



DONALD V. HELMBERGER
(Born 1938)

INTERVIEWED BY
SHIRLEY K. COHEN

May 27 and June 3, 1999

By Bob Paz, 2004. Courtesy CIT Public Relations

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Subject area

Geology, geophysics, seismology, planetary science

Abstract

Interview in two sessions, May and June 1999, with Donald V. Helmberger, Smits Family Professor of Geophysics and Planetary Sciences in the Division of Geological and Planetary Sciences.

He begins by recalling his family background and childhood on a farm in Northern Minnesota, one of thirteen children. Matriculates at the University of Minnesota in 1956 (B.S. 1961). Summer work at Scripps Institution of Oceanography, 1961; sails to Alaska on a research vessel studying the structure of the oceanic crust. Transfers to UCSD for graduate work in geophysics; works at Scripps with Russell Raitt (PhD 1967). Recollections of Walter Munk and Freeman Gilbert. Two-year postdoctoral position at MIT, with Frank Press and Nafi Toksoz; introduced to seismology. Works on upper-mantle modeling, supported by U.S. Air Force in connection with underground testing of nuclear weapons. Becomes an assistant professor at Princeton in 1969; following year, joins Caltech Seismology Laboratory as assistant professor of geophysics. After 1971 San Fernando Earthquake, works on high-frequency modeling of earthquakes.

Recollections of Seismo Lab when it was on N. San Rafael Ave., in Pasadena, and of the move in 1974 to South Mudd, on Caltech campus. Memories of Charles Richter. Recalls students: Charles Langston, Thomas Heaton, Thorne Lay, Terry Wallace, Stephen Grand. Comments on Hiroo Kanamori, director of Seismo Lab 1990-1998. Discusses National Science Foundation's establishment of earthquake centers and hopes for Caltech to get the first one; Caltech loses out to SUNY Buffalo.

Succeeds Kanamori as director of the Seismo Lab in 1998. Discusses evolution of directorship since Don Anderson's tenure (1969-1989) and effects of the move to Caltech campus. Discusses Kanamori's directorship and work on TriNet. Discusses Seismo Lab's relations with U.S. Geological Survey and the CUBE program [Caltech-USGS Broadcast of Earthquakes]. Concludes by discussing his own efforts as director of the Seismo Lab and his hopes for its future.

Administrative information

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH DONALD V. HELMBERGER

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

Caltech Archives, 2001

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Dr. Donald V. Helmberger
Pasadena, California

by Shirley K. Cohen

Session 1	May 27, 1999
Session 2	June 3, 1999

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COHEN: I think we'd like to start with your telling us about your family—your father, your mother, what they did.

HELMBERGER: Well, I come from a huge family, so it would probably be a whole session just on family.

COHEN: OK. Well, let's devote ourselves to your parents. [Laughter]

HELMBERGER: My parents come from the Midwest—Minnesota. A good Catholic family. I had six brothers and six sisters; I was the last of thirteen. My mother comes from Iowa originally, I believe. Her name was Klein. Her parents were quite successful farmers, but they did not believe in education, so my mother didn't go to school.

COHEN: At all?

HELMBERGER: I think she may have gone through a couple of grades. She didn't really learn how to read and write until she was a grown-up.

COHEN: Was that because she was a girl, or they just in general didn't believe in education?

HELMBERGER: I don't know, but they obviously didn't believe in it wholeheartedly or they would have done something about it, because I think they were fairly well-off people. My father came from Michigan. It was the only Helmberger family in the whole country at that time. I think his father worked on the Great Lakes, in the shipping business. My father did go to school; he went through the eighth grade.

COHEN: In?

HELMBERGER: In Michigan. And they got married and lived in Minnesota. Actually, before they moved to Minnesota they lived in South Dakota, and he and his brother were in the business of breaking broncos—wild mustangs—and turning them into farm horses.

COHEN: You mean they caught these wild horses?

HELMBERGER: Yes. His brother, Frank, would catch them and my father would tame them enough so that they could be used for farming. He was all scarred up. He had scars all over from mustangs kicking him. So I grew up with a lot of horses around. He was still doing that when I was a kid.

COHEN: So that was his vocation?

HELMBERGER: Well, he was a farmer, but he did that on the side. Farming in Minnesota—northern Minnesota—was never very successful. So he had a lot of other things he would do on the side, and one of them was breaking horses.

COHEN: So you were not part of the Scandinavian community up there in Minnesota?

HELMBERGER: Well, sort of. Yeah, sure, there were a lot of Swedish people in the area where I grew up, and Finnish as well. I grew up in a place called Perham, Minnesota. It's located about halfway between Fargo and Brainerd. If you saw the movie *Fargo*, then you'll know where that is.

COHEN: OK. North?

HELMBERGER: Yes. North, and very cold in the wintertime.

It's really interesting. As I said, my father wasn't very well educated. His first son was very gifted—this was John. John, it turns out, kind of led the family.

COHEN: This is your oldest brother?

HELMBERGER: My oldest brother. He was the first person in the whole Helmberger clan to go to a university. He led the way.

COHEN: So your father was quite receptive to this, even though he hadn't had an education himself?

HELMBERGER: Oh, yes. Yes, yes. Very supportive. My brother John went to the University of Minnesota and eventually got a PhD. It was a poor family, so he kind of showed the kids the ropes—how you do it, where you work. So there was a string of Helmbergers who lived in the same rooming house for about, oh, I guess fifteen years. There was John, and he was followed by Richard and then Peter.

COHEN: How about your sisters?

HELMBERGER: My sisters didn't go to universities. I'm not sure why; I don't think it was lack of enthusiasm. This was during the forties, fifties, so these people were growing up during the Second World War, that era. And it wasn't that easy. John and Richard were both in the Second World War, so they had the GI Bill. I guess women had the GI Bill, too, but—

COHEN: Yes, they did. The Wacs [Women's Army Corps] had the GI Bill. My sister-in-law was a Wac, so that's how I know.

HELMBERGER: My sister was in the Waves—in the navy.

COHEN: Yes, she would have had that, too.

HELMBERGER: I'm sure she probably did. I don't know why [she didn't go to college].

COHEN: Well, it was probably not part of the [expectations at the time].

HELMBERGER: Yes, probably. So they didn't go to school. But my brother John was a professor of economics at Minnesota for many years. And my brother Peter was a professor of economics at the University of Wisconsin for many years; he just retired a couple of years ago.

COHEN: Good for them.

HELMBERGER: So it's a long list of economists. Liberal folks. When [Senator] Eugene McCarthy [D.-Minn.] ran for president [1968], I had five brothers who went to the [Democratic] National Convention as delegates. Of course, they all lost. McCarthy did very poorly.

COHEN: Well, I always wondered whether he really wanted to win.

HELMBERGER: No, he wanted to win. He would have been a great president.

COHEN: Maybe. I worked for McCarthy, too.

HELMBERGER: You did, too?

COHEN: Yes [laughter], so when I make that remark, there's a reason. OK. So you finished high school in your little farming town?

HELMBERGER: That's right. I went to the University of Minnesota, following in the footsteps of my brothers.

COHEN: How did you support yourself there? I mean, you didn't have the GI Bill, did you?

HELMBERGER: No, I certainly didn't. I went there, and I washed pots and pans for five years [laughter], or maybe a year or two. I moved up the ladder of the food service business and became a floor mopper and eventually a manager of the workers. So that was fun.

