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DON L. ANDERSON
(1933–2014)

INTERVIEWED BY
SHIRLEY K. COHEN

February–April 1999

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
Geology, seismology, geophysics

Abstract
An interview in three sessions with Don L. Anderson, Eleanor and John R. McMillan Professor of Geophysics. Integrating seismology, solid state physics and geochemistry, Anderson is recognized for his work on the origin, evolution, structure and composition of Earth and other planets. He is a member of the National Academy of Arts and Sciences and the National Academy of Sciences, and has won numerous awards including the National Medal of Science (1998), the Crafoord Prize (1998) and the Gold Medal of the Royal Astronomical Society (1988).

Conducted by Shirley Cohen, the interview covers many aspects of Anderson's personal and professional life from his childhood onwards. Session 1 includes discussion of Anderson's education in Maryland and his early interest in geology. He reminisces about his time in the air force, including his research on the properties of polar ice, as well as his subsequent work for Chevron. Anderson also discusses the circumstances enabling him to come to Caltech and the difficult living conditions in California; he recalls his graduate work with Frank Press and

reminisces about Caltech faculty, including Arden Albee, Robert Sharp, C. Hewitt Dix, Charles Richter, and Gerald Wasserburg. The second session continues with Anderson's various appointments at the institute and the culture of the Seismology Laboratory in the San Rafael hills. He discusses his work on floating anisotropic plates and other geophysical research, as well as the attempt to maintain the collegial atmosphere of the seismo lab with its move to campus. The final session includes further reminiscences of Charles Richter and Anderson's attempt to understand the Earth's mantle with respect to geochemistry and helium 3; and recent research in surface geology, bathymetry, and plate boundaries. The interview concludes with the events surrounding the Crafoord Prize and the President's medal, along with Anderson's research philosophy and the importance of Caltech seismology.

Administrative information

Access

The interview is unrestricted.

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Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)793-8756
Email: archives@caltech.edu

In 1964 the National Science Foundation awarded Caltech over $400,000 for an intensive study of the San Andreas Fault. Some members of the investigative team pore over a map of what appears to be San Francisco. From left to right: Barclay Kamb, Stewart W. Smith, Don L. Anderson, and Frank Press. Photo by James McClanahan, courtesy of *Engineering & Science*, November 1964.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH DON L. ANDERSON

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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COHEN: Could we start this interview with you telling us a little bit about your family background: your parents, where you were born, what your parents did, where you went to school?

ANDERSON: I was born in Frederick, Maryland, in 1933. I have an older sister who is two years older and an older brother who is four years older. They are still living. We moved around quite a bit when I was very small. I remember living in Hagerstown and, I think, Cumberland and Chicago. We ended up in Baltimore, probably when I was about six years old or so. I did go to kindergarten or pre-kindergarten in Chicago. My mother was a schoolteacher and my father was an electrician.

COHEN: So how did that make them move so often?

ANDERSON: He worked for, I think, Edison Company, and they transferred him around quite a bit until finally he became a purchasing agent for Edison in Baltimore, Maryland. I went almost all the way through school, except for that little pre-kindergarten in Chicago, in the Baltimore public school system. I lived in northwest Baltimore. My sister, brother, and their families still live in Upperco, Baltimore and Randallstown, Maryland. I have quite a few cousins, nephews, and nieces that I'm fond of and visit them once a year or sometimes once every two years.

COHEN: But you have no southern accent.

ANDERSON: Well, I'm sure I've got a Baltimore accent. I've got some kind of a strange accent. But I've been away for a long, long time. I went to college in upper New York State right after I graduated from high school. So I essentially left the Baltimore area when I was seventeen.

COHEN: How did you find the school? I know there was a very good high school, Baltimore City College, I believe it was called.

ANDERSON: Well, I went to Baltimore Polytechnic Institute, BPI, which was the competitor of Baltimore City College. The liberal arts people went to City and the technical people went to BPI. They were the two, what you would call today, magnet schools and one had to work pretty hard to get into them. My brother went to Baltimore Poly four years before I did. BPI had a series of options, one of which was to get a year's worth of college under your belt before going to college. I didn't take that particular program. I went straight into the freshman year in college. But a lot of my friends in high school were taking the advanced courses. They got into college in the second year. It turns out that was not a good idea, so my friends who did that didn't make it, because that freshman orientation year is extremely important.

COHEN: We cautioned our kids against taking credit for the advanced placement tests.

ANDERSON: I would not recommend that. The socializing that first year is extremely important. To throw a young kid into college in his second year is not a good idea.

COHEN: And you don't do in high school what you do in college, no matter what.

ANDERSON: Exactly right.

COHEN: So what made you go to Troy, to RPI [Rensselaer Polytechnic Institute]?

ANDERSON: Well, that's a funny story. I was pretty poor, and needed a fellowship or a scholarship to go to college. I was interested in geology and collected minerals when I was a kid. There was a young man that taught at a local college then, and he very kindly taught mineral courses and took kids on field trips. So I actually toured over a large part of the eastern US collecting minerals when I was in high school.

COHEN: And this was your own interest? It wasn't pushed by your parents?

ANDERSON: No. It was my own interest. I was a Boy Scout also. I liked that kind of living. I became quite a mineral collector. At the time they were putting in a big water tunnel; that meant digging through the geology of Baltimore County. They went through a lot of pegmatite dikes and were bringing out just enormous crystals. I was the first one on the scene to collect some of these gorgeous crystals, so I had a very good mineral collection. I was also interested in other kinds of things. So I applied to about four different colleges, and each one was for a different major: geology at RPI, materials science, I think, at MIT, applied mechanics or something at Columbia—I forget; also Lehigh.

COHEN: And you did this all on your own? You didn't have some special high school teacher?

ANDERSON: I pretty much was on my own as I remember. I really needed the money, and the best scholarship I got was from Troy, New York. Rensselaer was a full-tuition scholarship, so that allowed me to go off to college. The others, I think, weren't quite full tuition. I literally picked the college and picked the field at the same time. If I had been given a better fellowship at a different school, I would have done something else.

COHEN: That's very interesting. What did you apply for at MIT? What would you have been?

ANDERSON: I think that I probably would have been a material scientist if I had gone to MIT. Another reason I picked RPI in the first place—I didn't know anything about it, except that it was a long way away—was that it was in a country town in upstate New York, and I was getting tired of the big city life in Baltimore. Baltimore was a grimy town at this time. Little did I know that Troy, New York, is about the grimiest little industrial town you can possibly imagine. It was quite a shock when I got there. It's still a fairly dismal town, but it's a fun place to go to college. It's not a big city, and it's not a New England type of campus.

COHEN: We lived in Ithaca for a long time, so I sort of know Troy.

ANDERSON: So that's how I got to RPI.

COHEN: And it was a good education?

ANDERSON: Yes, in fact it was unique. It was a good undergraduate technical college.

COHEN: There were just men at that college. Is that right?

ANDERSON: There were a couple of women. One, I remember, was an architect student. But, yes, it was basically men. In fact, while I was there I became active in the student government and newspaper. We needed a mascot, so I ran a contest to find one—the "Bachelors." We had a little cartoon character that went along with it. It was a good school because they didn't have a lot of graduate programs, so there was a very small number of graduate students. The people there were dedicated to undergraduate education. That's quite different from the schools that have a big graduate research program. They really, I think, gave a good undergraduate education. And I know that I took lots and lots of courses, a lot more than people tend to take nowadays. I'm a geophysicist, but I took an enormous number of geology courses. I was taking two or three geology courses every year, along with math and physics and engineering type courses. I think it was a good education.

COHEN: It was good. And then you proceeded to then want to go on to graduate work?

ANDERSON: No, going to graduate school was the furthest thing from my mind. I was in ROTC at college because I needed extra money, and was committed to go into the Air Force for at least two years after I graduated.

COHEN: I see. Because I have this here: "geophysicist with the US Air Force, '56 to '58."

ANDERSON: What happened was I had a commission when I graduated. I was a second lieutenant. I forgot to mention that I went to the University of Maryland for a quarter or a semester in between graduating from BPI and going to RPI. In Baltimore at the time they had two graduations a year, and I graduated mid-year, in January I guess it was. It was during the Korean War, and my mother insisted that I get into college as soon as possible to try to get deferred. So I went into the University of Maryland just as a temporary situation. So I took chemistry for a semester, and was in the Army ROTC. I switched to the Air Force at RPI.

COHEN: Where is the university? It's not in Baltimore.

ANDERSON: It's in College Park, Maryland. It's mid-way between Baltimore and the Greenbelt area, Goddard Space Flight Center. I used to hitch-hike there and back.

COHEN: Right. I don't connect it with Baltimore, but that's okay.

ANDERSON: So I went there. And it was a state school. You could literally walk up and apply and get in right away. In fact, I convinced one of my classmates who had a car to drive me, and we registered and became students.

COHEN: That afternoon?

ANDERSON: Yes. But that was just a short stay. It convinced me that I didn't want to be a chemist, just taking a semester's worth of chemistry. But then at RPI I had this commission, but I also wanted to work for an oil company for a while to see what that was like. So I was able to postpone my active duty and work for Chevron Oil Company for a year in Montana, Wyoming, and California. I was on what was called a doodlebug crew. That's seismic exploration for oil. The first assignment was in Cut Bank, Montana, which is right outside Glacier Park. That was fine until the winter came. If you ever read the paper in the wintertime, you might notice that Cut Bank, Montana, is usually one of the coldest places. It's very cold, which is interesting, because I later went to northern Greenland, and it was not nearly as cold there. I had better clothes, of course, when I went to Greenland.

COHEN: Did you enjoy that kind of work? I mean, it's sort of rough and outside.

ANDERSON: Northern Montana is beautiful country. My classmates all went to more classical oil company type jobs in Texas and Oklahoma and places like that. Montana was not known for its oil at the time—this was the initial exploration in 1955-1956 to see if the region had any production possibility. The way these crews work is they move around all the time. Most of the crew live in trailers that are towed from place to place. I only worked for Chevron for a year, but I was in Cut Bank, Montana; Glendive, Montana; Riverton, Wyoming; Santa Paula, California; and to Oildale, California, and then back to Santa Paula. So I moved about six times in the course of a year and saw a lot of country.

COHEN: Did they pay well?

ANDERSON: Well, they paid $400 a month, and that was kind of my goal in life, to make $100 a week, and that's what they paid.

COHEN: At that time.

ANDERSON: I thought I was doing pretty well. I worked for a year, but then I had my Air Force obligation.

COHEN: You still had to do that.

ANDERSON: I got assigned to Air Force Cambridge Research Center in Cambridge, Massachusetts. Actually it was in Bedford, Massachusetts, but it started in Cambridge. The reason I got that was because, when you are filling out a form when you get your commission, you tell what kinds of places you like to go and the kinds of things you like to do. I realized that the Air Force didn't know what to do with some of my geological colleagues so they put them in charge of gas stations and oil dumps, where they had the oil petroleum supplies. It has nothing to do with geology, but they thought petroleum maybe had some relationship. I knew there was an Air Force Cambridge Geophysical Research Center and specifically requested that. I got married just after I went there.

COHEN: Was this somebody from Baltimore?

ANDERSON: No, it's somebody I met in college. Nancy, my wife, was brought up in Catskill, New York, and she went to Russell Sage College, which is also in Troy. Her dormitory was right around the block from my fraternity. I lived in a fraternity in downtown Troy. We got engaged before I went to Montana, and got married about eighteen months after I joined Chevron. We were engaged about a year before that, while still in college.

COHEN: But she didn't go with you to Montana?

ANDERSON: No, I was not married when I was working for the oil company. I got married about a year-and-a-half after graduation. I graduated in June, and the following September I got married. And I guess I went into the Air Force in July—roughly a year after I graduated. They sent me to Greenland after I had been married for less than a month.

COHEN: You thought you were going to be in Cambridge.

ANDERSON: I thought I was going to be in Cambridge. They needed somebody to work on sea ice. I was on active duty for two years, and I kept going to Greenland two or three times a year to work on the sea ice, and for several years after that I returned on a part-time basis.

COHEN: It must have been interesting.

ANDERSON: It was fascinating. It was during that time—this was now two or three years after I graduated from Rensselaer—that I decided that I should go to graduate school. I was working on a very difficult scientific problem that required a PhD. I was doing the best I could but some of my colleagues—in particular one called Willy Weeks [Wilford Weeks] who had a PhD in geology from Chicago—convinced me I should do it.

COHEN: I write the names down because they are hard for the transcriber.

ANDERSON: Right. Wilford Weeks. Before I got a PhD we went to Greenland together and worked on the properties of sea ice. He was the one that convinced me to go back to school. In fact, he was a colleague of Jerry Wasserburg's [Gerald J. Wasserburg].

COHEN: At Chicago?

ANDERSON: Yes, they were at Chicago together. So I applied, again, to several graduate schools, including MIT, Lamont and Caltech.

COHEN: Let's go back to the experience you had in Greenland. This must have been quite invaluable. I mean, that was an education in itself, I would think.

ANDERSON: Yes. My job was to determine how thick the ice had to be to support aircraft that were in trouble. The Air Force wanted their pilots to land disabled planes on the sea ice, but the conventional wisdom at the time was that they would break through the ice and the crew would freeze to death. In fact, the emergency procedures were to destroy the airplane and bail out, so the plane could crash in case the Russians were around. But it turns out that aircraft can land very easily on ice that's not very thick. Even if the ice won't support the plane while it's sitting there, it will allow a plane to taxi long enough for the pilots to get out and then the plane can sink through the ice, or the wheels can poke through the ice. Our job was to study ice strength, and whether you could determine how strong it was before you landed so you would know where to land. This project lasted more than two years, because after I became a graduate student at Caltech I still went up to Greenland for several years. We wrote a bunch of papers on how fast sea ice grows and how strong it is.

COHEN: So this was not secret material? It wasn't classified?

ANDERSON: No, it was not classified. Toward the end of the project we landed fairly heavy aircraft, including a B-52, on the ice with no trouble at all. In fact, the ice was a better runway—this was off Thule, Greenland—than a runway which we made with shovels and things on the base, because it didn't crack and there were no hills. At the Thule Air Force Base runway there was a hump in the middle. It was all cracked up and rough, because the winters there are pretty bad.

COHEN: You were there during the winter, the summer, or other times?

ANDERSON: Just the wintertime. Several times I went up in September, which is early enough to start seeing the ice form. It starts to break up in June or July. That was my field season: from the time the ice starts to the time it breaks up. I wasn't interested in being there when there was no ice, so I was never there during the real summer time. Instead, I was in hot Cambridge.

COHEN: That's interesting that they used you to do something of value.

