldn't believe. The good thing about that USGS promotion I mentioned was that I was flying a lot in military aircraft, and by being jumped to that grade, I suddenly found myself with a rank equivalent to lieutenant general, which meant that I could go where other people couldn't go. That's the only significance that promotion had.

Working with the astronauts was absolutely stupendous. They're as bright and directed as any graduate students I ever met. Their motivation in outperforming all previous crews was a military challenge, you understand. Most of them, not all of them, were military. And each one met a different set of problems. They were just superb people. It was a great exercise.

But I hadn't given up my terrestrial research, either. I was applying the techniques of uranium-lead analysis to the mineral zircon, and working out the early geological history of southwestern North America. I had postdocs like Tom Anderson—he and I were doing the first work on the Precambrian rocks of Mexico. We were working on the total geological history of northwestern Mexico. Another postdoc was with me for a short period of time, working on the geological story of the Grand Canyon. I was working all through the southwest. I was working in California, in the San Gabriel Mountains. A lot of things coming off simultaneously in many different areas. It was hard.

One of the criticisms of me and my science is that I haven't published all I know. That's been my big problem—I set out to work on major problems, and I'm a data-gatherer. In the world of telescopes there are astronomers and astrophysicists, and I'm the equivalent of the astronomer. But I don't know any astronomer who doesn't make measurements based on the models in his own mind.

I had my interest in continental evolution—my lifetime project. I'm still working on it.

COHEN: So when did you really give up this moon stuff and the astronauts?

SILVER: About '74.

COHEN: And then you said, "I'm finished."

SILVER: I'm finished. I turned back all the lunar samples—they're all under super security, lock and key—and the guy who came to take them back from me was my old student, Mike Duke.

COHEN: So you came back in '74, and you went back to the work you were doing before. So now I want to talk about the San Fernando earthquake.

SILVER: I want to tell you a funny story. I had a training mission for the Apollo 17 crew, scheduled for two days after February 9, 1971. And we were scheduled to work in the San Gabriel Mountains. The San Gabriel Mountains have a large mass of a particular kind of rock which, we had become aware, was very important in the lunar crust. And I wanted them to see that rock under a variety of geological situations so they'd know what it looked like. They flew in to Inglewood, and I went down with my old yellow truck and picked them up. We came up the Interstate, I-5, past the Van Norman Dam and the power station which had been knocked out. The Van Norman Dam almost failed. And we were talking about the earthquake. You remember that the earthquake occurred almost precisely at 6:00 in the morning. So these guys asked me, what were you doing at 6:00? And I said I was sound asleep. How did it feel? I was in bed, which was moving up and down. [Laughter] Ten seconds later, my daughter was in bed with us.

Jack Schmitt and I were very close through all of this. I told him I was having a hard time, and he asked me if I wanted to quit. He had a different perspective. There was just too much going on, and my family was unraveling. I had a son who has passed away. He was subject to seizures. He had petit mals that developed into grand mals.

COHEN: An epileptic.

SILVER: An epileptic. We first learned about it in the late sixties. He went off to college in '69, and it was a problem for him all the way through. You live in dread. So this was another source of stress—I wasn't around, and all these things happened. In 1980 we lost him from a seizure. So times were very, very tough. And Jack did not want me to quit. He thought I was supplying an important role, which I was, but I didn't feel saintly or mission-driven. I did have a sense that I was helping make things work.

COHEN: Well, maybe it kept you going, too.

SILVER: It could have kept me going, too. And of course, Jack played a continuous role. He was one of the guys who educated me the most. Jack really worked—he had to prove to NASA that he could be a pilot and do all the things. So, having never flown, he learned how to fly. He was the backup landing-module pilot on Apollo 15. At that time, we thought there were going to be perhaps nine more flights. Then they started being canceled, and his chance of going was getting less and less. Ordinarily, he would have been on Apollo 18 the way the rotations were going. His commander was a very talented astronaut, Dick Gordon, who'd already had a lot of space experience, and they were all working together. I used to pit the Apollo 15 crew against the backup crew. We'd see how they'd do, and in the debriefings I'd use one to sort of needle the other. But it was all in a wonderful spirit. Jack is an extremely sensitive man, and he recognized that he couldn't be the scientist while the other guy was the pilot. They were both going to have to be pilots, and they were both going to have to be scientists. This is something I'd stressed all the way through. And it worked. That's the way it turned out.

COHEN: Everybody had to do everything.

SILVER: Everybody had to be able to do everything, because no one man could see everything and comment on everything. Jack helped to coordinate that all the way through. And I dare say that Jack's word was worth just as much as my word, just in a different way. He had to succeed in the most competitive company, and he did. But they finally canceled 18.

COHEN: Was that a budgetary thing?

SILVER: No, it was more than budgetary. It was national policy—how long were we going to keep on spending this money? We'd been to the moon. How many times do we have to go back? What are we learning from each of these missions that justifies keeping these monsters roaring and all these things going on? That was a legitimate set of questions, but the answers weren't right, from my personal perspective. There was lots to do, much science to learn, and, as a matter of fact, we've paid through the nose for not keeping that program going—launch vehicles and other things. But it had to be a difficult decision.

So far, we had been claiming that 11 through 16 were science missions. And here we had a scientist who'd proved himself, and he was supposed to go on 18. But if we used the original, scheduled crew on 17, we would never have put a scientist on the moon. This led to discussions and debates. All this time, Jack had been watched by, and I'd been talking to, several key people. There was George Lowe, the associate administrator who took over the Apollo program after the Apollo 1 fire destroyed the crew, who really brought the Apollo program back in many ways. He had an Apollo office manager by the name of Rocco Petrone, who also did an outstanding job. And down at Houston, there were two great people—Bob Gilruth, the center director who then retired; and Chris Kraft, his director of flight-control operations, took over. So all these people were watching us go through all the training, and they watched Jack's performance all the way through. Well, a very tough decision came about. All kinds of inputs were considered from various sources, and I gave mine. The commander of the designated crew for 17 was Gene Cernan. The landing-module pilot for that crew was dropped, and Jack was substituted, in time for him to go through the full mission training. But the commander Jack had trained with for eighteen months did not get to fly. He was an unhappy chap and so was the other guy. But Jack stepped into it and did an absolutely superb job. Over and over again, I've been asked what difference did it make, to have a scientist up there. It's very difficult to answer that question, because no single crew faced the same circumstances as another crew. They landed at different places with different equipment under different guidelines, and all the other things. But Jack's mission was an absolutely stupendous mission. If Apollo 15's was so rich, Jack's outdid it in weight of samples brought back and the quality of the photography and the observations. And what's more, when people work together as they did on those crews, they trained each other. They worked together so much that an uncommon characteristic developed—you could hardly tell their voices apart over the radio. They talked like each other. But by God, they did a stupendous job. And if I get a lot of recognition for having been a trainer of astronauts, the fact is that Jack Schmitt, more than anybody else, did the training of the astronauts. He did a stupendous job, so before I close out I want to say Apollo 17 was an incredibly good mission. I could say a lot about the Apollo astronauts, but that's enough for now.

LEON T. SILVER

SESSION 4

January 16, 1995

Begin Tape 4, Side 1

COHEN: Good afternoon, Dr. Silver. I was going to ask you about how the San Fernando quake in ’71 affected you and the department.

SILVER: Well, the scientific impact of the February 9, 1971 quake was very, very large. Our department, both geophysicists and geologists, contributed a great deal to the initial investigations and published a number of important papers. One of the major scientific impacts was, this was the first large earthquake in California that was associated with what we call a thrust fault, as opposed to the long-awaited movements on such faults as the San Andreas and the San Jacinto faults. In a thrust fault, the plane of movement is an inclined plane—sometimes at an intermediate angle, sometimes at a very low angle—and the plate that’s on the upper side continues to move up. In the San Andreas fault, we have what we call a strike-slip, or transform fault—a vertical fault with the two sides moving essentially horizontally past each other. The same is true for the San Jacinto fault. And we have focused over the years on these strike-slip faults, because the San Jacinto fault is active and has moderate to large—but not very large—earthquakes. And we have, of course, focused on the great San Andreas fault because of the 1906 earthquake, and the increasing evidence—accumulated, in a large part, right here in our own division—for repeated activity of that kind.

In 1971, the earthquake broke through to the surface. The deeply buried rocks of the region had come, over geological time, to the surface in a number of places along the San Gabriel mountain front, especially in the vicinity of the lower Pacoima Canyon drainage. And we finally appreciated how they came up to the surface. They came on an inclined plane, and the first-motion solutions of the earthquake said, this was a thrust. People didn’t want to use the word “thrust” initially. I remember my colleague, Clarence Allen, saying to me as we went into a meeting, don’t raise the word “thrust,” or people will start deemphasizing the importance of the San Andreas fault. I’m saying this for the record because I’ve never said it to Clarence since.

COHEN: Our last one [Northridge] was a thrust, wasn't it?

SILVER: What we're having now are mostly earthquakes generated on thrust faults, but many of these do not break the surface as nicely as the '71. So the '71 was a great demonstration. Here were these hard crystalline rocks and they were being thrust over the alluvial fan gravels that had been derived from them. In other words, they were going over their own recent debris. And we knew this had gone on a long time.

A number of things went on after the earthquake. The first thing is that the network of seismographs that we had in place at the time was disabled in a large area surrounding the epicenter. The instruments were not capable of taking the very intense shaking, and we lost some of the key records that we would have liked to have had. So one of the things that had to be done immediately was to bring in portable seismometers and seismographs to record the aftershock patterns. And that was done jointly by the US Geological Survey and Caltech.

COHEN: Who was in charge of this?

SILVER: It was not a single coordinated effort between the USGS and Caltech, as it is now. It was a cooperative effort, but it was also, in a scientific sense, somewhat of a competitive effort. There were three or four teams all doing similar things, mapping the surface effects. Barclay Kamb and I were leading a field group for Caltech. The US Geological Survey had a group. There were some people from UCLA and from USC also. It was not that well coordinated, but that's not detrimental because everybody added something to the picture. But what we learned was that we needed a new class of instruments, or better instruments. I can't give you all the details of this, but it led to a significant reshaping of our network over time. And now we've moved on to several generations of more sophisticated instruments. We have the instruments of our TERRAscope network right now, which have the capacity to change their gain almost instantaneously, so as to continue to record no matter what happens.