COHEN: And you were a good student?

HELMBERGER: Not real good, but good enough to go on to [graduate] school.

COHEN: So you had this inspiration from your brothers—that one didn't just stop after getting a bachelor's degree?

HELMBERGER: That's right.

COHEN: Was your interest always in geology?

HELMBERGER: Actually not. I was in physics. And one summer I got a job at Scripps [Institution of Oceanography].

COHEN: From Minnesota you came all the way here to California?

HELMBERGER: Right. There was a professor [of geophysics] at Minnesota named Harold Mooney, who was a very good teacher. He got his PhD from Caltech, at the Seismo Lab [Seismological Laboratory]. And Harold Mooney had a good friend named George Shor, who was a professor and a seagoing researcher at Scripps. And George was always looking for students to go on cruises. You'd get on one of the Scripps ships and go somewhere. And that's what I did in the summer of 1961.

COHEN: So you had gotten your bachelor's degree by then?

HELMBERGER: I had a bachelor's degree, and I was just starting graduate school in physics at the University of Minnesota.

COHEN: That was quite adventuresome, to go all the way from Minnesota to California to go on a ship.

HELMBERGER: It was. And it was, I would say, probably the most stimulating summer that I've ever had. And an eye-opener. We got on a research vessel, and the first thing we did was to sail for seventeen days to Adak Island, Alaska, which is going across the Pacific on one of the longest straight shots you can do. That was quite an experience. The weather was great. Then we went into the Bering Sea, which is to the north of that, and did some seismic work.

COHEN: Was that your first experience with seismic work?

HELMBERGER: Yes. It was a lot of fun. We were there for about a week, and we had a very bad storm—an exceedingly bad storm. It turned into a full gale, and the captain was pretty much worried that we would all drown. This was not a very big vessel, and it took some heavy damage on its superstructure. When the ship finally got back to San Diego...

COHEN: But it was able to get back?

HELMBERGER: It was able to get back, but it wasn't used at sea anymore; it was taken out of commission. That was quite a trip. But the seismology we did was very exciting. We were trying to determine the structure of the oceanic crust. At that time, it was known that the earth's crust underneath the oceans is a lot thinner than continental crust, but they were still trying to figure out how sharp the structures were, and the various seismic properties of the oceanic crust—density, and things like that—and how it varied as you moved around various parts of the ocean. I got quite interested in the data that was taken. And I loved Southern California, especially Scripps on the beach.

COHEN: After Minnesota, yes.

HELMBERGER: The first thing I did was go back and fill out an application to get into Scripps.
[Laughter]

COHEN: To go to UCSD, right. [Laughter]

HELMBERGER: And my grades were lousy, but a fellow named Russ Raitt...

COHEN: Russ?

HELMBERGER: Russell Raitt. He's dead now, but he was my chief mentor. Russ was a very interesting fellow. He was a physicist by training. He got his PhD here at Caltech under a fellow named Millikan, whom everyone knows.

COHEN: OK. He's known here, yes.

HELMBERGER: Russ did the exploration business like a physicist would, in that he paid attention to all the details of science.

COHEN: So he was a physicist?

HELMBERGER: He was a physicist, but he was working in the geophysical domain. But he behaved like a physicist.

COHEN: What do you mean by that?

HELMBERGER: Well, a physicist conducts an experiment in a way that someone else could redo it. So that means if you have a seismic source, like an explosion, you understand exactly what the pressure history looks like when the explosion goes off. And if you fire it in the ocean, it behaves differently as a function of depth. So the first thing you have to do is figure out the physics of the explosion underwater. That's not what people who were doing exploration were doing; they were just measuring the travel time of seismic pulses, not paying attention to how the pulse was made and what actually made it. And he was exactly the same way with the equipment. He knew what the gain of the equipment was and what the equipment was doing to the signals. Whenever you record nature, you don't record it—in those days, at least, you didn't

record it—exactly the way it was; you'd record some analog of it. And he knew all about that stuff as well.

COHEN: Now, was he a professor at Scripps?

HELMBERGER: Yes, he was. So what happened was that when I went there, it was very easy to apply physics to a subject that hadn't had physics applied to it directly before.

COHEN: Oh, because he mentored this approach.

HELMBERGER: That's right. So we set up my research program at Scripps—the two of us.

COHEN: Did he know you from the ship?

HELMBERGER: Yes.

COHEN: Oh, so that was part of why he took you on, even with poor grades.

HELMBERGER: Right.

COHEN: You're saying you had poor grades, but there had to be some reason.

HELMBERGER: Right. Exactly. To make things work in the lab—he liked that. At any rate, we set about trying to do the physics of the whole problem, treating it just as a physicist would. We set off the explosion in water; seismic waves, sound waves, travel through the water. They go to the bottom and interact with a complicated solid bottom, transmit waves across the bottom into the earth, and come back out and back up to the ship, where they're recorded. We tried to correct for all the physics involved and make a model of the whole thing.

COHEN: That was your PhD work?

HELMBERGER: That was my PhD work.

COHEN: It sounds interesting.

HELMBERGER: That, in fact, hasn't really changed. I'm doing the same thing I did in the early sixties. We don't use explosion sources anymore, but we use earthquakes. And we don't study the ocean bottom; we study the inner core, the outer core, the core-mantle boundary, and everything else in the earth.

COHEN: OK. So you finished up your PhD at Scripps?

HELMBERGER: That's right [1967]. And I had the good fortune, there, of...

COHEN: I was going to ask you who else you were involved with there.

HELMBERGER: I was involved with a couple of pretty interesting geophysics people. There was a fellow named Walter Munk.

COHEN: Yes, I know Walter Munk.

HELMBERGER: Walter Munk came from Caltech. Walter is one of those Renaissance people: he doesn't need a computer and doesn't need mathematics per se—he just can sit and think about the physical phenomenon and what the key things to look into are, in order to understand what's happening. So that was very interesting. I took his course and read his book, *The Rotation of the Earth*, which was all about the various parameters that affect the earth's rotation. I quickly discovered that my math was much better than his, and I made the terrible mistake of going through his book and correcting all his equations, because they were always wrong by pi or 2 pi.

COHEN: A detail. [Laughter]

HELMBERGER: Some silly detail. And I got into the mode of looking for integration mistakes. So I got an A, because he said, "Yes, you can do this stuff."

COHEN: Did you show him the corrections?

HELMBERGER: Oh, yes. I never asked him for a letter of recommendation, because I'm sure he thought I was hopeless. [Laughter] But Walter was a phenomenon. Years later, I appreciated him more and more and more.

The other fellow that helped me a lot was Freeman Gilbert. Freeman is one of the world's best applied mathematicians working in geophysics. He taught me all kinds of wave propagation, which I use to this day. I always felt that what I did best [at Scripps] was to interface with a number of people who were kind of isolated and didn't really have the interest to go all the way through from start to finish on a particular subject. And that's the niche I got into. Working between Russ Raitt and Freeman was a very good thing for me to do and it worked out very well for me.

COHEN: Did you know [Scripps physiologist P. F.] Scholander at all?

HELMBERGER: Scholander sounds familiar. He was an oceanographer, right?

COHEN: Yes. I think the boat's name was *The Amazon*.

HELMBERGER: Yes, right. Scripps by that time, the sixties, had already built a second building, which was for geophysics and planetary sciences. It was a separate building, so the school was kind of divided. I had a lot of oceanographer friends, but I wasn't really that much involved.