ANDERSON: It was quite useful. I learned a lot about properties of materials. Sea ice is a very interesting material; it's not like lake ice, because brine freezes in it from the sea water. As a result it has pores and is always draining brine. Plus, the crystals are all oriented, so it's an anisotropic solid. It depends on what direction you send seismic waves through it—it has different properties in different directions. It turns out that was very valuable, because the mantle, which I have spent the rest of my career working on, is very similar. It's partially molten and it's anisotropic. It is a rock with phase changes. It requires thermodynamics to understand.

COHEN: Okay. So then you finished up your two years and knew you had to go, or wanted to go, to graduate school.

ANDERSON: Yes. I got convinced that that's what I should do. It was never in my plans in my earlier education. It wasn't until just a year or so before I applied that I decided that was the next step. It was not my intention ever to go to graduate school. So I applied to MIT, Columbia, and

Caltech—MIT because I was near there. I had a trailer, anticipating the possibility that I might move someplace.

COHEN: You were living in a trailer?

ANDERSON: I was living in a trailer park near the Back Bay area of Boston, on the river.

COHEN: It sounds very nice.

ANDERSON: Initially I had an apartment. Then about a year into my tour I started thinking I might go to graduate school, but I didn't know where.

COHEN: And you were married at this time.

ANDERSON: I was married. We had a little daughter. While I was going back and forth to Greenland, my daughter was born. I was gone a lot of the time during the early parts of my marriage and my daughter's growing up. We bought a trailer and lived in a trailer park, because I said, "If I go to Caltech or to Columbia, I'll have a portable house."

COHEN: Did anybody counsel you about which would be good graduate schools to go to?

ANDERSON: Not really.

COHEN: I mean, MIT I can understand—you were there.

ANDERSON: I had to figure it out myself. I had now been out of school for three years, so I knew somewhat about who was doing what and what schools were doing what and what the various reputations were. I applied to MIT, because that would have been the easiest thing—to just stay where I was—but I really wanted to go to Caltech, because there were ice people here. There was Bob Sharp [Robert P. Sharp]. There were geophysical exploration people here. There were other seismologists I knew about: Frank Press and Charlie Richter [Charles Francis

Richter]. So I knew all those names. At Columbia I knew a lot of people, because I was a contract administrator for some of the research at Lamont [Lamont-Doherty Earth Observatory, Columbia University].

COHEN: While you were in the Air Force?

ANDERSON: While I was in the Air Force. They were doing some experimental work and I was there as contract monitor. In fact I did some experiments at Columbia so I knew them pretty well. And MIT—I took a math course there. I also worked with David Kingery, a ceramitist and metallurgist.

COHEN: Had you visited Caltech when you were out here doing your oil exploration? Because you said you were in Santa Paula.

ANDERSON: I never visited when I was with the oil company. I probably didn't even know about Caltech then. But by the time I was in the Air Force, the colonel, who was my boss was a Stanford graduate. He sent me back to Stanford to give a talk on sea ice when I was still just a young bachelor degree scientist. I didn't have any advanced degrees then. He also sent me to San Diego to work at the naval electronics lab. At that time I knew I was interested in Caltech. I had a two-hour layover at the airport, so I rented a car and drove to Caltech, not knowing anything about the distances involved. I managed to get here, but I couldn't figure out how to get into the geology building. You still had the front of the original building facing out to California, but it was locked then as now. It's never been an entrance. I couldn't figure out how to get in and I had to get back to the airport, so that was my visit to campus, just walking up to the front door which was—

COHEN: Which was locked.

ANDERSON: It was the entrance to the museum, which had some offices in it. It's where the main administration was. They still have minerals in display cases. As far as I can remember, they never used that particular entrance. I didn't have time to go around the building and find out how to get in. So, no, I didn't really visit. I just barely got back to the airport in time to get my plane. I was pretty cold as far as what Caltech was all about.

COHEN: But they did accept you right away here. So you already had some sort of reputation.

ANDERSON: They might have accepted me at that point. I can't remember when exactly that was. But, yes, I got accepted at all three of the schools, and I got some financial aid. The aid that I got from Caltech was good but not quite enough to allow me to transport my family across the country. I was still pretty poor. But just before I had to make a decision—oh, I remember what happened. I was in Greenland, and I told Nancy at the time that I was expecting these letters from these various places that I'd have to accept. I said, "If this happens, then you do this, and if this happens you do that," and so on. She had a bunch of decisions she had to make, because I was out of touch. But then there was also the NSF [National Science Foundation] fellowship, that I applied for. If I got that, then the equation would change. So it was a very complicated thing; she was very nervous about it.

COHEN: But she had to do all that.

ANDERSON: She had to do all that. So she accepted Columbia because we didn't have enough money to go to Caltech. Then the NSF [grant] came in and that changed the story a little bit, but not quite enough. And then a funny thing happened: my car got rear-ended. I was just sitting with my daughter Lynn while Nancy was in a store and somebody ran into the back of my car. It got appraised, and the insurance agent gave me, I think it was, $150. And then within a week I got rear-ended again in one of the rotaries in Boston. And it was exactly the same place. Probably I got hit because I didn't have any taillights—I had to fix the taillights. So I got another $150 and that made the difference. Three hundred dollars was enough for me to afford to drive across country. So I immediately called up Caltech and said, "I've changed my mind. I will come." I was nervous that I was too late, but they said, "That's okay. You can accept now, even though you've declined, or your wife declined, to come."

COHEN: Now, did you take your trailer across country?

ANDERSON: No. Then we got nervous about carting this trailer cross-country. So I started looking in the newspaper, and I discovered an ad—somebody wanted to repossess a trailer that had been sold in Boston and taken to California. They wanted somebody to go back and get it. And I said, “I wonder if you’d be willing to trade it. I’ve got a trailer here and you have a trailer out there, so maybe I can buy that one out there and we can trade it.” They felt that was a good idea. So I traded in my trailer for the one in California, sight unseen.

COHEN: I was going to say, “Had you seen it?”

ANDERSON: I had never seen it.

COHEN: And it was in Los Angeles somewhere?

ANDERSON: Yes, it was in Torrance. So I managed to find a trailer park and managed to find somebody that could tow it for me. Unfortunately there was no registration, and somebody had stolen the wheels.

COHEN: One wonders how one copes with that stuff.

ANDERSON: So I got this guy to pull this trailer, but he was very nervous because it wasn’t registered, and he could have gotten in trouble. So we stopped at the first motor vehicle—

COHEN: So did you drive across the country then?

ANDERSON: So we drove across.

COHEN: In your twice-rear-ended car?

ANDERSON: Yes. We drove across the country. We threw away the back seat and packed the back seat as if it were a suitcase.

COHEN: And you had this baby.

ANDERSON: Everything we owned was in this car. It was packed up to about this level and our baby was on top. We just perched our baby in a little pen on top of all that.

COHEN: Before car-seat ages.

ANDERSON: Yes. She was in a crib. So we got this trailer. Here's another funny story. We kept stopping at motor vehicle places. It was an enormous fee—$200 or something like that. Apparently it was almost arbitrary, so we kept going to different places. We went to about three places. Finally one guy said, "It will cost you $50." So we registered our trailer at that place. Apparently these formulas are somewhat arbitrary. I couldn't afford to pay the registration just to get it towed from one place in LA to another place.

COHEN: So you lived in that trailer here in Pasadena?

ANDERSON: Well, we found a trailer park in Glendora, and I commuted in. At the time that was about a forty-five-minute—almost an hour—commute.

COHEN: That would have been way before the 210 [freeway].

ANDERSON: Way, way, way. And after about a month of that, I got really very tired of that. All this commuting. Then I found out that most of the students were working late at night doing homework. I had been away from school for a long time, so I wasn't in this particular mode. I would just drive home for supper and that would be the end of it. Then I found out that I really had to keep working till midnight at least every night in order to keep up. At the time they had the Geology Club in the evening. And my wife and baby came with me one night to the Geology Club. And she got talking with Professor Noble's wife. Jim Noble was a professor here in the geology department. And he found out that we lived in a trailer. He said, "My son-in-law lived in a trailer when he was a graduate student." His son-in-law was George Shor, who is an oceanographer at Scripps in La Jolla. He said, "There is an old lady that lives at the top of Lake

Avenue at the top of Altadena in an orange grove. He just had his trailer parked in her orange grove. She has people living in the old kennels and in the gatehouse, just trying to make a go of it by having people live around this old estate. It's about a thirteen-acre estate at the top of Lake Avenue." In fact, Brad Sturtevant [Bradford Sturtevant]—also a graduate student at the time—and his family lived in the gatehouse. So Noble drove me up there and we asked this lady if it was still possible to live in a trailer in her orange grove, and she said, "Sure, but you'd better hurry up, because the rainy season's starting." This was September. So real quick I arranged to have somebody drive it in. We parked in her orange grove and ran a water line to the irrigation system and ran an electrical line down the hill to a neighbor who lived in the old gatehouse. There we were, hidden away in this orange grove at the top of Lake. And that's where we lived the whole time I was a graduate student. It was very convenient, just right up the hill.

COHEN: It sounds like you had a long-suffering wife.

ANDERSON: Oh, yes. We had an old washing machine, but no dryer, so we had to wring them out and hang them on the line. It was almost like in the woods. It was an idyllic spot.

COHEN: It's pretty up there.

ANDERSON: Beautiful spot. A little stream was running through, and bridges. She had people living in the barn. After we had our son—our son is four years younger than our daughter, and we had him while I was a graduate student—then we moved into the barn.

COHEN: You graduated from the trailer.

ANDERSON: And the barn was just beautiful. This woman—Marie Kaiser— just had a good eye for redecorating things on a very low budget. We had a good time up there, with Sturtevant in the gatehouse, just above us.

COHEN: So you then started living normal graduate student hours.

ANDERSON: Yes. My wife would pack me a lunch and a dinner, actually, and send me off in the morning. I'd come home after midnight, usually at two or three in the morning. She would literally pack me my two meals and send me off in the morning.

COHEN: So how did you find it here?

ANDERSON: Well, since I had been out of school for so long, it was rough that first year or two getting back into the rigor of it all. Plus, when you're away from mathematics for three years or so—that was really tough. I took a lot of mathematics. I minored in math. That was rough being away so long.

COHEN: Who was your professor here?

ANDERSON: Frank Press was my main advisor. In fact, most of the students that year and the year before were Frank Press students. Frank Press came in about 1956 or 1957. He was hired as a young professor out of Columbia, and he was a student of Maurice Ewing's, one of the more famous geophysicists who was at Columbia at the time. Before that, at Caltech, it was Hugo Benioff, Beno Gutenberg, and Charlie Richter. It was kind of a sleepy place. There weren't many graduate students. There wasn't a lot of research. There weren't big grants or anything like that. And Columbia was sort of a soft-money institute. Lamont Geological Observatory was like an oceanographic institution—it lived on contracts and soft money. So Press brought that particular kind of thinking to Caltech and started bringing in a lot of money and supporting a lot of research. He had a lot of graduate students. The students at that particular time all went out and became very well known.

COHEN: Who were some of your fellow students?

ANDERSON: Oh, Bob Phinney [Robert A. Phinney], who is now a professor at Princeton. Bob Kovach [Robert L. Kovach] who is now a professor at Stanford. Stewart Smith just retired from the University of Washington. Jack Healy [John H. Healy] just retired from the US Geological Survey. Ari Ben-Menahem, who is a theoretical seismologist, has been in Israel in Tel Aviv since that time. [M.] Nafi Toksöz is a professor at MIT. Shelton Alexander is a professor at Penn State, and Charles Archambeau became a professor of physics at the University of Colorado. These are just some of them, but it was an enormous collection of people who then really started modern seismology and diffused it all over the country and all over the world.

COHEN: It sounds like it was very exciting here. You must have been percolating all the time.

ANDERSON: It was pretty exciting. I forgot to mention Armando Cisternas at the University of Chile and then the University of Paris.

COHEN: What was his name?

ANDERSON: Cisternas. He's Chilean who escaped, barely, during the hard times there. There were more. It was a very active group.

COHEN: So this would have been in the early sixties, now. Is that correct?

ANDERSON: Yes, that's correct.

COHEN: When did Harrison Brown come?

ANDERSON: He was here when I got here, I think. I came in 1958, so he must have arrived before '58. And it seems to me he went to the University of Hawaii before I finished my graduate degree.

COHEN: Of course, he brought Patterson [Clair Patterson] with him.

ANDERSON: He was responsible for setting up geochemistry.

COHEN: And then Epstein [Samuel Epstein].

ANDERSON: That's right. He was the father of geochemistry at Caltech.

COHEN: But that was not an interest of yours at that time?

ANDERSON: Not really, not at that time. I was always interested in more than just seismology and certainly more than just earthquakes. I've gotten interested in chemistry since, but at that time, although I took courses from Jerry Wasserburg—

COHEN: He would have been an assistant professor here at that time.

ANDERSON: Yes. And I took courses, I think from Arden Albee, who is a geologist, and from Bob Sharp, of course, and a lot of the other people, including Hewitt Dix, Frank Press, Charlie Richter and Dick Jahns [Richard Jahns]. But I think Jerry Wasserburg's was the only course I took in geochemistry.

COHEN: That was just starting at that time. I mean, that was what Harrison Brown brought in.

ANDERSON: Yes. I guess it was brand new. Jerry was a young, aggressive professor—I remember that—trying to make a name for himself. He, Clarence Allen and Leon Silver all came at the same time, the idea being that only one would stay. They all stayed, and now all of them are in the Academy [National Academy of Sciences].

COHEN: Okay. So you never left.

ANDERSON: No, I never left, although I never intended to stay. I graduated in about three or three and a half years. My PhD took about three and a half years. And I was looking for jobs. I remember looking at Sperry-Rand in Massachusetts. The University of Chicago was interested in me. And Princeton, because Bob Phinney had just gone to Princeton, I guess before I graduated.

COHEN: Now, this would have been as an instructor or as a postdoc or as an assistant professor?

ANDERSON: These would have been as assistant professor.

COHEN: Yes, there weren't many postdocs in those days?

ANDERSON: Well, I was a postdoc for a year at Caltech after I got my PhD. Frank Press asked me if I'd be interested in staying on for a year as a postdoc, and I said, "Yes, but it's not my intent to stay, and that would give me time to look for a job." At that point I still hadn't decided what I was going to do. I wasn't cut out, I didn't think, to be a professor, so I wasn't looking for academic jobs. Some offers came that way, but I don't recall looking for academic jobs or deciding that I wanted to teach. It was just something that evolved.

COHEN: Those were quite good years. There was a lot of opportunity in the sixties.

ANDERSON: Yes, the opportunities were enormous then compared to what they are now. There was a lot of money in research, and departments were building up in geophysics. Geophysics was the big growth industry in universities. That's why most of these people that I mentioned were, and are, in universities. They didn't have too much trouble getting in. Again, Frank Press was well known, and his students were sought after.

COHEN: So you were still in the orange grove then?

