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CLARENCE R. ALLEN
(1925-2021)

INTERVIEWED BY
DAVID A. VALONE

April 1 and 4, 1994

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Geology, geophysics, seismology

Abstract
An interview in two sessions in April 1994 with Clarence R. Allen, emeritus professor of geology and geophysics in the Division of Geological and Planetary Sciences. Dr. Allen matriculated at Reed College in 1942, then spent three years (1943 to 1946) in the Army Air Corps before returning to graduate with a major in physics. He entered Caltech as a graduate student in geophysics in 1949 (MS, 1951; PhD in structural geology and geophysics, 1954). After a year as assistant professor at the University of Minnesota, he came to Caltech as an assistant professor in 1955, becoming a full professor in 1964.

In this interview, he discusses growing up in Southern California; his early interest in science and the outdoors; and his wartime career as a navigator in the Army Air Corps. He recalls his years as a Caltech graduate student and his thesis work on the San Andreas fault; his work on glaciology with Robert P. Sharp; his growing interest in seismology; and his work with Charles Richter, Hugo Benioff, and Beno Gutenberg in Caltech's Seismological Laboratory. He discusses the

interplay at the Seismo Lab in the 1950s between Richter and Gutenberg; the changes wrought by the advent of Frank Press as director in 1957; and the work of Kerry Sieh in paleoseismology. Praise for the division chairmanship (1952-1968) of Robert Sharp.

Comments on the “science” of earthquake prediction and failed prediction efforts by the Russians and the Chinese. The rise of Caltech to public prominence in the area of seismology. He discusses his own work on earthquake faults as they bear on the assessment of seismic hazards and notes the usefulness of probabilistic analysis in long-term planning to avoid earthquake damage.

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)395-4073
Email: archives@caltech.edu

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH CLARENCE R. ALLEN

BY DAVID A. VALONE

PASADENA, CALIFORNIA

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ORAL HISTORY PROJECT

Interview with Clarence R. Allen **by David A. Valone**
Pasadena, California

Session 1 April 1, 1994
Session 2 April 4, 1994

Begin Tape 1, Side 1

VALONE: I'd like to begin by talking about your family background and your youth. Tell me a little bit about your family. Where were you born?

ALLEN: I was born in Palo Alto, California, where my father was doing graduate work on his doctoral thesis at Stanford. Strangely enough, he was born in Pasadena. My paternal grandfather and grandmother came here sometime in the 1880s. They had a little money, which they immediately lost on the booms and busts of the real estate market back then. For the rest of his life, my grandfather was a carpenter; he actually built houses here. He met my grandmother, who was a member of the second graduating class at USC [University of Southern California]. He wasn't educated at all, but she was. They managed to put five children through college, even though he had never had any collegiate experience at all.

My father and mother met when they were students at Pomona College. At that time, Pomona College was a Congregationalist school, where many missionaries and ministers sent their children. It turns out that on both sides—my father's side and my mother's side—the families were in missionary work, in church work. So that's why they happened to be at the same place at the same time.

VALONE: You said your father was doing graduate work.

ALLEN: He was doing graduate work in education. He was somewhat of a mechanic at heart—

he did a lot of work in auto mechanics—and actually taught manual arts at Pomona High School after he graduated from Pomona College. Then he became principal of the high school up in Big Pine, California, just south of Bishop. He was up there for many years, and my brother was born there. I was born the year after he moved down to San Bernardino as assistant superintendent of schools. By that time, he had also gone to Stanford to complete his EdD—doctor of education—under [Ellwood Patterson] Cubberley, who was a very famous educator at Stanford. It's interesting that he started out teaching mechanics and blacksmithing but then got into school administration. Then, after he'd been assistant superintendent of schools in San Bernardino, they were starting a department of education over at the Claremont Colleges, in the graduate school, and he got called over there as a professor. It's not very often that people get into professorships having started out teaching blacksmithing. [Laughter]

At the time of my mother's birth, her family was living in Chihuahua, Mexico, where her father was doing missionary work with the Yaqui Indians, although they temporarily came up to El Paso, Texas, for the actual birth. My mother thus had a strong Mexican cultural background. In our early years, we used to take trips to Mexico quite often—I think partly because she spoke the language fluently and had a great love for Mexico. But she died when I was in sixth grade from some sort of heart failure—actually in association with the birth of my youngest sister. By that time, I had a brother a little bit older than I and sister a little bit younger, and then this sister who survived the childbirth, though my mother did not.

That, of course, was a traumatic experience for the entire family—particularly, of course, for my father, although I didn't appreciate that as much at the time as I do now. A year or two later, I think, he got called to Harvard University to be assistant dean of the graduate school of education at Harvard. So he picked up the family, all four children, and we drove to Boston. The kids were very unhappy there, partly because it was a traumatic period in our lives anyway from having lost our mother, but partly because, having grown up in the West and wearing Levi's, somehow Boston was a terribly strange, formal kind of place. And after two years there, we were so unhappy that he decided to come back here, solely for his family's benefit. So he returned to Claremont, out in the boondocks. Only in later life, having been in academia, do I realize what it meant to have quit a deanship at Harvard to come back to some school people had never heard of before. The fact that he did this entirely for his family is rather remarkable—something I really didn't appreciate until I got into academia myself and realized that deanships at Harvard are quite prestigious.

So we moved back to Claremont, and that's where I graduated from high school. Then, in about my junior year, my father remarried. As a matter of fact, my stepmother, who has been with the family ever since, is still alive and living in Mount San Antonio Gardens in Claremont.

The fact that I'm back in Pasadena is completely accidental, because by the time I came here, there were no family members left in the city. But it's sort of interesting that my family goes back to the 1880s in Pasadena.

VALONE: You said both your father's family and your mother's family came from missionary families. Did you have much religious training as a boy?

ALLEN: No. They were Congregationalists, which, of course, is typically what you'd call a liberal sect. They were not hard-line religious people in any way. Although I went through Sunday school and that sort of thing, my family has never been a terribly religious family.

VALONE: It sounds like you ended up moving around quite a bit when you were in elementary school.

ALLEN: Yes, we used to travel a lot. My father enjoyed driving with the family. I remember he built a whole bunch of things to go on the side of our 1926 Dodge so we could camp out. We made two or three trips across the country, camping out every single night. When my mother was alive, she did all the cooking and whatnot. I think all that travel inspired in me—and also in the other kids—an interest in geography and in other cultures and other places. My father enjoyed those things a great deal. I remember we had some cousins in Eugene, Oregon. One summer, my father decided we were going to visit them, but he was determined to drive to Eugene without ever getting on a paved road. He wanted to stay on dirt roads all the way. [Laughter] Somehow, we found our way over the San Gabriel Mountains and out across the Mojave Desert on dirt roads and all the way up through western Nevada and through eastern Oregon and finally over the Cascades into Eugene.

VALONE: Is that how you began to develop a love of the outdoors as well?

ALLEN: I think so. I was also deeply involved in Boy Scout work. At least at that time, Boy Scouts were very outdoor-oriented. For some reason, I got very much interested in maps. In fact, in high school I used to do a lot of map drafting. I sort of designed my own course in cartography.

This interest of mine was enhanced because of my World War II service. I went into the air force—then called the Army Air Corps—cadet program, and I became a navigator. I ended up on Okinawa, flying in B-29s in the Far East. Navigation for me was a natural interest, partly because of my former interest in maps. Also, I had already completed one year of college by that time, up at Reed College, studying physics and math. So the application of navigation in the Army Air Corps was right down my alley. I think my experience in the Army Air Corps, together with the early traveling my family did, has, no question, affected my subsequent career and the profession I ended up in.

VALONE: When did you get interested in science?

ALLEN: Well, that's hard to say. My father was not a scientist. There were no other scientists in the family. My brother, for instance, is a cattle rancher. He was determined to be a cowboy when he was young, and by God, he still is. I think in high school, particularly at that time—I graduated in 1942—math and physics were the most challenging aspects of the curriculum. And at that time perhaps more than today, if you were looking for a challenging thing to do scholastically, you tended to go into physics and math—especially the boys. Furthermore, it so happened that the physics teacher in my high school in Claremont was not particularly outstanding, but the math teacher just happened to be a Caltech graduate. He was fantastic. I'm not a mathematician, but the fact that I developed skills in mathematics and the fact that I enjoyed it was thanks in large part to a Caltech graduate.

So when I went to college—why did I go to Reed College of all places? Well, I grew up in a college town, in Claremont, and I very much liked Pomona College. I knew a bunch of the professors there. My father taught at the graduate school in Claremont. I liked the small liberal arts college atmosphere, so I was looking for another similar institution. I didn't want to go to school in my own hometown, particularly since I knew too many people on the faculty there. And perhaps with some of my father's guidance, it appeared to me that Reed College—at least at that time—was the other really intellectually inclined liberal arts school on the West Coast.

I'm glad I went to Reed. Now, Reed was a very different place from Pomona, in some ways. Reed was much more experimental. Locally, it was sometimes facetiously called "Red College," because of alleged communist influence, which was a common hysteria of the period. We can look back on that now with a little bit of humor, but at the time it was rather serious. Nevertheless, I found the program there excellent. I majored in physics. But after the first year, I went off into the Army Air Corps. I was old enough to get into the army, and I volunteered. World War II was so different from subsequent wars, in terms of the desire of the people to serve in it and so forth. The attitude of young people toward World War II was clearly very different from the attitude of young people toward the Vietnam War and other wars subsequently.

VALONE: Did you know Reed's reputation for being very liberal before you went there, or was that something you discovered afterward?

ALLEN: Oh, I think so. But that's not what attracted me there. I mean, I'm sure some people went to Reed because they wanted to wave the liberal banner. I guess my family and their politics were somewhat liberal, but certainly not avant-garde and certainly not anti-conservative or anything. In fact, I don't recall at that time in my life that my parents were very active in politics at all, nor was I, really. So I think what took me to Reed was the intellectual atmosphere and not this spirit of controversy, which is still typical of Reed, although it still has very high academic standards. One of the things it's done—I think this is still true today—is that it has a larger proportion of graduates who have gone on to get PhDs than any other university in the country.

The physics program there happened to be very strong. It was dominated by some people from the University of Chicago. You may recall that Chicago at that time was the leader in nuclear studies that eventually led to the development of the atom bomb.

VALONE: Did you go anticipating being a physics major?