COHEN: Now you're using the word "we." Who's in charge? Who does what?

SILVER: Right now, it's a cooperative effort. The US Geological Survey and the Caltech

Seismological Laboratory operate jointly. There's an excellent spirit of cooperation between the director of the seismo lab and the chief of the USGS office. There isn't anybody in charge; it's a coordinated effort. For example, when we have people like Lucy Jones and Kate Hutton addressing the media and the public. Kate works for Caltech. Lucy works for the USGS. But they cooperate beautifully, and they publish papers together. It isn't as if we want a single point of view, or a single mouthpiece. We have, in recent years, felt that it's been necessary to suggest that we need to be more constrained about what we say to the public right off the bat—right after an earthquake, when our information base is minimal and we're still trying to process the data and understand it. It's been very difficult, incidentally. Many of the records which were generated in 1971 have not been fully processed. Many of the records generated in the Whittier Narrows earthquake, which was six or seven years ago, have not been processed. Many of the records for the Upland earthquake and the Pasadena earthquake have not been processed, simply because the data acquisition was enormous and our ability to really process it has, to this point, been kind of slow.

COHEN: Do you need more computers? Or do you need more people?

SILVER: We need a more modern, better organized program, and that is now coming into position. We'll be able to do better in future earthquakes, but we still have to consider what we might have learned from these other ones. The relationship between the location of the San Fernando earthquake of 1971 and the Northridge earthquake has turned out to be intriguing. It tells us that we don't begin to understand the ways in which the front range [of the San Gabriel and Santa Susana mountains] will break. But here's what did emerge in our thinking—this is a combination of geological and geophysical theory. For years, we've known that the transverse ranges are a product of north–south compression. There's a bend [in the San Andreas fault] in Southern California, called the Great Bend, which extends from Fort Tejon to the head of Coachella Valley. The Great Bend produces a place where the plate carrying Southern California and Baja California is colliding with everything to the north. That's what produces the compression. That's what drives the mountains up. And thrust faults are what you expect as the failure mode in those circumstances—things are being squeezed up over each other. That's what we've seen in the '71 and '94 quakes. They are both compressional, with the upper plate moving up the slope of the fault surface, which we call the dip.

The 1971 quake taught us that—it was fascinating to work your way along the streets, see the damage to the homes, see the offsets in the curbs and streets and sidewalks, seeing apartment buildings smashed when the fault trace ran right through them. And in the end, we found that at the places of greatest movement, the mountain range had come up one meter and moved about two meters west. That was on the north side of the fault—not along the whole mountain range, but just that part of the range that was closest to the site of active breaking. Those displacements attenuated as you got more and more distant from the locus. Now that was not where the epicenter was—the epicenter was actually further north, under the range, at a depth of about thirteen or fourteen kilometers.

Within a half hour of the earthquake, Clarence and I were up at JPL, where we got in a helicopter and went flying over the area, trying to find evidence of ground breakage. But because the first locations calculated for the epicenter put the epicenter to the north, we didn't find any. So as we flew back an hour later, we happened to fly directly over Pacoima and down below we could see the collapsed hospital [old Olive View VA Hospital] where sixty-one or so people died. There was a brand-new Olive View hospital, not yet occupied, and it looked normal except that its full-height elevator wells had fallen off and were lying horizontal instead of standing vertically. We didn't appreciate from the air that, in fact, the new hospital had collapsed down one floor. And we further didn't know that the people who were trying to do rescue work at the old hospital were not in communication with anyone in downtown LA. We didn't know that we should have transmitted news of that disaster, because all other communications were out. So that had a profound effect on the civil-defense activities to put in place better communications. There was a lot of other very important damage. There were overpasses under construction which collapsed, one of which caught a pair of men in a pickup going to work. There were schools in the immediate vicinity of Sylmar that took terrible damage. This happened at 6:00 in the morning, so nobody was there, but the thought of what could happen to the student population shook up everybody in the community. I think that had a profound effect in our earthquake engineering group, and across the whole Southern California community.

COHEN: It was a wake-up call.

SILVER: It was a wake-up call to get on the ball. If you will, twenty-three years later, there was a test when Northridge came along. And that test said that we had done very well, but not well enough.

For me, the San Fernando earthquake was very interesting because early in my career, I'd been very interested in the formation of thrust faults and their mechanisms. In fact, one of my major areas of research has been the role of low-angle faulting in Southern California. I had found evidence for old thrust faults, but I didn't know how old, because the nature of the exposures wasn't as good as it might be. But my former grad student Jon Nourse and I have since been able to make the case that low-angle faulting has played a major role all through the last 80 or 90 million years of Southern California history.

So the San Fernando earthquake was a very important earthquake. Out of it came a much improved network. Out of it came an awareness amongst the geophysicists, who were less prepared to recognize this than the geologists, that low-angle faults are important. Now we're talking about what we call "blind" thrusts; that is, low-angle faults which have no expression at the surface, so that you have no way of recognizing them. The Northridge earthquake was like that; there had been no prior breaking on that fault, whereas for the San Fernando earthquake, there was good evidence of prior breaking. So there are a lot of low-angle faults in Southern California, some of which must be active, and now we're aware that there are low-angle faults that haven't shown their faces. Instead of having discrete breaking surfaces, they may pass into deformed and folded sedimentary strata above, or for that matter, even into what we call the crystalline basement below.

COHEN: But how would you recognize them?

SILVER: Well, we can recognize them after the fact. And now we're going after them. As a matter of fact, this last fall we ran a major seismic crustal reflection and refraction line across the LA basin and the San Gabriel Mountains to get a better handle on these faults. We're hoping to do more of that, but it's very difficult to do in an urban area. It was a cooperation between the US Geological Survey and the Southern California Earthquake Center.

COHEN: Did the San Fernando earthquake enable you to get better funding for these things?

SILVER: Yes, it really jacked up the funding. It made a major improvement in the ability to support the expansion and upgrade of the instrumentation, and the upgrade of the data processing. And the USGS and Caltech went to work seriously on a cooperative basis.

COHEN: Did you hire more faculty—young faculty?

SILVER: Not because of the earthquake, no. But we have hired more young faculty just in the course of replacing older faculty. And we're still hiring. We're in motion right at this minute, as we speak.

COHEN: And are you looking for people who are interested in seismology?

SILVER: Oh yeah, that goes without saying. We're always looking for good grad students. So much of the work is done by graduate students and postdoctoral research fellows. They're absolutely essential now. It's cruel to say, but they're inexpensive, and they're productive workers. And they are very good. In fact, it's been a tradition in the seismo lab to give good graduate students and postdocs a lot of free rein and a lot of responsibility.

I'll tell you what I've done since then. I've spent a lot of time trying to understand the last 125 million years of history of a region that extends from the southern Sierra Nevada down to below the Mexican border. To do this, I have to do a lot of aerial work, a lot of rock collecting, a lot of laboratory analysis, and what have you. After I developed this technique of using zircons to give good ages, I applied it across the whole southwestern part of North America, from Colorado to California, and from the Colorado–Wyoming border down into Sonora. The first thing I wanted to know was where were the oldest rocks? And we pretty well had that picture established by about 1970 or '73. I had a number of good students and postdocs, most of whom were learning the methodology which they then took away and used elsewhere. That zircon methodology, although it's been refined and many new approaches developed, is still basically the same. And it's used all over the world now.

But having done that, I came on an interesting thing. I found that the Precambrian rocks of the American Southwest were divided into provinces. And it appeared that those provinces had been offset along a great system of older faults—much bigger than the San Andreas. These rocks were between 1,100 and 1,800 million years in age, and had been offset, along with some younger rocks, at some time in more recent geological history. And the sense of motion was completely opposite to that which we see on the San Andreas, where the continent is moving to the south and the Pacific Ocean is moving towards the northwest. This had just the opposite sense of motion. That surprised everybody, and for a while it wasn't accepted. But gradually, it's being given more and more serious consideration. It's a phenomenon we think happened about 150 to 160 million years ago, and which has had many other things superimposed on it, including great masses of granite that came up into the crust, volcanic fields, and a lot of other stuff which conceals it. You're looking through windows to try and see it, but it fits very well with what is now generally accepted as the history of the motion between the North American plate and the Pacific plate.

The San Andreas is only the latest—and a modest—rip in the crust, as it goes. But the masses of the plates are so extraordinary that when the rip has an itch, it transmits so much energy into a region that all of man's structures are in peril. And that's why we get such devastating effects.

But I'm interested in the total history of the continent's evolution. I've not only worked in the Southwest, but I also did, I think, the first zircon work of significance in the eastern United States and southeastern Canada. And I applied it to places in Mexico. So it was in part because I was interested in seeing how the system worked and improving my ability to interpret the isotopic measurements, and in part because I was interested in the geological history. Basically, I'm a historian, and a historian writes about much more than dates. He writes about the whole environment—all the circumstances which lead up to a particular event.

COHEN: You say you're very dependent on postdocs and graduate students; and you impart the information and the enthusiasm and they proceed elsewhere. But there's no continuity then, is there?

SILVER: Well, I'm the continuity. And now I have at least one young colleague who spends full-time doing the same kind of things I do—Jason Saleeby, who's a professor in my division. And then there are several other colleagues who use the results. Another thing I spent a lot of time on, is trying to understand where the rocks we call granites come from, and what the cycle of granite formation is. The entire Sierra Nevada is comprised of about eighty to ninety percent granite and ten to twenty percent older rocks. The granites formed in three great big episodes. Down here, from Riverside south all the way to the tip of Baja, almost all the granites are there, and they're just as abundant as they are in the Sierra Nevada. Think of Yosemite Valley and those great masses up there. I'm talking about things that are not only exposed at the surface, but which extend down ten or fifteen kilometers, or even deeper. The San Jacinto mountains—the mountains right behind Palm Springs—are more of the same kind of rock, but not glaciated and not so spectacularly uplifted as in the Sierras. And that type of rock goes all the way on down to Cabo San Lucas. Palomar Observatory sits on one of those uplifted blocks. I've dated all the rocks around Palomar, and I've found some very interesting regularities which have been particularly edifying to people who were trying to understand where these granites come from.