ANDERSON: Well, I was in the barn. The first thing that happened when my son was born was that the trailer got kind of cramped. Mrs. Kaiser was the woman who owned this estate, and she knew a part-time carpenter, so we built a little shed on the side of the trailer. So for a year or so we had a trailer plus a shed. It was a room built on to the trailer—leaning against it but otherwise open to the elements. This woman had carpets and drapes all over her estate and gave some to us. In fact, most of our entertaining was up there. We just did it outside. The parties were outside. That was the way graduate students entertained. I don't remember going to movies or restaurants. The whole time it was graduate students entertaining each other. At that time most of the students were married and had kids. It was quite different then than it was at later times. A lot of people had been in the service.

COHEN: Maybe there's more pressure now, but people were a bit older, too.

ANDERSON: Yes. I remember Stewart Smith and Bob Kovach and Jack Healy and I, at least, had been in the Air Force or Army. I can't remember how many of the others were in the service but most of them were married and had a kid. So there was a range in ages and maturities.

COHEN: Well, of course, this was a time I know very well, too.

ANDERSON: Yes.

COHEN: What did you do for your thesis? Did you have a particular project?

ANDERSON: I was interested in ice, because of my previous research. The problem of ice was a mathematical problem I never did solve before. Vibrations of a plate and deformations of a plate are classical problems of mechanics and in seismology. In fact, Frank Press was the co-author of a book called *Elastic Waves in Layered Medi*a (New York, 1957), and the propagation of waves in a single plate was just part of the theory that goes into a layered medium. But ice is anisotropic. That means that the ice crystals, as they form, orient themselves. If you send a seismic wave horizontally, it has different velocities and different waves than if you send it vertically. That was a very complicated problem. The ice is an anisotropic wave guide instead of an isotropic wave guide. So I had to work out the theory for that. But then, in addition, it's floating on water, and water has low seismic velocities. The ice has high seismic velocities. That means a wave that is propagating in the ice will leak out and go into the water, and that's an even more complicated problem, which I was in no position to solve until I went to graduate school. So my thesis basically was wave propagation in anisotropic materials including the leaking of energy into a low-velocity fluid. I discovered that the mantle is anisotropic, just like the sea.

COHEN: So it was a mathematical thesis, really.

ANDERSON: It was almost entirely mathematical. I minored in mathematics. I'm not good in mathematics, and needed to learn a lot before I could solve these kinds of problems. So it was a short thesis, but it completely solved this particular problem. And I didn't think I had written a thesis. I wrote a couple of papers and showed them to Frank Press and he said, "Well, it looks like your thesis is about done." And that was a real surprise. I figured I'd be writing a couple papers, learning how to do things, and then I'd write a thesis. So I hadn't really picked a thesis topic yet; I was just solving some problems.

COHEN: It saved you from worrying about it .

ANDERSON: Yes, it was quite a shock.

COHEN: Oh, but it must have been very good.

ANDERSON: When I finally took my exam, my wife didn't know anything about it. I walked in one night and said, "Frank Press says my thesis is almost done." And I finally did turn it in. The exam was scheduled and completed so quickly I guess I forgot to tell her. Because I was as surprised that I had passed my PhD exam.

COHEN: Right. And then Frank [Press] asked you to stay on for another year?

ANDERSON: Yes.

COHEN: At which point you then just stayed on.

ANDERSON: I just stayed on, although I told my wife that I didn't like Southern California or Los Angeles and that we should leave—"Don't let me stay." But then, the following year I guess, I was offered an assistant professorship. Within a year or so it was associate professor. And then pretty soon it was tenure, and then director of the lab. So every year or two it seems like I got too good a deal to turn down. Every time I was thinking of leaving something good happened.

COHEN: They didn't let you go.

ANDERSON: They didn't let me go, so I've been here ever since.

COHEN: So you moved out of the orange grove, I'm assuming.

ANDERSON: Yes. When I was a postdoc, this little old lady that owned this estate—a wonderful, wonderful woman—decided that she was getting old and that she would sell her estate to a church and they would make it into a retirement community. She wanted to keep it from developers, so she formed a friendship and sold it to him. But it turns out he was a front man for a developer. She wanted to make sure people didn't get kicked out of this place. But they kicked us out; they evicted us all, so we had to leave with very, very short notice. I rented a house on Mount Curve Avenue in Altadena for a while. I felt very crowded in with neighbors, and I didn't like that, after having lived on the estate for years. So then we started looking around and we bought a house farther down the hill but in Altadena.

COHEN: And you just stayed there in Altadena?

ANDERSON: We've been in that same house for a long, long time. We were able to get it pretty cheap, I found out later, because Pasadena was getting integrated. We moved into an integrated neighborhood and people were leaving. Prices were depressed in these neighborhoods. I couldn't understand why I got such a nice house and nice property on this side of Lake. On the other side of Lake it would have cost twice as much.

COHEN: Right. You're saying you are west of Lake then.

ANDERSON: Yes.

COHEN: Yes, but that neighborhood has actually stayed quite nice.

ANDERSON: Fine. Our kids went through schools at the time when they were integrating them. People were all worried about that, but we never had any trouble. In fact, they almost bused our daughter all the way to the other side of town because they assumed that she was a minority. She almost didn't get a chance to go to neighborhood schools because of the busing.

COHEN: Right. We were a bit involved in that, too.

ANDERSON: The junior high school and the elementary school were both within walking distance, and the high school, in fact, you could walk to as well.

COHEN: So here you are, part of this department now, in the early sixties. Can you describe Caltech at that time?

ANDERSON: At the time we were in the Seismo Lab, which was in the San Rafael Hills, off-campus near the Annandale Country Club.

COHEN: How did you jump immediately into seismology? How did that happen?

ANDERSON: Well, I came to Caltech to study geophysics. And almost all the geophysics was, in fact, seismology. Hewitt Dix was an exploration seismologist, and the other people were earthquake seismologists. I wasn't particularly interested in either, but I've turned into the kind of seismologist that uses seismology for things other than finding oil or locating earthquakes. Seismology can be thought of as a tool, or as an end in itself. Or it can be thought of as just another technique to understand the Earth. That's what I was interested in. Even at that time I knew I didn't want to do either one of those things. I had been in the exploration business, and I wasn't all that interested in earthquakes. But most of the people here were studying earthquakes. Because of my background, I started trying to use seismic techniques to learn about materials with [certain] properties, the strength of materials, and the composition of materials, and I have since applied that to various parts of the Earth. You can use seismology as a microscope, where you look at a mineral and you can tell from the index of refraction, and you can tell from the colors more or less what's going on. Well, you can do the same thing with sound waves or seismic waves. You can determine the mineralogy of the mantle, the chemistry of the mantle, and whether you have melting or not just from the characteristics of the seismic wave. So that's what I do, that kind of stuff.

COHEN: So then, when did you become head of the seismic lab? What year was that?

ANDERSON: 1967. I was director of the Seismo Lab for about twenty-two years.

COHEN: A long time.

ANDERSON: I started five or six years after I graduated.

COHEN: So how many people would have been involved with the Seismo Lab? How big a staff would that have been?

ANDERSON: There would have been between five and eight professors during that time. [Tape ends]

DON L. ANDERSON

SESSION 2

March 10, 1999

Begin Tape 2, Side 1

COHEN: Hello, Professor Anderson.

ANDERSON: Good afternoon.

COHEN: I'm happy to have you here this March 10, 1999. I'd like to talk about the Seismo Lab. You came in what year?

ANDERSON: I came as a graduate student in 1958.

COHEN: So you would have known Gutenberg.

ANDERSON: Yes, certainly I knew Gutenberg. In fact, he was one of the reasons I came to Caltech. So when I came it was Press, Gutenberg, Richter and Benioff. And Hewitt Dix was here as a geophysicist; he was not a member of the seismological laboratory. So I think that was the staff at that time.

COHEN: Was it Caltech that was running the Seismo Lab when you first came?

ANDERSON: Oh, yes, Caltech had been running it for some time. But as you know from your reading, it was initially independent of Caltech. It was part of the Carnegie Institution of Washington. And the original date is a little bit hard to pin down, but it is something like 1927 or so.

COHEN: I think that's the date. It was San Rafael Hills in 1925-27.

ANDERSON: Right. So it was set up as a result of a committee of world famous geophysicists from around the world. I guess it was Arthur L. Day [Arthur Louis Day] who was chairman of that committee.

COHEN: Was he from Carnegie?

ANDERSON: Yes, he was with Carnegie. So it was a result of his report that the seismological laboratory was set up to deal with, as they called it, the earthquake problem in California. Then at some later point, Caltech hired Beno Gutenberg to be a professor at Caltech, but he worked at the seismological laboratory. So for some years it was a joint effort.

COHEN: Was there a reason why it was over in the San Rafael Hills?

ANDERSON: Yes, it had to be a quiet site. The instruments had to be on bedrock or granite exposure, so it couldn't be in the middle of a big sedimentary basin. And it had to be away from population centers. So at the time the San Rafael Hills, geologically and culturally, was the ideal place to build a building specifically for the instrumentation for the people that were to work at the seismological laboratory.

COHEN: So Carnegie recommended to Caltech to do it? I mean, how did that work?

ANDERSON: I think Carnegie just decided to go ahead and do it. And their affiliation with Caltech—and this was long before my time—was fairly indirect, but then Caltech decided they wanted to get into the earthquake business. I think Gutenberg was the first one, although [John] Buwalda was quite interested in the geology of earthquakes, and I think he had a lot to do with getting Caltech into the seismological business. And then, many years later, Caltech actually took over the whole operation of the seismological laboratory.

COHEN: So their relationship with Carnegie was smooth?

ANDERSON: I don't know too much about the history.

COHEN: I know the astronomy had not been smooth.

ANDERSON: Yes. You almost have to talk to Clarence Allen and Bob Sharp to find out about the early days.

COHEN: They've both done oral histories.

ANDERSON: That was long past by the time I got to the seismological laboratory. But it was off-campus and was a separate facility from the Division of Geological Sciences. So the Division of Geological Sciences was founded, I think, at roughly the same time as the Seismological Laboratory, but they were independent entities run by different groups. It was much later that the seismological laboratory became part of Caltech, and then the Division of Geological Sciences. So the heritage is quite new.

COHEN: So it existed by itself.

ANDERSON: The seismological lab was off by itself. The students worked out there. It was roughly three miles from campus. Students would take their courses on campus, but then spend almost full time at the laboratory. There were both benefits and disadvantages of being isolated. But because the lab was out in the beautiful San Rafael Hills, it developed its own culture, that was quite distinct from the way the Division and Caltech were operating. It was more like a commune: people cooperated and shared data, students, and facilities. It was quite different from the little individual empires that typically grow up at a university. We learned how to do science in a different way that we tried to transplant when we moved to campus.

COHEN: Can you go into that a little bit? What do you mean by "a different way"?

ANDERSON: Well, we were in a mansion, with a huge spiral staircase and a nice big lobby. It had very large rooms, living rooms and dining rooms that came from the heritage of a mansion.

COHEN: Now, this was after you moved in 1958?

ANDERSON: Well, in 1958 I came as a student. And most of my first year was on campus taking courses.

COHEN: Right. And then this mansion was bought.

ANDERSON: The mansion was purchased several years before. Bob Sharp was the main person who arranged to buy the property. The seismological laboratory was built in the twenties out in the San Rafael Hills. Then the Seismo Lab outgrew it. In the late fifties this house just up the hill from the original Seismo Lab came on the market.

COHEN: It says, "A.C. Thorsen mansion." From Walgreen [drug stores]?

ANDERSON: Walgreen gave money to buy the mansion. One attractive feature was that it had a huge tunnel, carved into the side of the hill underneath, that was used as an entryway to the garden and the tennis court. The building also had an elevator. You'd take the elevator down to the tunnel, and go out the tunnel to the garden and the tennis court. The garden was very attractive.

COHEN: And the whole thing sat on granite?

ANDERSON: Yes. The attractiveness of the tunnel was [related to] Hugo Benioff designing and building very large instruments, including strain gauges. These are typically put into tunnels. When Benioff found out about this tunnel, he was very excited. The first thing they did was put a big, long quartz strain gauge into this tunnel and isolated it. And then the elevator wasn't functional for very long, so we walked up and down the hill after that. But the mansion was bought, and that became the place where the faculty, the students, and the postdocs were, and where most of the research was done. The original laboratory down the hill was the shops and the main instrumentation. Most of the instruments were down the hill, plus the seismogram collection from the twenties.

COHEN: You didn't give up the old site?

ANDERSON: No, we maintained both until we moved to campus in about 1975. At that point the Donnelley laboratory in the mansion, or upper Seismo Lab, as we called it, was sold. The original seismo building was called the Kresge Lab.

COHEN: Let's go back a bit. You were saying [the Seismo Lab] had a different philosophy than on the campus or at most universities.

ANDERSON: Yes. This mansion had a spiral staircase in it which facilitated inter-floor communication. We had a coffee break twice a day where we sat around in the furnace room. [It was] very cramped quarters, but all the faculty and staff and students got together and drank coffee—more importantly, they discussed the problems of the day or the earthquake of the day. We developed a unique esprit de corps. I've never seen it since, and I've never seen it anywhere else. It's very rare. We developed the philosophy that all the students should be completely supported but not assigned to any particular professor. They would come to the lab and work with several people on different projects. Sometimes students would work with two, three, or four professors.

COHEN: That meant that people didn't have to go out and get their own grant money?

ANDERSON: Well, initially Frank Press was very well known, and he knew how to get large grants. At the time, $100,000 was a large grant—enough to support all the graduate students in the lab. Then, as time went on, money migrated from one agency to another. When he first came it was the Air Force, or maybe it was even ARPA—Advanced Research Projects Agency—or DARPA [Defense Advanced Research Projects Agency] that was in charge of detecting nuclear tests—seeing if the Russians or Chinese were setting off nuclear bombs. At the time, instrumentation was developed to put in all over the world. This was a new class of instruments that required training new people. People in the government at that time realized that they had to upgrade this particular science. The science of seismology needed new instruments, new ideas, and new theory and new people, so government agencies were very generous in supporting various groups, but in particular the seismological laboratory. Consequently, all the students at the time were working under this one grant but on different aspects of the same problem. The problem wasn't narrowly defined, however, it was learn all you can about the Earth and about earthquakes and about sources of seismic energy in the Earth. This meant we were really working on quite different things, none of which were very closely related to the nuclear test problem. There was so much to learn about the Earth that anything was acceptable.

COHEN: But you were supposed to be keeping track of that also?

ANDERSON: Yes.

COHEN: The nuclear kinds of things?

ANDERSON: Yes. We all knew about the nuclear business, but very few of us were actually monitoring nuclear devices or using this data as a primary source of information. One aspect that enhanced our esprit de corps was that most of the students had families and kids, which is different from today. Many students had served in the military and were starting their careers relatively late, so it was a fairly mature bunch with lots of kids. The families weren't very well paid—graduate student pay wasn't all that great—so we tended to have parties and dinners and do our own entertainment.