COHEN: So you were how many years in La Jolla?

HELMBERGER: Let's see. I started there, I guess, in '61 and I graduated in '67. I was there for a while.

COHEN: And you liked living in Southern California?

HELMBERGER: I loved it, just loved it.

COHEN: I was in La Jolla yesterday. There's a lot of traffic there these days, I'll tell you.

HELMBERGER: It's changed. During the sixties, it was really nice.

So I got my degree there. I went to a meeting and gave a talk about modeling. And some fellow I didn't know, named Frank Press [the former director (1957-1965) of Caltech's Seismological Laboratory], offered me a job at MIT.

COHEN: I see. Well, he must have had some clues from his friends in La Jolla, too.

HELMBERGER: Maybe so. But I didn't know who he was. When I got back and asked Freeman who Frank Press was, he thought it was very funny.

COHEN: And he said, "Take the job"?

HELMBERGER: Yes, he said, "Take the job." [Laughter] So I went off to MIT for a couple years as a postdoc.

COHEN: Were you Frank Press's postdoc?

HELMBERGER: Not directly. I was working with Frank and Nafi Toksoz. Nafi graduated from the Seismo Lab as well [1963].

COHEN: Is Toksoz a Greek name?

HELMBERGER: It's a Greek name. Nafi introduced me to earth seismology, in that he knew about earthquakes. And up to that time I had never attempted to model an earthquake or even think about seismograms of the type that people use in that kind of research. So that was a new thing. I learned a lot from Nafi.

COHEN: Now, he was a professor there?

HELMBERGER: He was a professor at MIT. I believe he was an assistant professor then, or he could have been an associate. I'm not sure.

COHEN: So this was a new kind of living for you. You went now from California to the Boston area, Cambridge.

HELMBERGER: '68, '69. Yes. Boston is a lot different from La Jolla, but it's still much nicer than Perham, Minnesota. [Laughter] So I was there for quite a while and did some papers with someone named Ralph Wiggins, who was a very good computer person. We started working on structures deeper than the crust. We started working on upper mantle modeling—modeling triplications from the upper mantle.

COHEN: Was this a big department at MIT at the time?

HELMBERGER: Yes, it was. A lot of people.

COHEN: Because I know Frank [Press] would have built a big department.

HELMBERGER: Frank was primarily interested in free oscillations at that period. He was working primarily on deep structure. And at that stage I was still thinking in terms of top down. We were working on the upper mantle stuff, and we were primarily supported by the air force. The air force was interested in monitoring explosions and deciding how big the explosions were that the Soviets were firing.

COHEN: This was underground testing they were concerned about?

HELMBERGER: Underground testing was a major issue during the late sixties and early seventies, and MIT had a great big grant to monitor that. So what we did was to develop some computer models for figuring out how large these explosions were and what kind of waves they produced and that kind of stuff. I did that for two years. Then I moved on as an assistant professor at Princeton.

COHEN: How did that happen? You gave a paper at a meeting and someone offered you [a job]? [Laughter] Or you just applied for jobs at this point because your postdoc was finished?

HELMBERGER: Yes, I just started applying for jobs. There was a fellow at Princeton named Bob Phinney, whom I had known for quite a while. And Bob Phinney was particularly interested in modeling, so he offered me a job. I was there from 1969 to 1970. I really liked Princeton. It's a great place, a very beautiful campus, good people. But they had a huge milk farm right next to Princeton.

COHEN: A huge milk farm?

HELMBERGER: It was a big dairy farm, and they manufactured milk and fertilizer, and it reminded me too much of home. [Laughter] Then Don Anderson [director of the Seismological Laboratory, 1967-1989] offered me a job at the Seismo Lab, and it was really hard to decide whether to come out here or to stay put. It was one of the biggest decisions of my life.

COHEN: Is that right?

HELMBERGER: Yes.

COHEN: But, of course, you were coming back to Southern California.

HELMBERGER: I liked Southern California a lot. At that time, the smog here was a lot worse than it is now, so it had its drawbacks. But my wife was from La Jolla.

COHEN: When did you get married? Was that in your La Jolla days?

HELMBERGER: Yes, while I was a graduate student at Scripps. And we had two daughters, and she wanted to move back here so that she would be closer to her parents.

COHEN: She was from that area?

HELMBERGER: Born and raised in La Jolla.

COHEN: I see. Was her family part of the academic community?

HELMBERGER: No, they were merchants—her father ran a big furniture store. So I moved back to California in 1970.

COHEN: To Pasadena?

HELMBERGER: To Pasadena.

COHEN: You must have already known Caltech, from previous visits. Or not?

HELMBERGER: No, I didn't really know Caltech very well. I tend to settle into wherever it is I am and go at it.

COHEN: OK.

HELMBERGER: A farm-boy mentality, unfortunately—staying put. Coming to Caltech was, again, a big turn. And when I first arrived here, I was still primarily working on modeling explosions and earth structure.

COHEN: And where would the data come from? Would it just come into some central place and you could get the data?

HELMBERGER: The data was produced by a network of stations all over the country.

COHEN: It seems as though science is more and more done that way.

HELMBERGER: So I was just analyzing the records. But at MIT they didn't really have all these records. You had to special-order them or go somewhere. And at Caltech the old Seismo Lab was one of the few places on earth in which there was a catalog.

COHEN: So all the records came in.

HELMBERGER: All the data was there. That was because of these fellows [Charles F.] Richter and [Beno] Gutenberg [Seismo Lab director from 1947 to 1957]. Those two guys were very interested in global seismology.

COHEN: They felt they had to keep track of the whole world.

HELMBERGER: That's exactly right. They had incredible collections of old earthquakes. Both of them were interested in earthquakes, for all kinds of different reasons.

COHEN: So Caltech, in some sense, was unique—in the sense that all these data were here.

HELMBERGER: Oh, no question about it. The Seismo Lab and [Columbia's] Lamont Geological Observatory were the only two places that had really big data sets—collections of all the earthquakes, and so on.

COHEN: Did people come here just to work on the data, as visitors?

HELMBERGER: Sure, all the time. At any rate, I'd been here for about a year when we had the 1971 earthquake, the San Fernando earthquake, which was quite impressive. It woke us all up. I got out of bed and cut my feet on some glass that had fallen out.

COHEN: And you said, "This is not a model."

HELMBERGER: It really got my attention.

COHEN: Where were you living in Pasadena?

HELMBERGER: In Altadena. Page Drive in Altadena. Way up there. So, of course, that means you have to model the earthquake. So that's what I did.

COHEN: That really shifted your interest.

HELMBERGER: It really did. And we started modeling earthquakes, in terms of describing what kinds of forces are involved, how those forces are going to couple into the ground...

COHEN: But people must have already been working on that here.

HELMBERGER: They were already working on it but not really modeling—not the way I’m talking about, high-frequency modeling.

COHEN: What do you mean by “high-frequency modeling”?

HELMBERGER: Well, people at that stage were working on really long-period seismology—periods of 50 to 200 seconds. Those are real long wavelengths. Those are the periods you look at if you’re going to study how the earth vibrates as a whole.

COHEN: OK.

HELMBERGER: They call it free oscillations; it’s a long-period vibration.

COHEN: OK.

HELMBERGER: But what I’m talking about is very short wavelengths—the kinds of wavelengths that do the damage in strong motions and tell you about the details of the orientation and rupture properties. That’s what we started.