The importance of these granites is that they are the principal materials for building continents. You may break off a piece of continent from one place, and drift it to another place, and collide it, and make a new continent by addition. But the raw materials have gone through a cycle where they've become granites. The granites have a greater capacity to float on the mantle of the Earth, because they're lower in density than the kinds of rocks that form in the oceanic environments, like Hawaii. They're the floaters and drifters, and you need things with low density to make continents. If it's not the granites, then it's the sedimentary rocks, which are derived from the weathering and erosion of the granites. So granites have been essential, and the measure of the maturity of a planet is the extent to which granite has formed. Granite is, if you will, the fine wine of the whole terrestrial chemical system. It's the end product, and understanding something about it is vital. And much of the metal resources of the Earth are associated with one kind of granite or another. So I've been trying to understand the interaction between these younger granites, which formed in the last 120 or 130 million years, and the older rocks of the region. One of the basic questions is, how much of the older rock are you recycling? Early granites are converted by one thermal mechanism or another into what appear to be younger granites, and that's where the dating method I've developed has been exceedingly useful. I'm not a seismologist. I'm a geologist who listens with awe, and a certain amount of skepticism, to what the seismologists can do. But I'm enormously impressed with them overall. And to the extent that they want geological advice, I'm there amongst other colleagues like Clarence [Allen], to provide advice.

COHEN: So you just use the results of this dating method?

SILVER: I use them and they use them, and we feed back and forth. And that's been a wonderful thing. You know, for many, many years, the seismo lab was located on a very grand estate in the Annandale area. Bob Sharp and Barclay Kamb worked for a long time to bring that group into closer contact with the campus. And that's when they came to South Mudd.

COHEN: Okay, so that's very good, and you've been able to successfully get funding for all these projects over the years?

SILVER: For a long time. It's gotten harder and harder.

Geology in general is suffering from lack of funding. Geological history isn't seen as a national urgency. But on the other hand, I have to say that we will not understand the real nature of our earthquake hazards here, or our metal resources in other places, unless we do this fundamental, basic research on the history and processes by which continental masses evolve. But it's gotten very difficult. Part of it is my own fault, because I have a reputation as being slow to publish. One of my closest friends and colleagues said to me, the trouble with you, Lee, is everybody's afraid of what you know and haven't published. There's a legitimate basis for that. And my response to that is, I set out to solve certain problems. I know when I have made a significant improvement and when I haven't. Just pouring out data—I've made many measurements, I could have been the most prolific of paper writers. But those papers would not have done anything as coherent as what I'm trying to do. Because when I make a measurement, it's a guide to my next measurement, to my next measurement, to my next measurement. On the other hand, I have a pretty extensive bibliography. And what's more, I've worked in a greater variety of areas than any of my colleagues over the years. I take some pride in saying that I'm a general geologist, even though I'm a historical geologist. I work in many areas. I don't know everything, but I know a lot about a lot of geology. And that's what Harrison wanted me for when he first hired me. That's what Bob Sharp wanted me for. It's what I think the division, to some extent, still values me for. I'm a graybeard who's seen a lot.

COHEN: There's another big thing I want to talk about, and that is your work with the alumni, and the Associates, and these wonderful trips. But first one more thing, just for the record. Who has funded you over the years?

SILVER: Oh, I've been funded by NSF [National Science Foundation], US Geological Survey, NASA, Department of Energy, and I've had a lot of funding. But the last few years have been particularly difficult.

COHEN: Okay, well let's talk about your activity with the alumni and the Associates. I mean, you've even had doings with banditos in Mexico.

SILVER: Yeah, but that's a side aspect.

COHEN: When did this all start?

SILVER: It all started about 1974, '75, somewhere in there, when Gene Shoemaker and I were down at the Space Center in Houston. We were getting on a flight back to LA, and the plane had an electrical problem. And they sat us there. It was back in the days when if you were stalled, they started serving free drinks. So Gene and I sat there drinking and talking. Now in 1968, Gene had led a wonderful expedition, following the route of Major John Wesley Powell when he did the initial exploratory float trips through the Grand Canyon. And Gene had tried to reoccupy the site of every photograph taken by Powell's photographers, E. O. Beaman, J. Fennemore and J. Hillers. You see, Major Powell eventually founded the US Geological Survey, and this trip was in preparation for the Survey's centennial and also for the centennial of Powell going through the Canyon. It was a brilliant concept, and he executed it brilliantly. And when he got to the old rocks in the Inner Gorge of the Canyon, he asked if I would go along with him. So I spent a little more than a month down at the bottom of that canyon with him. We had a very fine expedition, and I think it was extremely profitable. I had already been doing work in the Grand Canyon as part of my exploration of the old rocks of the Southwest.

So six or seven years later, we're sitting on this plane with a couple of drinks, and we're talking about the fact that our division doesn't have a single endowed professorship. Gene is no longer anything but a visitor—he's now with the US Geological Survey—the astrogeology branch, which he had founded in Flagstaff. He is a dedicated alumnus. So I suggested to Gene, you know we need to do something really special to get the money for a professorship. Why don't we take the friends of Caltech down the Grand Canyon? Charge them an exorbitant price. He said that's a great idea, but you're going to have to go and sell it.

So I went and sold it to Barclay [Kamb], who was the chair. Barclay was very enthused. I wanted Barclay to participate in the actual start. And of course we had one ace up our sleeve. One of the earliest living geologists to do the Grand Canyon was Bob Sharp, who wrote two famous papers about going down the Grand Canyon when he was a Harvard student after he'd finished at Caltech. And he went down with Ian Campbell, who was on the faculty here. And so Barclay and I and Gene talked it up some more. And Barclay and I went to see Bill Corcoran, the vice president for development and professor of chemical engineering. And Bill liked the idea. So then he broached the idea to Harold Brown. And Harold Brown liked the idea. So then, amongst us all, we proceeded to set up what we called the "Trip of a Lifetime." And it was going to be a deluxe trip. Now I had been going down the Canyon, so I knew how to arrange the logistics and do all the other things that were involved, and I set it up. A number of the trustees were very interested—and wealthy associates. We got two large, powered rafts full.

COHEN: What did you charge them?

SILVER: We charged $50,000 per individual, $75,000 for a couple.

COHEN: We're talking twenty years ago. That's a lot of money.

SILVER: That's a lot of money. Well, we finally did three of those trips, every one of which was fully subscribed. The leaders were Barclay Kamb, Bob Sharp, Gene and myself. And by the end, we had about $950,000, and we managed to get some matching money from somebody else. It came to a million dollars, and we had our chair. And all the way from Gene Shoemaker's and my initial discussion, we had the objective that the chair would be called the Robert P. Sharp Chair, and that he was going to be the first occupant of that chair. Of course, Bob did not know this. And we pulled it off. You have to understand that that was the first time that any faculty initiative on this campus had done anything like that.

Well, when Bob retired, he thought that those trips were so great that he proposed to do more of them. Bob then became the moving force, but whenever Bob asked me, I went with him. Phyllis Jelinek was the first Alumni Association director who participated with Bob, and she was great. She was adventurous. You have to take some risks. You don't know whether you're going to sell your product until you try. But it worked. And the strength of it has been Bob Sharp's wonderful personal style and his ability to educate. They were real travel-study operations, but they were fun. We were going to such good places. So Bob did a couple of these, and then he asked me if I'd help him. The first thing we set up were trips to Alaska, and we did either three or four trips.

COHEN: What year was that?

SILVER: Long gone. I'd say about 1980.

COHEN: And how long were these trips?

SILVER: These were seven or eight days. Bob did the first scout trip to Alaska, but I went along with him on the trip itself. And then as we did further things, he and I worked on them together. Bob was the major force, and he set the tone and style. And I have to remind you that when I came to Caltech, my first job was as Bob's teaching assistant. His influence on me has been profound. If I have to acknowledge any single individual as having affected my career, number one is Bob Sharp. And he hired Sam [Epstein], he hired Gerry [Wasserburg], he hired Frank Press, he hired Barclay Kamb, he hired Clarence Allen, and on down the list.

COHEN: So you went on these trips. How often would you go; maybe once a year?

SILVER: We'd do two or three trips a year, but the big trips were where Bob needed my help. By 1980, Bob was already in his early to mid-seventies.

COHEN: Of course, he was making a lot of small trips; that is, just trips in California.

SILVER: Yeah, he does such a wonderful job, and I learned from him. So I've helped him—

three or four trips to Alaska, three or four trips to Yellowstone. And Bob encouraged me to do some of my own trips.

COHEN: Now did you continue to make a lot of money on these?

SILVER: Oh no, these trips were basically institutional support efforts for the Alumni Association and the Associates. And as a matter of fact, Bob and I have talked about whether we can draw on the good will. He's reluctant to do it. I'm trying to push him into it. All I wanted was his permission to raise funds for a cadre of Robert P. Sharp Graduate Fellows in the Earth sciences. To use his name.

COHEN: He said no?

SILVER: He got mad at me—took a couple of months before he'd talk to me again. I had persuaded Tom Anderson [vice-president for institute relations] and all the powers that be. I talked to the president and everything, and they wanted to do it. Bob felt he could no longer operate that way, but sooner or later, I'm going to make the effort to help establish the Robert P. Sharp cadre. His thesis has always been that the future of the Institute lies in young people. Bob and I and Barclay once went on a fishing expedition to visit Dean McGee of Kerr McGee, who at that time was a trustee. We tried to sell him on just that theme. This is all for young people, and that's what we need here.

COHEN: There's been another endowed chair since, because you have a named chair.

SILVER: Oh yeah, there've been a lot of chairs since. But we broke the ice. How influential was this? I'd say there must be seven or eight chairs now. I have not recently kept count. Barclay, Don Anderson, Sam Epstein, Ed Stolper, all these others.

COHEN: Is there any one of these trips that you'd like to talk about in particular?

SILVER: No, they're all excellent trips. They take a lot of hard work. I'm now setting up three: one jointly with Clarence, and two by myself. The first one will be a three-day trip to the Anza-

Borrego desert in flower time, so we can talk geology and wildflowers at the same time. Second will be the one with Clarence, in central Colorado, where we'll look at some of the great mining districts, now ski resorts. We'll talk about the history and geology of the Front Range, going from Denver through the Front Range and back to Denver. The third one will be in southern Colorado and northern New Mexico, where I'm trying to take a group along the course of the northern Rio Grande. The trip's called "Rio Grande del Norte."

COHEN: I see. Doesn't it add to your office load to do all the real work?