COHEN: You did speak of that.

ANDERSON: We took care of each other's kids. But that was the social aspect. We had a good social thing going. But the research was very collaborative, because by and large we used the same facilities, the same instruments, and the same laboratory. We built individual little things, perhaps, but overall it was a matter of sharing. We helped each other.

COHEN: The whole group worked together.

ANDERSON: Seismology is a fairly unique science: you have to share data with colleagues all over the world. You can't run a seismological observatory and then say, "This is my data," because all the data in the world has to be used together to locate earthquakes and learn about the Earth's structure. Beno Gutenberg established ties with all the seismologists around the world to get data and records. So the interchange of data was quite unique.

COHEN: Cooperation is sort of built into geology?

ANDERSON: At least in geophysics cooperation was built in—I was amazed to see other sciences operating in what I thought was a very selfish way—keeping their data and not sharing. We shared data and ideas at coffee breaks.

COHEN: A rigid break in the morning and afternoon is very European.

ANDERSON: Is that right? Well, we noticed that people in the Division on campus worked in a quite different way. They had what I would call little empires. They built little walls around these empires and there was little interaction between these groups. I thought they had a more selfish attitude, whereas we were open and free. Having a spiral staircase, we discovered, was very important, because if you were on the third floor or the second floor or the first floor it didn't matter. You would go up and down these stairs and run into people. So every day you saw everyone, whereas some of the buildings that you see on campus have elevators and outside or unattractive staircases that isolate the floors.

COHEN: This building is probably the prime example of never meeting anybody.

ANDERSON: Right. When we moved to campus we realized that part of our success was isolation, and another part was the interaction between the floors and between everybody in the building. Once we were essentially forced to move to campus, we took the new building design very seriously. We wanted a spiral staircase, because we saw MIT's Green building that is sort of like a Millikan Library style of building, a huge skyscraper with an elevator that you have to wait forever for. When you get up to the tenth or twelfth floor you don't see anybody except the people on that floor. It was the opposite extreme from our seismological laboratory. So we insisted on a spiral staircase, but the architect and the people on campus didn't like that. It was an added expense and a fire hazard. But we stuck to our guns and they figured out a way to have a water curtain that would allow for a spiral staircase. It helps a lot to maintain collegiality between floors and prevent isolation of individual groups that don't interact too much—I can't exaggerate how bad that is for science. We kept the coffee break going too. Twice a day we have a coffee break, because we've learned that is an essential part of doing frontier science. And we incorporate the students into it right away as soon as they come. Everybody's welcome. The faculty is always there and available. It's deteriorated over the years, because people are getting their own interests and are more isolated from each other here than they were at the old lab. But to some extent we still have these coffee breaks. Our coffee breaks are world famous. Postdocs and visiting professors have visited us from all over the world, and tried to transport this coffee break kind of collegiality. But it really doesn't work unless you have two or three or four key people that religiously attend so that the students know they can come there every day. Once you say you're too busy, then it falls apart.

COHEN: Did you ever have the problem where some people don't go back to work? I mean, sometimes that can be a problem.

ANDERSON: Sometimes we get a really exciting thing going and the coffee break might last for two or three hours. But there are also a lot of papers, a lot of ideas, and a lot of theses that start in that room. We solve a lot of problems. We try ideas out. And we get new ideas. Somebody knows something you don't know. So you very rapidly are able to learn a lot and discuss and think through various problems.

COHEN: Okay. So let's go back to your own work at the Seismo Lab.

ANDERSON: As I mentioned last time, I was in the Air Force for two years. And I worked in northern Greenland on the problems of sea ice. Sea ice is, of course, a plate of material that freezes out on the surface of the ocean. You can walk on it after a while. It's rigid and strong, but it's also partially molten because of the salt content. It's also anisotropic, because the ice crystals all line up in the same direction. I got interested in that problem, and it turns out that's very similar to the Earth problem. The lithosphere is a plate, the asthenosphere is essentially a weak layer underneath the plate. The outer parts of the Earth are anisotropic because they are made out of minerals. The upper mantle is partially molten. I didn't know all this at the time, but trying to sort out this sea ice problem got me thinking about things that a lot of geophysicists weren't thinking about, in particular anisotropy. Seismologists have traditionally assumed that the Earth is isotropic. And that simply means that the seismic waves go at the same velocity no matter what direction, whereas in a crystal the index of refraction, for example in optical crystallography, is based on the idea that light waves go through a crystal at different speeds and different directions. The same thing happens in a rock with sound waves, or in the Earth with earthquake waves. At the time there were indications that some data weren't being fit as well as they could have been. I started working on the problem of wave propagation in anisotropic material. At that time, scientists at Lamont had just worked out the theory for a layered medium. In other words, how do you calculate the propagation of surface waves through a stack of layers—a stack of layers being an approximation to any distribution of seismic velocity of the Earth. I had been in the Air Force, and was a contract monitor for these people, so I was familiar with the techniques that were being developed. In fact, I worked at Air Force Cambridge with Norm Haskell [Norman A. Haskell], the person who invented this technique. So it turns out that I was able to work out the theory for elastic wave propagation in an anisotropic plate. Because of this new theoretical development and the fact that big computers were coming online, I was able to generalize this immediately to layered anisotropic media. My thesis itself was a floating anisotropic plate over a liquid half-space, which is also a very complicated problem, because waves in the plate leak continuously into the material underneath. In technical terms this means all the roots of the theory equation are complex. It makes it a very complicated problem to find these roots. So I would spend my lunch hour every day in the computing center with an old Burroughs computer looking for roots of this equation. I would find maybe one or two each day, so it took many months to determine this dispersion curve that I was looking for, which is a series of roots. I was helped by Bob Phinney, a very smart young theoretician who was a graduate student at the same time. He was working out the theory of complex roots in wave propagation, so I was able to utilize some programs that he wrote to solve my particular problem. The final program was written by David Harkrider, another graduate student who was a super programmer. Together we wrote about four or five papers on wave propagation in anisotropic media. That was typical of what was happening—all of us interacting with each other. We helped each other and we wrote lots of papers together.

COHEN: This was before you had your degree?

ANDERSON: As a student I was writing papers with a lot of people, including David Harkrider, Charles Archambeau, Ari Ben-Menahem, Nafi Toksöz, and Bob Phinney. At that time we were solving a lot of very important problems. But it was also an exciting time, because some big earthquakes were happening. We had instrumentation that could record these big earthquakes, so the free oscillations of the Earth were discovered during this time and lots of good theory was worked out. I also wrote a paper on ice with Carl Brewer who is now a professor at the University of Alaska.

COHEN: So when you felt the earthquake you didn't panic. You thought, "Ah, good data coming in."

ANDERSON: Oh, yes. Everybody would crowd around the seismic drum recorders and try to figure out where this earthquake was coming from. It was fun to see Richter and Gutenberg go to the drum and argue with each other about where this earthquake might be coming from while it was still coming in and the needle was still vibrating. The phone would start ringing, of course, right away. And Charlie Richter was very clever. He would find out where they were calling from, and pretty soon he knew exactly where the earthquake epicenter was.

COHEN: From the telephone calls?

ANDERSON: Right, from the phone calls. So that was my thesis. Before I got that far, though, I interacted with Carl Benson, an older graduate student who came here two or three years before me. And since I had been in Greenland and he was working on a thesis on glaciers, we got interested in some problems of ice and glaciers. We wrote a paper together about the formation of ice from snow. That wasn't even a seismological paper, but it shows the kind of interaction among students that was happening at that time. Incidentally, before I ever came here I had already written about three or four other papers on various kinds of problems involving sea ice.

COHEN: Right. I was reading one of them that you had given me. It's always interesting when one's military service in no way disrupted the career but rather enhanced it.

ANDERSON: Well, it certainly changed my direction. I wasn't planning to go to graduate school. It was never my intent to have a PhD or to teach or to be a professor. That all came much, much later. It was doing this interesting research that led me to realize that I needed more education. That had never occurred to me during the time that I was working for the oil company, or for the first year or so that I was working for Air Force Cambridge. The idea of going to graduate school was just never in my thoughts at all.

COHEN: So this evolved from your military service?

ANDERSON: It was in the Air Force that I realized I liked research. I liked to try to solve problems. But it became crystal clear to me that I didn't have enough knowledge to solve these problems. In fact, that problem of wave propagation in an anisotropic partially molten solid with high attenuation and a physical intrinsic attenuation because it's leaking energy into the layer underneath is one of the most challenging in geophysics. People are still working on that problem. It certainly occupied me for quite a while, although I transferred from applying it to ice to applying it to the Earth in general. It is also related to plate tectonics.

COHEN: So you finished your degree and you stayed on here.

ANDERSON: Yes. I finished my degree. I guess you have the date: '61 maybe.

COHEN: It was the early sixties, yes.

ANDERSON: Early sixties.

COHEN: I have here that you were only here for six years as a professor before you became director of the Seismo Lab.

ANDERSON: That's probably true. I graduated. I handed in my thesis. And I think I mentioned earlier that I thought I was writing a paper. I handed it to Frank Press—he was the director—and he said, "Well, it looks like your thesis is almost done." It floored me, because I hadn't been here very long. I guess I graduated in three and a half years maybe, something like that, which was short. I didn't realize I had written a thesis. I was still trying to learn things. I certainly wasn't as smart as all my colleagues, or didn't have as good a background.

COHEN: It's the second, probably.

ANDERSON: I solved this neat little problem, and I guess Frank Press thought it was decent. In those days people would spend five years writing enormous theses. In fact, most of my colleagues have very, very thick three- and four-inch theses, but mine is about three-quarters-of-an-inch thick.

COHEN: I could have seen it actually. Right next door there is where they are.

ANDERSON: It's pretty thin—basically a couple papers that I wrote. I wrote a bunch of papers on anisotropy that grew out of my thesis. But then I kind of dropped everything I did for my thesis and worked on other things. In fact, I was embarrassed to talk about sea ice or anistropy, because I didn't want people to think I had a one-track mind. So I graduated and Frank Press brought me into his office. He said, "Would you be interested in studying as a postdoc for a year?" And I thought that wasn't a bad idea. I had started looking for a job. I didn't have one yet, I guess, or I didn't have one I really wanted, so staying around for a year sounded like a pretty good idea. I forget the details of just why I made that decision, because I remember having told my wife that I didn't like Los Angeles or Southern California. It was too dry, not enough rivers. It would be nice to go back East. I even told her, "Don't let me stay in Los Angeles."

COHEN: She didn't like it here?

ANDERSON: Well, she's from the East, too, so it took us both a while to appreciate California. It takes a long time if you're from the East, particularly the northeast, or Maryland in my case.

COHEN: Did you continue along some of the same lines, or were you already leaving the ice behind?

ANDERSON: Oh, I had long since left the ice behind. I can't remember what papers I was writing those first couple years, but they had to do with attenuation and the structure of the Earth's mantle. I never really did much work on earthquakes; it was always the structure of the Earth and interpretation of seismic data in terms of things like mineral physics, physical properties, and chemistry.

COHEN: So you wouldn't be going out to the deserts to look at rocks and things like that?

ANDERSON: No. One reason I got into earth science was because I was a rock collector when I was a kid. So I collected outside and I liked doing the fieldwork. In fact, I taught field geophysics with Bob Kovach for several years here when I was a young professor.

COHEN: Is that where you take the students out into the field?

ANDERSON: We took the students out in the desert, the field, and did all kinds of geophysical experiments. So my first courses, in fact, were geophysics field courses, working with Stewart Smith and Robert Kovach. If I had a list of my papers in front of me I could sort of tell you what I did those first couple years. I just don't remember the timing of what I did then and what I did later. But I was looking for a job. I think at the time I had offers from Princeton and Chicago.

COHEN: I've got one from '65, "The Interiors of the Terrestrial Planets," by Kovach and Anderson.

ANDERSON: Yes, I got involved in the terrestrial planets a little bit later and wrote a bunch of papers on planetary physics. But as far as my postdoc and then professor, I was getting, I think, $6,500 a year.

COHEN: That sounds about right.

ANDERSON: It was low. I had two kids and a family, and I just barely survived. And I was quite anxious to leave after that. But then I mentioned to one of my colleagues, "Boy, it's hard to live on this." He mentioned something to Frank Press, and Frank Press gave me a raise. But then within a year—I think it was within a year, you can check all of this—they made me an offer to be an assistant professor. So that sounded okay, although I still had no idea whether I wanted to teach or not. Bob Sharp was the chairman at the time. He was very supportive. He wanted to know what I was going to teach and gave some suggestions. He developed a course on the physics of the Earth's interior, which I updated into a graduate course called "Physics of the Earth's Interior." Then I think the way the history goes is that every year for a while I got some kind of promotion, either a promotion in grade or a big raise in salary or a directorship or something like that. So one thing led to another. No matter what kind of offer I got outside, there was always something better here, so that's the reason I stayed.

COHEN: You were at the best place, I would think.

ANDERSON: Yes, after five or six years.

COHEN: Then you were asked to be director of the Seismo Lab. How did you find out about that?

ANDERSON: Frank Press was offered the chairmanship of the department at MIT, and he had only been here six or seven years. I think he came in '56 or '57 and he must have left in '64 or '65, something like that. People didn't expect him to leave so soon. But he was very ambitious and very good. I guess it's understandable that he moved. He became chairman of the department at MIT while I was still pretty young. All of us were young; we were all about the same generation. Although we had been in the military and had been out, we were still academically pretty young. The Division didn't know quite what to do about the Seismo Lab, so they appointed Clarence Allen as interim director even though he wasn't a member of the seismological laboratory. He was a geologist in the geology department. Allen moved over into the seismological laboratory and became the interim director. He didn't want to become the permanent director, but he liked the environment in the Seismo Lab. So he became one of the people at the Seismo Lab. He liked the way it operated, and became partially a seismologist.

COHEN: Now, the geochemists were separate from you guys?

ANDERSON: Oh, yes. Geochemists, geologists, planetary scientists.

COHEN: Because that whole group came from Chicago in the fifties. I mean, with Harrison Brown.

ANDERSON: That was a completely different group. The new geophysics group was almost entirely homegrown. Particularly after Frank Press left, we were entirely homegrown. We were virtually—look, Gutenberg died, I guess, early on in my graduate student career.

COHEN: 1960, yes.

ANDERSON: But Benioff and Richter were still around. And Hewitt Dix was still around, although he never was a member of the Seismo Lab. But after Press left, it was myself, Stewart Smith, Bob Kovach, Charles Archambeau, and Bob Phinney. At one time or another we were assistant professors.

COHEN: All together in the same class, so to speak.

ANDERSON: Right. Frank Press didn't mind hiring his own graduate students. He routinely did that, which is frowned upon nowadays. This alienated us a little bit, I guess, from the Division. We were not only isolated from them but we were now ingrown and homegrown.

COHEN: And younger.