COHEN: When you say “we,” who were you working with?

HELMBERGER: Students. My first student was a fellow named Chuck [Charles A.] Langston, who modeled the 1971 San Fernando earthquake. He modeled the signals at large ranges—signals that were produced all around the world.

COHEN: Was this data coming into Caltech?

HELMBERGER: Yes. So he was studying the signals that were propagating through the earth all over the place, and then interpreting those signals in terms of how deep the earthquake was and the complexity of rupture. This was one of the first papers [to show] that the rupture started off with one kind of a fault plan and when it got to the surface it started changing its orientation—changing the direction in which it was slipping. It was not a regular rupture—it was irregular, very complicated. About the same time, there was a student named Tom [Thomas Harrison] Heaton, who is now a professor of engineering seismology here. He started working on the theory of near-end strong shaking and data that occurred right above the earthquake. Pacoima Dam is a very famous series of records, because it was one of the first times that accelerations were higher than G. Before then, there had been speculation that earthquakes could throw things up in the air, but the record at San Fernando proved that things left the ground. No question about it! So that was exciting.

COHEN: Oh, I remember that! It lifted things up and threw them on the floor; they didn't fall over.

HELMBERGER: That's right. And then they fell over. [Laughter]

COHEN: Yes, right, but there was a lifting first.

HELMBERGER: Right.

COHEN: You mean that that [phenomenon] had never been noticed before?

HELMBERGER: It had been noticed before, but never recorded well enough to [verify it]. If it's not recorded in some kind of a record that leaves no doubt about it, people argue. This was the first time it was very definitely a strong recording. And the Pacoima Dam record was used as an example of how bad an earthquake could be. It turns out that it wasn't really all that bad. We now have a lot more records that are far more dangerous than that. But it was the start of a field.

So Tom Heaton then started modeling; he primarily modeled strong motion. And then we started using earthquakes as sources for modeling. We knew their orientations, and we knew what kind of a source earthquakes really put out, and we started treating that as a known source,

and we'd look at what happened when those waves bounced off the core-mantle boundary or wherever. There were a whole bunch of students who spun off from that.

COHEN: Now, you were doing all this work from the Seismo Lab?

HELMBERGER: Oh, yes.

COHEN: You weren't [on campus] in South Mudd yet? Had the move [from San Rafael] taken place?

HELMBERGER: We moved [to South Mudd] in 1974. I think these guys—Tom Heaton and Chuck Langston—were definitely out at the old lab [Kresge Seismological Laboratories, 220 N. San Rafael Ave.].

COHEN: Because I wanted to ask you a little bit about the philosophy of the old lab. But go ahead and finish this.

HELMBERGER: Well, let's do that. The old lab was...

COHEN: Separated physically from the rest of the...

HELMBERGER: Separated physically, and really a different kind of experience. In the old lab—because it was a house—everybody was kind of together. And the communication about what everybody was doing was a daily thing.

COHEN: I got the sense from Don Anderson [that] the central stairway was where you met.

HELMBERGER: In the basement of that building was an old work area that Richter hung out in. It had all the catalogs of seismicity in Southern California since he took over. He had lots and lots of data down there. It was right next to the heating system, and for some reason or other that was the coffee place. There was a big coffee pot down there, and people would lean against the wall around this furnace area, which was nice and warm, and talk seismology.

COHEN: With Richter? He was part of it?

HELMBERGER: Not Richter. Richter didn't really have much to do with it. Richter was a very individualistic kind of person, probably a bit on the arrogant side. He knew the answers to things, and he didn't really need to discuss them with all these dummies.

COHEN: Did it occur to anybody to go ask Richter anything? Or did you get the sense that he didn't value what you had to say?

HELMBERGER: Yes, I think that's it. [Laughter]

COHEN: So people didn't go to him with problems?

HELMBERGER: No, they really didn't. He ran his operation on local seismicity...

COHEN: All by himself?

HELMBERGER: Pretty much by himself. He had an assistant who worked with him. And they did whatever they did. It was a separate thing.

COHEN: So he was a shadow there; he wasn't really part of what was going on.

HELMBERGER: Yes, I think that's right. I don't know how that all came about, but probably it wasn't the case when Gutenberg was there.

COHEN: He would have been a contemporary of Richter's.

HELMBERGER: That's right. They published a lot of papers together, and they worked together. I think when Gutenberg died [January 25, 1960], Richter kind of just went into his own research. And I don't know what transpired; I don't know why that isolation occurred.

COHEN: Did you ever meet Gutenberg, or was that before your time?

HELMBERGER: Gutenberg died before I arrived.

COHEN: He was very different from Richter, I would imagine, coming from such a different background.

HELMBERGER: Oh, I'm sure. Gutenberg was a lot like a modern scientist, in that he had contacts all over the world. And he was interested in everything and knew the physics of things to go with the seismology of things.

COHEN: That was quite different from Richter.

HELMBERGER: Quite, quite different. Richter was primarily interested in earthquakes and the strong shaking produced by earthquakes.

COHEN: So, here you are in this nice, congenial, small space. Did you miss the institute? I mean, [did you miss] meeting other people from the faculty?

HELMBERGER: Well, I didn't. I really liked it out there, because it was quiet. It was on a big estate, so you could walk around. There were oranges all over the place, and avocados, and a tennis court.

COHEN: It sounds like a country club.

HELMBERGER: It was like a big country club. It was beautiful—quiet and a nice place to work.

COHEN: And you were quite congenial with the group that was there?

HELMBERGER: I liked it a lot. You could think about being a seismologist, and that was a good thing. You didn't have to worry about being a geochemist or a geologist and all these other things that happen to you when you become part of earth sciences. So moving down here [to the Caltech campus] was different, but it had a lot of rewards, too. As you say, [when] you're exposed to different thought processes, you start to change.

We had a string of very good students. I'm not sure it was the right thing to do, but what I did was to try to break the earth up into different distance ranges and different structures, and then one of the students would take over the modeling of that part of the earth. There was a fellow named Thorne Lay who started studying the core-mantle boundary. He's now at the University of California at Santa Cruz. Another fellow, named Terry Wallace, worked on the crust as a waveguide. So he worked at distances from the source out to about 1,000 kilometers. Whatever in the earth that controls signals from there to there, he was the world expert on that.

COHEN: Now, I'm assuming that these were PhD projects.

HELMBERGER: Oh yes, these were PhDs. Terry is now at the University of Arizona. They have kept those interests to some degree. Even though they left here, they started a new set of studies doing this globally. A fellow named Steve [Stephen P.] Grand, who is now at the University of Texas, modeled the upper mantle triplications and then got into doing global studies of lateral variation of these structures. And he's done very well with that.

COHEN: Now, you didn't go out into the field to do any of this? You just sat with your numbers in the lab?

HELMBERGER: You need computer models to do all this. You write codes to take the data...
[Tape ends]

Begin Tape 1, Side 2

COHEN: So you had these students doing these different things and then putting together the model.

HELMBERGER: Yes, right.

COHEN: And in 1974 you all had to move over here.

HELMBERGER: Yes, that's right. I don't know whether some of the students were at the old lab for more than a year or so before they were here on the main campus.

COHEN: Tell me how you felt about moving over to this big building and joining the real world of Caltech. You must have come over before, for things like seminars and so forth, didn't you?

HELMBERGER: Oh, yes. And for the computer, too. The computer was always over here.