ALLEN: I think so. People have asked me why I did that, and I guess it's because it seemed like the hardest thing to do. And when I came back—I was in the army for three years—in '46, I continued in physics. But I minored in history. As it turns out, the most intriguing courses I took at Reed were in U.S. history and Northwest history, because of a very excellent instructor in that field. And my first scholarly publication was in history, not in physics. Nevertheless, I graduated in physics with a minor in history.

Now, back in my junior year I realized I was not a mathematician at heart. I couldn't think in abstract terms quite like a real mathematician could. I knew that wasn't my real forte; probably theoretical physics would not have been either, for allied reasons. So I was sort of searching for some way to use that physics background in a different way.

One summer I was working at the Naval Research Lab in Washington, and I drove back to the West Coast with my parents. My stepmother got very ill in Madison, Wisconsin, and we stayed with some friends of her family there. He happened to be a very eminent geology professor at the University of Wisconsin, and I was just chatting with him one night. And he asked if I had ever heard of the field of geophysics? I had never even heard the word before. But I got reading up on it a little bit, and it sounded intriguing to me. So when I got ready to go to graduate work in my senior year, I was looking at schools of geophysics—not knowing exactly what it implied. Geophysics then—probably more than today—was oriented mostly toward petroleum exploration. That is, using dynamite explosions, using gravity and so forth, trying to understand Earth's structure to find valuable deposits. I'm not even sure I realized that seismology, for example, was a branch of geophysics. But to me, it looked like a way that I could get out of pure physics, which I thought I wasn't going to really succeed in anyway, and into something where I would have the chance to be outdoors a little more. Even in college, I did quite a bit of hiking and skiing.

My professors at Reed weren't very happy, because they wanted me to go to the University of Chicago. As a matter of fact, out of our graduating class of nine seniors in physics up there, I think four of them did go to Chicago. They thought I was a little strange, going off to a program in geophysics. I think in actuality I made the right choice, in the sense that I don't think I ever would have been a great physicist, particularly not a theoretical physicist. I don't mean to say I'm a great geologist, but I think I've been more successful in the field I chose than I would have been had I stayed in physics.

So I applied to a number of places. Interestingly enough, Reed College taught no geology, so I had never taken a geology course. There was a chemistry professor who was a mountain climber who was interested in geology, so I had done a reading course under him where I read some of the common geological texts of the time. I remember reading [J. H. F.] Umbgrove's *The Pulse of the Earth*, which had to do with the Milankovitch cycles and so forth. And I was intrigued by that. I even read an elementary geology textbook while I was there, which convinced me even more that geology sounded fascinating to me. So I applied to a number of schools that had outstanding geophysics programs—among them Colorado School of Mines, which was always famous in that area, particularly in what we call applied geophysics, that is, using geophysics to find ore deposits and find oil. Caltech also had a strong program. I didn't realize at that time that it was oriented more toward seismology. I also applied to UCLA, which had a strong geophysics program. I remember that I got an offer of a fellowship from UCLA which was rather attractive financially. I wasn't offered any money by Caltech. On the other hand, I received a letter from Ian Campbell, who was then executive officer of the Division of Geological Sciences—now Geological and Planetary Sciences—and the letter was so nicely written and so welcoming, even though they couldn't offer me financial aid, that I eventually decided to come to Caltech. Now, you should bear in mind that I had the GI Bill available to me, which made my financial situation quite a bit different than it would be today.

I guess I had some parental support in my freshman year, before I went into the army. But after I came out of the army, I never had any more support from my parents at all, so I had to make it on my own. But with the GI Bill and the California State Aid for Veterans as well, I was able to make it through. After one year at Caltech, I did well enough that they decided I should get financial aid.

I should point out that since I had never had any formal geological work at all, I decided that for the summer between the time I graduated from Reed and the time I started at Caltech I would try to find a summer course in geology. I went to the University of Colorado at Boulder and took elementary geology and some other kinds of things there, which brought me up to a reasonable level. When I started here at Caltech, however, I still didn't have nearly as much geological background as most of our students coming in. But nevertheless, I had no trouble, and my physics and math background was a lot better than that of most of the other students. I don't regret for a moment that I did my educational work in the way I did—that I majored in physics. The fact is, in geology, because of the sequence of courses, it's rather easy to go back and make up things. You don't necessarily have to have elementary geology in order to understand paleontology. Whereas in physics and math, you can't take advanced calculus without having taken elementary calculus. So, all in all, I'm very glad, and I think my undergraduate work stood me very well. I still don't have as much geology as some of our students did, but so what?

VALONE: I want to go back and talk a little bit more about your experience in the war. You said you were in the Army Air Corps. How did that happen?

ALLEN: I volunteered. In that day and age, almost all the young people who were physically fit enlisted. A lot of the universities had military programs. You know, Caltech had a large navy contingent. Up at Reed, they had an Army Air Corps pre-meteorology program, where students stayed for about a year studying physics and math and then were sent to meteorological school. That intrigued me, because it was in my own field, so I applied to go into the pre-meteorology program. I remember talking to my father about this. My father had served in World War I, in aircraft gunnery, I think. But he was recalled in World War II and went into army personnel and ended up in the South Pacific. He said, "Oh, hell, you don't want to go into meteorology. Why don't you go into the cadet program and fly?" It's rather interesting, because in those days many parents, particularly mothers, were trying to keep their kids out of the cadets—somehow the idea seemed very dangerous. Actually, both of my parents encouraged me to go into cadet training to be a flyer. It may have even saved my life, because it turned out that there were far too many meteorologists—they had half the college kids of the country studying pre-meteorology. When they suddenly realized they didn't need all these people, they all got transferred to the infantry and sent to Europe, and the casualty rate on that was much greater than it was in the air corps.

So I went into the air corps. I eventually had, after preliminary training, my choice of whether to be a pilot or a bombardier or a navigator. I chose to be navigator. So then I went through navigation training places all over the United States. Eventually I ended up on Okinawa. I arrived there shortly before the atomic bomb was dropped on Nagasaki and Hiroshima. I remember flying over those two cities within two or three days after the bombs were dropped. That's a sight I will never forget.

I found navigation absolutely fascinating. I remember flying around the Pacific, particularly after the war was over. For the first few months, we did a lot of flying around. We'd go down to the Philippines to get milk or something. And we would fly over volcanoes, because they were intriguing. Nowadays, particularly in a B-29, you can sit up in the front of that dome and get a 180-degree view of the world beneath you. Many times since, I'd give a million dollars if I could repeat that experience, now that I know some geology.

We also did a lot of flying over China. One day we went out because there was a rumor that there was a higher peak than Mount Everest up in central China. I remember spending eight hours flying all over central China trying to find this place. So I'm very grateful for my military experience. But I didn't think twice about getting out of the army. I'm not a great enthusiast of the military discipline system. I mean, I appreciate it's necessary, but it's not the way I like to live. At the same time, I don't think I've ever done any work in my life that I enjoyed more than aerial navigation. It was challenging. I did a lot of celestial navigation, which was very interesting. But I got out of the army at the first opportunity, when the war was over.

VALONE: Was it 1945 when you got out?

ALLEN: Actually it was 1946, and I went right back to college. Then I was recalled in the Korean War, because I had been in the reserves. It happened that the last thing I'd done on Okinawa was to plan and direct a program of aerial photography over the 38th parallel in Korea, trying to identify bridges that one could see on the radar. So the Korean War wasn't more than a week old when I got recalled. On the other hand, I was already in graduate school here and I was studying advanced geophysics. And for some reason, the air force thought it was more important that I stay in school. I think they thought geophysics meant physics of the upper air, or something. They didn't realize that I was studying how to find oil and studying earthquakes and so forth. [Caltech president] Lee DuBridge wrote a very nice letter for me. So actually I put in only five days in the Korean War and then came right back and continued my graduate work.

VALONE: What were your initial impressions of Caltech in 1949, when you first came here for graduate school?

ALLEN: Well, almost all of the students were veterans of World War II at that time. It was a very compatible group of people—probably not as able or as well trained as the graduate students today but nevertheless a very competitive group. I very much enjoyed field geology, partly because field geology is a matter of going out and making maps and so forth. Given my natural interest in maps, I just fell in love with doing geological work in the field. So even though I obtained my master's degree in geophysics [1951], when I started my PhD work I ended up in structural geology and geophysics, and my main emphasis for my PhD work was in geology. I did that under Dick [Richard H.] Jahns, who left Caltech shortly thereafter, went to Penn State, and eventually was dean up at Stanford. I did my thesis studying the San Andreas fault in the San Gorgonio Pass area, between Beaumont and Palm Springs. I very much enjoyed that. It got me looking into fault structures, which was one of the reasons I subsequently turned more toward earthquake work. But as a teaching assistant here, or research assistant, at one point I was assigned to [Hugo] Benioff, who was one of the three famous seismologists here at that time. I worked with him on developing a new seismometer, and that provided the opportunity of getting to know the other people at the Seismological Laboratory—[Beno] Gutenberg and [Charles] Richter. And since I was sort of between geology and geophysics, I took all the courses that Gutenberg offered and all the courses that Richter offered. So that by the time I graduated [1954], I was already half geologist/half geophysicist. Also Bob [Robert P.] Sharp, who was teaching geomorphology, was doing some work in the summers up in Alaska. After my first year, I think, he dragged me along with some other students, up to Alaska to do seismic sounding on the glacier there. He was working on the Malaspina Glacier, where he was trying to understand some glaciological aspects; it was critical to know how thick the ice was, which was a geophysical problem. We spent all summer up there, camped on the ice. That was my introduction to glaciology, which I did a fair amount of later; after I graduated, I still pursued those interests for quite a while. This was all thanks to Bob Sharp. Eventually I got cold feet and gradually stopped doing glaciology and concentrated more on seismology and geological aspects of it.

Certainly an important influence for me at Caltech was my thesis advisor, Dick Jahns. He also taught field geology and developed in me a very great love of that. My own impression is that I was very good at it. Bob Sharp certainly was also a very important influence. And he did a lot of this, grabbing students and taking them out on these various projects he was working on. Ian Campbell, although not such an eminent scientist, was the reason I was here, because of a very nice letter he wrote, which reflected his concern with students. He was a very good instructor and a person who was very good at making students feel at home and helping them.