ANDERSON: And much, much younger. So the Division kind of didn't like this idea of us sitting out there by ourselves, so that was one reason they brought an established geologist out to head it.

COHEN: Oh, so that's why Allen came.

ANDERSON: No, there was no friction or any problem there, but the rationale for doing that can be questioned. In later years, in fact, the Division quite commonly would try to corral the Seismo Lab in one way or another to reduce our independence. It usually served to decrease the esprit de corps and the strengths. Bringing the group to campus was part of that, too. Eventually we had to move to campus. But I was always worried that our uniqueness had a lot to do with our isolation, our independence.

COHEN: So the fact that you were asked, I mean, you were really just part of the group. I mean, except after Allen. He was the interim director for how long?

ANDERSON: I think it was just a year.

COHEN: And then they asked you to take it?

ANDERSON: Then they asked me. Then later Benioff told me that they should have appointed me right off the bat, early on, but for some reason they didn't trust me or didn't like this idea of elevating someone from this young group up. So apparently—I was never privy to it —there was a lot of discussion among the geologists about what to do with seismology.

COHEN: So what kind of decisions did you have to make being head? Or did everybody just do their own thing? How did it work?

ANDERSON: I felt my responsibility was to make this the best geophysics group in the world, and to make sure we had money and students. So I looked at my role, not as guiding research at all, but in making sure we had good facilities. We had a constant in-flow of money, which was hard to do since obtaining money was different in those days. By the time I finished as director we had about fifteen contracts to support the lab. In the early days it was just one or two big ones. I had to do things like get money from NASA and then become the principal investigator of some planetary mission, partly to get money to support our technicians, students, postdocs, and other needs. One of my major jobs was to raise money. I wrote a lot of proposals and served on a lot of committees.

COHEN: You kept the same scheme there where there was blanket money and all the graduate students were paid from that?

ANDERSON: Yes. Other professors that came in the meantime were Jim Brune, who was from Columbia. And he didn't quite understand the situation. We would just absorb new professors and he wouldn't have to worry about the money and I would write a proposal putting this new person on. Since he didn't know the system and since he had very strong feelings about accepting money from the defense department and for doing anything for the military, there were some problems. The same thing happened later on with some students during the sixties and seventies. We had to tread very carefully. There were some rough spots as far as accepting money from the defense department.

COHEN: And that was spawned internally by students?

ANDERSON: Yes. In fact, I remember a meeting very distinctly at the old Seismo Lab. We had all the students and faculty and postdocs, everybody. And there was a big peacenik-type movement that said this was dirty money. I had to explain that we could refuse all defense department money, but that there would be no scholarships, no fellowships, and we'd have to fire about half the employees, and that we were doing peaceful research. We were actually doing research that had nothing to do with building bombs. In fact, it was the other way around; we were detecting other people building bombs. So it was a worthy endeavor, I thought, and they were taking money away from people building bombs. I was very sympathetic to this attitude. And we all agreed that it was okay.

COHEN: That you'd take the money?

ANDERSON: It was okay.

COHEN: There were many problems. I mean, there were places where the students didn't listen.

ANDERSON: Well, we were always open. Part of the coffee break was so students could talk about what was on their mind. You establish an esprit de corps that way that's quite different from just making an appointment to go see a professor when you've got something really bugging you. Things never really got out of control. We were always there and talked things out. I don't like ordinary meetings. I hate meetings. We had these coffee breaks, which were almost always science, but if there was a pressing issue we settled that, too.

COHEN: You then continued your own research, which was quite fruitful, during this time.

ANDERSON: Oh, yes. But I was talking about what I felt my responsibilities were. One was to make sure the money kept flowing and we had facilities. The second was to make sure that we always had a good crop of students in. Then, as now, the applicants go up and down. Sometimes we don't get a lot of good applicants or they don't accept our offer. So many times I would have to take off and beat the bushes. I'd go to other universities or go to meetings and specifically try to find good students that I'd then try to attract here.

COHEN: So you wouldn't just wait for the applications to come in.

ANDERSON: We have quite a few students who we recruited. Many would never have applied to Caltech. One of them I found in Hawaii. He was working for the Geological Survey. His name is Dave Hill. You see his name in the paper all the time, because every time there's an earthquake in Mammoth he will get in the paper. He's in charge of the earthquake hazard problem on Mammoth Mountain. He was in Hawaii at the time—he was a vulcanologist—and I convinced him to go to graduate school at Caltech. He didn't think he was good enough, but he came and turned into a very good student. Another time we weren't getting enough applicants, so I started looking at the people who applied to physics, mathematics and astronomy, and other departments, to see if there were some people that might be interested in geophysics. We got several students just by looking at applicants to other departments. Sometimes I'd go to a meeting and a person would give a nice paper. I'd get him aside and convince him to come to Caltech.

COHEN: You were very active in recruiting students?

ANDERSON: Getting students was absolutely essential. We discovered early on that having a critical mass of students was important, and that passing information on from one generation to another was important. If you go two years without a good graduate student crop, you can lose continuity and have to fill it up all over again. Students learn a lot from each other. That became very clear early on, that we needed a good continuity of students.

COHEN: Was there any feeling about looking for women students?

ANDERSON: Yes. I remember our first woman student. We were sensitive about not having many women students. We had one of the first ones in the Division, I believe. Sue Raikes. She was from Britain. Early on we only had one or two at a time, and that was kind of rough. We realized that we needed more. We've got quite a few now. But, yes, during those early years when there was only one or so we tried very hard to increase the student body. I can't remember what we did specifically to try to attract women, but it was just a general problem of getting good people here. And we were sensitive to the fact that this one woman student, who was the only one we had for quite a while, was isolated and really needed colleagues.

COHEN: There's something I'm not quite understanding here. You went after your own graduate students as opposed to, say, the geochemists?

ANDERSON: Yes.

COHEN: Or other groups in the Division?

ANDERSON: Right.

COHEN: Everybody went after their own students.

ANDERSON: I think we were unique. Nobody else went after students. Students would apply. Every year you'd get a supply.

COHEN: But they'd apply for a certain group within the Division?

ANDERSON: That's right.

COHEN: I see.

ANDERSON: In fact, we had some problems. Especially when the geology professors accepted a student who then wanted to be a seismologist only to find that we would not have accepted them from the basis of their background—not enough math or physics or something. Sometimes after they get here then they decide they want to transfer over, and that's a little bit complicated, because there's a big mismatch between these people that were accepted by the geology professors and the ones who were accepted with different standards by the geophysics professors.

COHEN: I see.

ANDERSON: So I mentioned the students and the money, but the faculty is also important. It is my view that it's always important to be looking so that the next year you get the best people and young people. It's very important to have turnover and have people constantly coming in, so I was always looking for faculty. I was looking to expand the faculty. My goal was to make

Caltech the best geophysics group in the world, not the best earthquake group or the best seismology group, so I was interested in going into other fields that are complementary to what we were good at in seismology. Tom Ahrens [Thomas J. Ahrens] is a mineral physicist and a high-pressure physicist, so he was one of the early ones that I was able to attract that was quite different from our other faculty in what he was able to do and what kinds of talents he had. Much later we got Brad Hager [Bradford Hager], who was a geodynamicist: somebody who tries to understand the dynamics of the whole Earth. We still kept hiring seismologists. Jim Brune was a very good seismologist. We hired Dave Harkrider. He was one of the students I was telling you about. He went away to Brown and we hired him back, because at one point we lost a theoretician, Charles Archambeau, I guess, and we needed a theoretician. We couldn't train our students adequately without [giving them] a good theoretical background. Our niche at the Seismo Lab was always sort of between observation and theory. We had good observational data, but we realized it had to be interpreted with theory. At the time, Lamont Geological Observatory was very data-oriented, very observationally oriented. They turned out people who were very good at interpreting data, but they didn't have a good theoretical background. The other extreme was the University of California at San Diego—the La Jolla people. George Backus [George E. Backus] and Freeman Gilbert were down there, and they were very heavy on theory but not so good on the experimental work. And our niche was right in between. You've got to be good at both. And in fact the other groups realized that after a while. La Jolla—the University of California at San Diego group—hired Jim Brune away from us to bring them observational talent. And Lamont hired one of our graduates, Paul Richards, who happened to be this physicist I was telling you about who applied to physics and we convinced him to be an earth scientist. He became a very good theoretician. He went to Lamont and started their theoretical program. So this niche that we had started that made us unique was recognized as being pretty important.

COHEN: The way to go.

ANDERSON: So we contributed to helping our competition.

COHEN: And then you continued your own work on top of all this.

ANDERSON: Oh, yes.

COHEN: You were busy.

ANDERSON: I continued to write a lot of papers, and had a fair number of students. Also, as director of the seismological laboratory I felt obligated to be a spokesman for seismology in this country and around the world, so I spent a lot of time in Washington on committees. If you look at my vita you'll see that I spent a lot of time on committee work with the NSF, NASA, the Air Force, and the USGS.

COHEN: What sort of committee work did you do then?

ANDERSON: We worked to start new programs and initiatives, to evaluate proposals, and to plan lunar exploration and Mars exploration—things like that. They were high-level types of things. Plus a lot of academy design committees. Typically the National Academy of Science would have committees that were dedicated to the future of seismology or the next twenty years of space exploration—things like this. I was involved with a lot of those things. In fact, I was involved in more of those kinds of things than I was on campus. So I was really an outside person more than an inside person as far as helping to run Caltech or serving on a lot of committees. I never did that, but I spent an awful lot of time on airplanes flying to Washington. And you can see that in my vita.

COHEN: Yes, I can see that. I'm trying to put all this together: you still had time to do all these different things? I mean, it was quite amazing.

ANDERSON: I worked all the time. In those days you'd just work all day and most of the night. When I was a graduate student I would typically go to the laboratory first thing in the morning and I'd come home after midnight. I mentioned that my wife would pack me lunch and dinner. Even after I was a student I worked very late at night almost every night. When I went on vacation with the family I would take work with me.

Begin Tape 2, Side 2

[Off tape, the conversation turned to the move from the old Seismo Lab to the new building on campus—ed.]

COHEN: Why did this happen?

ANDERSON: Well, we ran out of space. It was relatively small, so we were crowded. Some of the money that was given to build the Millikan Library was left over. That was seed money for our new building. Bob Sharp was still chairman at the time. I can't remember exactly. Somebody decided to raise money for the Seismo Lab. Maybe some of it came from the defense department.

COHEN: Let me just go back to one more thing, because I am reminded of something. That is, when I drive up California and I see all those trucks with the whirligigs there, I know there's been an earthquake someplace. Was there that kind of fanfare out at the old Seismo Lab?

ANDERSON: Oh, yes. I should mention something about that. Caltech is very famous, of course, for the Seismo Lab because of the earthquakes. But earthquakes is just something we do as a public service. At least from the time that Frank Press, and then I, directed the lab, earthquakes were just a service. We'd locate the earthquakes routinely and report everything we knew to the media. And every time there is an earthquake you drop everything and pay attention to the media.

COHEN: Well, you hired those ladies to do this for you, I think.

ANDERSON: Eventually. That was very much later. But during most of my time it was just the professors and a lot of students who had to drop everything and satisfy this public need. And we always thought of it as a public service, something we did. Periodically the division that we're in, and sometimes Caltech, confuses this with our real role, which is to train students to do research and to do things more than locate earthquakes. We've done it too well. We've gotten too famous and too good at it. Sometimes the Division will say, "Well, you need a geologist to interpret the faults," or something like that, implying that the Seismo Lab's main goal is to locate earthquakes. I consider that to be a very small fraction of what we do. Assuming we were just an earthquake lab, that would be much too narrow a role, particularly to train students in research. In fact, you can't understand earthquakes unless you understand a lot of other things too. So that's why I tried to broaden the lab. In fact, at one time I thought we should change the name to Geophysical Laboratory instead of Seismological Laboratory, because people thought seismology was earthquakes.

COHEN: Yes, of course.

ANDERSON: I mentioned Tom Ahrens but there were other people who were in that distinctively different mode of operation. In recent years it's been Mike Gurnis [Michael C. Gurnis], for example, who is not a seismologist—he's a geodynamicist. Rob Clayton [Robert W. Clayton], on the faculty right now, is an exploration-type seismologist. Brad Hager, I mentioned, was here for around five or six years. He was certainly not a seismologist. To be a complete geophysical laboratory you need a very large spectrum of talents. It's certainly a mistake, and I always viewed it as not our goal, just to have seismologists or just earthquake people on the staff. Once in a while the Division says, "Well, this guy has studied faults, so he belongs in the Seismo Lab." It's a different environment and a different tradition. People at the Seismo Lab are physicists or geophysicists that understand math and physics, and they think of problems in a completely different way than geologists or geochemists or planetary scientists. So in a way, when you try to focus on earthquakes rather than on the broad discipline, you can defeat your purpose by getting too narrow and too specialized. I'm sure I didn't mention all the people that we've had on the faculty over the years. But my intent was always to make it as broad in geophysics and as good in geophysics as possible. For many years our students and our research, and our breadth were just unexcelled. All of our students are well placed and solid.

COHEN: So it was really a matter of space that made you come down here.

ANDERSON: Yes. Some of us were very reluctant, because we understood the good thing we had going, the isolation and the camaraderie and the facility itself—the spiral staircase that I mentioned. And we knew that when we moved into a new facility—I forget whose law it is, Parkinson's Law or Murphy's Law or somebody's law—that things will deteriorate.

COHEN: If something bad can happen it will.

ANDERSON: Yes. So we were very, very sensitive—I was anyway—about trying to transfer from that environment to this environment, where we're now embedded in a division that's basically always been run by geologists. We were more likely to be under the thumb of what other people thought we should do. The geophysicists were always ridiculed for always being together as a group. Their perception was that we always voted the same way on faculty issues. So there was a lot of resentment between the mainstream geology part of the Division, as they called themselves, and geophysics. And to some extent the same kind of a thing happened when we patched on planetary science. It was not classical geology.

COHEN: I see. So you moved down. But what year was Mudd finished?

ANDERSON: I think it was 1975 when we moved to campus.

COHEN: I remember when they built that.

ANDERSON: I mentioned that we tried very hard to design the building in such a way that it was a free-flow of people and a free-flow of ideas and interactions between people.

COHEN: Because the stairs are certainly prominent.

ANDERSON: Well, we had the good example of our old lab and a bad example from MIT. The people at La Jolla, the Institute of Geophysics down there, have done the same thing. They realized the importance of the physical plan, and went out of their way to design a very attractive, utilitarian, interactive type of building. But lots of academic buildings are just the opposite. Because the idea is that this group has this lab and this group has that lab, and the interaction is ignored. An important part of our building besides the staircase was the coffee room. We have a big living room type of arrangement, with couches and chairs and blackboards and slide projectors and whatever. That's where we have our coffeehouse. It's quite a different environment from the furnace room, where we used to have it. It's a living room type of environment. And I know that the Seismo Lab will finally have reached the end of its run when somebody decides to break that up into offices or labs or something. These parts are essential.