SILVER: No, the Alumni Office does a lot of the work. Bob and I have to have the accommodations at the places where we will be at the end of the day. And so everything is really built about our study plan and then we work with the Alumni office. The most recent Associates trip, Nancy York traveled ahead, and sort of left Clarence and me to fill in the gaps. I don't think that's the best way to go. The way Bob did it is much more effective. He had someone, Phyllis Jelinek or other people from the Alumni Office, traveling with him on the preliminary trip.

COHEN: So it really is a big effort.

SILVER: Oh, it's a major effort. What's more, I have to go on each trip by myself, just before the actual trip, to make sure it's all still go. For example, the trip in southern Colorado-northern New Mexico is intended to catch the aspens changing. How much time we spend looking at aspens depends on whether there are aspens there. It takes a lot of work. And why am I doing it?

COHEN: Why are you doing it?

SILVER: Well, because it's partial payback. I have been lucky to be a member of the California Institute of Technology. I've worked with people I respect and enjoy. And I think, as an institution, Caltech is extremely important. Over a number of years, I've spent time serving on committees—the council of the National Academy of Sciences, the governing board of the National Research Council. I'm convinced that as a representative of a class of institutions, this is the most important element in our whole national scientific program. It's a site for great science and it generates great scientists.

COHEN: You mean Caltech itself.

SILVER: Caltech, MIT, and the other great universities. And of course, this is a time when we all wonder what the future of scholarly science and basic research and major educational institutions really is. And I don't know, but I want to do everything I can…

COHEN: So in some sense, this is your thank-you to Caltech.

SILVER: It is in a large part.

COHEN: And you enjoy teaching.

SILVER: It is a teaching process and I'm following my mentor, Bob Sharp, who clearly enjoys teaching. I enjoy teaching, so it's a form of payback. I'll never be able to give Caltech the kind of money some of these people do, but I may, in fact, have far more influence than their money. And that's because I don't believe concrete's important—I think young people, ideas, and continuity are the important things.

COHEN: Well, that's really terrific. So of all the things you've done, do you feel this is the most valuable?

SILVER: Well, Shirley, we all have multiple values. I am satisfied that I have explored a part of the natural world in a way that nobody else had before me. I'm also satisfied that there are good people coming behind me, and I've had a hand in helping them get the leg up—this business of standing on the shoulders of those who precede you. I wouldn't call myself a giant. It's just institutional memory. One of the reasons you're keeping the record here is to see the way in which each generation of us coming through has influenced the following generation.

LEON T. SILVER

SESSION 5

January 25, 1995

Begin Tape 5, Side 1

COHEN: Good afternoon, Dr. Silver. Today I want to talk about activities that you are involved with away from Caltech, or things that you do concurrently with your stuff here at Caltech.

SILVER: Well, I've already told you that for a number of years, I had a part-time employment relationship with the US Geological Survey. And that was in two phases—one as a young geologist, when I got invaluable experience working part-time for eight years in various places in the western United States. Then, when the Apollo program came along, the training program that I initiated here at Caltech with the support of the division was eventually assigned to the US Geological Survey, so I was once again employed for a while by the US Geological Survey.

COHEN: So your training of the astronauts was actually a job you were doing for the Survey?

SILVER: At the end. But it was initiated strictly by young Jack Schmitt and then Gene Shoemaker. These are the guys who got me involved and I initiated that actual training program myself. And then, as it got a little further along, I started working collaboratively with the Survey. And I worked very well with the guy who replaced Gene Shoemaker as the head of the lunar science surface-investigation team—a geologist by the name of Gordon Swann. He became a very good longtime friend. He's now long retired from the Survey, but he had been Gene's right-hand man in the surface-investigating team.

Then I did a fair amount of work for NASA, in several different capacities. I think I've mentioned that, over the years, I've given introductory geology lectures to all the incoming classes of astronauts. And of course, I served on many, many committees—so many that I'd have to look them up to figure them all out. And I really did serve very intensively on committees during the Apollo program, perhaps because I was an easy target for the engineers. The engineers could talk to me in a way that they couldn't talk to a number of the other, more brilliant investigators. Remember, I had been a civil engineer myself, way back. So I did a lot of work for them, and, particularly in the last three major missions, I was on almost all of the major committees that they had.

COHEN: And this was while you had your full-time duties here at Caltech.

SILVER: I had my full-time duties here at Caltech. Then, starting a long time ago, but picking up in the seventies, I started doing work for the Geological Society of America. Eventually, I served on the council of the Society, and then I became vice-president and president.

COHEN: What kind of an organization is that?

SILVER: The Geological Society of America is a North American geological organization—it covers Mexico, the US, and Canada—and it is the largest geological organization for publishing basic research right now. It's a great professional society. I got very heavily involved, and while I was president, we adopted a program called the Decade of North American Geology, which led to the publication of probably thirty-plus volumes and maps. I was the founding chairman on the steering committee for that, so I got deeply involved in that program. I served on the steering committee for a number of years, and pulled out because I was getting increasingly involved in National Research Council activities. I did work for the National Research Council on various kinds of committees.

COHEN: Was this after you joined the National Academy?

SILVER: I started before, but it picked up. I was elected to the Academy in 1974. It was at the end of the Apollo program, and they started using me more and more. And I served on various boards and commissions and finally, I was asked if I'd like my name to be nominated for the council of the Academy. And I was elected to the Academy council.

COHEN: What year was that?

SILVER: I think it was 1989, and it's a three-year term. And council members carry on other activities—I was asked to serve on the governing board of the National Research Council, and I served on it. All of these took a heck of a lot of time.

COHEN: I was just going to say, that's a lot of trips to Washington.

SILVER: A lot of trips to Washington—piled up lots of frequent-flyer miles. I then went on to serve as chairman of the NRC Committee for International Programs, involving ICSU and other activities.

COHEN: ICSU is what?

SILVER: It's the union that Harrison eventually became president of—the International Council of Scientific Unions. Let me talk about Harrison right here. Harrison, after he became foreign secretary of the National Academy, became very active in international science affairs. There were some wonderful programs that Harrison organized.

COHEN: How was an organization like this funded?

SILVER: This was funded by contributions from professional science societies and from the countries which sponsored them. It's the society for international professional societies of all kinds. There's an astronomical union. There's a geological union. There's a geophysical union. There's a chemical union and an applied-chemical union. There are many unions. They're headquartered in Paris. And I was responsible for US programs related to it. And then beyond that, I did a great deal of work for the Department of Energy on various committees. I have served on advisory committees to Los Alamos. I was a member of the Basic Energy Science Advisory Committee for five or six years. Then I became vice-chairman and then I was chairman. This is all DOE—big programs.

COHEN: Would this be several meetings a year?

SILVER: Yeah. Far more, actually.

COHEN: You were commuting to Washington, almost.

SILVER: All over the country. There were meetings for the national labs and what have you. They all overlapped, and it was an enormous burden. And of course I served on committees for the National Science Foundation. I got deeply involved in planning and implementing a program of deep scientific drilling along or near the San Andreas fault. I served as assistant chief or co-chief scientist—I forget the exact title, but I was really responsible for the ongoing science operations. That project took three years, from about 1985 to about 1989.

COHEN: Who did this project?

SILVER: That was a joint National Science Foundation–USGS project, with some Department of Energy involvement. And I've been on advisory committees for the National Science Foundation for various kinds of programs. I'm now out of most of those—maybe out of all of them.

COHEN: Now, did you do this because you felt this was being a good citizen? Which it is, of course—or did you really enjoy it?

SILVER: Well, I enjoyed it when we made progress. But I also began to learn that an institution like the California Institute of Technology is so far more effective and relevant to American science than many of these advisory committees or the institutions they advise, that my interest in them began to decline very, very markedly. I was still doing all my teaching. I was still carrying my load here, and it meant lots of travel and lots of heavy-duty stuff.

I had very little in the way of outside consulting—I deliberately turned away from it. But in 1979, the then-president of the Academy, Philip Handler, came to me informally with a request that I consider service on an advisory committee that the then-president of ARCO [Atlantic Richfield Company] was putting together. And that president was William F. Kieschnick, who has been a trustee—and even the vice-chairman of the trustees—for a long time. It was a committee of about six or eight scientists from different disciplines. Harry Gray was on that same committee, and Harry and I served together for a period of that time. That was quite a lucrative time.

COHEN: So you were paid a consulting fee on that one?

SILVER: But I didn't take the consulting fee. I directed it to Caltech with the understanding that it would be applied to my chair, so I could use it in the pursuit of my research, to support grad students and other things. I've never taken consulting monies except on a very, very small scale—a trivial scale. Probably, all told, not more than a couple thousand dollars in the history of my work.

COHEN: Is that a matter of principle?

SILVER: Yes, it's a matter of principle, but also the money is applied in ways that were beneficial to my research. So it was self-serving to some degree. But I really have avoided that, and I'll parenthetically make the remark that, in contrast to many faculty members, for me to be an active consultant would have detracted from my ability to perform my basic research or my teaching.

COHEN: You felt there was a real conflict of interest.

SILVER: There's a real conflict of interest, and I also think that the Institute does not always recognize this well. And it comes into many issues which the Institute has had to deal with. Members of the faculty form their own companies, and brilliant members who form successful companies make a lot of money. Now that I'm investigating retirement, I find that the terms of retirement are based on the assumption that you put aside a lot of money from your outside income, which would allow you to go half-time early, or which will allow you to take these bonuses which are currently being offered, and that you'd supplement this with your other monies until such time as you take your retirement. That works, I'm sure, for many people in engineering, chemistry, biology, and what have you.

COHEN: You know, actually, engineers have said to me that they feel that that is a part of their job, because then they know what's going on.

SILVER: Yeah, I know they do. I'm not quarreling with that. What I'm quarreling with is the assumption by the Institute in its retirement policy that we've acquired all of this extra money. Because it's not uniformly distributed—I happen to think that astronomers don't make very much money either. I just think it's an uneven policy. It's not right. But the Institute's been so good to me, I'm not going to gripe.

Anyway, I enjoyed that service and made a very interesting friend, Bill Kieschnick. He has served Caltech very well, although he's minimizing his role now because of Mrs. Kieschnick's illness. But he has done very well.

COHEN: When did you get your endowed chair?

SILVER: I've had the chair for ten, twelve years.

COHEN: And your chair, what is the name?

SILVER: It's called the William M. Keck Foundation Professor for Resource Geology, and it was really created by the life trustees of that foundation.

COHEN: The Keck Foundation.

