CHARLES RICHTER
(1900-1985)

INTERVIEWED BY
ANN SCHEID

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Abstract
Interview in 1978 with Charles F. Richter, professor of seismology emeritus, in the Division of Geological and Planetary Sciences. A pioneer in seismology and active in the seismology and earthquake engineering fields for over fifty years, Richter's name is known for the earthquake magnitude scale he developed in the 1930s for local earthquakes. Richter received his PhD from Caltech in 1928. In 1937 he joined the Caltech faculty and worked alongside Harry Wood in the Seismological Laboratory, which that year was transferred to Caltech from the Carnegie Institution of Washington and was situated in the San Rafael area of Pasadena. The interview covers a wide range of topics, including his graduate student years at Caltech, then headed by Robert A. Millikan; recollections of Harry Wood, Beno Gutenberg, and Hugo Benioff and their work in the early years of the Seismological Laboratory; the prospects for earthquake prediction; the role of the Bikini atomic tests in studies of the propagation of seismic waves; the tectonics of Japan; the importance of earthquake engineering; his consulting work with the L.A. Dept. of Water and Power; and his views on latter-day developments in the geology division and at Caltech.

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Charles F. Richter

AN INSTRUMENTAL EARTHQUAKE MAGNITUDE SCALE*

By Charles F. Richter

In the course of historical or statistical study of earthquakes in any given region it is frequently desirable to have a scale for rating these shocks in terms of their original energy, independently of the effects which may be produced at any particular point of observation. On the suggestion of Mr. H. O. Wood, it is here proposed to refer to such a scale as a "magnitude" scale. This terminology is offered in distinction from the name "intensity" scale, now in general use for such scales as the Rossi-Forel and Mercalli-Cancani scales, which refer primarily to the local intensity of shock manifestation.

The writer is not aware of any previous approach to this problem along the course taken in this paper, except for the work of Wadati cited below. Total original energies have been calculated for a number of shocks, using seismometric and other data; but such a procedure is practicable only for a limited number of cases, whereas it is desired to apply a magnitude scale to all or nearly all of the shocks occurring.

Mr. Maxwell W. Allen states that he has for some time employed an arbitrary scale for rating large earthquakes, based on the amplitudes of earth motion calculated from the reports of distant stations. This laborious procedure is not far removed in principle from that adopted in the following discussion. Doubtless it has also occurred to others, but has failed of general application because of its paucity of dependable results.

In the absence of any accepted magnitude scale, earthquakes have occasionally been compared in terms of the intensity on the Rossi-Forel or some similar scale, as manifested near the epicenter. Even when reliable information is obtainable, this method is obviously exposed to uncertainties arising from variations in the character of the ground, the depth of the focus, and other circumstances not easily allowed for. In a region such as Southern California, where a large proportion of the shocks occur

* [Received for publication June 17, 1934.]

Charles Richter's original paper, with signature, from 1935 on the creation of the earthquake magnitude scale that today bears his name.

California Institute of Technology
Oral History Project

Interview with Charles F. Richter

by Ann Scheid

Pasadena, California

Caltech Archives, 1979

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Scheid: Let's start with some of your background, your childhood and early life. Would you like to begin with that?

Richter: To begin with, the name Richter is actually my mother's maiden name, which she resumed after a divorce, with court approval, and I have never been known by any other name. And it is the name, of course, of my maternal grandfather to whom I owe practically everything I am and have, in terms of support and education. My great-grandfather Richter was a brewer in Germany, Baden-Baden. He became involved in the political disturbances of 1848, and had to leave Germany in a hurry, bringing with him his small son who was then about four years old. That was my grandfather, known as Charles Otto Richter.

The family was at first in New York, and not long before the Civil War they moved to Richmond and then had to leave there in a considerable hurry. In later years, my grandfather was with the large firm manufacturing stationary engines at Hamilton, Ohio. He owned a farm and house about seven miles from Hamilton, and that is where I was born. The family moved to California in 1909 and included my grandfather, my mother, myself, and an older sister.

Scheid: And your grandmother--you didn't mention her at all.

Richter: She died in Ohio when I was about seven years old, and that was, I think, one of the reasons which decided my grandfather to move.

Scheid: Why did he move exactly? Was it his work, or. . . ?

Richter: Well, let's see, he had retired from business about two years prior, so he had nothing really to retain him in Ohio, and he had had rather glowing accounts of California and finally decided to move and take his family. So we came out here in January, 1909.