COHEN: You are not director of the Seismo Lab anymore?

ANDERSON: Not for a long time. Hiroo Kanamori and Don Helmberger [Donald V. Helmberger] have been directors since my time as director.

COHEN: Yes, I have that number. So there wasn't a five-year pluck?

ANDERSON: In those days there was none of this. It was instituted somewhere along the line, but since I wasn't ever in the records I never came up for a review. I was grandfathered in by oversight. About half way through my twenty-two-year term I think the institute had a five-year review kind of thing. But I went through two or three cycles of that without anybody ever bothering me, because I wasn't in the books. I knew how to do the job and nobody else wanted it. It was an obligation.

COHEN: So why did you finally give it up?

ANDERSON: How frank are these things supposed to be?

COHEN: Very frank.

ANDERSON: I outlived many turnovers of administration: presidents, provosts, chairmen. Every time there was a new turnover they would look at the Seismo Lab and recognize that it was unique. It had some budget and it seemed to have too much independence. So there would be a hassle about cutting the budget or doing something to change things and make us more like them.

COHEN: Now, this would be your interaction with the provost?

ANDERSON: Well, we were always under the chairman, so the provost always interacted with us through the chairman.

COHEN: The chairman of geology?

ANDERSON: Right. So I think Christy [Robert F. Christy] was the first new provost after Bacher [Robert F. Bacher] that I had to deal with.

COHEN: Bacher you wouldn't have had trouble with. I mean, he was really a man with vision.

ANDERSON: No. When I started out it was Bacher and Bob Sharp and DuBridge [Lee DuBridge], and they were all fantastic. They were very supportive and very understanding. As I got older they started bringing in younger administrators who panicked just like I did when I became director. I looked at the budget and said, "Oh, we're going to be broke in three months or something." I was absolutely panicked about getting enough money and not running out. Any time a new chairman or a new provost came in he would go through the same exercise. He would panic and he'd try to do something to the lab. Every one of them. Every time there was a new provost or a new chairman, he would cut our budget or stop the increasing of the budget. After a while it became very, very painful. They were trying to do their job and after they got to understand the situation in a year or two things would ease off. At that point, I was the longest-lived administrator on this campus. Every one of the new ones tried to change things, although Peter Wyllie understood the workings of the lab. When Jerry Wasserburg became chairman it became so painful that as soon as he stepped down I stepped down. It was just not fun anymore.

COHEN: I see. Who was your provost at that time?

ANDERSON: I think Barclay Kamb was provost during all that time.

COHEN: So you had all geologists.

ANDERSON: Yes.

COHEN: Maybe that was the problem.

ANDERSON: Yes.

COHEN: I see.

ANDERSON: It was painful under Barclay Kamb as the chairman of the Division. He is very conservative and really cut our budget. I almost left Caltech when he was chairman. I continued to get offers from outside, including several from Harvard, and I kept staying here because it always was better. But at certain periods of time it got pretty bad, because the chairman was always trying to make the Seismo Lab more like the Division and less independent. Peter Wyllie was an excellent and fair chairman. He raised salaries and got at least one endowed chair for geophysics. Prior to that, they were all given to geologists and geochemists.

COHEN: Jerry was not chair for very long.

ANDERSON: No. Two or three years.

COHEN: That was a difficult time for the Division.

ANDERSON: He was particularly rough on the Seismo Lab and me in particular. I couldn't quit while he was giving me a hard time, because that would have been giving up. As soon as he stepped down, however, or as soon as he got thrown out, I handed in my resignation.

COHEN: So you had just had enough. I mean, twenty-two years is a long time.

ANDERSON: Yes. It was enough.

COHEN: Kanamori took over at that time?

ANDERSON: Yes, I think so. Maybe Rob Clayton [Robert W. Clayton] was an interim [director] for a little bit, but essentially Kanamori succeeded me.

COHEN: Who was president at that time?

ANDERSON: I remember Harold Brown. He was another example of a new president coming in. He wondered, when he came and started looking over things, why we had a seismo lab and why we needed a seismo lab. That was just before the '72 [1971—ed.] earthquake that knocked down, or damaged, the administration building.

COHEN: Did you arrange that?

ANDERSON: We never had any problems with him after that. He finally understood why the Seismo Lab was so prominent at Caltech and why it was good for Caltech to have this kind of visibility. It comes at an expense. The Seismo Lab is expensive. It costs money to run all these facilities, which we weren't doing for our own research staff. Very little of the data that we were collecting and analyzing for the earthquake location problem was the kind of data that was good enough for us to use in our day-to-day research. Clarence Allen used it a little bit. On the other hand, when one of the provosts gave me a hard time about why we were spending all this money on the Seismo Lab, I did a lot of statistics and showed him that seismologists were bringing in about three times as much money from outside as the average professor. It went both ways; we were costing overhead money but we were bringing in lots of overhead money. Then when we built our new building, Harold Brown took a tour of it and was furious about the size of the offices.

COHEN: They are big.

ANDERSON: I remember his comment. He said, "These offices are bigger than some of our Nobel Laureate offices." But they were all laboratory offices. We need big areas to spread out maps with seismic data. We have few labs. We had central facilities that we shared. The individual labs that most professors have in the Division are bigger than the average Seismo Lab office.

COHEN: How did you do with Goldberger [Marvin L. Goldberger] in there?

ANDERSON: He was no problem. He was sort of a teddy bear. The hardest president was Harold Brown, because he was a very efficient, money-conscious kind of guy. Christy [Robert F. Christy] was really tough, too, for the same kind of reason as Barclay Kamb. They were all trying to be good business people. I think they were counting beans more than realizing how you do science and the uniqueness of the Seismo Lab and geophysics at Caltech. You have to allow for different ways of operation. You just have to have flexibility when you are a scientific administrator.

COHEN: So then how did you feel when you stepped down from being director?

ANDERSON: Oh, I was delighted. I never felt better in my life. I'm not a good administrator; I hate administration and I hate meetings. Something's always on your mind. You are always worried. That is what eats away at you, particularly when you're not getting support from the levels of administration above. You expend emotional energy just defending what you're doing, when you shouldn't have to worry about things like that. Caltech is so small that if there's any problem in the chain of command, someone who's not interested in having Caltech the best or most enjoyable place to be but has other things on the agenda—then it can be bad. A place like Caltech should be the best place in the world for research. There's no reason in the world why administrators should give faculty a hard time just because of a difference in philosophy.

COHEN: Well, one hopes that is the case here.

ANDERSON: By and large it's been the case. I've really had no trouble with the administration at Caltech. There were a few little rough spots with new provosts, but by and large the Caltech administration has been fantastic. It's difficulties inside the Division of Geological Sciences that have been emotionally charged. I guess that's always true.

COHEN: People see each other every day.

ANDERSON: There are a lot of skeletons in closets, and a lot of antagonism. All the people that I was dealing with in the Division were here as professors when I first came as a student. Once you're a student, you're always a student.

COHEN: But as a professor you tried to avoid these difficulties and continued, then, trying to find out what the Earth is made of?

ANDERSON: Yes, that's been my overriding passion—not any particular specialty, but how the Earth works. I've dabbled in lots of different things, but I've had to learn a whole bunch of different fields in order to understand the Earth. It's not enough to do the seismology, because the seismology's controlled by the minerals and the minerals are controlled by high-pressure physics. You have to understand the magmas, the geochemistry, planetary science—the other planets are certainly important in understanding the Earth. If there are some peculiarities in the seismic waves, you might have to learn anisotropy or the physics of attenuation, material science. In order to study the Earth I've had to become fairly interdisciplinary, and I've discovered that most people are really specialized. I tread on a lot of toes by working in fields where I wasn't trained.

COHEN: Let's pursue that. Any toes here in particular?

ANDERSON: My last couple of papers have been on noble gases, which is to some extent Jerry Wasserburg's field. I'm not a geochemist. I don't measure things. I read papers and make calculations. And I've discovered some problems in the way people study the noble gases, or the rare gases like helium.

COHEN: I'm a chemist by training.

ANDERSON: They are called the noble gases, the rare gases, and the inert gases.

COHEN: And then I used to tell my students they could have been named after [William] Ramsay, who discovered them.

ANDERSON: I'm a bit of a philosopher, too. I enjoy logic and philosophy. I've discovered that sometimes you can read a bunch of papers as an outsider and finally discover what the assumptions are, and discover logical fallacies in what people are doing. I think I've uncovered quite a few of these. And once you identify the fallacy, once you state it as a syllogism, for example, you can discover why you have a lot of paradoxes. And in the noble gas business there are a lot of paradoxes. I think I've resolved a lot of them just by looking at that problem.

COHEN: How did you get to the noble gases?

ANDERSON: One of the ways you understand the evolution of the Earth is to look at the chemistry of the atmosphere or the chemistry of the crust or the chemistry of the ocean. And to some extent they hold a record of a long-integrated tree of evolution. Noble gases are unique because they are inert, they don't react with anything. Helium 3, for example, is something that we don't make much of in the Earth. There are not many nuclear reactions that make helium 3. So we know there's been some helium 3 in the Earth for a long time, or else it's been brought in recently by interplanetary dust particles and subduction into the mantle. Since helium 3 is still coming out of the mantle, a myth has grown up that there's a part of the mantle that's never been melted or never provided any material to the Earth's surface before. It's the so-called primordial reservoir. It's called that because helium 3 is a primordial isotope rather than a radioactive-decay product. But that's logically fallacious. Just because you have something in the Earth that's been around forever—most of the elements in the Earth have been around since the beginning—it doesn't mean that nothing has ever happened to them. You can outgas, and you can transfer a material around and then get it out at a later time. I just discovered that the volcanoes that provide high helium-3-to-helium-4 ratio are generally interpreted as being primordial reservoirs since they have helium 3. But it turns out that they've got very, very low helium 4, and that's why they have this high ratio. So I just picked up the other end of the stick, as some philosophers say. It resolves a lot of the paradoxes in the noble gas literature—to my satisfaction. I think I've solved an important problem, but I've never measured a noble gas in my life.

COHEN: This was just from looking at the papers written on this?

ANDERSON: Yes. There are lots of unstated assumptions that you don't notice. If you're an outsider, you just accept the conclusions. But if the conclusion doesn't agree with things you know from other things you have done, then you say, "Well, I trust my work and I trust my assumptions, and that's a good, tight case, but it doesn't agree with what these guys are saying. So what's wrong?" So you have to go into the science and learn what their assumptions are. And then you find out their assumptions are the opposite of your assumptions. That's why their conclusions are the opposite and they can't justify their assumptions, for example.

COHEN: So you're having fun now.

ANDERSON: Oh, yes. I'm having more fun than I've ever had, because I've tried to get rid of all these administrative responsibilities. They were always a duty. I never liked it.

COHEN: Well, you obviously did it all right.

ANDERSON: Well, I did it, but it was always a duty. I've been president of all kinds of things, and the head of this and the chairman of that, but I never volunteered or ran for any of these things.

DON L. ANDERSON

SESSION 3

April 13, 1999

Begin Tape 3, Side 1

COHEN: Dr. Anderson, welcome back.

ANDERSON: Good afternoon.

COHEN: When we talked about the Seismic Laboratory—

ANDERSON: Seismological laboratory.

COHEN: The Seismological Laboratory. You told me about the marvelous spirit there and the young guys. We didn't talk about Richter. What was Richter doing all this time? I mean, he must have been there. And in some sense, because of the scale, he is a well-known geologist. Can you talk a little bit about Charles Richter?

ANDERSON: Charles Richter is an interesting person. His love was earthquakes, so every time there was an earthquake he was very happy and very busy, particularly when Beno Gutenberg was still alive and they would be all business and even compete a little bit trying to locate the earthquake. They would talk about it and try to understand it, and make sure this information got out to the public. Other than that, I don't remember an awful lot about Charlie, as we all called him. He was always in the measuring room, which is where we measured the photographic paper, or seismograms in those days. So he virtually lived down there with a woman called Vi [Violet] Taylor, who was his chief assistant in locating earthquakes. He was supervising that operation. Virtually he did nothing else, except to locate earthquakes and to file away the data. He taught once in a while. And he wrote a very nice book called *Elementary Seismology* (San Francisco, 1958), that is still a very, very good introduction to earthquakes. I remember his courses as being—well, they weren't very vivacious. He wasn't a vivacious person. They were sort of pedantic but very good. He was a good educator, but there was nothing really creative. He was a good historian. He liked the history of earthquakes. And his book, in fact, is an excellent history of earthquakes. Beno Gutenberg was the innovator and the motivator. He was the great scientist of the two. Charles was more or less his assistant while Beno was alive. That's really all I remember. And then, after Charles Richter retired, he was still a presence at the lab all the time, even after he gave up his responsibilities in the measuring room. He would definitely show up whenever there was an earthquake and participate in the discussion.

COHEN: But he wasn't part of your morning and afternoon coffee hours?

ANDERSON: Oh, yes. He was there a large part of the time, but he never had anything to do with new frontier science. We always turned to him to put this new earthquake into historical context. And he would be an encyclopedia of what other earthquakes had happened in that particular region in the past. So he was a participant, but again, mainly as a link to history rather than the new exciting things that were going on, particularly after Frank Press arrived.

COHEN: Did he in any way try to understand these new things, or was he just not interested?

ANDERSON: Oh, yes. He tried to understand. He was on top of most things. He was a very smart fellow. He certainly didn't disdain the modern, newer developments. He just never really participated in them, although he did like to play with computers. He was a good mathematician.

COHEN: By the time you moved to Mudd that was long after his retirement. Did he come along?

ANDERSON: Yes. In fact, he had an office in the new building. I'm really trying to remember things now—I think he was retired, but he did have an office. One curious thing about Charlie was that at some point he stopped opening mail, so after he died his office was completely full of unopened mail. Cleaning out his office was a little bit harder to deal with than most retired people's.

COHEN: When did he stop opening his mail? When he retired or earlier than that?

ANDERSON: I'm not sure, but it went back many years. After he died there were many, many years of unopened mail just stacked up in his office. So he's got the fame because his name was attached to the magnitude scale. But the great scientists of the early seismological laboratory were clearly Beno Gutenberg and Hugo Benioff, and earlier on, Harry Wood [Harry Oscar Wood]. So of the early people that founded the lab, Harry Wood was the first and then Benioff and Gutenberg. Richter was more or less the assistant in those early days.

COHEN: I read Richter's oral history. It follows what you say—discussion of earthquakes. I asked somebody if he was ever asked to be director and they said, "Oh, no." Evidently doing something like that would not be one of his skills.

ANDERSON: No. It definitely was not. He was very absentminded, and very single-minded.

COHEN: So that he would not have some feeling of unhappiness that somebody else was asked to be director when he had seniority?