COHEN: Did you do any teaching? I mean, you must have done teaching.

HELMBERGER: Yes, we'd come over here to teach. But that was kind of funny, because we'd come over here and teach the same people who were in the lab.

COHEN: You mean everybody had to come over here and then you'd all go back? [Laughter]

HELMBERGER: Right. But the other classes were here, too, and the people in the PhD program were taking classes from other people all along, so they were over here for classes. So it was much more convenient to move over here. But I guess the spirit of the lab and the emphasis on geophysics is now different. And every year we become less and less like the old lab—less specialized in geophysics and [moving more into] earth sciences.

COHEN: What is the difference? Tell me from the beginning the subtle changes. Define for me "geophysics" and what you do.

HELMBERGER: Well, geophysics, when I first came here, was a specialized discipline in which you learned the physics and math of what you were doing. That was a specialty. You put all your energy into that.

COHEN: Into the physics and math of the phenomenon, whatever it is?

HELMBERGER: That's right. And doing seismology. And there weren't so many introductory courses. You'd jump right in with the physics of the problem. No introductory courses, just the

real thing. And what's happened is that students now have to know a lot about geology, geochemistry, chemistry, biochemistry.

COHEN: Is that because the emphasis has changed, on what seismology is?

HELMBERGER: Well, as you get this field folded into earth sciences, it's all in the same division [the Division of Geological and Planetary Sciences]. And they keep on getting new faculty, and these people all have to teach, right?

COHEN: You're implying that [seismologists] didn't really need all those courses.

HELMBERGER: It depends on what you're going to do.

COHEN: Well, students don't really know what they're going to do; they have to have exposure in order to—

HELMBERGER: That's right. So what's happening is that the students we're producing now are far better educated. They're very educated. They know a lot about a lot of things, but they're not specialists.

COHEN: So that's already a difference in philosophy.

HELMBERGER: No question about it.

COHEN: When you came over here in 1974, did you still stay together as your small group, or were you suddenly—

HELMBERGER: Well, the modeling group I had started just stayed as it was. I don't think it changed any. We still do the same kind of thing. We just do different parts of the earth.

COHEN: And back then you had very little to do with what else was going on at Caltech?

HELMBERGER: Well, as time went on, we interacted more and more with people who were doing convection models. And these days I work with Mike [Michael C.] Gurnis [professor of geophysics], who runs dynamic models. We have a rather nice working relationship, with a few students who work [with both of us] on PhDs. I haven't really worked very much with Hiroo Kanamori [John E. and Hazel S. Smits Professor of Geophysics and director of the Seismo Lab from 1990 to 1998]. Hiroo is more of a Walter Munk type.

COHEN: Intuitive, you mean?

HELMBERGER: Yes. We've shared some students, too, over the years—who've gotten probably the best of both worlds, or the worst, depending on your point of view. [Laughter]

COHEN: Did you have very much to do with the people over in [the Division of Engineering and Applied Science], like Paul Jennings [professor of civil engineering and applied mechanics]?

HELMBERGER: Paul Jennings and I did a very successful workshop, where we tried to get engineers and seismologists together to try to solve strong-ground-motion problems together. That got off to a terrific start.

COHEN: When would that have been?

HELMBERGER: I would say about '77. I guess in the early eighties that was still going on. The NSF [National Science Foundation] sponsored some of these workshops; I think we had two or three. And we had some large meetings, with large groups of seismologists and earthquake engineers talking about problems. And then the NSF changed its style. It set up earthquake centers—centers for earthquake engineering research—and there was a big scramble about who was going to get the [first] center. Paul Jennings assumed it was going to be here.

COHEN: Is that when [the State University of New York at] Buffalo got it?

HELMBERGER: Buffalo got it [the Multidisciplinary Center for Earthquake Engineering Research, est. 1986]. From my point of view that was terrible, because immediately the NSF

Division of Earthquake Engineering would have nothing [more] to do with seismologists. The NSF funding for seismology in strong motion just died.

COHEN: So that was really a disaster.

HELMBERGER: A disaster. I don't know why they would have an earthquake center in Buffalo, and I don't know what those people are doing. But it was a disaster for Caltech.

COHEN: Were the reports properly written up? Or did Caltech just assume that without too much effort they were going to get it?

HELMBERGER: I don't know. But there was some nasty bickering going on, [to the effect] that the Buffalo people in fact had copied things right out of articles that Jennings and [George W.] Housner [Carl F. Braun Professor of Engineering, now emeritus] wrote. The Buffalo people just picked things up and put them in their proposal, with no citing where this stuff came from or anything. So there were some nasty letters circulated.

COHEN: But the NSF didn't rescind its decision.

HELMBERGER: No, it didn't. All this did was to make the NSF really unpleasant to Caltech proposals for the next ten years.

COHEN: Oh, really?

HELMBERGER: Yes. Very bad things happened.

COHEN: Happened because you objected to the decision?

HELMBERGER: Right.

COHEN: That's very interesting.

HELMBERGER: It was pretty bad. I think one of the reasons Jennings went into administration [Jennings was vice president and provost from 1989 to 1995—ed.] was simply because he had been isolated by NSF.

COHEN: It was a bad time.

HELMBERGER: I think it was a bad deal, yes.

COHEN: So the idea was that everybody had thought that there would be an earthquake center here.

HELMBERGER: Oh, yes. I think everybody assumed that that would be the case, for all kinds of reasons. First...

COHEN: You had all the records.

HELMBERGER: Well, we have the earthquakes.

COHEN: [Laughter] OK.

HELMBERGER: Right? The exposure to damage in San Francisco and Los Angeles is obvious.

COHEN: Yes.

HELMBERGER: Right? And we live here. Second, the people, like Jennings and Housner, were...

COHEN: Well, they were world famous.

HELMBERGER: They were world famous. They wrote the specs for all the high-rises built in downtown Los Angeles. These guys were the engineers who were doing it. Third, they realized that modeling was a very important part of this. At that time, some of the best students in

theoretical seismology were engineers; they were coming over and taking classes with us and vice versa.

COHEN: And that combination was unique to Caltech?

HELMBERGER: That was unique to Caltech, and there was funding.

COHEN: Was it all NSF funding?

HELMBERGER: It was NSF funding.

COHEN: So do you have any clue why [Buffalo got the center]? Was it the congressmen? The decision must have been political. Or there must have been a board that sat and made the decision.

HELMBERGER: I don't know how it happened. But at that time Tom Heaton was here, as a postdoc—this was some fifteen years ago. And a fellow named [Stephen H.] Hartzell was here as a postdoc. These were two of the first people to model the strong motions of earthquakes systematically. They wrote papers about how dangerous earthquakes are and what kinds of motions occur. These people were all supported by NSF engineering [money]. And I think there were students in the Engineering Division who were supported by the same program and working on the same kinds of problems. It was a very healthy, good [partnership].

COHEN: There was a cooperation between the two divisions.

HELMBERGER: Right. And that whole thing was just killed by NSF, basically.

COHEN: And nobody has a clue as to why that was? Or you don't have a clue? I mean, there must have been some...

HELMBERGER: I don't have a clue.

COHEN: It's interesting that nobody I've talked to [in the Geological Division] has mentioned this before.

HELMBERGER: Well, I was the only guy [in the division] who was getting reasonable support from the [NSF] engineering community. That support had been going on for, oh, I would say about six or seven years, and what we were doing was modeling strong shaking in earthquakes.