Scheid: You said that he worked in a company that made engines?

Richter: Well, he was actually secretary with some other duties. The Corliss engines, and the firm name was, let's see, Hooven-Owens-Rentschler Company. I think they're still in business.

Scheid: So he wasn't in the technical side of it, then?

Richter: No, although he had some things to do which involved technical knowledge. He occasionally acted as trouble-shooter. When some client would report that they were having trouble with their installation, he would go out and try to find out what was the matter.

Scheid: So, then, where did they settle in California?

Richter: We took a house in Los Angeles at 8th and Kingsley Drive, which is in the general Wilshire area. We were there from 1909 to about 1925, when we moved to another location on Bronson Avenue. And about 1936 I moved with my wife to Pasadena.

Scheid: So you went to school in Los Angeles?

Richter: Yes, a short time in the public schools and then at the age of twelve I entered the preparatory school for USC. At that time they were running this school on the high school level, which was at first Southern

California Academy and later University High School, which was discontinued. I owe it a considerable debt for a very solid foundation in elementary mathematics, in which, it turned out, I had some ability, and consequently it more or less affected my subsequent education and career.

Scheid: Was there a particular teacher there who was important to you?

Richter: Yes. Hugh Willett who was principal of the Academy and also the chief instructor in mathematics; he was very alert to the developments in mathematics and in mathematics instruction which were current at that time. So what we got was very sound and well established as of that date.

Scheid: Was there something new happening then that affected his teaching?

Richter: Mathematics at that time, at least instruction in this country, was very much under the influence of the ideas of the great German mathematician, Felix Klein.

Scheid: And so that was reflected in your high school education?

Richter: It was.

Scheid: What about physics? Did you do any physics in high school?

Richter: Only at the high school level. I should explain, my first scientific interest was in astronomy, and for many years I had the idea that I would eventually be going into that. It only came about later that there was a shift, and I went through a progression of chemistry to physics, theoretical physics, and of course the entry into seismology was more or less of an accident.

Scheid: Then you were already pointed toward science, though, at an early age? Do you attribute that to anything in particular?

Richter: I suppose it was merely natural. There were children's books around the house with some information about astronomy, as well as other things, and I developed an interest in the stars, the solar system, and what have you. And it stayed with me.

Scheid: You spent nights outdoors looking at them?

Richter: Well, at least I got out at night, even as a small child.

Scheid: You went to college?

Richter: My first year of college was as a freshman at USC, but from there I transferred, and went to Stanford University. There, as I mentioned, I took a chemistry course first and that didn't seem to be a satisfactory adjustment. Gradually I got into physics, which was more congenial.

Scheid: Why did you move to Stanford from USC?

Richter: I think the actual determining reason was that my sister had gone to Stanford and was enthusiastic about it, and of course I got exposed to the very attractive situation at Stanford, and decided, well, after all I would try to go there.

Scheid: You mean the campus, or the teachers, or what do you mean?

Richter: Stanford is a state of mind, and I don't think one can sum it up very easily in a few words.

Scheid: I think that is what a lot of alumni say about Stanford. Was your sister studying science too?

Richter: No. She was in English.

Scheid: Were there any particular teachers at Stanford that were important? You ended up in physics; was it due to someone there?

Richter: Only to a minor degree. I could mention two or three names, and they were all very competent and agreeable men, but I think one of the deciding factors was merely that at that time I was quite nervous and tended not to be neat, particularly with my hands, and this is fatal in a chemistry laboratory. So after some unfortunate experiences, I felt that this wasn't for me, and of course, naturally, in parallel with my instruction in chemistry, I had received quite a bit of physics and was brought in that direction.

Scheid: So you finished Stanford at quite an early age? How old were you?

Richter: Twenty.

Scheid: You must have finished high school very early as well.

Richter: Sixteen.

Scheid: And then you came back to Los Angeles right away?