SILVER: Yes. That's the same Mr. Howard Keck who is building the telescopes. Actually, I didn't know him as well as I knew William Keck, Jr., whom I got to know quite well, but who died. The Keck family's differences have been publicized so frequently and widely that I don't want to go into it.

Probably the reason I got the chair is that I had paid a lot of attention to natural resources. I served on natural resource committees for the National Research Council—I chaired a number of studies for them, especially in the era of our great hunt for uranium resources. I'm honored to have had the chair.

Other outside activities—well, the work with Bob Sharp for the Alumni Association. I did it initially to give Bob assistance, and again I've continued to do it because I'm astonished that Bob is able to maintain the pace he does at his age and in his physical condition. Bob has been my benefactor here. He's the guy who hired me. He was the professor I first TA-ed for when I was a grad student, and he's a great man. It's been a privilege, so I'm there to assist him whenever he wants me to come. Bob has said to me that he'd like me to carry on this program. But I think, from the point of the view of the institution, we need to involve many people in this thing.

COHEN: Well, they really have two offices sort of devoted to doing these things.

SILVER: Yes, they have the Associates and they have the alumni, who are two very different communities. The alumni are very educated in science, and have a lifelong interest in science. Almost all of them have taken a course from Bob Sharp at one time or another, and a few of them have taken courses from me and other faculty members.

COHEN: Aren't many alumni Associates also?

SILVER: Quite a few, but mostly I've taken trips with the President's Circle Associates.

COHEN: They are wealthier than Caltech graduates.

SILVER: I'm unhappy to say that there aren't that many wealthy Caltech graduates. A few, okay. I've also done a trip or two for the National Academy's President's Circle, and…

COHEN: Who are those people?

SILVER: They're wealthier—a very impressive group. One or two of them overlap with our Associates. That's it for outside activities that I can think of right now.

COHEN: Okay. So among your honors and your awards are, of course, your membership in the Academy and the chair. Is there anything else that has meant a lot to you?

SILVER: Oh, there are a number of other awards from NASA. And, when the Apollo program ended, there was a sheaf of certificates of commendation for service on all these committees. There was a medal from the Geological Society of America and the American Institute of Professional Geologists, and things like that. There aren't that many awards—just what comes along when you've done a lot of service. It's interesting how much you get for being in service, as opposed to recognition for your science. But being elected to the National Academy is the central recognition for science.

COHEN: That's good. I'd like you to be a little philosophical now. You love this place and it's been very good to you. Where do you see it going? I mean, with the young people that have been appointed. Do you think that it's going to keep its direction?

SILVER: I have to look at several levels—the level of the students, both undergraduates and graduates; and the level of the faculty. I'm only talking about my division here. Actually, there's another kind of service I haven't mentioned, and that's my service on a wide variety of Institute committees, which have made me very aware of some things in the Institute. I've got lots of impressions about the institution. I also have strong impressions about the sequence of presidents and provosts who have served while I've been on this campus. I'll always remember the grace and the complete focus of Lee DuBridge. I remember Bob Bacher's bluntness and directness, but dedication. I remember the service of Harold Brown, who I thought was a superb president.

COHEN: You did like his term of office.

SILVER: I liked his term of office, even though it was a term of financial shrinkage, of cutting back. However, there was no cutback in support for interesting intellectual endeavors and/or for any interesting new ideas. Remember, he accepted the proposal to do these river trips. He had Bill Corcoran working with him, and that was good.

COHEN: And, of course, he established the Social Science Department within the Humanities Division. I don't know how you feel about that.

SILVER: Well, I think it was important. I think it's an essential element of our institution. I served under Bob Christy as acting president and as provost. I got a very tempting offer in the UC system that I never really considered seriously, but after talking to the chair, Barclay Kamb, I decided to go talk to Bob Christy. I didn't ask for anything for myself. That was a time when we needed young people, and I asked Bob to assure me that we'd be able to do certain hires. And whether I was influential or not or whether Barclay sold it all by himself—which he could well have done—I don't know. But we got those young people and I did not go. Earlier, I also had gotten an offer from the Carnegie Institution which I did not pursue. There weren't that many job offers, but there were a number. I had continuous offers from the US Geological Survey, which I did not pursue. I think I can honestly say I never leveraged on those offers.

I worked very well with Murph [President Goldberger]. And I was very disturbed to see the way in which Murph was treated during the army think-tank episode [the Arroyo Center—Ed.]. In part, he brought it on himself, but I think things were not graceful. And Robbie Vogt, who had just become provost, was not graceful. But Robbie was never a truly graceful man. He was very good with trustees. He could be as gracious as he could be, but he was not graceful. I had been chairman of the Institute Academic Affirmative Action Committee, at the request of Murph Goldberger. We were dealing with some problems involving female faculty members, and Robbie made some comments and statements which gave me great difficulty. I went into his office prepared to resign, but you don't resign to him—you resign to the president. Robbie assured me that he was really going to support affirmative action. But in the long run, the great movement in affirmative action at this Institute was created by Barclay Kamb as provost, and I never felt that there was a true in-depth commitment to affirmative action before that. It's now being supported on a national basis. I've always had the problem with Tom Everhart [current Caltech president], that I feel he says what is the politically correct thing to say at the time. I could never see that he really put his weight to the program. And eventually, despite his asking me to stay on, I retired from the Affirmative Action Committee. I've never been happy with it, and I have to say that I think the Institute has used the advantage of its being a special place, a very stable environment, to not do as well with its staff affirmative action as it could. Even though a lot of progress has been made, and I won't criticize the personnel officers, the remuneration and the benefits extended to female staff are so far different from those extended to the professors. [Break in the tape]

I do feel that the Institute has not adequately taken care of the non-academic staff, and particularly women on the non-academic staff. And I have to recognize that the extent to which the Institute was able to get service from them at low cost provided the rest of us—the professorial users of the benefits of the Institute—great advantages. But there is the matter of equity and social consciousness, and I think that problem is still there. I don't know how to beat it. I think the real responsibility doesn't lie just with Dave Morrisroe, who is the financial power of the Institute—it lies with the trustees. Dave's working with the trustees, and I've talked on occasion to trustees. I won't name names, but we felt there was a need for some revision. And I think there's been a tendency to revise, but over the long term, this has to go back to the leadership of the Institute. There's been a history of underpaying, and the Institute will claim it's within the salary scale for the region for people of comparable work and accomplishments. Maybe I'm just a liberal do-gooder, but I think there's massive inequity that still needs to be addressed. And I don't know how we're going to address it, because I also at the same time recognize the intensified national budgetary pressure that the Institute is facing and will continue to face. And I'm worried about big projects and the need to have a lobbyist in Washington.

COHEN: Shall I say the word LIGO [Laser Interferometer Gravitational-Wave Observatory], or do you want to say the word LIGO?

SILVER: Well, LIGO is symptomatic. I'm only modestly informed on LIGO, and it's all based on secondhand information. I do know many of the personalities involved. I can see that LIGO was an accident waiting to happen. I strongly criticized two of my colleagues who were members of the Provost Search Committee who recommended Robbie to the appointment, the provost position. I had served on committees with Robbie, and I've listened to him at faculty meetings. Robbie is relatively unstable, and I'm not going to get involved in the details of the LIGO thing, but I'm concerned by what the fact that we have become so dependent on a large project like LIGO means financially to the Institute. If we face a future where we must recruit large projects, then this institution isn't going to be what it has been. We will not succeed in attracting some of the best minds that I'd like to see us get. We'll attract some of the best science promoters instead. I'm even concerned about the Beckman Institute and its role, even though I have great respect for Harry Gray, both for his intellectual accomplishments and for his general wisdom. I worry about projects or people having special status, rather than our being what we used to be over the years—a true collegium.

COHEN: Maybe that doesn't exist anymore.

SILVER: Maybe it can't exist anymore in the real world of financing, and maybe great progress in science must be done in consortia that can build telescopes of super size and dimension. We have found at Caltech that we always made great progress when we had great advantages in instrumentation, and so there have to be great efforts in instrumentation since instrumentation grows more expensive all the time. So we do have to get involved in more and more expensive projects. But the community should be selected and propagated on the grounds that it includes truly intellectual individuals of stature, with high intellectual energy, and with long-term goals.

COHEN: Do you see that happening with the young appointments in your department? How do you feel about them?

SILVER: I feel good about almost all the appointments in our department, and at the time of their appointments, I supported all of them. Not all of them always turn out the way I wanted them to be, and there is a gamble. But I think we have a high batting average. I think we have a very distinguished young to middle-aged faculty. On the other hand, we missed so many opportunities. Sam Epstein can tell you about all the things he suggested that were not heeded. And, to a large extent, he was correct. I have supported Sam, and I sometimes am more effective than Sam is in selling some of these things. But sometimes we never get off the ground. One of our great problems was that we had a couple of chairs who weren't as effective.

Barclay was a distinguished chair, just as I feel he was a distinguished provost, and the people he hired are our best hope. They are our strength right now. Both as chair and as provost, he was a true leader of first quality, with the highest goals and objectives. He had the ability to evaluate people, and the whole thing. After Bob Sharp, Barclay was extremely important in creating our division as it is now, with the reputation that it has. And that's got nothing to do with the fact that he's a superior scientist, and a gentleman. He's a dedicated citizen who loves the Institute about as much as anybody does. But after Barclay stepped down, we brought in Peter Wyllie, who was a good man and a fine scientist, but he came in from the outside. He didn't understand the Institute. He came from a different milieu, and he could not handle things. And eventually dissatisfaction, not anger, led to a request that he step down. And when he stepped down, we appointed G. J. Wasserburg chair. This man's idiosyncratic manner and egocentric methods were not compatible with the honest forms that we have always had, and that set us back. We lost people whom we never should have lost—whom we could use desperately right now—because of his self-serving insensitivities. Brad Hager, who went to MIT, is a major case.

COHEN: But he only served a short time. How long did he serve?

SILVER: He's a very influential man for good and for ungood. And then we had Peter Goldreich come in, who put in about four months of healing and did a super job. Peter's intellectual standards are such that he could do that. And then we got Dave Stevenson to take the job. Dave is another superior intellect, and a very decent fellow—very dedicated. He ran his term, but towards the end of his term, his last year, he said he wanted out. I think he was wise—he had earned the out. He had done many good things.

COHEN: So he served one term.