COHEN: OK. And you were the only people doing that?

HELMBERGER: We were the only people in the world doing that, as far as I could see. Well, the Japanese were probably doing some of it.

COHEN: OK. So other people [in the Geological Division] were not getting support from the NSF?

HELMBERGER: Not from its engineering division. I don't think [Don] Anderson or Hiroo [Kanamori] ever got any NSF engineering money. These were funds to [support research in studying] the motions close in to buildings, the motions that knock them down.

COHEN: So were you part of the group that then wrote up the proposal to get the earthquake center at Caltech?

HELMBERGER: No, because I think it was felt that the engineers here would not appreciate that. There was already some ill feeling among them about what these modelers were doing in their field.

COHEN: OK, I'm beginning to understand this. It was people in the Engineering Division who wrote the proposal to get the center. It wasn't the seismological people—so that's why none of them have talked to me about this. It was an engineering thing.

HELMBERGER: Yes. I'm sure that if you were to interview Paul Jennings, you'd learn a lot.

COHEN: Who funded your work after that?

HELMBERGER: Nobody funded that work, and I stopped working on earthquakes. We still do some work on strong motion, but it's funded by the USGS [United States Geological Survey], and those are small grants. [The NSF] used to put out double the money, which allowed you to get postdocs. And to have a postdoc for a couple of years working on a particular problem is a way to get research done.

COHEN: OK. Maybe this is a good place to stop.

HELMBERGER: OK. [Tape is turned off]

DONALD V. HELMBERGER

SESSION 2

June 3, 1999

Begin Tape 2, Side 1

COHEN: We talked about the work you were doing, and what was particularly interesting was that you worked with the engineers and you're one of the few people who had close connections with them. So in 1998 you took over the directorship of the Seismo Lab from...

HELMBERGER: [Hiroo] Kanamori.

COHEN: Yes. Now, Don [Anderson] said something interesting to me. When Don talked about being director of the Seismo Lab—which he said he enjoyed—he said that every time there was a new provost or a new executive officer they'd try to cut the budget of the Seismo Lab, and that got very discouraging. Were you affected by that at all? Or were you even aware that that was going on?

HELMBERGER: There were a lot of things that affected Don's directorship. When he first got the job [1967], he was out at the old lab [on San Rafael], which he ran pretty tightly, so he could control the research that went on there. He had his pick of whom he wanted to hire. The professors who were in geophysics were pretty much dominated by him. Decisions about which students would be housed where, and everything about the lab, were made by him.

COHEN: So there was no interference from the general group?

HELMBERGER: No interference. Because it was really a scientific institute [in its own right], and the classes that were being taught were structured in many ways to support the research directly. It wasn't trying to produce a well-rounded earth-sciences type of person.

COHEN: You mentioned that last time. You said that you felt that that was actually a strength—

just concentrating on the one thing.

HELMBERGER: Right. So when we moved over here, various division chairmen started to try to pull the Seismo Lab into their division. And that made the boundaries of this institute more transparent. So things started happening. First of all, instead of the director of the Seismo Lab being in charge of students' affairs—such as, does a student get advanced candidacy, or does he qualify for all the courses—decisions with respect to scholastic achievement in geophysics were sent off to someone else.

COHEN: Someone who handled it for the whole division, you're saying?

HELMBERGER: Well, who handled the geophysics part, [which now] was separate from the director. So you no longer had direct control over what students were coming in and what kind of science they were doing. It was removed one level from the direct involvement in research connected with geophysics.

COHEN: Was that true of all the other departments in the Geological Division?

HELMBERGER: Yes. It's not that geophysics was separated out; it meant that geophysics was now going to be treated like geochemistry, or like geology, and not [as the province of] the Seismo Lab.

COHEN: OK. That was a difference.

HELMBERGER: That was a difference. That's just symbolic of the way things changed. And before long, instead of the director's saying who got such-and-such an office, that was handled by the space committee.

COHEN: I see. So you were integrated into the main body of the division.

HELMBERGER: Exactly.

COHEN: And you guys weren't used to that—nor were you very fond of it, I gather.

HELMBERGER: Well, I'm just talking about how frustrating it was for Anderson to see his directorship become essentially watered down, or changed. Let's just say changed.

COHEN: Now, would you attribute that change to whomever was executive officer at the time of the move, or to the division chair?

HELMBERGER: Division chairman. The chairman integrated the Seismo Lab into earth sciences. There was a whole series of people who were involved. I think Gerry Wasserburg [John D. MacArthur Professor of Geology and Geophysics] was probably the most influential.

COHEN: As far as trying to make changes?

HELMBERGER: Right, yes.

COHEN: That wasn't smooth, I know that.

HELMBERGER: I think that was probably a long-standing disagreement between Don and Gerry. They had different opinions about what geophysics really was. And I think they're both right. They both had their points, but they did just kind of duke it out. It wasn't the right thing to do, I don't think. [Laughter]

When Don finally retired [1989], Hiroo Kanamori became director of the Seismo Lab. Hiroo's style was totally different from Don's. He was perfectly happy to let all these people decide where the space was and do this and do that. And he concentrated on upgrading the seismology instrumentation. I don't know if you've talked about this previously, or not. It's a program called TriNet.

COHEN: Yes, he did speak of that. That was a network of stations that they...

HELMBERGER: Yes. I think he put a lot of effort into making that system work. And he was lucky, in that we had a bunch of earthquakes. Whenever we have earthquakes in Southern California, funding for instrumentation will follow. It started with the Whittier earthquake [1987]. And the L. K. Whittier Foundation coughed up a million dollars to buy some new equipment.

COHEN: This equipment was for monitoring the earth itself?

HELMBERGER: Yes. Good equipment—it worked very well. Loma Prieta, in northern California, happened in 1989, and Landers in 1992. And then ARCO put up some money—another million dollars for more equipment. Then there was Northridge [January 1994], and Northridge was really a mother lode, because it occurred right here in Los Angeles. Then TriNet was formed, which is a complicated system that involves the state of California—the bureau of mines—and also the USGS and an organization called FEMA [Federal Emergency Management Agency].

COHEN: That's the agency that gives money for rebuilding, too, doesn't it?

HELMBERGER: Yes, it's a disaster organization that provided funding toward the TriNet system. I would imagine Hiroo talked [to you] about this. The system is still growing all the time, and the amount of data coming in is very exciting.

COHEN: From all these stations that have been installed all over California?

HELMBERGER: That's right. The big job now is to try to figure out how to keep it going and how to store all the data [and to] deliver all the things that were promised about the nature of strong ground-shaking in Southern California: early warning systems and so on. I've inherited a bunch of problems, most of them good, in fact. I have to figure out how to exploit some of the data.

COHEN: So you feel that's been your mission as director, to deal with all this stuff?

HELMBERGER: That's right.

COHEN: What is your relationship with USGS?

HELMBERGER: What is Caltech's, or mine?

COHEN: Yours and Caltech's. I mean, it's hard for me to understand that.

HELMBERGER: Well, it's complicated. The USGS is a relatively conservative organization that is pretty much dominated by geologists. And they sometimes act very strangely. They once tried to hire me; I didn't take it, thank goodness.

COHEN: When was this?

HELMBERGER: About ten years ago. It's been a love-hate relationship all along. They can't decide whether they hate me or not.

COHEN: So who's in charge [at the USGS]? Are you talking about somebody in charge here, or somebody in Washington?