At that time, a lot of the graduate students lived in the Athenaeum—on the open-air balcony on the third floor, for two dollars a month, I think. We used to eat dinner there quite often, back before it got fancy in the evening. I remember eating dinner quite often with [Robert A.] Millikan, and often with a group of graduate students. That was a very interesting and enjoyable experience. He was a very formal person, but his ideas were very interesting, and I think the graduate students enjoyed talking with him.

VALONE: And he was willing to discuss any number of issues?

ALLEN: Sure. As I say, he was somewhat formal. Those were different times, when he was around. There were other instructors and people here. I took some work in physics here, because in the geophysics curriculum you had to, and I got to know Bob [Robert L.] Walker and some of the people in the physics department at that time. All in all, the graduate student work I did here I found very exciting. Thank God for the GI Bill, so that I could afford it—and then later, of course, I had some support from Caltech, from teaching and assistantships. Teaching as a graduate student, I helped Bob Sharp in the introductory geology course, and that paid off. I got to learn quite a bit of introductory geology. [Laughter] There's no way to learn a subject like trying to teach it.

VALONE: You said you came and started in geophysics. How did your migration from geophysics to geology take place?

ALLEN: It was a matter of what I was interested in. Particularly, I discovered I had this love for field geology, the mapmaking aspect of it, and trying to figure out what the three-dimensional configuration of geologic structures was. At that time, and even today, I hope, there's no great boundary between the two fields. No one was particularly worried that I had to be one or the other. When it came time to do my thesis, I decided I'd like to do a geologic field problem. And that's what I did out in San Gorgonio Pass. I wasn't giving up on geophysics, but I think that by the time I left here I was probably more of a geologist than a geophysicist.

VALONE: What was your basic graduate training here like? What were the main courses you had to take? Which fields did they emphasize?

ALLEN: Well, field geology was very important then. It's still important, although I think it's not as important as it used to be. The geophysics work depended very heavily upon some of the work in the physics department, sort of the elementary graduate physics courses. I took a wide variety of courses. I even took a course in vertebrate paleontology at one point. I took some more work in Northwest history. Rodman Paul, who was an instructor here, offered a course in Northwest history. I took that and enjoyed it because that's what I had been studying up in Reed in my history minor, and that's where my first publication was, in that area. I enjoyed my humanities work here. At that time, I think, as graduate students we were required to take one humanities course every term. I don't think that's generally required of graduate students today.

The work I found the most intriguing was field-oriented geology. I enjoyed getting out in the field and trying to figure out what was going on.

VALONE: What was the state of seismology at that time? What were the big leading issues that people were working on?

ALLEN: I was not so much in seismology then, as a graduate student. It's after I came back on the faculty [1955] that I got together more with Benioff and Richter and Gutenberg. I would say that Gutenberg was certainly the greatest of the seismologists who were here, and at that time certainly one of the two leading seismologists in the world; he and Harold Jeffreys at Cambridge were certainly the two leading figures. His concern was primarily with the interior of the Earth, trying to use seismology as a tool for understanding the interior of the Earth. But Gutenberg had very broad interests. He was also interested in Southern California seismicity. Although he wasn't particularly concerned with seismic hazard, nevertheless he realized it was important to do the kinds of studies that would allow seismic hazard analysis to be done. Charlie Richter, even though not a geologist in any sense, was more interested in the relation between faults and earthquakes. His book has a lot of emphasis on that.[1] He traveled around a lot, in New Zealand and whatnot, and he was intrigued by the tie between seismology and geology and the major faults. So was Benioff, in a somewhat different way. Benioff was trying to understand the mechanics of that process, particularly in what we now know as subduction zones, which was something he was getting a feel on. Of course, the concept of plate tectonics had not come along

[1] *Elementary Seismology* (New York: W. H. Freeman, 1958).

yet, but Benioff was doing things that formed some of the foundations for plate tectonics. There's a region still known as the Benioff Zone, where the oceanic crust is being pulled or pushed beneath the continental crust, because he first pointed out, on the basis of seismological evidence, that it looked as though there were these earthquakes on a slanting plane, going down under the edge of the continent, that could represent the oceanic plate being pulled underneath the continent. It's also interesting that Gutenberg was one of the few defenders in the Northern Hemisphere of continental drift. Continental drift was sort of the predecessor of plate tectonics. They're not exactly the same, but certainly the concept of plate tectonics had its start in continental drift. Gutenberg had always been a great proponent of continental drift, even though almost no one else in the Northern Hemisphere was. There may be three reasons for this: One is that the other great geophysicist of his era was Harold Jeffreys, and Harold Jeffreys said that continental drift was physically impossible. So of the two leading figures of the world, it's not surprising they should oppose each other on a number of issues. Secondly, the father of continental drift had been a German scientist, [Alfred] Wegener, who was a meteorologist, who had first taken to comparing, particularly between South America and Africa, some of the fossils that were very similar, indicating similar climates. Wegener, even though he was a meteorologist, published, I think, the first really scientific book on continental drift. It so happens that Gutenberg was also German. Gutenberg had also been trained as a meteorologist. So to some degree, I think he had sort of sympathy for Wegener's point of view. Maybe that's just idle thinking on my part, but nevertheless Gutenberg completely defended the concept of continental drift. And although some of those aspects turned out to be wrong, the fundamental concept was right—namely, that South America, Africa, and other continents really had separated from each other.

Gutenberg was a very interesting individual—a very distinguished, very formal kind of person. I don't mean formal like Millikan. Millikan was formal, but Millikan was almost—"haughty" is not quite the word. He sort of put himself on a pedestal and tended to give orders. Gutenberg was very formal but a very cordial person, very gentlemanly toward students, and all the students really liked him, despite the fact that he was Germanic and formal in the way he did things. It was a very interesting experience, studying under him. And of course he and Charles Richter were just an utter contrast, because Richter was a very different kind of a person. Presumably we'll talk more about him later.

VALONE: How were they toward students? Did they compete at all for students? Were they very cooperative in terms of helping their students?

ALLEN: Generally speaking, yes. Although it's strange that Gutenberg, despite the fact that he was by all odds the greatest pure seismologist in the world then, if not the world's greatest geophysicist, never produced many really outstanding students. He was a great researcher himself. Whereas Perry Byerly, up at Berkeley, who was the chairman of the seismology department up there and a very eminent seismologist, produced many, many students, who are still active in the field. For some reason, Gutenberg just concentrated on doing his research more. He didn't make a great effort to go out and get students to work with him. Nor did Benioff. Benioff sort of enjoyed doing his own thing. And Charlie was a little bit that way. So strangely enough, in that day and age, even though the Seismology Laboratory was perhaps the greatest seismological institution in the world, it was not the greatest producer of students. It was only when Frank Press came here, somewhat later, that we really began to put out very good quality and enthusiastic students. And I think since that time we perhaps have been the eminent institution in the world in putting out seismology students. And that was after I'd been away and come back.

VALONE: You mentioned earlier that you did some of your work under Benioff.

ALLEN: When I say "worked"—I took a course he offered. Also, we had at that time graduate assistantships, where you were assigned to a professor to be his slave, so to speak, in order to make money to eat with. And in this day and age, almost all of that money is used to support doctoral work. At that time, students were assigned to different professors, partly to give them an education, not because you were necessarily going to work under that professor. But for about a year, I worked under Benioff, who gave me an assignment—well, one of the problems: How do you calibrate seismographs? It's not easy to do, because it's not easy to build a table, for example, that can shake in some known way that you can then use to calibrate a seismometer that's sitting on that table. A shaking table is a very sophisticated kind of thing to build, and at that time there were very few of them.

Benioff had the idea that maybe if we could put a pair of capacitor plates, one tied to the mass of the seismometer, the other tied to the frame, and then put a varying current across the capacitor plates, we could drive the seismometer at a given frequency. So that's the sort of thing he turned me loose on, and I did a lot of work on that. It was not a thesis project. I guess what happened is that good shaking tables became more available, and this technique didn't seem to be so necessary at a later time. But that was the work I did with Benioff. **[Tape ends]**

Begin Tape 1, Side 2

ALLEN: Even today at Caltech, and in many schools, if you come here as an introductory graduate student, you are assigned to a certain professor by some sort of a lot system. And if you decide you don't want to work under that professor for your actual research work, or your thesis, then it can become very awkward. Sometimes students even have to change schools, because it's hard to go out of one research group into someone else's.

That was never the case with our division. I think it's still true today. We encourage students to do research projects under a number of different professors. It's generally accepted that the student can talk to a lot of professors about what they might want to do for thesis work, get ideas, and finally go to a professor and say, "I would like to work under you. Can you support me?" And if the professor can support them, which hopefully is the case, then it's the student who makes the choice.

That was certainly the case when I was here. I went around and talked to a number of professors. The one who was most closely aligned to my field, in structural geology, was John Peter Buwalda, who was a very eminent, old-time professor. He had been chairman of the division for a long time. But I decided that what he was doing wasn't as exciting as what I might be doing under Dick Jahns, whose emphasis was a little bit different, not so much structural geology. Eventually it was Dick Jahns, I think, who suggested that I look at this area out in San Gorgonio Pass. I also talked with Bob Sharp. I was into structural geology then, more than either glaciology or geomorphology, which were his two areas. I suspect that had I gone to Bob and said, "Look, I want to do a thesis on the glaciology of such-and-such glacier," or on the geomorphology of sand dunes, or something, Bob would have been very happy to take me as a graduate student. It so happened that at that time I was still looking at structural geology. In structural geology, you're looking at faults and folds, the kinds of things that are rather close to geophysics and seismology. But let me make it clear, my thesis was not a geophysical thesis. It was entirely a field-mapping project, albeit on the San Andreas fault.

VALONE: Was the San Andreas fault already recognized as a major seismic hazard at that time?

ALLEN: Oh, yes, ever since the time of the 1906 [San Francisco] earthquake. And even prior to the 1906 earthquake, some parts of the fault had been recognized. But I think the 1906 earthquake led people to realize for the first time that it was capable of generating big earthquakes. As a matter of fact, the 1906 earthquake report, a very famous report done by the Carnegie Institution in Washington, had traced the fault clear down south, through the mountains.[2] Most of that report had to do with the area around San Francisco in 1906, but one of the chapters of that report has to do with the San Andreas fault in Southern California. So, yes, it had long been recognized as a major source of earthquakes. And by that time we knew a good deal about the 1857 earthquake, which is the last big earthquake in the southern part of the fault. There were a lot of structural complications out in San Gorgonio Pass. The fault didn't seem to go through the pass area as a single line, and that's why it was particularly intriguing to try to understand what was going on out there. It's still a problem. I think I made a contribution, but I certainly didn't solve the problem. Kerry Sieh's still working in some of the same areas I worked on, adding more information, new information. We certainly have a better feel for it now than we did when I was there, although I am in no sense ashamed of my thesis.[3] I think I made a major contribution.