SILVER: One term of five years. He could have had a second term with the full support of the division. So then we had to go through the search again, and we now have a fine chair, Edward Stolper, who is in his, I guess, fourth or fifth month. He's working very hard. He's in his very early forties, and he has taken on the responsibility very well. He worries about his job. He's going to be one of the strong figures in the history of the Institute. It's very good to have him in the IAC [Institute Administrative Council] group alongside such people as Peter Dervan. And having Steve Koonin come in as provost is great.

COHEN: You think that's good news.

SILVER: I think it's good news. I have interacted with Steven in a variety of ways. I was on the steering committee of the faculty board when Steven was chair, and I've watched him work, and I liked everything I saw. I think his performance in the LIGO thing, although it's received some criticism, has been very appropriate. Given that nobody will remember the state of affairs at each meeting, you understand.

I think Steve is a very broad-gauged man. He'll do a good job. I think Murph made his own contribution. I think we made a significant misstep in selecting an engineer who's not really interested in science to be the current president. I have great respect for his honor and integrity and good intentions, but he is not the caliber of man who should be the president of the Institute.

COHEN: Do you think the trustees just wanted an engineer at this point? Were they just tired of eccentric physicists, or something?

SILVER: Yes. Well, they were. Murph was too much of a character for the likes and tastes of some. And Murph can be wobbly on occasion.

COHEN: He likes to be nice to everybody.

SILVER: He likes to be nice to everybody. He was also given to off-the-cuff statements which probably get him in trouble now. But Tom is not the right guy, and I'd wish he'd retire soon in honor and with honor, and let us get about the business of finding a distinguished president. I'm disappointed, because I believe the president of the California Institute of Technology should be an articulate and reasonably frequently heard voice on matters of science policy and what have you. Tom has not risen to that level. There are very few presidents that rise to the level of intellectual policy, like Derek Bok of Harvard.

COHEN: They may be overwhelmed with the money-raising aspects.

SILVER: Well, they are overwhelmed, but they should be working on changing the basis on which federal monies come to universities. I think that federal monies are doing great damage, intellectually, to universities. There should be a certain amount of money that comes to a university—perhaps a full fifty percent of what it gets should come in [the form of] block grants. And those block grants should be internally competitive awards, with external people making inputs on the qualifications of the in-house people. It would give the Institute a much better feeling for the quality of its work, and also provide a stability which is sadly lacking. What's happened as the number of institutions and individuals requesting funding has increased is that the terms of the grants become shorter and shorter. The time spent in writing proposals increases terribly, and the success rate drops off. The factors which should be considered—long-term performance and the nature of the research problems—are gone. I got incredibly tired of having people review my proposals who were far less informed than I was. That may sound like intellectual snobbery, but it's a fact. I know how carrying on my own research as best I could, with what funding was available in my own way, has moved me far further along than if I had been in this rat race.

COHEN: But you managed to get your grants.

SILVER: With very conspicuously decreasing success over the last five or six years.

COHEN: Well, you're not alone in this.

SILVER: No, everybody's had that problem. The scale of my effort has gone down, and, to some extent, I concede that what money is coming in should be coming to young and middle-aged investigators, by and large, to keep them afloat and alive. We need that more than anything else. And I don't like the way in which the managers of those programs I've been associated with allow reviewers incredible license to depart from responsibility, accuracy, and decency.

COHEN: Well, I guess as resources get less and projects get more expensive, there's a problem.

SILVER: The community starts turning in on itself, and even eats itself. I just don't think that the people who've exercised leadership in American science have really addressed the question of how one goes about doing reviews. NIH [National Institutes of Health] probably does better. NIH's standards are higher.

COHEN: Well, people are interested in health. I mean there's always money for cancer.

SILVER: I think there are serious difficulties that have not been properly addressed at the national level—at the level of the Academy and the National Science Foundation. The bad thing about the National Science Foundation is that they have a governing board—the National Science Board is what it's called—that does not have enough professors on it. It has too many college presidents. The college presidents are there to make sure that the funding systems work.

I'm interested that the science works. That's a major criticism of mine, and I don't think we'll ever get away from it. I would like to have a leader at Caltech who is more interested in how the science works, as well as dedicating himself in service to making the system work. I like Tom. He says many good things, and I like him, but I don't think he's the leader.

COHEN: Vision is what you're talking about.

SILVER: A vision. I recently served on a NASA advisory committee for the redesign of the space station. The chair was Chuck Gray, who's been president of MIT, and his effectiveness far exceeded anything I've seen the Caltech president do. And Chuck can speak very well. Everhart has not, in my own mind, done as good a job as he should in speaking on behalf of the national science programs. But we allow science policy to be dictated so extensively now by congressmen and by political appointees to such things as the Office of Science, Technology, and Policy, which is the President's and the Congress's joint appointment. As a matter of fact, we took the head of Congress's OTA [Office of Technology Assessment] and made him Clinton's science advisor. And of course, I can be accused of not being realistic about what science should be doing. Well, we should never have built as many research universities.

Begin Tape 5, Side 2

COHEN: Can you envision solutions to some of these problems?

SILVER: I see that there's going to have to be a painful downsizing in science, but it can be done in better ways than is currently contemplated. The people who should be leading the creation of new designs for how science is done are not speaking up very well.

COHEN: Well, who should that be?

SILVER: College presidents. There is a committee right now that Frank Press is chairing. Frank did not want to do any more chairing, but this is a committee—created by legislation sponsored by Tom Hart, the Nebraska senator—to set priorities in the allocation of funds for science. It's such a difficult thing. I hope, basically, that what it's going to produce is a report on how one goes about this, and not a series of specific recommendations. Because the nature of science is such that you cannot tell where the greatest contribution will be to the general scientific bank of information and understanding, nor can you tell what will be relevant to the societal applications. We're losing respect for the importance of building a general understanding of the processes which operate in our universe. What really is disappearing is the attractiveness of science as a vocation. When grad students see the kind of struggles that professors go through to raise funds, they don't want to get into that rat race. It's much easier to become a lawyer, or a doctor, or a stockbroker. The rewards are more immediate, and more material. Of course, when you get to be a doctor, you will struggle and strain. You get to be a lawyer, you'll struggle and strain. Even as a stockbroker or banker, you'll struggle and strain, but not, perhaps, in the ugly ways.

There are many different ways to do science. I'm involved in hands-on science. But there are so many ways to do physical science now that are not hands-on—where you have twenty grad students and eight postdoctoral fellows, and you meet with them periodically, but you don't do the hands-on science yourself. I love my hands-on science. Perhaps I'm an antique, but I really love it.

So science is changing, and I just want to express my gratitude for having been in the right place at the right time to have had a number of mentors and sponsors who helped get me into new areas. They provided incentive and initiative to go ahead. I know that I have explored some areas of the natural world that had not been explored before. I know that I've made some contributions which have been useful, and which are being used right now. They will probably be supplanted, in time, and that's legit. I think I moved the body of understanding forward, and I'm not through. I'm going to keep on trying. Caltech has been wonderful to me. The Division of Geological Sciences has been wonderful to me. The geological profession has been wonderful to me. I can look at all kinds of instances that I'd rather not have experienced, but I have been a very lucky fellow in my professional career.

COHEN: And it's been fun.

SILVER: It's been fun and it continues to be fun. The fun comes now in sharing it with young people.

COHEN: Well, I think that's a good interview. Is there anything else you'd like to say?

SILVER: No.

LEON T. SILVER

SESSION 6

February 23, 2000

Begin Tape 6, Side 1

COHEN: I'm glad you've come to give us some supplemental stories we may have missed, including the discovery of the Indian Sipapu.

SILVER: I want to talk about an episode which was kind of fun. In involves Gene Shoemaker. And again, Shirley, I don't know how much of this I've talked about. My recollections may not even be consistent. I got to know Eugene Shoemaker, who was for four years chairman of our division. And he was a professor for about eight. I got to know him just after he left Caltech with a master's degree. He went to work on a project for the US Geological Survey which is known as the Colorado Plateau Uranium Project. This was the first great national hunt for uranium to fuel our nuclear ambitions, both military and peacetime. This had to do with a region called the Colorado Plateau, from which we extracted most of the uranium—not all of it—which supplied our nuclear ventures at that time.

COHEN: Now, I don't think you spoke about this. I'm not remembering this.

SILVER: Okay. I don't want to get away from the fact that I'm going to talk about Gene Shoemaker and the Sipapu. But those ventures meant that the Survey expanded its program immensely. It was on a great national hunt for uranium.

COHEN: So this was on contract with the AEC to do this?

SILVER: It was supported by an AEC budget and carried out by the US Geological Survey, which expanded its power. Gene was working full time for this project, and he very quickly became very expert. Also he was an independent thinker; he didn't necessarily follow the generally accepted scientific line. And I want to give him credit for that, because he was probably more right than all the conventional wisdom at that time. But I came to work for the

USGS for a summer; it was the summer when I was on my way to Caltech as a grad student. And I got to know him for the first time. I liked him. We got to know each other. We stayed in touch after I left and went to Caltech.

COHEN: He was with the Survey?

SILVER: He was with the Survey full time. He did not go back to graduate school until he'd been with the Survey for probably eight or ten years. His job was to look at various kinds of craters and volcanic features—a few of which had already been identified as being associated with concentrations of uranium—and try to decide what geological principles governed the distribution of that uranium and what that might guide us to in the search for major deposits. One of the places he became interested in, of course, was known as Meteor Crater. And he became aware of the work of G. K. [Grover Karl] Gilbert—that's one of the giants in the early history of geology. And Grover Karl Gilbert was one of the founders of the National Academy of Sciences. If not right at the very beginning, he was one of those who built up its role. He worked for the US Geological Survey—one of the giants of the Survey. He had looked at Meteor Crater and wondered whether or not it was an impact feature or whether it was a volcanic feature. At the same time, he just happened to be doing telescopic studies of the surface of the moon. He put together the existence of these circular features and tried to debate whether the moon's surface was dominated by impact or volcanic features. On the moon he came down on the impact side. He was the first man to do that. Harold Urey was a great admirer of his for that. This place which we call Meteor Crater was called Coon Buttes when he looked at it.

COHEN: Coon Buttes?

SILVER: Okay.

COHEN: I have been there.