HELMBERGER: This was in Menlo Park. There's a great big group up there, and they kind of dominate the US Geological Survey's earthquake program. Then there's a USGS office down here that's very small.

COHEN: And that's the one right here on Wilson.

HELMBERGER: Yes, right across the street. And I guess they've been kind of dominated by Caltech. But not really—they have their own agenda. I think the working relationship over the years has been splendid. They don't compete with us; we work together.

COHEN: Yes. When there's an earthquake and they go on-air, it always seems as though it's one group working together.

HELMBERGER: It does. It's amazing how well [the partnership] has worked. I don't quite know how it got to be that way.

COHEN: Well, it's probably the people involved.

HELMBERGER: Maybe it's just lucky. Tom Heaton was the last leader of that group.

COHEN: Oh. So he's a USGS person?

HELMBERGER: He was until he resigned and became a professor here.

COHEN: That's what I thought. But he was there originally and then came over to Caltech?

HELMBERGER: Right. He and Hiroo Kanamori are great friends. They publish papers together a lot. It's worked out very well. The new director is Lucy Jones. She's probably more interested in managing the office than Heaton [was]. As far as I can tell, she's doing a splendid job. Occasionally, there's a bit of a flare-up, because Caltech has a little program on the side called CUBE [Caltech-USGS Broadcast of Earthquakes]. And they send out earthquake-information beepers to organizations that contribute money to a fund that pays for the equipment, and essentially for the upkeep of the equipment, producing these warnings. It's like real-time seismology. When an earthquake occurs anywhere in Southern California, it will be located in seconds, and there's a little beeper that various people around Southern California have, that lets you know that there's an earthquake and where it's located and how big it is.

COHEN: How do you get on this list? By donating money to this organization?

HELMBERGER: That's right. You donate money to something called CUBE.

COHEN: And then you get on this list?

HELMBERGER: You get on this list and you get some special information.

COHEN: I'm sure it's all right, but it sounds a little strange.

HELMBERGER: That's what Lucy Jones says, too. Her feeling would be that anything quake-related, anything to do with a hazard, should be freely accessible to anyone who needs to know. And we're moving in that direction. But to get the whole thing started, we had to have some people put up the money to do something. It's not clear that anything ever would have happened until there was someone who had information that other people thought was valuable. There wasn't enough public interest in this to warrant anyone's developing the system.

COHEN: I see. OK.

HELMBERGER: This isn't that uncommon. There are a lot of government agencies that started off essentially being in charge of something. It could be with respect to, say, the airline industry. And they monitor safety, and this and that. But some private organization will come up with some device that makes it easier to land an airplane in the fog, say—and since they developed it, they'll sell it for a profit. Now, suddenly, the planes that have [this device] are safer. Should it be licensed or shouldn't it be? So the government has developed various kinds of partnerships with industry, so that industry can go ahead and produce something about which the agency can then say, "Hey, this is just what we need for public safety."

COHEN: Maybe that's [problematical], because there have been problems with the pharmaceutical companies. The [government] supports the research, and then the [companies] want to have first dibs at developing the drug. So that's kind of—I don't know—funny.

HELMBERGER: It is. And there's a fine line—did the government finance this through the university, or didn't it?

COHEN: Yes, in a sense you're taking money with strings attached.

HELMBERGER: That's right.

COHEN: And universities are not supposed to do that, particularly.

HELMBERGER: Well, it's funny how that whole thing works—I don't really understand that. In the CUBE program, we have this little pager now, but that's all going to be updated. It's all going to be [based on] the technology of Web sites—the next generation of secure Web sites. So that you don't have to worry, when you dial in, whether it's busy or not. There's a whole new set of systems in which you can get a special relay system. So once you get Netscape, you can get in. There's a whole series of things like that that are being developed.

COHEN: We're really going into the unknown, as far as communications go.

HELMBERGER: Communications—and then what's going to happen with them? Who's going to use what? That's another big issue. It turns out that there are a lot of alarm systems. For instance, the National Weather Service has a [warning system]—a certain code that can make your radio turn on and go *beep, beep, beep, beep, beep*. You don't hear it in California, because there isn't any weather that warrants it. But if you're in the Midwest and there's a tornado on the way, your radio will start to beep and you will be given a warning and other pertinent information.

COHEN: That's a public service.

HELMBERGER: That's a public service. And there's no reason why, for instance, [something like] that couldn't be activated in terms of an earthquake warning. Suppose there's a big earthquake on the San Andreas Fault within Los Angeles. You could probably get a ten-second warning.

COHEN: Yes.

HELMBERGER: So classrooms could have these radios. They probably all have radios anyway.

COHEN: Loudspeakers of some kind, yes. Sure.

HELMBERGER: Right. This thing could beep and the kids could head under the table. A lot of people would think that was not bad.

COHEN: So you're saying that there's good reason for this kind of cooperation?

HELMBERGER: Yes. It's a complicated business.

COHEN: Well, I guess some office of ethics has to figure all that out. But it's a matter of controversy between you and the USGS? They don't want selected people getting information?

HELMBERGER: That's right. I guess my feeling is that I could argue on either side, and I have.

COHEN: I see. OK.

HELMBERGER: When we need the money, I'll argue on one side, and when we don't need the money I'll argue on the other side. It's a question of trying to get the money to model and to understand what you've got. And it turns out that most government agencies, unless it's the NSF, will have all kinds of strings attached. They'll say that they want you to buy computers. They'll want you to buy hardware. They'll want you to buy something. But they don't want you to think. They can understand why you need this equipment, but they don't understand that you need graduate students and postdocs and some time to think about what you're going to do with this data once you've gotten it.

COHEN: I see. So what's the major source of your funding these days?

HELMBERGER: The lab gets money from everybody. The money that comes from the NSF is probably the best used, in terms of getting some science out of it.

COHEN: Does that come to individuals? It doesn't come to the lab as such?

HELMBERGER: It comes to individuals.

COHEN: How many people are actually involved in the Seismo Lab? How many professors?

HELMBERGER: About ten. And a lot of them are involved but it's not their main interest.

COHEN: And you've been director for a year now?

HELMBERGER: For a year.

COHEN: And is your style different from former directors? How do you act as director?

HELMBERGER: I still act like I'm trying to be the director.

COHEN: [Laughter] You're still learning the job?

HELMBERGER: Yes. It's very different from anything else I've ever tried to do. It's also complicated, because this TriNet project, which is so big, is run by Kanamori, and he's doing a good job. And what I'm trying to do is not mess it up. I stay away from the things that are working really well and simply try to fold in any kind of modeling I can, around the edges. So I would say that I'm primarily supporting this big project at this stage, trying to make it move more and more toward a modeling effort as opposed to a public service.

COHEN: I see. So you're urging him in that direction? Or does he want to go in that direction by himself?

HELMBERGER: Well, he'll go in any direction he wants.

COHEN: Right, right. OK.

HELMBERGER: And the only thing I do is, whenever I can influence things, that's what I do. He [Kanamori] sees TriNet as a big humanitarian effort. Early warning, real-time seismology, and earthquake preparedness. Where was this event located, and what kind of strong shaking occurred, and which kind of buildings are going to fall down and which are not? That's what he's thinking about. He's thinking about the human aspects of seismology, and the social concern, and that kind of thing—which is very good to do.

COHEN: Now, would you think that that's a good USGS objective, rather than a research institute's objective?