VALONE: Was your thesis one of the first attempts to map that particular section of the fault, or any long section of the fault?

ALLEN: Yes. Earlier, a University of California student by the name of [F. E.] Vaughan, back in the early twenties, had mapped the entire San Bernardino Mountains area, including the San Andreas fault along the south side of it. He wasn't concentrating only on the fault but also on many other things. So some work had been done. As a matter of fact, he was living in Pasadena at the time I embarked on my thesis work, and he was very helpful to me. But I think mine was

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[2] Lawson, A.C., chairman, *The California Earthquake of April 18, 1906*: Report of the State Earthquake Investigation Commission: Carnegie Institution of Washington Publication 87, 2 vols. (1908).
[3] Allen, Clarence Roderic (1954) *The San Andreas Fault Zone in San Gorgonio Pass, California.*

the first effort to concentrate on the fault itself in that region.

However, a Caltech student by the name of Bob [Robert E.] Wallace, in his 1944 PhD dissertation, had mapped a section of the fault northwest of Palmdale.[4] I think that was the first time anyone had looked at a section of the San Andreas fault itself, trying to understand the fault and not just the regional geology. Bob Wallace's thesis is a landmark in itself, sort of the first intense geologic study of any given segment of the San Andreas fault. Mine may have been the second, and in a different area.

VALONE: How did you end up getting your first job, at the University of Minnesota?

ALLEN: Well, I wanted to go into academia. After all, my father was a college professor. The research area fascinated me, as well as the teaching aspect of it. Still, I did have some thoughts about going into the petroleum industry. At that time, seventy-five percent of all geologists were hired by petroleum companies. The first summer after I graduated, as a matter of fact, I worked for Union Oil for four or five months, which I found to be a lot of fun. They gave me a Jeep and a carryall and a house trailer, and I was with a paleontologist and a cook. They said, "Find out all you can about western Utah." Shell Oil had just struck oil up near Ely, Nevada. It was a surprise to the oil industry, because nobody knew what the geology was in that area. So they asked me to go out in the boondocks of western Utah and just do regional mapping, trying to understand what the mountains were made of. That experience was a lot of fun. But nevertheless, I had already made up my mind I wanted to go to academia, and I already had the job at Minnesota by that time.

Getting jobs then was not nearly as difficult as it is today. Universities around the country were expanding. Geology departments were expanding. It was a different day and age, believe me! So I had talked to various professors at Caltech about where there might be openings available. I think Bob Sharp might have mentioned Minnesota. He had taught there himself, at one time earlier in his career. There were also a couple of Caltech graduates on the faculty there, in the geology department.

So I applied to a number of places. I wanted to stay west of the Mississippi. For some reason, I considered myself a westerner—as a geologist, even more so. But it's interesting that I

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[4] Wallace, R. E. (1944), *A portion of the San Andreas rift in Southern California.*

didn't know much about Minnesota. I'd spent a winter in the army in Superior, Wisconsin, just across the bay from Duluth, so I knew something about Minnesota winters. But I had never spent any time in Minneapolis.

Of course, the Mississippi River goes almost through Minneapolis. So I thought, in keeping with my desire to stay west of the Mississippi, I should at least look at the map. Sure enough, Minneapolis was west of the Mississippi. So I agreed to give a talk up there when I and other people were being interviewed for this job. Much to my surprise, I found out that all of Minneapolis, except for one little bit, is west of the Mississippi. But that one little bit was where the university was. So eventually my first teaching job was east of the Mississippi. [Laughter]

Anyway, I accepted their offer to go up there as an assistant professor, and I enjoyed it a great deal. I taught their summer field camp, out in the Black Hills. The people up there were very nice to me. Unfortunately, the department had some problems. There was a lot of infighting, more so than I had thought. I suspect, as a student, I didn't appreciate the infighting that perhaps also went on in the division here at Caltech.

Nevertheless, during my year there [at Minnesota, 1954-55], John Peter Buwalda, who had been the structural geologist in the division here, died. So the division here was looking for a replacement in the area of structural geology. When I got the call to come back, I didn't really think twice about it, partly because I thought the research opportunities were much greater in this area and partly because Caltech, frankly, was just a much better school than Minnesota at that time—and I think still is—in the geological area. So, even though I enjoyed my work there and I think they were honestly sorry to see me go, I'm glad I came back to Caltech. I think the decision was a wise one on my part. I was certainly made welcome back here, even though I had left here as a student and came back as a faculty member.

VALONE: So Caltech called you to ask you to come back?

ALLEN: Right. Bob Sharp was then chairman. He gave me a phone call and wrote me a letter. And I remember that letter; he said some interesting things. He said, "I should point out that all members of our faculty are required to take thirty days of vacation every year, which none of our professors have ever been known to do." [Laughter] So I came back here [in 1955] on a twelve-month salary of $4,500.

VALONE: It seems like the salaries hadn't gone up from Millikan's day. Millikan was well known for keeping the salaries low.

ALLEN: Well, I'm sure they had gone up some. This was more-or-less competitive, I suspect, with what was being offered elsewhere. $4,500 went a lot farther then than it does now.

When I came back here, I was still working with Bob Sharp on some glacial projects and did for many years, through the late fifties and into the sixties, I guess. We worked together on the Blue Glacier in Washington State. But I turned my primary attention to faults, since I knew the seismology group here was perhaps the most eminent in the world. I was a structural geologist, but I felt, well, here's an area where I can find a niche, working in geology and seismology, understanding the two together. As a result, most of my career has been in this area of trying to understand the mechanics of fault systems. In more recent years, I've done more with trying to understand them in terms of seismic hazards. So I worked together with Charlie Richter and others on a number of similar problems. That was part of the reason I came back—to take advantage of this area and the expertise here in seismology. And I'm glad I did.

[Tape ends]

CLARENCE R. ALLEN

SESSION 2

April 4, 1994

Begin Tape 2, Side 1

VALONE: I believe we left off last time just after you'd returned to Caltech as an assistant professor. I wanted to ask you more questions about your early years and your work in the division when you first returned to Caltech.

I believe one of the first things you did, after you came back to Caltech, was the Blue Glacier project.

ALLEN: Well, Bob Sharp had got me involved as a graduate student in the work on the Malaspina Glacier up in Alaska. Then later, one of my colleagues, Mark Meier, was doing a PhD thesis on the Saskatchewan Glacier in Alberta, Canada, and I helped him with geophysical work. Yes, I was, to some degree, into glaciology at that time, even though my primary research effort was still in structural geology. But you see, there are some aspects of studying ice that are not completely irrelevant to studying faults and folds in the Earth's crust. You mentioned the Blue Glacier, which was a project that Bob Sharp put together. It was the result of, I think, the IGY [International Geophysical Year]—when the IGY first started, they were putting lots of money into Antarctica. Bob Sharp apparently saw millions of dollars being poured into Antarctica, and he said, "Well, give me just a couple of thousand dollars, and I can do some very important things on a small glacier right here in the United States." So he got a very minimal grant to sponsor work on the Blue Glacier, working out of Port Angeles, Washington. We chartered a small ski-wheel plane out of Port Angeles for occasional delivery of supplies to our glacier camp, although we always walked in and out ourselves on the Hoh River Trail—usually in the mud and rain. For the most part it was a bare-bones operation.

One of the things I did up there: Initially I was doing seismic sounding to see what the thickness of the glacier was, but then I got interested in the deformation of the ice, because this is a glacier that's made up of two ice streams coming over two ice falls and then joining at the base of the ice falls. And it gives rise to a very peculiar, double-spoon-shaped structure within the ice of the lower glacier. So I spent quite a bit of time doing mapping, just like I would be mapping rocks, only mapping foliation in the ice and crevasses and things like that. We eventually published a paper on the structure of the Blue Glacier, which I think was important, and it was right within my area of structural geology.[5] Although Bob Sharp was the leader of this project, he always had a gang of people up there—students working with him. Barclay Kamb was among those, and Ron [Ronald L.] Shreve from UCLA. I was up there for, I guess, parts of seven different summers. It was a very rewarding experience to me, but I gradually got to the point where it was clear that my future didn't lie in glaciology. I was really getting more and more interested in earthquakes and the relationship between geology and earthquakes. So I eventually dropped out, if you might put it that way, of glaciology and turned my entire attention to these other fields.

VALONE: You mentioned the International Geophysical Year. What was your involvement with that?

ALLEN: Well, my only involvement was through this Blue Glacier project. The International Geophysical Year went on for a number of years, but I think '57-'58 was the first official year. It involved a lot of international cooperation on projects, particularly those involving climate, environment, and so forth. A lot of effort was in Antarctica, because that was an international arena. This project [Blue Glacier] was just a very small part of it, and as far as I know, that was my only involvement in the IGY.

VALONE: After you returned to Caltech, you started to expand the work that you had done previously on faulting, and on the San Andreas fault primarily?

ALLEN: Well, I maintained my interest in the San Andreas, but I didn't really expand my thesis area. As a matter of fact, after I got back here, it took me about a year, including the year I had spent in Minnesota, to get all that stuff in preparation for publication. In geology, particularly in those days, you tended to publish rather large, long articles that required a lot of preparation. I had to draft all the maps myself, including the color separation plates for printing. So it was two years after I finished my thesis before it was published. But that was sort of a major publication

[5] Allen C. R., et al., "Structure of the Lower Blue Glacier, Washington," *Jour. Geol.* 68:6, 601-25 (1960).

at that time.

Of course, I maintained an interest in other parts and aspects of the San Andreas fault, but I also got interested in similar features elsewhere in the world. I remember, for example, that Hugo Benioff suggested a rather interesting idea. He was just full of ideas like this. I'd say at least half of Benioff's ideas probably turned out to be wrong, but the other half included some real imaginative contributions. Overall, his contribution to science was very great, even though a lot of things that came out were sort of half-baked and wrong.