ANDERSON: I don't think so. I think Hugo Benioff would have liked to be director. He was a more ambitious person in that sense, and he was a great scientist.

COHEN: But he was not asked?

ANDERSON: As I understand it. That was before my time.

COHEN: Okay.

ANDERSON: You can get that out of Bob Sharp's memoirs.

COHEN: Cropping up continuously in your talking about what you did and what you were interested in comes our marvelous element of helium. Could you make sort of a cohesive story of this for me? I mean, briefly about your interests.

ANDERSON: Well, it's just temporary.

COHEN: Okay.

ANDERSON: I started out as a seismologist and began developing techniques to determine the structure of the interior of the Earth and the evolution of the Earth. Then I got interested in other planets and started to learn a little bit more about how planets got put together and what their internal structures were. But I had never considered myself only a seismologist. I thought of seismology as a very important and necessary tool, and one of the most exciting tools to learn about the structure and evolution of the Earth's interior. I was always interested in the next step: what does this mean in terms of composition and temperature and evolution and physical state?—whether the seismic velocities mean there's partial melt down there, magma chambers, and things like that. Over the years I slowly developed a general idea of the way things worked. I tried to incorporate petrology and mineral physics and material science, and then, later on, geochemistry. The model that I had of the inside of the Earth was a dynamic one, but most of the action was right near the top: plate tectonics and low-velocity zone and asthenosphere were all upper-mantle entities. I could understand to my satisfaction how the Earth was put together and why it should be layered in this way and why plate tectonics was primarily a characteristic of the outer one-half or one-third of the mantle, not the whole mantle. The ideas I developed meant that the crust and the upper mantle came out of the lower mantle. During accretion the Earth melted, and the light stuff floated and became the crust and the upper mantle, and the dense stuff sank and became the core and the inner core and the deep mantle. And it didn't seem possible that the deep interior of the Earth could communicate with the surface today. Then geochemists came along, and they developed a completely different story. In fact, it was almost the opposite story. And in many respects they adopted some of the things I had done, like the discovery of a 650-kilometer discontinuity in the mantle, which in 1967 I showed was a very major change in physical properties. It was due to a phase change, which means a change in crystal structure or in mineralogy but not a change in composition. The geochemists, including some at Caltech, kind of took that division as a profound chemical boundary and started talking about the upper mantle as being one material and the lower mantle being another material, even though it was quite clear to geophysicists and petrologists that this was a phase change that you get even—

COHEN: It was all the same stuff?

ANDERSON: Yes. Just like how ice and water and steam are all the same.

COHEN: Sure.

ANDERSON: But they developed this idea that the lower mantle was chemically different and in fact was primordial, undegased, and primitive star-type material. And this just didn't seem right to me at all, but their data was good. To bring you up to date very quickly, the helium story is important today because there are some volcanoes that generate magmas with very high ratios of helium 3 to helium 4, that is, the primordial isotope versus the radiogenic isotope.

COHEN: You dropped this other [topic], and I would like to finish it. But maybe you're going to bring it up again.

ANDERSON: I'll back up in a minute. But today, I think, most people believe that there is no primordial mantle, no mantle that has not been through the mill and is not differentiated or has not been exposed to outgassing. But this idea, that there is a large part of the mantle—most of the mantle, most geochemical models—that has never been differentiated. It's still in its primordial state and therefore has a lot of helium 3 and the other noble gases. In fact, it's just primitive in all respects. It's something like the carbonaceous meteorites that fall to Earth today. Now, I know this is wrong, but I'm not a chemist, and I couldn't show that they were wrong. All I could do is say that all the geophysical data indicates just the opposite—that the whole Earth has been through the mill, and the outer parts of the Earth are the distillate of the rest of the Earth, rather than any of the Earth being primordial. You just can't make a planet the size of

Earth without melting and vaporizing it. So I had to learn about geochemistry, and I'm not a geochemist. And it takes a long time to understand somebody else's language. Well, let's cut to the chase. The evidence, it turns out, that there is a primitive or primordial or undegased reservoir is based on an assumption. The helium-3-to-helium-4 ratio of some volcanoes, in particular Hawaii, for example, and Iceland—volcanic islands that are called hot-spots—is higher than in most magmas that come out of the mantle. The assumption is that if you have a helium-3-to-helium-4 ratio that's greater than the average ratio that you get in mid-ocean ridges, which is the most common place for volcanoes and the most abundant kind of basalt, then these high helium 3:4 ratios must mean that you have excess helium 3. The helium 3 is the primordial isotope. It had to be in the Earth since the beginning. And since you have a higher helium-3-to-helium-4 ratio, it must mean there is a reservoir somewhere in the Earth that is providing primordial gas. That means it's never been melted, never been degased, never been brought to the surface before. As I said, this just didn't seem right. So I finally discovered that there was a semantic trick going on here. Of course, when you have a ratio like the helium-3-to-helium-4 ratio, if it's high compared to something else, then it could be because you have more helium 3 than this other thing or less helium 4 than this other thing. And much to my surprise, I found out that the geochemists had never investigated both alternatives. They had always just assumed that a high helium 3:4 ratio meant high helium 3. In fact, they went to a shorthand designation. They stopped referring to high helium 3:4 ratios and started talking about them as high helium 3 or excess helium 3 or magmas from an undegased primordial reservoir. So now I became interested in philosophy of science and semantics, because they clearly misled themselves. And all the evidence, when you look at it carefully, supports the idea that these places that have been called primordial or high helium 3 places are actually low helium 4 places. They don't have much helium 3 at all—very little helium 3 compared to mid-ocean ridges. They are low helium 3 places but they are called "high-3He" places!

COHEN: And helium 3 is the primordial stuff.

ANDERSON: Right. Now, the helium 4 is important because that's made every day by decay of uranium and thorium, so it's a daughter product. So as time goes on, the helium-3-to-helium-4 ratio decreases, because any rock that has uranium and thorium will generate helium 4 but not helium 3. So the natural reinterpretation of their data was that these basalts that they thought were coming from deep, primitive mantle were actually coming from very shallow mantle that was very low in uranium and thorium. And it's old. So, for example, if you take a rock [from] two billion years ago or one billion years ago, it will have a high helium 3:4 ratio. But then, as time goes on, that ratio will decline because the helium 4 is being made. However, if you take a rock and pull a melt out of it, at some point the melt loses its gas, and this gas can get trapped in the residual rock, with little uranium and thorium. Since melts can wet grain boundaries, they can migrate right up to the top and come out of volcanoes. But any trapped gas gets trapped in the grains, and it doesn't wet grain boundaries, so it gets trapped in the rock. So I just turned the story upside-down. I'm not a geochemist, but I can recognize a logical fallacy when I see one. By correcting that logical fallacy and investigating the other alternative, everything fell into place. The kind of Earth that I was looking at was the same kind that they were looking at; we just had a different assumption about what this particular ratio meant.

COHEN: Did you have problems when you came out with this?

ANDERSON: Oh, yes. For the geochemists this was one of their favorite, standard, venerable assumptions. I went very slowly. I started writing papers and circulating the papers, and then giving talks and circulating reprints. As near as I could tell, there was nothing wrong with this idea. But none of the geochemists liked it, and I could not get it published, so I finally published it in the *Proceedings of the National Academy of Science*. By this time I had had about twenty reviews of this paper and this idea. They had lots of objections, but nothing that negated the idea involved. So as far as I was concerned, the idea was tested and approved. It was just bias and paradigms and dogma that—

COHEN: *PNAS* really had a role to play over these years for ideas that were unacceptable to the major players.

ANDERSON: Yes. I played by the rules. I submitted this paper and got the comments back. I rewrote it and submitted it again, and got comments back. I rewrote it. It would get rejected and

I would have to send it off to another journal and take into account all the objections. But it still could not get published in the standard, mainstream literature. So I finally decided to just—

COHEN: To go the other route. Now, was this the late sixties or the early seventies?

ANDERSON: Oh, no. This was very recent. This was a couple years ago—the helium part.

COHEN: Okay.

ANDERSON: But you're right. The idea that there's a primordial mantle was developed— oh, more than twenty or twenty-five years ago—largely at Caltech as a matter of fact, by the geochemical group. The first rocks were measured with the new technique that was developed here in the geochemical lab. There were rubidium-strontium techniques used, and uranium-thorium techniques were used, but as mass spectrometers got better and better, then Caltech geochemists started analyzing other kinds of elements, like the rare earth elements, which are much harder to work with. And the first rare earth isotope measurements were on rocks that were very close to having the same isotopic ratio as certain kinds of primitive meteorites—carbonaceous chondrites. So the idea developed that there was indeed a primordial reservoir, and that's when I started digging into the geochemistry. And I showed early on that though there wasn't a primordial reservoir, there were enriched reservoirs and depleted reservoirs, and sometimes they mixed and sometimes they'd look like they were primordial. And I had to show that, if you melt the carbonaceous chondrite by ten percent then most of these elements that the geochemists were using were in the melt. Very few of them were left behind in the crystals they were on. That means that the ratios of these elements, and eventually their isotopic ratios, stayed the same as the original unmelted rock. So there was another fallacy: that the only way you could have these primitive ratios was to have a primitive rock. But if you melt that rock and take the melt out, you've inherited all those primitive ratios, and then you can proceed along and get to these things that look primitive, but you also get some that look enriched. So early on I showed that there were enriched reservoirs and depleted reservoirs rather than primordial reservoirs. You might accidentally get one that looked like a chondrite that had never been melted, but that was just a coincidence. So that became clear to everybody, and pretty soon the geochemists started talking about enriched reservoirs. And in fact, they even called them EM1 and EM2—funny acronyms for these things that were basically non-ocean ridge basalt but non-primitive—they were enriched. And later it became clear that these geochemical signals were recycled materials such as sediments and continental crust and sea water, so it was as far as you could get from the primitive idea. So the fact that there might be a large primitive reservoir sort of was laid to rest, until the helium came along, and that resurrected it. Geochemists, going back to Harold Urey, really like the idea of primordial matter. So we've had to start all over again trying to kill this idea.

COHEN: This is accepted now? There's no problem?

ANDERSON: With what?

COHEN: With this theory that the Earth has all been through the mill, so to speak.

ANDERSON: It depends on your background. If you are a planetary scientist or a geophysicist or you worry about accretional heating and separation of the Earth into its various components or you look at the other planets—all the other planets are clearly differentiated—then you are favorable to that point of view. Many geochemists now are starting to adopt the point of view that recycling the Earth recycles a lot of its materials, and there might not be much room for a primitive reservoir. But for the people that study noble gases this is still their preferred model—that there is a huge, undegased reservoir in the mantle. And it's really just because of that one assumption.

COHEN: I see. Well, it's good that there are still arguments going on. Are you still working on these gases now? I have a paper here that's very recent, from '98: "How to Explain Various Paradoxes Associated with Mantle Noble Gas Geochemistry." That's quite recent.

ANDERSON: Yes. I'm almost done. As far as I'm concerned it's a solved problem. But the hard part is convincing other people.

COHEN: So what are you doing right now?

ANDERSON: I'm more or less becoming a surface geology type. I spent a lot of time worrying about the inside of the Earth, and now I'm trying to connect things that we see from satellite altimetry, satellite gravity measurements, and magnetic field measurements. I'm trying to relate this to the inside of the Earth. So, for example, we now have exquisite global maps of the topography and the bathymetry of the Earth, so we can see the mid-ocean ridges and the great fracture zones. And I'm trying to relate the volcanoes and the earthquakes to the fabric of the surface. I think I'm showing that the surface plate, the lithosphere, allows volcanoes to happen where they happen, and it's not much to do with the convection in the mantle. So, for example, if a plate is extending or if it has cracks in it, then the magma can actually break its way through and you have a volcano. If the plate is strong or thick or old or under compression, like an arch or a dome, then it manages to keep the magma down. So the basic idea there is from seismology. Underneath the plate we have what used to be called the low-velocity zone. Gutenberg discovered that here. It's now called the asthenosphere, which means the weak layer. And I showed some years ago that the properties of this layer require the presence of magma, or the presence of melt, and that the only thing keeping this melt down is the lithosphere. So the question is not, "Why are there volcanoes?," but, from my point of view it's the other way around: "Why aren't volcanoes everywhere?" And the answer is: you have to understand the plate and you have to understand the lithosphere and you have to realize that it's not a rigid plate—it's broken up and in some places it's weaker and in some places it's thinner and this is where the volcanoes are.

COHEN: So that has nothing to do with—I mean, what I thought I knew was that volcanoes happen where the plates come together.

ANDERSON: Oh. I'm talking about what are called "mid-plate" volcanoes. We have the plate-boundary volcanoes, which I think everybody thinks they understand. Those are the mid-ocean ridges and the island arcs.

COHEN: Okay.

ANDERSON: Mid-ocean ridge volcanoes are by far the biggest source of magma. And then there is the subduction zone, or convergent-margin volcanoes: the Island Arcs, the Ring of Fire. We understand those volcanoes. But it's the volcanoes that are in the middle of plates, like Hawaii, or the volcanoes that are near ridges but have more magma or are bigger than the mid-ocean ridge volcanoes, or they come above sea level. So it's these that people have been trying to understand in terms of narrow plumes in the mantle, but which I'm suggesting are places where the lithosphere is particularly weak or where we can break through the lithosphere using the magma as a buoyant pressure source, or where magma is focused.

COHEN: Why would it mean that there are some places where the lithosphere is weak? I mean, is that just because it happened that way?

ANDERSON: Well, these maps show very clearly that the outer shell of the Earth is a mosaic. It's made up of many, many pieces. There are only about twelve major plates that move coherently, but if you look at the individual plates you'll find out that every plate is assembled from dozens of smaller plates. And where they come together the ages are different, the geology's different, the composition's different, and the chemistry's different, and you tend to develop weak zones where plates have become sutured together—that's the technical word.

COHEN: I see. So there are systems within the system.

ANDERSON: Right. So any plate that you're looking at is a combination of lots of old plates. If you change the boundary conditions—for example, if North America, which had a trench off-shore, runs into a ridge, then the ridge is where a new plate is made, and the subduction zone, or the trench, is where you destroy a plate. When they come together they both annihilate, so that changes the boundary condition. That means that what used to be one big plate in the middle of the Pacific breaks up into three or four plates. Or it could be the other way around: three or four plates could, all of a sudden, decide to move in the same direction. Then, by definition, they become one plate.

COHEN: So you have been doing some writing lately. Is that correct? Would you like to talk about that? You had mentioned something about your becoming a philosopher.

ANDERSON: Because of my difficulties in changing fields and then getting the ideas published that derived from my previous experience but then were consistent with somebody else's data, I've discovered that it's a quite common phenomenon for people to have their own little clubs or fraternities or groups or, as it's now called, a paradigm. And everybody in a given paradigm makes the same assumptions, uses the same data, and thinks/knows what's important and what isn't important. It's sort of like the City of Troy: defenders of a paradigm are very much like defenders of a walled city. And the only way you can topple that city is, not from the inside, but bringing in something from outside, like a Trojan horse. So I wrote a little article about that.