HELMBERGER: I would say that's exactly what the USGS should be doing. So from that point of view, I want them to take over the operation of raising the money and getting the equipment, alarms, and notifications out.

COHEN: I see.

HELMBERGER: And I would like us to be using the data to understand earthquakes in the fundamental sense.

COHEN: And let [the USGS] take over the humanitarian effort.

HELMBERGER: Yes. That's their job, in my opinion.

COHEN: But that's not how Kanamori's running it.

HELMBERGER: Kanamori thinks that unless we get in there and show people how to do it, they won't do it. And he's probably right on.

COHEN: So you can see both sides of it.

HELMBERGER: I think he's achieved what he wanted to achieve. He's kept the thing going. It works. When an earthquake occurs—*bang*, all this stuff comes out.

COHEN: Are any graduate students working on that?

HELMBERGER: No.

COHEN: OK. So then it's not really a university research effort. I mean, that's a good way to judge, I suppose.

HELMBERGER: That's right. But most of my students work on the data it produces.

COHEN: You do modeling, then, from the data?

HELMBERGER: Yes. And he does, too. So everybody's going to be modeling the data.

COHEN: So it really serves both purposes.

HELMBERGER: It does. At any rate, Hiroo is back to his old directorship time, getting this system going.

COHEN: OK. As somebody said, the Seismo Lab is a public relations [organization], in the sense that it's very visible, and people know Caltech for that reason.

HELMBERGER: Yes. The history of how this all goes is interesting. The debates that go on now are the same debates that went on in the thirties: Is the Seismo Lab a research institute that's trying to understand what's inside the earth, or is it supposed to be running a local array in Southern California telling people about their local earthquakes? A local earthquake notification system versus a scientific program.

COHEN: Well, I think the institute has taken the public relations part very seriously. I mean, they've even given them parking spaces over there that nobody else can park in. [Laughter]

HELMBERGER: Yes. And I think that as far as Caltech's accomplishments go, the Seismo Lab probably has the best reputation in the world for geophysics, and that was due to the yin and the yang of the program—going into the reporting of earthquake activity and studying the geophysics of the earth in general. So we had both things going all along, and I think it probably worked out well to do that.

COHEN: OK. If you have the news on, they'll say, "We'll go to Caltech." That's the first thing you hear when there's an earthquake. And I think Caltech doesn't mind that.

HELMBERGER: Right. [Tape ends]

Begin Tape 2, Side 2

HELMBERGER: Then there is the old lab out there in the hills—part of it is still operating. And it has instruments in a tunnel that's going into the hill. That was the reason the lab was there originally, because it was a very quiet seismic station. There was this big, old three-story building—it's full of old records that have been accumulating since the thirties. And we haven't done much about that.

COHEN: So the building is just closed up and the records are there?

HELMBERGER: Yes, stored in big boxes. And occasionally the building leaks. And there are a lot of spiders. I don't know what's in all the boxes now. But we have to do something about archiving those records. We're probably going to scan them or do something with them.

COHEN: And is that one of the things you feel responsible for?

HELMBERGER: Yes. That's something that hasn't been done yet and needs to be worked on. That will be a summer project—to try to start some program to do something about that. And it would be good, too, to be able to get this [data] in a modern form, so that when an earthquake occurs somewhere—in San Bernardino, say...

COHEN: You can go back and look at the older events...

HELMBERGER: We can look at the last one and say, "Hey, this is what happened."

COHEN: You don't have that capability now.

HELMBERGER: No, we don't have the information on-line. We have some papers that are written on old earthquakes, maybe. But you wouldn't be able to look at the records that were

produced unless you went out there and dug around for half a day or so to find the right box of paper records. It's a big job to try to figure out how to handle those old records.

COHEN: How to do all that?

HELMBERGER: Yes, like a librarian stuck with this huge library that's been put together.

COHEN: So will [these records eventually] be put on microfilm, or something?

HELMBERGER: Well, we won't microfilm them, because there are things called scanners, which can create an electronic picture of it.

COHEN: Yes, right. We have all the [Archives] photographs done that way. People get on their own computers at home, as a matter of fact, to see if they want the picture.

HELMBERGER: That's right. So we would scan the whole lot and then archive those scans under some system so that they could be pulled up quickly if you wanted to see what was there. And they'd have some codes whereby you could turn those scans into seismograms and trace records that you work with. There are a lot of things that need to be worked out better.

COHEN: Yes. So this is part of what you have to do. But it sounds like it's not exciting, innovative work. It just has to be done.

HELMBERGER: Yes. That's the way it is.

COHEN: But meanwhile, you continue your own work.

HELMBERGER: Always.

COHEN: The directorship does not take a huge amount of your time?

HELMBERGER: No. Well, it probably could if I really was a director. But as long as I'm a play director, it doesn't seem to.

COHEN: You mean that you let everybody do what they want to do, and never mind?

HELMBERGER: I think that's the way [the directorship] is set up now. There isn't really much of a job, as far as I can tell, as long as everything's working. I suspect that if it didn't work, then I would have to roll up my sleeves.

COHEN: So what you're saying is that Kanamori does the big job of running CUBE and TriNet, and that's the main job of the Seismo people?

HELMBERGER: That's the main job, so he's doing all the work.

COHEN: And the rest of you guys are just doing your research?

HELMBERGER: That's right.

COHEN: [Laughter] OK, OK.

HELMBERGER: [Laughter] Not a bad deal, huh?

COHEN: So that's how you would sum up what's going on there?

HELMBERGER: Yes, that's how I would sum it up.

COHEN: So you don't care how long you have this job? It's not a big deal?

HELMBERGER: No, it's a great deal. I like it.

COHEN: I see. OK. Does this mean you teach less or you have fewer other responsibilities?

HELMBERGER: Well, I would say that the other responsibilities haven't really changed that much. You still have to worry about raising funds and doing the things you do as a professor. And since my interest is in modeling, it's natural just to try to hire more modelers—to get more postdoc modelers.

COHEN: And that's what you want to do. Now, I get some sense that all is not terribly smooth between the different sections of the Division of Geological and Planetary Sciences.

HELMBERGER: I'm sure that's true in every division.

COHEN: But I hear more nice things—that people get along and it's more like a family over there.

HELMBERGER: The whole earth sciences, you mean?

COHEN: The whole earth sciences. That's not so?

HELMBERGER: I don't know what it's like to be in another division, so I can't really say. But typically, people get along. There are some cat fights once in a while, and you can't figure out why these people are acting like kids. They usually yell and scream, but before long they are hugging each other.

COHEN: So it goes on OK.

HELMBERGER: It seems to work itself out. I would say that what I inherited is really a pretty great deal. I guess what I'm mumbling about is that I have nothing to do.

COHEN: As far as being director?

HELMBERGER: That's right.

COHEN: OK. Well, then, the next question should be easy to answer. How have you found your career here at Caltech? It's been a good experience?

HELMBERGER: Fantastic! Fantastic!

COHEN: Do you find that you interact with many other people outside of your field?

HELMBERGER: No.

COHEN: No, that's not your style. So, if you had it to do over again you'd do the same thing? You'd come to Caltech when the opportunity presented itself?

HELMBERGER: I think so.

COHEN: Is there anything else you'd like to put on the record here that I haven't thought to ask you?

HELMBERGER: I guess that's about it. If you ask me again five years from now, I'll give you a different story. [Laughter] [Tape is turned off]