SILVER: He felt he couldn't really make a definitive scientific argument on what he suspected. He wrote a wonderful paper, because it's clear he was bending over backwards not to let his prejudices show. He did a very cool evaluation of the thing. Gene loved that. And of course, ultimately it was Gene Shoemaker and his colleagues who found the definitive proof that it was an impact feature. Nobody ever found the meteor itself, although there were lots of efforts to drill and do gravity surveys and all kinds of other things there. But Gene and a couple of his colleagues established the presence of a mineral, which had only been formulated in the laboratory as a compound, that had the same chemistry as quartz, SiO_2, but it was a high-pressure polymorph. And the laboratory studies showed that it required extreme pressures to form. And he found that this material, which was called—well, there were two polymorphs. Coesite and stishovite were to be found in the sandstone which makes the ridges on the outside of the crater, and which was thrown back in the impact from the [meteor]. And that proved that it was an impact. Well, that was something that we all admired terrifically. We all thought it was great stuff. And that's one of the things which brought him back to Caltech, first as a visiting professor, then as professor and chair. And then, after he stepped down from the chair, later on he did some wonderful things. But he did them in Flagstaff. Now, he had gone to Flagstaff to establish the Survey's astrogeology branch—a nucleus of scientists who were planetary scientists who were studying the surface of the moon, in particular in preparation for Apollo, but doing many other things. And Gene was the first principal investigator on the interpretation of the Ranger photographs, the Orbiter photographs, and the Surveyor unmanned landings. Well, in the course of all that, I was very interested in what he was doing. I had been on the Colorado Plateau with him. I was interested in all of his work in planetary science. I was working on meteorites. So there was a crossover in there. So Gene and I became increasingly friendly.

COHEN: That was while he was here or after he got back?

SILVER: That's while he was here and after he went back. And of course, I mentioned earlier that it was Gene Shoemaker who persuaded me to undertake astronaut training. But the main story [is that] Gene organized a wonderful expedition to go down the Colorado River from Green River, Wyoming, all the way to Pearce Ferry. This took him three months. He did it in three big legs. And he would bring geologists who were familiar with the various legs of this journey.

COHEN: Now, this was for scientific, educational purposes?

SILVER: No, it had a specific objective.

COHEN: I mean, it wasn't raising money, like your other trips?

SILVER: No, it wasn't like raising money. It was doing science. It was done in 1968. On the second expedition of the exploration of the Grand Canyon by Major John Wesley Powell, John Wesley Powell had a photographer with him.

COHEN: Now, was Gene essentially repeating this Powell trip?

SILVER: Yes. He was not only repeating the Powell trip, but he organized a group which included a first-rate camera man who put together a camera with exactly the same optical properties—focal lengths, fields of view, what have you—as one of John Wesley Powell's photographers, Beaman, who was very good. And the idea was to try as closely as possible to reoccupy the sites from which about 140 or so—not exact—magnificent photographs had been taken by Powell's photographer. This would have been something like ninety-seven or ninety-eight years later. And the question is, what could you learn about what had happened to the terrains which were visible in these places? What could you say about the way in which the river did its work? How closely could you do this? And in the course of doing this, he got me to go down the river with him, because I had been working on the old rocks at the bottom of the Grand Canyon.

COHEN: Now, who was supporting all this?

SILVER: The US Geological Survey and me and Gene. I mean, it was minimally supported. In fact, there was a key group that Gene conned into running the river with him. This was a group called the John Wesley Powell Society. And the John Wesley Powell Society was comprised of people in the Denver area who loved to do white-water rivers.

COHEN: As an adventure?

SILVER: As an adventure. And these people were led by a group of several pathologists who worked in the Denver city and county morgues and what have you.

COHEN: [Laughter] These stories are always so weird. Yes?

SILVER: They're kind of interesting, aren't they?

COHEN: It's wonderful. Yes.

SILVER: They're superior people. And then a number of US Geological Survey people who loved to run white water. So I ran the river with Gene. And that was a really wonderful occasion. I'm still getting to a key point before we get to the Sipapu. And of course, in 1969 when the Apollo 11 landing occurred, Gene was the principal investigator for the surface experiments. He had not only been the principal investigator, but he had been seconded by the Survey to provide scientific guidance for Apollo. He was the number one science man for Apollo. Gene was not too happy with the way in which Apollo management was responding. After Apollo 11, he stepped down from that job, and that's when we got him [at Caltech].

COHEN: Did he step down because he was unhappy with how it was going?

SILVER: That was how it was going—with the idea that NASA really wasn't interested in the science. Let me give them the benefit of the doubt and say that Gene, who had always wanted to be an Apollo astronaut—and who would have been if he hadn't had a very serious kidney ailment—was impatient. NASA's number one obligation was to get the people there and get them back. And requests to organize the science design in the beginning were not very encouraging for Gene. I'm not the only one who can talk to that; there are many people who were more directly involved. I was not involved at that time. And Gene did some heroic things. Again, I don't know what I've said in the past interviews.

COHEN: We didn't talk about Gene very much.

SILVER: Well, let me talk about Gene a little bit [more]. He'd had to suffer through the failure of the Ranger series, which we operated out of JPL. We didn't get a good Ranger flight until

Ranger 7. They blew up on the pad, they misfired one way or another, they lost control. Finally on Ranger 7 we got pictures going into the lunar surface. And the design of the Ranger was impact and be destroyed. And we got all the photography that we could from things that had been videoed back to us. They were the first close-up pictures. And then things got better and better—again, largely from planning that Gene had implemented long before he quit. Well, two Caltech grad students and one Harvard grad student who had gone to work with Gene—two of whom had been students of mine, and one of whom had been a joint student with Gene supervising, when Gene was here—all applied to be astronauts and got accepted for astronaut training.

COHEN: They all got to be astronauts?

SILVER: To a point. And then slowly they failed. And the only one who hung on was Jack Schmitt [Harrison Schmitt]. And that requires so much more discussion. But Gene and I had had great fun supervising jointly. We supervised a guy by the name of Tom McGetchin, who so much wanted to be [an astronaut], but he had a physical deficiency. And the other guy was Mike Duke, who also came up with an inability to handle the Gs that were necessary in the test system.

COHEN: Now, these were Caltech students?

SILVER: These had been Caltech students. But understand that Gene had hired them to work for him in the astrogeology branch. And they were surrogates for him. It's one of the qualities of Gene. There's going to be a biography on Gene that's being done by a guy by the name of David Levy [as in] Shoemaker-Levy [the comet]. And I sicked him on Joe Kirschvink, because Joe was one of those completely captured by this wonderful man. But so were these students. The fact that Gene and I supervised together had made a connection. Jack Schmitt—who did a great deal to improve the science on the Apollo missions—and Gene got to discussing it. And they asked me if I would be willing, if asked, to work with starting mission geology training for the forthcoming crews. And this request came at about the time that Apollo 11 landed. I said that Gene was a hero. The available photography was so poor that they didn't know the details of the landing sites very well. When Neil Armstrong was approaching the lunar surface in the landing module, he recognized that the targeted area was just as bouldery as it could be, with boulders probably about the size of the landing module. He couldn't make a landing there. And he took over manual control of that. And then *he* did a heroic thing, Shirley. He kept dancing around above the surface, manually guiding the module. When he landed, it is reported, he had somewhere between fifteen and thirty seconds of fuel left for the descent. But it was landed. The success was famous. There was just one difficulty. The Houston control room and the science back rooms, which operate on the periphery, or perimeter, did not know where Neil was. So Gene, who was running the surface geology experiment, worked like hell for several hours and finally figured it out. So he found him. That was vital to reprogramming the ascent. And I can't give you the details of it, but I've been told over and over again by the people who were there, with whom I subsequently worked very closely, that it was Gene's finding him that was a critical aspect. And that never became part of the general story. They didn't want to admit that they lost him.

COHEN: Ah, of course.

SILVER: But that's Gene. In his science, Gene always came through with solutions where nobody else could see solutions. And he was a great scientist. Well, later on Gene was still doing some kind of consulting. While he was here, I talked him into writing a general paper. He was willing to do it, but he hated writing. It was a general paper on impact phenomena and what it meant for the samples that came from Apollo 11. Many of my colleagues could not appreciate that those samples came from all over the moon. In a small planet with a small gravity field and no atmosphere, when an impact of any size occurred, almost certainly some of that material flew around the moon, either out of the gravitational field or brought back on down. And if you look at the history of impact on there, the regolith—a word which nobody appreciated till Gene really defined it.

COHEN: Regolith?

SILVER: Regolith. That's the debris layer on the upper surface that had been hammered out of all the rocks which underlay it. And as I say, not only hammered out of the rocks that were directly below, but they could have come from anywhere. Gene wrote a very important paper on that which set the tone for our later understanding of sampling and everything else. And Gene, in the course of his time as a science advisor, helped establish all kinds of sampling and photographic protocols which were right on the money and which I subsequently used.

Anyway, they asked me to take over, and I started doing the [astronaut] training.

Now we're coming to the period from 1974 to 1976 when we raised the money for the Robert P. Sharp professorship. "Three trips of a lifetime," we called them, "down the Grand Canyon."

COHEN: Right. You talked about that.

SILVER: On the second trip, Gene and I thought to ourselves, "Well, for lunch we're going to tie up at the junction of the Little Colorado River and the Grand Colorado." We had both heard about this place called the Sipapu.

COHEN: That's an Indian name, I'm sure.

SILVER: Yes. But it's a name that the Pueblo Indians use, and particularly the Hopis. Now, Shirley, do you know what a *kiva* is?

COHEN: Yes.

SILVER: And you've got this image of a *kiva* with a hole in the roof and a ladder sticking out?

COHEN: Yes.

SILVER: That is symbolic of the Sipapu. And the *kiva* is a place only for rituals and for religious ceremonies of one kind or another. It's not a common living place. It holds the secrets of every clan. Every clan has its own Sipapu system and what have you. But the Hopis named these things as the Sipapu, after a place on the Little Colorado River. When they made their annual trips from the Hopi villages to get salt on the edge of the Colorado River, they came to the Little Colorado, down the Little Colorado, and then crossed over to the salt. This is incidental. It forms as little stalactites, coming out of the Cambrian sandstone at the base of the canyon. It's being leeched out in the dry desert climate. So they used to go down and collect these little stalactites of salt and bring them back. But they found the place that they were sure—or maybe they knew of the place so that they were sure—was the place where the Hopi people emerged. Now, you have to know that the myths and histories of the origins and creation of the Hopi people have the modern Hopis living in a fourth world—the highest of four worlds—that the people had come to. And they had moved, by various elements of their mythology, up to it. The Sipapu was where they emerged into the fourth world.