Richter: Well, they weren't very clear years. I did finish for my AB in physics, and then my intention was to go on as a graduate student in physics, and I even had some research planned. But there was a definite nervous breakdown. I left the university bag and baggage, returned home, my mother had the good sense to refer me to a psychiatrist, and I was under advice and supervision for a number of years, in the course of which I found other things to do, and in particular, employment. My first employment was as a messenger boy at the Los Angeles County Museum. And after that I was, for a couple of years, working in a warehouse for the California Hardware Company in Los Angeles. That will account for the years about 1920 to 1923. In 1923 the former Throop Institute of Pasadena was reorganized as Caltech and Dr. Millikan came to take charge and also to lecture, and of course, with my interest in physics, I couldn't miss the opportunity to hear his lectures. The result was that very soon I gave up my employment and entered Caltech as a graduate student.

Scheid: So you came up to Pasadena from Los Angeles to hear Millikan. What do you remember about your first encounters with Millikan?

Richter: Merely sitting in his audience. He was giving a series of lectures on the developments in physics, particularly atomic physics, which were then new. Of course, this involved his own ground-breaking determination of the charge on the electron, but there were a great many other features involved. It was a very rapidly developing and exciting period in experimental as well as theoretical physics.

Scheid: What was he like as a lecturer?

Richter: Well, I don't know how to characterize him. His delivery was clear and occasionally somewhat slow. His lectures were always very well organized, since naturally he had been giving them in the same form for a long time. I remember that part of the time Professor Epstein followed him with a lecture on the even more recent developments, particularly on the theoretical side. At that time I found I could not follow. My preparation wasn't sufficient, and Dr. Epstein had not yet shed his German accent, so it was a little difficult to follow. As you know, eventually I became Epstein's student, and I owe a very great deal to him.

Scheid: You mentioned something about coming up earlier for Epstein's lectures.

Richter: Well, of course it was Millikan's lectures which drew me, but Epstein was there and immediately following, so on a few occasions, I simply remained in my seat.

Scheid: But your preparation wasn't sufficient, even though you had had a good foundation in mathematics?

Richter: Yes, but not sufficiently on the mathematical side of physics.

Scheid: So he was doing graduate school level lectures?

Richter: I suppose that was the case. I really am not too clear about the precise circumstances anymore, but I do recall hearing Epstein talk on one or two of these occasions, and finally realizing that I couldn't follow very well and would have to get my background otherwise. Then it was in the following year that I came around and registered as a student, and naturally some of my first courses were lectures by Epstein. By that time his English lecturing had improved somewhat and I had learned something, so I began to get a very great deal out of his instruction.

Scheid: What was the campus like at that time, do you recall anything about it?

Richter: It occupied the same general main area. The expansion of the grounds across San Pasqual didn't happen until later, but the campus was between San Pasqual and California Streets, and between Hill and Wilson, just about as it is at present, and many of the present buildings were in existence. Of course there were the two Bridge Laboratories and Gates and Throop Hall, which was older than the change of name to the Institute.

Scheid: What about student quarters? Were there any?

Richter: I should remember but I don't. I think those very first years they were using as dormitories some old buildings which had been constructed as army barracks during the war.

Scheid: Were you living there then as a graduate student?

Richter: I was living in Los Angeles.

Scheid: So you commuted every day?

Richter: Yes.

Scheid: So you were essentially living at home then during those years?

Richter: Yes.

Scheid: Let's get back to some of the courses you took and the teachers. You mentioned Epstein already. He was a very great influence on you it seems.

Richter: Yes, he was. He provided the real direction. Naturally, I attended lectures by Millikan and others in physics and mathematics, but they were not as critical as Epstein's courses were for me. Naturally, there were regular seminars which I usually attended, and we had distinguished lecturers on the campus, and I attended those. Lorentz lectured, Schrödinger lectured, Born lectured. These were all courses of lectures, not individual visits, usually for a full term or more. This was just at the time when quantum mechanics was evolving and the whole atmosphere of atomic mechanics was changing. Sommerfeld came and lectured somewhat later, after some of these critical changes had already taken place.

Scheid: Would you like to describe Epstein a little bit--as a person, as a lecturer, as a teacher?

Richter: He was a very beautiful lecturer in that his lectures were always carefully planned and organized. He had a number of odd mannerisms, some of which were Germanic and some of which were individual. I remember he was something of a pacer, and there was one particular lecture room which had a loose board or something at one end of the lecture platform, and he almost invariably hit that with a plunk. I'm not sure whether it was completely an accident.

Scheid: That was in Throop or in Bridge?

Richter: It must have been in the Bridge Lab. That's where at the time a large part of the Bridge Lab, and especially West Bridge, was in use for lecture rooms. The different rooms were fitted up and had connections that we used as laboratories, but they were not being employed in that way at first.