One of Hugo's ideas was that the whole Pacific Basin was spinning with respect to the continents around it. He had observed strike-slip right-handed faulting in the United States—that is, the faults were clockwise in their displacements. We knew the same thing occurred in New Zealand and a couple of other places around the Pacific. All of these faults, all the way around the edge, were basically right-handed. He also had been working on some big earthquakes, and some seismological evidence from Kamchatka suggested that they were right-handed as well.

Well, from what we know now, that's not very viable. For one thing, it's not exactly a circular boundary, it has a lot of corners in it. And we also recognize now that the primary movements at the edge of the Pacific are not horizontal but vertical—although in some places, such as here in California, they're primarily horizontal.

At any rate, I remember reading somewhere of an alleged strike-slip fault in the Philippines that was semi-parallel to what we now recognize as the plate boundary there. And Hugo thought, "Well, gee, if that's right-handed, that will help prove my theory." So Hugo urged me—and I think he may have helped support me—to go to the Philippines in 1960 to look at this Philippine fault zone. It had been described very generally in the literature, without any really detailed mapping at all. I spent several months, working and doing field work in the Philippines, particularly in Leyte.

It turned out to be very rewarding. The Philippine fault indeed existed; it was a very major feature, very comparable to the San Andreas in its degree of activity and things like that. But I firmly concluded that the fault was left-handed and not right-handed—in other words, the opposite sense of rotation from the San Andreas. I came back and reported this to Benioff. He wasn't happy, but at the same time, he didn't argue with me. He was willing to accept it. But my finding was one of the things, I think, among others, that rather quickly led to the demise of that particular idea—of the Pacific Ocean rotating.

On the other hand, the idea that the Pacific Ocean was somehow separated by continual faults from the continents around it is consistent with what we now know as plate tectonics. The Pacific Rim is, indeed, one of the great plate boundaries of the world. This was 1960, of course, before the advent of plate tectonics, which really didn't come in until the mid- and late sixties.

So I got interested in these international faults, similar to the San Andreas. A friend of mine—one of my fellow graduates here, Pierre Saint-Amand—was working down in Chile for the State Department; he was teaching at the University of Chile on a State Department grant. He found what he thought was a similar feature in Chile. So with Frank Press's help—Frank was then at the Seismological Laboratory—I went down to Chile and spent several months. I almost stayed there, because I got infectious hepatitis and just about left the scene. [Laughter] It turned out to be another very intriguing fault system down there.

It was these episodes that got me interested in major active faults—particularly of the strike-slip variety but not limited to those. So that's guided a lot of my subsequent work in looking at alleged features of similar types in Venezuela, southern Argentina, Sumatra, and Taiwan. More recently in China, where I've been fourteen times. At least half those times I was doing research projects involving major faults—sort of like the San Andreas but in a different part of the world.

In any event, back in the late fifties and early sixties, I was changing the direction of my research and doing more seismology. I forget when it was that I became acting director of the Seismo Lab; I guess that was when Frank Press left, which was in 1965. Anyway, I was working a good deal with Charlie Richter. He and I had done some seismological work on some specific earthquakes here in California. We were doing work on trying to correlate seismicity with geologic structure and understanding seismic hazard. So even though my office was down here on the campus, I was spending an increasing amount of my time up at the laboratory, working with people up there. Then when Frank Press left, I was asked to become acting director of the Seismo Lab and I moved out there. My office is still at the Seismo Lab, although, of course, we've subsequently moved the whole laboratory back to the campus. It was Frank Press's departure that, I guess, put me into the Seismo Lab, per se. Then we persuaded Don Anderson to become permanent director of the lab a couple of years later, in 1967. But I stayed there as a member of the laboratory staff, even though I was doing both geology and geophysics at that time.

VALONE: I want to talk a little bit about the relationship you saw between progress in theory regarding seismic activity and observations. It sounds like a lot of your own early work was more observational.

ALLEN: Oh, yes, I'm not a theorist and never laid claim to that.

VALONE: But did you have a sense that theory was moving ahead of the observational data, and that there was a great need to go out and actually test these theories? Or was there much more emphasis on working on the theoretical aspects?

ALLEN: Well, I think both were going on. At that particular time, there was a great deal of theoretical work going on by people at the laboratory—Frank Press and his students. It was very quantitatively based. I wouldn't say it was completely theoretical, because in many cases it depended upon observations not from the field but from seismographs. I would say those records are the greatest contributions from that period, and maybe even still today, from the Seismological Laboratory—at least in the area of seismology, since there are things that the lab does that are not seismology. Those were observations in the sense that they were based on seismographs, but they also involved a lot of theory on how to interpret them.

For example, one of the burning issues of that time that came up from a practical point of view was, How do you distinguish between natural earthquakes and underground nuclear explosions? This was a matter that had a lot of political import. In those years, we got a fair amount of money—from the air force in particular—to try to help understand this. It was all unclassified work. We were trying to find differences in the kinds of signals put out by an underground nuclear explosion and a natural earthquake in the same area. That involved a lot of theoretical work.

For example, Charles Archambeau, who was one of our students at the lab, his work was almost entirely theoretical—trying to understand the mechanics of what happens during an explosion versus what happens during the shearing phenomenon we know as an earthquake.

I think when the Russians and the Americans first started worrying about this problem, the assumption was that earthquakes should look very different. Because, after all, an underground explosion is one that just simply expands, and it's expanding in all directions. Whereas an earthquake, by definition, is caused by a movement on a fault, which gives rise to a very different kind of radiation pattern—at least, you would think—in the way the signals are picked up around the world.

Well, much to the surprise of the Americans and the Russians, the big underground nuclear events in Nevada and in the Soviet Union looked surprisingly like earthquakes, even those we knew full well were underground nuclear events. Apparently part of the reason was that since the areas in which they were being set off was under tectonic strain—that is, in seismically active areas—when you set off an explosion in rock that's already under strain, part of that signal gets converted into something that looks very much like an earthquake. So it turned out that the problem was a lot more difficult than either the Russians or Americans had thought. Even today, everyone recognizes there's a certain limit—say, below magnitude 5 or so—beyond which we cannot adequately discriminate if we don't have instruments right in the area where the nuclear events are being set off. Of course, in that day and age we didn't have instruments inside Russia and they didn't have instruments here.

VALONE: So underground nuclear testing had a big impact on furthering the theoretical study of earthquakes?

ALLEN: Oh, absolutely, no question about it. The air force support we had was exceedingly valuable. Now, the air force's interest, I guess, was not in basic research. But they came to realize that we were never going to solve this problem if we didn't have a better understanding of the mechanics of how these processes take place.

Now, having said that, I was not directly involved in that program. Because I was not primarily one who was reading seismograms or doing theoretical work. But you were asking about the broader interests of the laboratory. I would say that it's always been observationally based, particularly on seismograms, although there have been some very important theoretical considerations at the same time.

My own work was observationally based, in terms of the geological aspects of earthquakes. That is, what can we say about an earthquake on the basis of observing the faulting that took place during an earthquake? In that sense, I guess there was no one at the laboratory who had quite those same kinds of interests, because I was at least half geologist. I got good support; I don't mean to say I wasn't welcome. But nevertheless, my interests and research were quite different from most of the things going on at the laboratory.

Charlie Richter himself had quite an interest in this area. His book, *Elementary Seismology*, has a lot of geology in it. It's not a textbook of theoretical seismology at all.

VALONE: Do you think the discipline has changed quite a bit over the thirty or forty years of your career? Did you have a sense in the late fifties and early sixties that the disciplines were coming together—geophysics and seismology?

ALLEN: I'm not sure that the discipline has changed in overall direction. Certainly there have been exciting changes. Take the whole new field of paleoseismology: Up until, I guess, about 1968, no one even thought that you could observe the histories of prehistoric earthquakes on excavated trench walls and thereby tell something about the sequence of past earthquakes in a given locality. That's been a very exciting development in the whole area of trying to understand seismic hazard.

In seismology, time and again, new instruments have come in that have completely revolutionized our ability to look at earthquakes. Hugo Benioff was basically an instrumentalist. He developed many of these instruments; he pioneered that work at Caltech—at least, many of his greatest contributions were in new instrumental developments. More recently, we've seen the broadband, high-dynamic-range seismometers that now make up our TERRAscope Network, here at Caltech. It's a very recent instrumental development that's just revolutionized the way we're doing a lot of seismology now. Again, that's not my field as much as it is Hiroo Kanamori's and Don Anderson's and people working in that area.

So the disciplines have certainly changed. But fundamentally, yes, we're still trying to understand earthquakes; we're still using earthquakes to try to understand the interior of the Earth. That was true when Gutenberg came here in 1930; it's still true today. Most of Don Anderson's work, for example, is to try to use earthquakes in new and different ways to understand more about the interior of the Earth—its chemical makeup and this sort of thing—and understand plate tectonic processes.

VALONE: I'd like to talk a bit more about your work with some of the founding fathers of earthquake research and to get your impressions of those people. Tell me a bit more about your impressions of Charles Richter and his influence on seismology at Caltech.

ALLEN: Well, as I mentioned earlier, Charles Richter was a very different sort of person from Beno Gutenberg. Particularly in his final days, I probably knew Charlie better than anyone else at Caltech, because we worked a fair amount together, particularly following Gutenberg's death. While intellectually brilliant, Charlie did not get along comfortably with other people and was always somewhat of a troubled person. He was particularly sensitive to what he often visualized as people making fun of him or his science. He could be absolutely charming when he wanted to be, but he didn't always want to be, and all of us at the lab fell under his wrath at various times. Some of his tantrums are legends in their own right, particularly among former lab graduate students. But I liked and deeply respected Charlie, despite his idiosyncrasies. He's not at all easy to describe honestly, and I worked very hard and almost painfully on my note following his death, which was published in the *Bulletin of the Seismological Society of America* [vol. 77, pp. 2234-7], "Charles F. Richter: A personal tribute."

As you well know, I think Charlie's greatest contribution to science is his textbook, which is on the shelf of every seismologist around the world. It particularly concerns the geological aspects of earthquakes. Richter was, quite frankly, not the eminent seismologist that the public would seem to think from the fact that his name is attached—and legitimately so—to the magnitude scale. Certainly, both Benioff and Gutenberg made far more profound, fundamental contributions to seismology and plate tectonics than did Richter. I don't mean to belittle Richter, but there is no question that because of his name being associated with the magnitude scale the public thinks he's the greatest seismologist who ever lived. Well, that's simply not true, or in keeping with the facts.