COHEN: Where these rivers came together?

SILVER: Not where the rivers came together. It's on the Little Colorado River, seven miles upstream. Have you ever done the Colorado River?

COHEN: No.

SILVER: You've got to do it while you're still young enough. It's a fabulous experience. There's no better place to study stars, and I'll tell that to Marshall [Marshall Cohen].

COHEN: Did you know this story before then?

SILVER: Yes, we knew this story. We knew where it was. There's no trail to it, except for the Hopi trail. And that's a trail that we wouldn't take. So Gene and I determined that we'd walk upstream.

COHEN: And are the Hopis still around there?

SILVER: No. The Hopis only go by this place. They live about sixty to eighty miles southeast of the Sipapu.

COHEN: But they would still come to this spot.

SILVER: Oh, yes. They were still collecting salt there. The salt that they collect now is for religious ceremonies. It's a special salt, kind of a kosher salt. [Laughter]

COHEN: Yes. And they come these miles to get this salt.

SILVER: Yes. So Gene and I both heard of it, and we wanted to see the Sipapu.

COHEN: Of course.

SILVER: I had started working on the Colorado Plateau way back in the forties. Gene had worked on the Colorado Plateau. We loved the Colorado Plateau.

COHEN: So you knew all these Indian stories.

SILVER: We knew all these Indian stories. Gene knew them far better than I did, because he'd done far more work there. I had worked in other places. Now, the Little Colorado River is a gorgeous river. It differs from the main Colorado [River]. The main Colorado is muddy. But with the building of the Grand Canyon dam, it's not as muddy as it used to be. It's sort of a dark greenish-brown. It's got some translucence to it, at least. The Little Colorado, when it isn't raining upstream, is the most wonderful milky blue. And it's a milky blue because, in the lower part of the river, there are a number of springs which are charged with CO_2 and calcium carbonate. There's so much in there that as the springs discharge into the river and the CO_2 is released, it forces the precipitation of extremely minute crystals of calcium carbonate. And you get a Tyndall effect, just like the sky—it's blue. And it's gorgeous. Not only do you get a Tyndall effect, but over and over again where there's turbulence, which causes the release of CO_2, you get little natural dams of travertine. Now, if you want to know what travertine is, it's on the floor of the Athenaeum.

COHEN: It's a kind of marble.

SILVER: It's a kind of marble. And if you've been to the Three Fountains, that's travertine. Which is why it's on the floor of the Athenaeum.

COHEN: Right. I'm thinking of destroyed plumbing in San Diego from all those carbonates from the Colorado River. [Laughter]

SILVER: Well, that's exactly what it does. It does destroy plumbing. Okay. So we had a topo map. And on the topo map there was a feature that was labeled "Sipapu" by the topographic mappers of the US Geological Survey many, many years before. So we knew how far we had to go. So Gene and I were on the second trip, and people wanted to look at the blue water and frolic in the blue water, because it was cleaner than the river and all that other stuff. So Gene and I started walking upstream.

COHEN: Was it 100 degrees? How hot was it?

SILVER: Hotter. It was probably between 100 and 110, but that's no problem.

COHEN: The water's cool.

SILVER: The water's cool, and all that. But we were walking up, finding our way through the bushes and all the other stuff, because right along the river's edge, both in the Grand Canyon and at the Little Colorado River, there's a suite of plants—shrubs which include willows.

COHEN: How about snakes?

SILVER: There are snakes. There are snakes everywhere, lots of snakes in there. He and I walked up, and it got hotter and hotter. So we started stripping down. And I took pictures of the natural dams that were about that high that were backing up the water. They formed from the precipitation of travertine. And I took pictures of Gene. Finally we got up to this place, and it was an astonishing feature, probably longer than from this wall to the far wall over there.

COHEN: So was that thirty or forty feet?

SILVER: Probably more like sixty or seventy feet. Eighty feet even. And the feature was as wide as this room. It kind of tapered.

COHEN: Okay. Let's get a width.

SILVER: This is probably twenty to thirty feet—somewhere in there.

COHEN: We are talking about the Archives' rare book room. [Laughter]

SILVER: Yes. And Gene and I by that time were so hot that we had stripped off all of our clothes. We were in our tennies, our river shoes.

COHEN: Snakes?

SILVER: We had lived with snakes for a long time.

COHEN: Okay. [Laughter]

SILVER: So we were in our tennies. And this feature was so striking that, from a geological point of view, it was something of an unexplained mystery. It probably can't be rationalized.

COHEN: When you say "feature," do you mean a dam-like thing?

SILVER: No, no. Take a look at a blue whale.

COHEN: Okay.

SILVER: Imagine the shape of the blue whale without the fins at the end, but with a blunt head.

And that blunt head was right on the river. The river water was, in fact, eroding the blunt head. And the tail extended back, away from the river, toward a series of strata that just marched upward.

COHEN: This is a rock…

SILVER: Formation—a travertine formation in a long mound. Give me your paper there, Shirley.

COHEN: Well, we've got to describe it. It's on the tape.

SILVER: Okay. It's a long mound which was about fifteen feet high and twenty to thirty feet in transverse dimension, tapering with slopes on all sides. The head was blunt like a whale's head, to the extent that the river was eroding simultaneously on this thing. And the tail tapered very nicely back some distance. When we climbed up on top we were astonished to see that there was a hole on top. This was the orifice through which the Hopis believed they came out. But when we looked in the hole, down about four feet…

COHEN: How big was the hole?

SILVER: The hole was about—I'd have to guess, five feet in diameter.

COHEN: Okay. So we're talking about something big.

SILVER: A big hole on top of this big feature. And in the hole, about four feet down, was sort of an oily-looking, roiling green spring.

COHEN: And it was coming up?

SILVER: It was up to a level four feet below the surface of this mound. Or five feet, or whatever it was—I can't remember exactly. So Gene and I wanted to try that water. Now, what was mysterious about it was that that water level was at least twelve feet higher than the river right there. What was keeping that water at that level? Why didn't it just run out at the blunt head where the river was undercutting it? So it had a pressure head which was able to sustain that level. When we first looked at it we thought it was going to be hot, because it was moving all the time. So what were we going to do to get to testing it? It was pretty damned hard for me to hold him by his ankles while he went down to feel it, and vice versa.

COHEN: You didn't have a pail with you, or anything?

SILVER: What we had with us is the reason why Ed [Stolper] wants this recorded.

COHEN: Oh, Okay.

SILVER: What we had with us was a one-gallon canteen, supplied to the US Geological Survey from the warehouses of the US Forest Service, with a bright orange cover. By Gene's taking his belt and me taking my belt and using the straps on the canteen, we were able to lower the canteen down into the spring and pull up a full canteen.

COHEN: Weren't you worried about the spirits of the Hopi getting you?

SILVER: Gene and I were interested in the scientific phenomenology. First of all, the Hopi spirits—the *kachinas*—knew that we respected who they were and what they meant. So we brought this thing up and, lo and behold, it was cold. So what we were seeing as roiling water had an internal circulation system—driven by what, we don't know. So Gene and I looked at it and tasted it. It wasn't very good. It was loaded with salts—calcium carbonate, sodium chloride, and the other stuff. But we decided that it was so magical that we just put the cap back on that canteen and carried it back down the river.

COHEN: Did you take some pictures?

SILVER: Oh, I did. Shirley, I have been looking for those pictures. I had them as recently as several years ago, and they're around somewhere, but I have moved so much lately. But I have a picture of Gene that I want to show Carolyn Shoemaker. She's never seen it. He's stark naked, except for his six-inch tennies. But he's far enough away and served as the scale for this larger feature. We brought that back. On that river trip with us was the then chairman of the division [Barclay Kamb]. So we thought we'd have a little fun. And that night, where we camped on one of the beaches along the Big Colorado, we made a presentation of our [discovery] to Barclay. That canteen, because it is a rare geological and geochemical sample, has been in the office of the chair ever since Barclay was chair. Ed had never heard this story, except that when he took over the chairmanship I told him to take very good care of this [because] it was not easily replaced, it was very special, and all the other stuff. So recently we were sitting in the Athenaeum having lunch and I mentioned this story to them. And he said, "That's a good story.

If that canteen's going to be with us, it should be recorded." I didn't know he was going to call you.

COHEN: Okay. Well, evidently he told this to Judy [Goodstein] or something.

SILVER: Well, what do you think?

COHEN: Do you think it could have been carbon dioxide that kept that water going like that?

SILVER: It wasn't effervescent.

COHEN: Yes. But there was enough pressure, maybe, coming up.

SILVER: Oh, that's quite true. And what we had no sense for was what the shape of the orifice was at depth. And it might be rather tortuous. Do you know what I mean?

COHEN: Yes.

SILVER: If you go to any deep-seated spring—you've been to Yellowstone and other places?

COHEN: Right.

SILVER: But this wasn't driven by thermal circulation.

COHEN: I see. Now, is there any written record of this anywhere that you know of?

SILVER: No. I haven't run across it. I mean, record of the Sipapu.

COHEN: Yes.

SILVER: Every clan of the Hopi calls that opening in their *kiva* the Sipapu.

COHEN: This specific one?

SILVER: No, no. The hole in any *kiva*. And not only they, but almost all the other people who share a language with them, call it the Sipapu. So Sipapu as a term applied to the holy entrance to the holy *kiva* is quite widely used.

COHEN: Okay. Now, this particular location—is that only a specific tribe, or is that—

SILVER: For the Hopis in particular.

COHEN: For all the Hopis.

SILVER: That's all I know about, Shirley.

COHEN: I see.

SILVER: I haven't really investigated. Nowadays not many of their people make this great religious—what will we say?

COHEN: Pilgrimage?

SILVER: Pilgrimage is the best word.

COHEN: Now, did you see salt [deposits] while you were there?

SILVER: Oh, yes. We've seen those. I've done so much work in the Grand Canyon. That's a very common feature.

COHEN: I see. So that could be almost anywhere.

SILVER: Well, not almost anywhere. It requires that you be at a place where the local groundwater is continuously traveling through the sandstone, leaching out salt, and then evaporating in the desert environment and making these deposits. And it's most conspicuous right there at the junction. The sandstone is the Tapeats sandstone.

COHEN: Well, that's really fascinating. What an adventure!

SILVER: Oh, yes. Well, Gene and I had a number of adventures together.