Scheid: Did you know him outside of class at all?

Richter: Not at first. Some years later, especially after I was married, my wife and I would visit him at his home, and there would usually be some other people there for something in the nature of a party. But I never got to know Epstein very well outside of his professional capacity. He was very much absorbed in his subject anyhow.

Scheid: You did your thesis under him?

Richter: Yes.

Scheid: Did he suggest the topic to you?

Richter: No, actually that came about through Dr. Millikan. Millikan had received a letter from Ehrenfest, which was in German, which Millikan could read perfectly well, describing the results that Uhlenbeck and Goudsmit, working under Ehrenfest, had obtained by bringing in the hypothesis of a spinning electron, which made sense of a lot of apparently contradictory items which had been coming up in atomic theory just at that time, of which I was fairly well aware. So Millikan gave me this and said, "Would you look it over?" And I did and checked on it and found that indeed it promised to be at least a partial theoretical answer to some of the matters that were troubling him. Finally this developed into matter for a thesis.

Scheid: So you know German then?

Richter: Well enough to read, especially in my subject; I had no difficulty at all. German was always easy for me. Granted my family did not speak German at home, but they were all familiar with the language and had the background, so that I acquired it, I would say, almost as a matter of course.

Scheid: But you never did study in Germany?

Richter: No, I never studied abroad, unless you want to count my Fulbright Scholarship in Japan.

Scheid: What was your thesis topic?

Richter: Oh, let's see--what was that exact title? Anyway, it was on the hydrogen atom with a spinning electron. And actually it developed into two theses, because I had taken up the investigation first on the basis of the classical mechanics and found that it would give results similar to those already obtained applying classical mechanics to atomic problems. Then just at this time along came Born, Heisenberg, and Schrödinger with the quantum mechanics, and I had not completely polished off my thesis, so that there was almost a second thesis dealing with the same subject from that point of view.

Scheid: So you were right there at that time; rather fortuitous. But you had to rewrite your thesis.

Richter: Hardly more than a few introductory words. That is, the first part of it was presentation of the material from what had been the standard approach up to that time, and it was worked out to show how far the matter could be carried under those assumptions. And of course the second part took it up from the point of view of the wave mechanics.

Scheid: You mentioned that Schrödinger came here. Do you remember him at all?

Richter: Quite vividly. He was a decidedly good lecturer, and he was speaking of a very fresh and new subject which was not completely worked out, as he pointed out himself. I owe to him one very priceless general remark which I found opportunity to squeeze into my textbook many years later. He was dealing with the generalization of the mechanical treatment, which had originally been set up on a non-relativistic basis. Now the problem was how to generalize it, so that it would take into account the special theory of relativity. So he was about to outline the procedure which he favored at that time, saying, "Now, of course the generalization is not unique." He stopped for a moment, then said, "Of course not, otherwise it would not be a generalization." I always enjoy repeating that because it is a very profound observation.

Scheid: You said Schrödinger was here for about a term you remember.

Richter: Yes, he gave a course of lectures.

Scheid: On what subject specifically?

Richter: Essentially the wave mechanics, the quantum mechanics in the form in which he had set it up. I probably have a notebook around here which would show about how far he carried it. Naturally, there were many other outstanding points for investigation at that time.

Scheid: How was his English?

Richter: Certainly no problem. I would say there was probably less trace of accent in his lecturing than still remained in some of Epstein's. Eppy retained some particular peculiarities after years, and they were a source of entertainment.

Begin Tape 1, Side 2

Scheid: You were talking about Robertson.

Richter: Howard P. Robertson was an extremely brilliant fellow and contributed greatly to the general theory of relativity. Unfortunately, he wasn't with us very long. Since this is anecdotal, I do remember his coming in one day. He said, "Well, I have finally completed my thesis, and now I begin to understand how a woman must feel just after she's had a baby."

Scheid: You mentioned this story of Epstein forgetting the factor when he wrote down an equation, and a student pointed it out to him. Was that a student that you remember?

Richter: I remember quite well. I had a lot to do with him. He's no longer with us, either. He was for years on the staff at Cincinnati. His name was Boris Podolsky, and it happens to be that it was through him that I met Lillian Brand, who became my wife.

Scheid: You met her on the campus?

Richter: Actually, no, at his home.