VALONE: Do you think his being at Caltech was instrumental in making Caltech into a world leader in seismology?

ALLEN: I think so. But he worked very closely with Gutenberg. Had he not been allied with Gutenberg, I'm not sure he would have been productive at all. Gutenberg had many of the ideas. Gutenberg would sort of keep things under control in getting Charlie working on good projects. It was a little bit of a symbiotic relationship. The volume that Gutenberg and Richter put out on the seismicity of the Earth is one of the most important seismological references ever published.[6] It involved a whale of a lot of work, detailed work, to get all those catalogs of earthquakes put together. That's the sort of thing Charlie could do very well. I'm not sure Gutenberg would ever have been willing to sit down and spend the days and days and days just getting things in order. But synthesizing the data and understanding what all these earthquakes meant in terms of global processes is where Gutenberg really shone. And the book is an honest-to-God contribution by both of them. To some degree, it wouldn't have been as good had either one not been involved. Gutenberg would have been a great scientist in any event. Charlie Richter, I think he depended more on Gutenberg's existence for his fame. Had Gutenberg not been here, it might have been a somewhat different story.

VALONE: Tell me more about Gutenberg and his theoretical contributions.

ALLEN: Well, I'm not the best person to do so. I'm not the kind of seismologist that Gutenberg was. He was primarily an observationalist, looking at records and drawing conclusions. He also had a very brilliant mind and was great at synthesis, with a strong theoretical background. And to him we primarily owe our basic understanding of the nature of the interior of the Earth. Now, other people were working on this, and he wasn't the first one to recognize some of the discontinuities in the Earth. But he certainly made profound contributions to that field.

It's interesting how out at the old lab, at that time, we just had one seismograph that was visibly writing a record in "real time." Nowadays, of course, we have 250 of them constantly sending us signals. But at that time, there was one local instrument that was hooked up to a seismograph that we could stand there and watch. Now, there were other instruments in Southern California recording, but they were all recording photographically, and we didn't get the records in until several days later. But when a distant large earthquake came in, it gave these broad, very obvious signals on the seismograph. I remember we all used to stand around the recording drum, students and faculty, arguing about where these signals were from. And Gutenberg would say, "Oh, it looks like something from Java." And Charlie would say, "Oh, no. It's from Saudi Arabia," or some such thing. I remember that both of these were men with a great deal of pride. Quite often, they would get into sort of a heated argument as to where the

[6] Gutenberg, B., and Richter, C. F., *Seismicity of the Earth and Associated Phenomena* (Princeton NJ: Princeton University Press, 1954).

earthquake was. They'd finally part ways and go off to their own offices, and then almost invariably, an hour later, they would come back and apologize to each other formally—Gutenberg, in particular, with a very formal, Germanic background. And if they ever disagreed on something, they would always apologize to each other later. It was a very interesting thing to watch, from the students' point of view. I would say Gutenberg was probably more often right than wrong.

Benioff sort of stood off at the side. He was a little bit more of a loner on this sort of thing. He sort of did his own thing, more so than Gutenberg or Richter. Most of Benioff's contributions are single authorship—not that he wasn't a very sociable person. He was. He just loved jokes, particularly those with a somewhat off-color tinge to them. But in his academic work, he sort of did his own thing with his own people, his own students. We were all in that one building, crowded in a little building—all crowded together. So everybody was thrown in on top of one another. When I was a student, I was working in the same office with Charlie Richter, because we were all in the same room there.

VALONE: I want to talk a little about the transition from the generation of Richter and Gutenberg and Benioff to the younger generation. How did that transition work out? Did it go smoothly? Was there much tension?

ALLEN: It came about primarily because of Frank Press's directorship [1957-1965]. Frank Press was a different kind of seismologist from Gutenberg or Richter, trained by more modern techniques, with, I think, a broader interest in tectonics. This was the era of the beginning of the concept of plate tectonics. Frank's influence was very profound. He took over as lab director when Gutenberg retired. That was a major change, and a very positive change in the laboratory. For one thing, he was more active in the teaching program. He was much better at working with graduate students than Gutenberg had been. He put these people on tough problems and worked with them very closely. And he was very successful as a producer of PhD students, which, quite frankly, was not really Gutenberg's great forte. Frank also brought new ideas into the laboratory, particularly emphasizing tectonic kinds of things. Some of his initial students, for example, were not doing seismology of the type that Gutenberg had known at all. One or two of his students spent most of their time in the Imperial Valley, doing seismic reflection work and gravity interpretation, trying to understand the tectonics of that area. That would have been almost unheard of under Gutenberg. So, in a sense, there was a broader program, perhaps with more emphasis on geological aspects. Although Frank was—and is—through and through a seismologist. So I think that transition was not painful, and it worked out very well.

VALONE: What were the circumstances that brought Frank Press to Caltech?

ALLEN: I don't really recall. At any rate, it was clear that Gutenberg was nearing retirement and we were anxious to get some younger people on the staff. For a while, Leon Knopoff from UCLA was also on the staff here. He was formerly a Caltech student, a Caltech PhD, but after several years he decided to return to UCLA. But then we did add other people to the staff here: Don Anderson, Stewart Smith. Charles Archambeau was on the faculty for a while. I, of course, was on the faculty.

VALONE: Was the department itself expanding at that time?

ALLEN: Yes, the entire division was expanding. Those were the days when Caltech, in general, was expanding. Whether the lab expanded more than the division, or more than the average for Caltech, I don't know. But, yes, we were expanding.

As you recognized, the laboratory is part of the Division of Geological and Planetary Sciences. Partly because of its physical isolation—at that time, being out beyond the Rose Bowl, on the other side of the Arroyo—the Seismological Laboratory operated fairly independently of the division. The only time Gutenberg came here to the campus, for instance, was to give lectures. And Charlie Richter spent all of his time out there. Hugo Benioff allegedly claimed he had some sort of an agreement with the administration where he didn't have to teach—although that could have been debated. Bob Sharp was then chairman of the division [1952-1968] and probably the most successful and forward-looking division chairman we've ever had in this division. There was a great deal of respect for Bob Sharp. And I don't mean to say that Bob got on bad standing with either Gutenberg or with Frank Press.

Bob, I think, felt that he would like to see better integration between the Seismo Lab and the division. The Seismo Lab had its own budget—although it's a budget that they got through the division. There was some unhappiness, I think, because people in the division outside of the

Seismo Lab felt that the people in the Seismo Lab were getting better research support than they were. Even to this very day, that's always a bone of some contention.

Frank Press, when he came here, my hunch is—I don't know this—but I think he wanted to have greater autonomy for the lab. I think he would have liked to have had the Seismo Lab made independent of the division. Not that he was particularly unhappy with people in the division, but I think he felt that the Seismo Lab deserved a more direct line of communication to the administration, and therefore they should have their own budget completely, without even going through the division. That's my hunch, and it's also my hunch that one of the reasons Frank eventually left [in 1965] was because he didn't have as powerful a position as he subsequently had at MIT, where he was chairman of the entire department.

Of course, there's long been a feeling at Caltech that we don't want to have a bunch of separate institutes that are outside of the divisional structure. We've seen it at the University of California, at UCLA, how a lot of these institutes in some ways are not very healthy, because they tend to reduce the stature of the teaching divisions. To some degree, they develop first- and second-class professors. The first-class professors are members of the institutes. The second-class professors are the ones with higher teaching loads. Caltech has always tried to avoid that. Even in the case of the Beckman Institute, there's some tugging and pulling there as to whether it's wise to set up an organization that is not within the regular framework of the Biology Division. But the Seismo Lab, you see, was always—and still is—part of the Division of Geological and Planetary Sciences. Palomar Observatory is part of the Division of Physics, Mathematics, and Astronomy; it's not a separate group, although it has its own budget coming down through the divisional structure.

VALONE: So you feel that the Seismo Lab and the geology division really do complement each other well, and there's not too much administrative overlap?

ALLEN: In my opinion, yes—that is, they *do* complement each other. Now, in more recent years, after Don Anderson became director of the Seismo Lab [1967-1989], that issue was exacerbated, partly because Bob Sharp eventually stepped down as chairman. We've had other chairmen who were not as effective in binding the two. There's no question, I think, that Don Anderson felt unhappy with some of the aspects of the division's treatment of the Seismological Laboratory.

You can get different points of view on this, depending on whom you talk to. But I'm sure Don is among those who would have liked to have had greater independence from the division. Caltech has always stood firm, saying that, no, the laboratories—like the Seismo Lab and Palomar—shall be part of the divisional structure.

VALONE: You said that you thought Bob Sharp was probably the most effective chairman of the division. What made him so effective?

ALLEN: Well, there are many factors. In the first place, he was—and is—a very forward-looking person. He doesn't like to look backward; he likes to look forward. In that sense, his vision has been good. Now, there are some forward-looking people whose vision turns out to be not as good as you might hope or who are unrealistic. But Bob, I think, led the division in some directions that were very important.

In particular, he started geochemistry at Caltech. That was his idea. He got the division behind it—he didn't do it alone, by any means. He brought Harrison Brown and Sam Epstein and Clair Patterson from Chicago. And Gerry [Gerald J.] Wasserburg. And right now—or at least up until recently—geochemistry has been one of the preeminent sciences within our division. That was essentially Bob's doing.

At the same time, we had to give up certain things. When Chester Stock died [1950], there was a long, painful discussion as to what to do about vertebrate paleontology. For several years, we brought visiting people on a year-to-year basis, hoping that one of these might turn out to be a person who could lead us into the future on vertebrate paleontology. Finally, the division decided—and I think Bob led us in this—that we shouldn't try to continue vertebrate paleontology.

Well, considering that a lot of our graduates were vertebrate paleontologists, the mail we got because of that was rather shocking. Many people thought we were abandoning a critical field, that we were changing direction. I think basically we made the right decision. And Bob led—but he also had the ability to get the division behind him. One never had the feeling he was being a dictator. One recognized he was a leader.

Bob is basically a very unselfish person, which was also terribly important in this. He was leading the division in the directions that he thought were best for the division, not for him.

I think he would have liked to have seen glaciology expand, but he didn't push that. And when the division on its own didn't do it, he didn't push it. That hasn't been true of a lot of divisional chairmen at Caltech, including others in our division. But it was his honest-to-God unselfishness that was terribly important in his ability to lead.