COHEN: Where did you get that published?

ANDERSON: I've developed a pen name. I call it "Dev L. Advocate." I periodically submit a short article to *EOS*.

COHEN: *EOS*?

ANDERSON: It's the house organ of the American Geophysical Union. So it's a secret; nobody's supposed to know who writes those little articles. They are kind of tongue-in-cheek comments.

COHEN: So other people besides you do this sort of thing?

ANDERSON: Well, I've tried to get other people to do it so I can honestly say it's not me only, but every time I've solicited an article and then had it submitted it was rejected. In fact, I'm the only one that's been writing these articles that have gotten accepted.

COHEN: I see.

ANDERSON: Now I'm also writing a book. The working title is something like "What Planet Do You Live on Anyway?" And it's a discussion of the various cults, or subdisciplines, in earth sciences and how they have completely different ways of looking at the Earth—in fact, their Earths are all quite different. That's what I'm trying to do—integrate a lot of data and come up with one Earth instead of a different Earth for every specialty.

COHEN: But how it's looked at. I see.

ANDERSON: It's like the elephant.

COHEN: Right.

ANDERSON: The Hindustanis and the elephant.

COHEN: I see. So I can tell by your smile that you are having a lot of fun doing this.

ANDERSON: Yes. Well, I was doing that during the evenings on my sabbatical last year. I doubt very much if it will get published.

COHEN: But anyway, you have produced this manuscript.

ANDERSON: Yes, I have produced a manuscript.

COHEN: Okay. Well, you never know. But now you are back from your sabbatical.

ANDERSON: I'm back to more normal kinds of—

COHEN: Well, serious or whatever, but maybe this other is serious.

ANDERSON: It's dead serious, but it's done tongue-in-cheek.

COHEN: Okay. Well, those were the things I wanted to catch up on. Before we go on to the next topic of business, was there anything that you thought of that you thought you should say?

ANDERSON: Oh, I had some ideas last time, but I think you filled it in pretty well.

COHEN: Okay. I wanted to talk about your visiting positions, like when you were away last year, but I think you've already answered that. You spend more time doing these philosophical things.

ANDERSON: No, not *more* time. Actually, it was quite an interesting year.

COHEN: Where were you?

ANDERSON: I started out in La Jolla, at the Scripps Institution of Oceanography and the Institute of Geophysics and Planetary Physics.

COHEN: IGPP.

ANDERSON: IGPP—in January. But I had made up a plan for myself to visit about eight different institutions and work with certain people and solve certain problems, many of which were related to trying to interconnect various disciplines. My first step was to go to La Jolla, where Dave Sandwell is a professor. He is an expert on using satellite techniques to determine the bathymetry of the sea floor.

COHEN: What is that word? I'm not catching it.

ANDERSON: Bathymetry?

COHEN: Yes.

ANDERSON: It just means the depth of the ocean. It's like topography, only it's inverse. Oceanographers refer to bathymetry and geologists refer to topography, but it's the same stuff. So we drew some maps of the world that not only had the bathymetry and topography, but also the ages of the sea floor and volcanoes and earthquakes and magnetic stripes and large volcanic provinces—just a wide variety of tectonic indicators. When you put it [all] together you see the stresses in the plates and you see where volcanoes should be and where volcanoes shouldn't be. We haven't started writing this up yet, but the first step was to take about ten different kinds of data sets and put them all in one map and then sit back and try to figure out how the top of the Earth works and how it relates to the inside of the Earth. So I spent three months doing that. Then I went up to Stanford for another three months, working with people who are experts on volcanoes and dike injection and cracks and stress, because I was starting to look at the Earth from a completely different point of view. Instead of the inside convecting away and breaking the crust or the lithosphere—which is sort of what I've done all my life, I always thought the inside was more important than the outside—I started wondering about that last step, how does the plate itself influence convection of the mantle, and what does it take to make a volcano? So I learned a lot about dikes and things there from experts in that area. I also went to Paris and RPI. Then I went to Hawaii for a couple weeks to work with another mapper, drawing maps of different projections looking down the North Pole and the South Pole. Different kinds of maps than the standard Mercator. You can't always look at the Earth in the same way. You have to look at it with fresh angles. You tend to ignore the regions near the poles because of the way Mercator maps are made. About that time I got informed I had won the Crafoord Prize. That was very exciting. Then, while I was in Hawaii, I got a request to fill out an FBI form for some honor that I might get. That turned out to be the Presidential Medal. And I was on sabbatical using a Guggenheim fellowship.

COHEN: Was your family with you?

ANDERSON: No. My wife teaches at UCLA, and she couldn't take off much time. But I arranged it so every two weeks or so she would come visit me or I would come home. So I sort of did the whole sabbatical in two- and three-week bundles. We did go to Hawaii together. We managed to do that. And we went to Sweden together.

COHEN: Well, you'll tell me about that separately.

ANDERSON: And then when the weather got better on the East Coast, I started migrating east.

COHEN: So this was really a sabbatical of looking at data from different viewpoints. You weren't actually doing any experiments or anything on this sabbatical.

ANDERSON: No. We were making maps using computer capabilities at these various places. Except for Hawaii and La Jolla where we worked pretty hard making these beautiful colored maps, most of the rest of the time was just talking to people, giving talks, reading papers, writing, and learning a lot about things that I didn't know much about. That's sort of been the story of my life: I take off a bit of time and try to learn a new field that seems to be important at the time. I don't become an expert in it, but at least I'm able to talk the language of these other people. And I've been through fields like petrology and geochemistry and planetary science. Noble gas geochemistry was the most recent, and then surface tectonics and what's called geodynamics.

COHEN: Well, I think that's traditionally what a sabbatical is supposed to be.

ANDERSON: Yes. Then I went to Woods Hole. There is an oceanography group there, and a lot of geochemists. So I spent most of my time there arguing about geochemistry. And then [I went] to the Department of Terrestrial Magnetism, which is a branch of the Carnegie Institution in Washington. So I spent some time down there interacting with both geophysicists and geochemists. They have a good high-pressure capability—mineral physics, which is another one of my interests. So I covered a lot of different areas, but I'm not sure that's the best thing to do—to travel around from place to place and just spend a little bit of time in each place—but I think it worked. I enjoyed it.

COHEN: I think it's a little wearing on the body.

ANDERSON: It was wearing, yes—wearing on the family, too, because I was separated quite a bit from my wife at that time.

COHEN: So then you heard about this Crafoord Award. But that's not the first award you got.

ANDERSON: No, it was the only one with money.

COHEN: I see. It takes on a different importance.

ANDERSON: Yes. Most of them give you a gold medal, a pat on the head, and you have to buy a plane ticket to fly to England or some place to pick it up.

COHEN: Tell us about it. You knew you were going. That was last September, wasn't it?

ANDERSON: Yes. I was at Stanford on a weekend, or a Friday maybe, and Nancy had come up to visit me for the weekend when I got this message to call so-and-so in Sweden. And the message was garbled. I was supposed to call Dr. Norby at the Royal Swedish Academy of Sciences, but whoever took the message down garbled it. It said that I was supposed to call the Norway Swedish Academy of Science. I finally sorted that out and talked to Dr. Norby, who happens to be the president of the Royal Swedish Academy of Science. They are the ones that give out this prize. It's the same group that gives out the Nobel Prize. The Crafoord family is interesting. He made his money in artificial kidneys. He invested in the early models and made a fortune. He decided to endow a Nobel-like prize to be given by the Swedish Academy to recognize those fields that are not traditionally honored by the Nobel award. And I think there are about four fields that it rotates around. So any given subdiscipline in any field takes a long time. It's not like the Nobel Prize, where you get two or three winners in a given field every year. earth sciences only gets rotated into about every four to five years. And then it depends on what subdiscipline they're interested in that year. So it's a miracle. There's no reason why my particular field was picked.

COHEN: Do you think they decide on the field or the person?

ANDERSON: I don't know. I don't know the inner workings of this thing, but I suspect that after they honored geochemistry—when Wasserburg got it—they decided to do something in geophysics or something with the Earth. I think they had general ideas of what the next area was.

COHEN: So you were actually after Wasserburg in the next geology project—in some sense, the next geology winner.

ANDERSON: His was about 1986.

COHEN: Well, I know Murph Goldberger was still here.

ANDERSON: There might have been another one or two in there. I think Willi Dansgaard and Nicholas Shackleton received the award, and Adolf Seilacher and James Van Allen as well. They are earth scientists, in quite diverse disciplines.

COHEN: I know the astronomers, but I don't know the geologists that got the prize. I'll tell you another story about this later.

ANDERSON: So I called him. It was five o'clock in the morning. You have to call at that time to get the phone call through. So Dr. Norby informed me of this thing. It was really quite a shock and quite startling. I could hardly tell my wife what was happening, because I couldn't speak. But we went out and had a nice breakfast at a fancy hotel in the Stanford area. We took our cell phone with us and we called the kids. We decided then and there to make it a family get-together.

COHEN: So they let you know when? In what month did they call you?

ANDERSON: It would have been about April.

COHEN: April. And then you had to go the following September.

ANDERSON: So it was a lot of lead-time. It was a good time. The Swedes really know how to do these things, because they do so much of it. It was very well done.

COHEN: Now, you weren't there with a big group of people though?

ANDERSON: No. That's the amazing thing. It's probably more interesting than a Nobel Prize, because there were just two of us. Adam M. Dziewonski of Harvard and I split the award. So they had this big ceremony, big dinner, and big celebration just for two of us, instead of twelve or fifteen or whatever the standard Nobel Prize ceremony is like. So it's a lot less formal. The king and queen don't wear their get-up; they just wear ordinary clothes. And they seemed to be more comfortable than when they're dressed up with their medals and their gowns and their crowns.

COHEN: Now, you had to give some talks. I mean, that's part of it, right?

ANDERSON: Yes. We went to Lund, which is the university town in the south of Sweden. It's a medieval university and cathedral town. That's where the Crafoords lived. So we flew to Copenhagen and took a ferry over to Lund and spent a couple days there giving talks and being wined and dined. Then we flew up to Stockholm, where there were two or three more events. There was a symposium around our work, so there were about eight or nine people that gave talks during our two-day symposium. Plus there was a little speech to the king and queen during the ceremony.

COHEN: And then you came back.

ANDERSON: We came home.

COHEN: And then this Presidential Medal, which for some reason I thought you were going to last week.

ANDERSON: Well, it's been delayed and delayed and postponed. Finally, it's on the 27th of April.

COHEN: Okay. Now, when did you find out about this?

ANDERSON: I got the FBI form to fill out when I was in Hawaii.

COHEN: And you didn't know why you had to fill it out?

ANDERSON: No, no. No idea at all.

COHEN: Oh, that was for getting into the White House.

ANDERSON: I guess before they give a medal of this type that comes from the President you have to have a FBI check. So I think I was—

COHEN: I was going to say, "[So] the President won't be embarrassed," but I don't think our president [Bill Clinton] minds being embarrassed.

ANDERSON: I think any time you get something like this they've got to make sure you pass the FBI check. They wouldn't want to give it to a spy or somebody with a criminal background or whatever.

COHEN: I see.

ANDERSON: So when did I find out about that? I'm not sure. It was probably in December, because when I was informed they said, "Well, the ceremony will probably be in January or February." And here it is late April.

COHEN: Now, is the delay on their part because the President is so busy with this war and everything?

ANDERSON: Well, it was long before the war. I think it had to do with other things. He was busy being impeached.

COHEN: I see. Okay.

ANDERSON: I think his social calendar—

COHEN: Was put aside.

ANDERSON: Yes.

COHEN: Okay. So now you're going next time. Now, that doesn't come with money or anything?

ANDERSON: No. But I understand that's a nice ceremony. A lot of people at Caltech have been there, and I've been told it's a nice family-oriented ceremony. I was advised by Bob Sharp, who got it, to make sure I take as many of my family members as I can, because it's a nice experience.

COHEN: Well, Peter Goldreich told me about it. I think John Bahcall will be there also.

ANDERSON: That's right. So I guess it's three people in our division, then. And Eugene Shoemaker and Edward Stone, I think, got it too. So it's quite a common award.

COHEN: Well, here everything is common, but in the wider world that's not so. Okay. Well, do you want to make some observations about being at Caltech for thirty, forty—how many years?

ANDERSON: It doesn't seem like that long. Time went by. Caltech's a great place. It's an absolutely unique place. I hope everybody who is ever responsible for its future understands this. Once in a while we get a president or somebody who comes in and tries to change things to make it more like someplace else.

COHEN: Did you read the newspaper yesterday?

ANDERSON: No, I didn't read it.

COHEN: There's a special section of the business section with our president on top, about turning this into a place that generates high-tech companies.

ANDERSON: I don't mind a change, as long as it recognizes the uniqueness of the place. We've always done new things and unique things. I guess you can always quibble about changes, but it's got to remain small and it's got to remain number one and it's got to remain unique. We don't want to be like MIT or Stanford. We shouldn't be jealous of any other place, because we've got more going for us than any of those other places because of our strengths.

COHEN: If you look back at your career, can you think of any people along the way that have been particularly influential for you?

ANDERSON: Well, the unique thing about Caltech is there's so much interaction and so much collegiality. I could not possibly have done what I've done—whatever it is—anyplace else, because I don't have any particular skills of my own and I'm not particularly smart. Just being able to rub elbows with people and talk to people about things—collaborate on things—it's just an absolutely unique place. Of course, it depends on the discipline, too. The Seismo Lab has always been like a commune, with a cooperative and sharing kind of environment. But other groups in our own division are little empires—little empires with walls around them—and that's a different way of doing science. Everybody has their group, and they're very successful too. But I prefer the—

COHEN: The open doors.

ANDERSON: Complete open doors, and complete interchange. Nothing is hidden away. If somebody wants something—ideas, data, programs, shared students—give it to them.

COHEN: Geology does seem to me like much more of a family than some of the other places.

ANDERSON: Not all parts of geology, but geophysics certainly has always been like that. It's a different kind of science. Seismology has always had to share data, whereas in other fields you tend to generate and hoard your own data until you're damned well ready to put it out, and even then maybe you don't share. But seismology would never get anywhere if everybody who had their seismic station kept their data to themselves. We never would have figured out the inside of the Earth. We needed all the data from everybody. And there's nothing wrong with sharing or even giving away data. Some people think, "Well, they're going to scoop me here. They're going to discover what I was going to discover." But there are so many undiscovered things about the inside of the Earth that that's a very shortsighted attitude not to share your data. I don't see any downside to sharing. You get more people interested and talking together. Things just advance so much quicker when you are completely open and sharing. Everybody's career advances even more if you share your data rather than hide it. And I can't understand why all science isn't like that. [Tape Ends]