When Bob took over the chairmanship of our division in 1952, it was a mediocre division—I think certainly not in the top ten in this country, not by any means. From about the time when he stepped down [1968], and maybe even perhaps before that, it's been the number-one department in the country, according to the polls of department chairmen. There is a lot of concern right now that we may not be sustaining that, because you're always looking about five years behind when you're making these polls as to who's leading the country. But at any rate, Bob Sharp can take credit for that.

He was also very effective, I think, in the institute's administration. He was highly respected by the powers that be here at Caltech and worked very effectively with them. Even subsequent to his retirement [1979], he's been very active in various kinds of search committees and so forth. I think any one of the past presidents and past provosts you talk to would say that Bob is one of the great statesmen in the interests of Caltech. I'm sure he could have been president had he wished to be, but for a variety of reasons he didn't want to do that. His health has always been a problem, so his traveling has been quite restricted. At any rate, it's primarily thanks to Bob Sharp that we owe what preeminence we have right now in our division in the field of the Earth sciences. I think everyone you talk to in our division would say the same thing.

VALONE: So the division really bears his stamp to this day.

ALLEN: Oh, no question about it. If you have to point to one thing in particular, it's the development in geochemistry. And geochemistry was certainly not his own field—anything but.

VALONE: But he led the development of it.

ALLEN: Oh, yes. We essentially stole the department from the University of Chicago.

VALONE: I would like to turn now to the changes that you've witnessed in seismology and geophysics over the course of your career. You mentioned at several points that one of the most important things has been the development of new seismological techniques over that period. What are some of those changes, and how have they led to significant developments in the field?

ALLEN: Well, in terms of instrumentation, I'm not really the best person to talk about it. But there's no question that Hiroo Kanamori, for example, has taken advantage of this new generation of instruments. Don Anderson essentially pioneered in getting us to see the need for these instruments and initially in getting a few of them. But Hiroo's the one who has really cashed in, in terms of his science. It's not only instruments but the imaginative ways he's chosen to use them. Now, that's not my field, so I really shouldn't talk about this. But let me say [something] in another area, paleoseismology—one I'm much more familiar with, the area of Kerry Sieh's eminence. Kerry's thesis work at Stanford University dramatically showed how powerful it could be to excavate these trenches across the San Andreas fault and study the history of displacements and be able to say a lot about what the prehistoric, geologically very recent history of the fault has been. How often has it moved? How much has it moved? And that's been a real revolution in terms of seismic hazard evaluation. Kerry is certainly among the fathers of that field. Now, a lot of us were involved in it at various times, but that's one place where we really cashed in, and Kerry is known throughout the world. I think he's probably the foremost exponent of those ideas. What we're able to do from geological evidence is go back and say what the history of fault movement has been—something that in seismology we just can't do. We can measure little earthquakes along the San Andreas fault for fifty years, but that really doesn't tell us much about when the next earthquake is coming or what it's going to be like—less than I think we expected it would. I think the hope at one time was, if we got enough seismometers out there, we'd be able to predict earthquakes, or at least tell something about when the next earthquake was coming. Generally speaking, however, I think the greater advances have been the other way, from looking back in the prehistoric history.

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VALONE: You were talking about the development of paleoseismology. Have you been involved in that directly?

ALLEN: Well, yes. In fact, I was one of those who encouraged Kerry Sieh to do his thesis work.

He almost dropped out of Stanford; he came down here and talked to me, and I urged him to talk to one of the professors up there with similar interests. He did, and subsequently he got reinvigorated. His thesis work on the San Andreas fault, as I mentioned, is really a landmark study. [7] So we've been involved in some of the same things.

Now, let me make it quite clear, that's not seismology, that's geology. I happen to think it's very important for our understanding of earthquakes, but it's not seismology in the classical sense. And I think, in terms of understanding seismic hazard—and that's where I have really spent the latter part of my career, trying to understand seismic hazard—I think that it may be even more important than the seismological observations. It gives us a chance to look back at what has happened in the recent geologic past and therefore gives us some idea of what might happen in the future, from a statistical point of view.

You see, I consider myself a member of the Seismological Laboratory. I think Kerry, even though his office now is in that same part of the building, feels a little bit of strain as to whether he is a member of the Seismological Laboratory or not. So there are still some problems remaining in that area.

VALONE: Let's talk a little bit about your work on seismic hazard. That's occupied the last twenty years of your career, approximately.

ALLEN: You see, looking at these major faults, the obvious concern is what do they tell us about where big earthquakes are likely to occur; how often they're likely to occur; how big they're likely to be. So one of my efforts is to try to use these major faults to understand the answer to those questions: When did it last break? What's the average interval between times of breaks? What's the average size of breaks? What's the maximum size? All this sort of thing. It's been very fascinating.

It was made even more fascinating by the realization, over the past few years, that a lot of our earthquakes here in the folded terrain in Southern California are not occurring on faults that come through to the ground surface. That means that in studying surface faults, you're only studying one class of earthquakes. How do you go about evaluating the significance of the others? That's a very difficult problem. Probably the most challenging problem we have right

[7] Sieh, K.E. (1977) *A study of holocene displacement history along the south-central reach of the San Andreas fault*.

now is trying to understand seismic hazard on these buried faults. We can look at a fault like the Raymond fault. We have put trenches across it in a number of places. At least when we did this—ten, fifteen years ago—it looked to us as though the last major earthquake was about 2,500 years ago, and that earlier earthquakes had occurred on a somewhat comparable time schedule, once every few thousand years.

OK for the Raymond fault. But what about these buried structures, like the so-called Elysian Park thrust, that don't come through the surface? What can we say about them? That's a very difficult problem. They're reflected in growing anticlines, or folded structures. I think we're seeing that geodesy is going to play an important role in trying to understand those structures. That is, how is the Earth deforming day by day? We can understand this only by very accurate surveying, which we can now do cheaply and quickly via the GPS—the Global Positioning System. We couldn't five years ago, even.

But then I've also been involved in a lot of probabilistic studies, where you try to establish probabilities of earthquakes occurring. For instance, I've been involved in two of the studies that the USGS [United States Geological Survey] did here on the probabilities of earthquakes on major faults in the Bay Area and down here, where we were trying to use the slip rates of faults—the average rate at which the fault slips, over thousands of years—and use that as the primary basis of estimating how likely big earthquakes are. Even if you know exactly how rapidly the fault is slipping, though, you have to know how big the earthquake is going to be—that is, how much the displacement is going to be—before you can use that slip rate to get a time interval between major earthquakes. Obviously, small earthquakes would occur more frequently than big earthquakes. So you have to make some judgment of what is the typical big earthquake on that fault. Is it magnitude 6; is it magnitude 8? If it's magnitude 6, that earthquake might occur once every hundred years. If magnitude 8, it might occur once every few thousands of years. So these are the kinds of things we're looking at.

VALONE: So how realistic do you think the potential is for predicting earthquakes?

ALLEN: Well, you'll notice I haven't used the phrase "earthquake prediction" as yet. Although, that's an area that, particularly about ten, fifteen years ago, there was a lot of excitement about—probably more excitement than there should have been. When we say "earthquake prediction," I think what most of us mean is predicting within a few hours, days, or weeks a major earthquake on the basis of some sort of physical precursor. That is, something that happens—some telltale thing that happens—before the earthquake: a sudden shift in the ground, or an increased rate of strain, or changes in water levels. As opposed to the probabilistic approach, where you merely look back thousands of years and use that solely in a statistical way to look forward. You don't pretend to be recognizing precursors to individual events.

Well, I think we were unduly optimistic fifteen years ago that we were going to find a lot of these precursors. It's clear that some earthquakes have been preceded by precursors. The Chinese predicted one earthquake, in Haicheng in 1975. And it's obvious, I think, that there were some effects on groundwater and so forth that occurred in the days and weeks before that earthquake. To some degree, they were also lucky, because the Chinese also had a number of predictions that didn't pan out. In fact, a lot—more than they were willing to admit at the time. Nevertheless, in one case, it did, and that has led to optimism. Some of us still retain the hope that someday maybe we'll be smart enough to recognize what those precursors are.

One of the discouraging things has been that the more we look at earthquakes, the more we realize how different they are from one to the next. Although that's scientifically exciting, that's not very encouraging from the point of view of prediction. Because the more different earthquakes are from one to the next, the less likely it is that they're going to have common kinds of physical precursors.

I spent a lot of time in China looking at their earthquake prediction program, which got a lot of ballyhoo during the Cultural Revolution. And some of that was, if not dishonest, at least heavily weighted in the way the evidence was presented. It was an interesting program, because it got a lot of political support from the Communist regime.

One of the ideas was that the kinds of things that preceded earthquakes could be seen by farmers out in the field—by an increase in the number of rats, or by animals doing various strange things before earthquakes. What could fit in more nicely with Communist theory than these ideas where the broad masses of the people are the ones who might see the things that are going to allow us to predict earthquakes? It doesn't have to be done by the intelligentsia or the bureaucrats—no, it's the broad masses of the people who can predict earthquakes.

Anyway, they got a lot of financial support from the Communist government. Although a lot of intriguing things resulted, I think it also turned out that they really weren't any more successful in their earthquake predictions than we were. They tried to predict a lot more earthquakes, so they may have had a few more successes. But they also had failure after failure after failure. Some of the things they were doing were absolutely silly. Other things were quite valuable.

Also, the Russians announced that they had discovered that certain relations between the S and P waves in the period before an earthquake were different from normal periods. They felt it was a telltale sign that an earthquake was coming. Now, the Russians have some very competent seismologists and geologists, but I think, in retrospect—and there was a lot of excitement in this country that that technique would work—it's turned out that that has not borne much fruit. So I think, all in all, we would have to say that our hopes that we were going to have meaningful, short-term predictions of earthquakes within a ten-year period were optimistic and naïve. That has been disappointing.

On the other hand, I should emphasize that the long-term prediction of earthquakes through statistical extrapolation has turned out to be far more promising and exciting than we had ever thought. We can now point to given faults, like the Sierra Madre fault, and say, "Well, the probability of an earthquake on that fault in the next forty years is this kind of number." That's been very promising, and much more so than we had expected. So, at the same time that we've been disappointed in our progress in short-term earthquake prediction, I think we've been very excited about our progress in what we call long-term prediction.

VALONE: Do you see those kinds of probabilistic analyses of earthquakes being useful to the wider public? Or is this just purely a scientific enterprise?

ALLEN: Oh, I think in the long run they may be a lot more useful than short-term predictions. That is, if we announce tonight that there's going to be a big earthquake in downtown L.A. tomorrow afternoon, what do we do with that information? How can we effectively use that? It's a very difficult problem.

On the other hand, if we point to a fault and say that in the next fifty years there's a seventy-percent probability of an earthquake on that fault of a given magnitude, then it allows us to do land-use planning. It allows us to design building codes that are not absurdly conservative or too lax. So, in terms of effective action for the public, I'm not sure that the long-term effort isn't even more important. After all, if we have the world's most precise short-term prediction—let's assume I do predict an earthquake, and turn out to be right, for a big one tomorrow at 5:00 P.M. in L.A.—if the buildings aren't built to withstand this, so what? The only way we're going to build buildings properly is by knowing what kinds of things we should be designing against.

For example, some people say we should design against the maximum possible earthquake. Well, that's absurd! We don't really know what that earthquake is. It's sort of like a meteor impact. You know, within the next ten seconds, a meteorite could impact this room here—it's certainly physically possible—and we'd all be destroyed. We could guard against that, I guess, if we wanted to build our cities underground. But we don't do it because the probability is so low.

And you have to use, to some degree, the same approach in earthquakes. You've got to prioritize your effort—put your effort where the probabilities and vulnerabilities are highest—and then make up your mind what kind of a cut-off you want to have, below which we are just willing to accept the consequences without trying to prepare for it.

VALONE: Is our understanding of the actual mechanics of earthquakes now to a point where we can successfully design structures to withstand them?

ALLEN: Well, that's not really my field. But we're learning a lot more every day. The Northridge earthquake [January 1994], for example, was an eye-opener in this regard, because for the first time we got a lot of what we call strong-motion ground records with the actual quantitative measure of strong shaking right in the center of a reasonably large earthquake.

There's going to be a symposium next Thursday at the Pasadena Civic Auditorium, and some interesting questions are going to come out on that. I think some people are going to claim that we have not designed our buildings adequately. Others, I'm sure, will say we have done so. But I'm not an engineer, so I really shouldn't talk about the design of buildings.

VALONE: That does, however, tie into another point that I did want to talk about, which is the growth in public awareness of earthquakes and earthquake dangers. That seems to have largely taken place over the past thirty years. Was there a large public awareness of earthquake danger when you first came to Caltech?

ALLEN: Well, the Long Beach earthquake in 1933 was terribly important, because that occurred late in the afternoon—fortunately, after schools were out. The damage to school buildings was so great, however, that it was immediately clear to everyone that had school been in session it would have been a really terrible tragedy, compared with what it was. So the Long Beach earthquake was really a very historic event, not only in Southern California but actually, to some degree, worldwide. Because following it, new and very much improved standards were put through for school buildings, and building codes were changed for every other kind of structure.

Now, the public forgets these things very quickly, particularly where money is involved. It sort of goes by fits and starts. After every earthquake, the public gets aroused again, and then they tend to forget. Like at Northridge, when we had parking structures fail and so forth, people said, “Well, why don’t we do something about it?” And yet five years from now, they’re probably going to say, “Oh, gee, we can’t afford to build really strong structures.” So public opinion is a bit fickle.

In Southern California, and greater Los Angeles, the public is now reasonably well informed about earthquakes, primarily due to television. I sort of regret that, because I don’t like television that much as a medium. But the fact is that ninety-five percent of what people know about earthquakes comes from television.

Television has also done us in, on some occasions. They blow up some of these ridiculous predictions—like the one for the New Madrid fault in central Missouri last year, which was almost entirely a media event. I regret that sort of thing. But all in all, the documentaries, the pictures of earthquake damage, have meant that public awareness regarding earthquakes in California is just a whale of a lot farther advanced than it is in most parts of the world. What you see typically in earthquakes, and particularly in the Third World countries, is utter panic, because people just don’t understand what’s going on. For the most part, people here—you know, you may get terribly scared and so forth, but most people have some understanding of what an earthquake is. It doesn’t mean you still can’t be killed, but at least it’s not some completely unexplained kind of phenomenon, and also it leads to much more orderly planning for earthquakes.

One thing is very clear: In Southern California, we have to be able to live with an earthquake—staying here during and after the event. The idea of evacuating Southern California is totally absurd! So all of our planning has to be contingent on the fact that, come a big earthquake—even larger than Northridge—we're not going to be able to get out of California. We have to live with that earthquake right here. And that, I think, is leading to a lot of emergency planning and features that will be very helpful.

VALONE: What were the circumstances surrounding Caltech becoming a media center for earthquake information?

ALLEN: The original seismographic network was established here by the Carnegie Institution back before 1930, in an effort to understand the earthquake problem of Southern California. It was also a scientific network to try to understand something about the interior of the Earth. But right from the word go, the purpose of our network here was, at least in part, to try to understand the earthquake problem.

Once the network got started here at Caltech, no one else went into competition with us. Now, there's no reason why UCLA or the county government couldn't have set up their own network. But that didn't happen—partly because that would have been a duplication of effort. I think that over the years, Caltech has demonstrated that it's done a competent job of running the network. No one has come in and said, "You idiots, you're not doing it right." So it's a matter of historical accident, to some degree, that Caltech has been the only major seismographic network in Southern California.

Now, several years ago, when a lot of our money for support of the network began coming from the U.S. Geological Survey, they obviously wanted to expand the network. They had money to support it. They could have built their own network down here. As a matter of fact, that's what happened in the northern part of the state. Berkeley had formerly run the network up there for the northern part of the state. They did not want to cooperate with the survey, so the survey went ahead on its own. So now there are sort of two networks up there, but by all odds the largest one is the one run by the U.S. Geological Survey.

Down here, we decided initially it was best for both groups if we would cooperate. So actually we run this network jointly, which is sort of an unusual operation for a university, to work together with the federal government—particularly a university like Caltech, which prides itself on its freedom from U.S. bureaucratic controls. Basically, that's been successful, I think. Whether it will always remain that way, I don't know.

Caltech has continued to support the network because we felt it was important for research—not primarily because of its public-relations aspects. If we didn't think we were advancing the cause of science and our students couldn't get good data out of this network, I think we would turn it over to the government and say this is not really for us to do. But, at least at the present time, we still continue to get tremendous research value out of the network and the stations associated with it. But someday that may change; the time may come when we just say we're no longer in the business of running a public-service facility.

There's no question, also, that the network has provided tremendous visibility for Caltech. The Development [Office] people say, "Boy, there's nothing that gives Caltech more visibility and public support than the network." Fine and dandy, and we should make sure that continues, if appropriate. But that in itself is not an adequate reason for running the network. Fundamentally, it has to be a scientific tool if we're going to continue to operate it.

VALONE: It seems that, since around 1987, the public-relations aspect has really taken off. Perhaps because there have been some major earthquakes in the vicinity.

ALLEN: Yes, I think one of the reasons is that we've had more earthquakes the past few years, particularly in the L.A. area. We've had a much larger number of damaging earthquakes locally than we'd had previously.

Furthermore, every household has TV; they can see everything that's going on simultaneously. Plus, the network is a lot better than it used to be—so that we can, within a few minutes, tell where the earthquake is. That didn't use to be the case. I remember when I was working at the lab, we'd get a call that would say, "This is Mr. Smith. We just felt an earthquake. Where is it?" And we'd say, "Where are you calling from, please?" We didn't have the means then to really give out information rapidly. Now we do. So that's increased the interest in the news media of getting on the air quickly.

VALONE: I'd like to wrap up by asking you about your thoughts about the future of seismology at Caltech and the future of funding for seismological work. How do you see those things progressing from here?

ALLEN: Well, you know, we have a very strong program at Caltech—certainly one of the strongest, maybe the strongest in the world. I'd like to see us build strength on strength, as long as the science and the technology continue to be exciting and moving into the future.

Now, there always comes a time when certain fields of science tend to lose their excitement and other fields pick up. And maybe that will happen to seismology one of these days. At the moment, I don't see that. I think that at the moment it continues to be an exciting area. We're learning about the interior of the Earth, the nature of plate tectonics, the nature of the mountain-building processes—not to speak of increasing our understanding of hazards and helping society in that way. And I would hope that Caltech would continue to cash in on that.

Partly because of the fact that there are some very useful aspects to seismology, it's been able to get reasonably good funding—not that we aren't suffering along with everyone else, but at least we're not having the rug cut out from underneath us completely. I think it's clear—and clear to the public—that increased understanding of earthquakes can help us face the hazard better. There are also relevant problems, like those having to do with nuclear testing and the discrimination between nuclear events and earthquakes. I discovered just in the past month how important that will continue to be, because our previous arrangements have all been to try to understand what's going on in Russia and the Russians trying to understand what's going on here. It now appears as though our greatest hazard, or threat, comes not from Russia but from other parts of the world that have not yet had nuclear weapons. So it's a more difficult problem when the geography is spread that widely.

I should hope that the program should continue to thrive and that the funding will continue. I worry about funding for science in general, and particularly if we would like to make a major effort in an area that is not so directly related to seismic hazard or to nuclear blast detection—can we get funding in those areas? That's really the proof of the pudding these days: Can you get money to support what we call basic research? Things look somewhat bleak in that regard at the moment.

It's also clear that we are working more with the other sciences and fields of engineering. For example, seismic hazard is now much more dependent on geology than it used to be. It used to be purely within seismology. Now we're finding that our people trying to understand earthquakes are working more with people in mechanics, aeronautics. Brad [Bradford] Sturtevant and some of our people are working a lot on trying to understand some wave-mechanics questions. So I have no question that seismology's not going to be as isolated a science as it used to be but is going to overlap more with other, adjacent sciences.

So I'm generally optimistic in that regard.

VALONE: So you see prospects for growth in seismology and in geophysics, partly through these sorts of bridges with other disciplines?

ALLEN: Yes. For example, one of the most exciting things to happen in the past few years is the development of this GPS System for rapid, cheap surveying. It's just revolutionizing our whole science—not only seismology but a lot of other aspects of geology. It's bringing geodesy into the picture very closely. So here's an area where an instrumental breakthrough is having a major effect on how our science is being conducted.

[Tape ends]