Scheid: Was she from Pasadena?

Richter: No, she was living with the Podolskys at that time and helping them out. She was actually born in Los Angeles. She always considered herself very exceptional, being the native daughter of a native daughter. There aren't many of those.

Scheid: That's right. So that was when you were a graduate student? Were there many students activities at that time? You did have friends who were students and went to their houses, and so on, but was there anything that was organized particularly, or was it very informal?

Richter: I think there were all sorts of the usual things going on, but I was not particularly social, and didn't participate in much of that kind, with the important exception of Professor Judy's discussion groups. Judy would organize these discussions with an announced date, and often some topic being particularly presented by a member, and the group would meet at his house in San Marino, and some of the most brilliant, better known members of the staff and associates were present, so they were really worthwhile discussions. People like Fritz Zwicky and Robert Oppenheimer were fairly regular attendants. I remember once I had raised some point which involved biology and genetics and who should turn up but Bridges. To my embarrassment.

Scheid: What sort of topics were discussed at these meetings?

Richter: It could be scientific, but that was somewhat on the rarer side. It was more likely to be philosophy or literature.

Scheid: And they were monthly or how often?

Richter: I have a feeling it was every two weeks, but I couldn't answer for that now.

Scheid: And one person was selected to give a talk and then there would be a discussion afterwards?

Richter: Yes.

Scheid: Do you remember specifically any one evening or question that was discussed that caused a lot of controversy?

Richter: Unfortunately, I remember mostly the arguments I started, and that is rather an embarrassing recollection. But we did have some very interesting things presented, and I learned quite a bit. I remember a remark of Oppenheimer's, which was characteristic of the man. I think I and a number of other people got into the matter of the justification for pure scientific researh. Points of eventual practical utility were raised, and Oppenheimer said, "As far as I am concerned that's of no importance. I do this kind of work because it interests me, and it is there."

Scheid: So that was his personal justification.

Richter: I think it is of a great many scientific men who are honest with themselves. They are asked always for justifications. We know objectively that their work usually is justified, but the prospect of practical use or even of making a reputation for future generations isn't as important as the fact that there it is, that is what one likes to do.

Scheid: Well, that was later, though, because Oppenheimer was in the thirties, wasn't he? That was when you were no longer a student.

Richter: That is right. And that would overlap into the period when I was working at the Seismological Laboratory, but still coming over frequently to the campus and joining groups such as this.

Scheid: You did talk about other lectures. You mentioned Lorentz and Born, particularly. Do you have any recollections of them and their lectures?

Richter: Only in the briefest way, because generally they were covering material which was either already published or about to be published, so I don't distinctly recall the material of those lectures as separate from what I remember of the general trend of the subject.

Scheid: You don't remember them as personalities?

Richter: No. I did not meet them personally actually, except I remember getting an autograph out of Lorentz, but. . . .

Scheid: He must have been quite old by that time, or not?

Richter: No, no.

Scheid: Born was also a good friend of Kármán, I think.

Richter: I just don't have anything on that at all. But these were almost too interesting times. The subject was developing rather rapidly.

Scheid: Sommerfeld came in, you said, a little later. Was that when you were already at the Seismological Lab?

Richter: Probably. Yes. That was about the time of the publication of the revised addition of his big book, and I think I had my thesis in order and was completing the second part of it.

Scheid: When Sommerfeld came?

Richter: I think so. I'm not sure about the order of events there.

Scheid: Do you recall anything about him?

Richter: No. Once again I attended a few lectures, not even all he gave. If I remember rightly, he was not there for a full term, but only for a short series of lectures. And I didn't get to all of them.

Scheid: You mentioned going to Judy's house. Did you go to Millikan's house at all when you were a student? He and his wife were concerned about students.

Richter: Once or twice that I can think of, but certainly it wasn't a frequent occurrence. Naturally at first I was living in Los Angeles, and later on, after I married and moved to Pasadena, I remember going to the Millikans' once with my wife. At least once. I've never been very socialized.

Scheid: You mentioned Robertson. Were there other students that were around when you were there?

Richter: There were people like William Houston, Carl Eckart, Ira Bowen, who was there still carrying on research under Millikan's gentle supervision. I remember Carl Eckart particularly for one special occurrence. He came in one day and he had a result which was apparently the exact thing he had been looking for. It had something to do with collisions of the second kind. But he said, "Well, this looks so good that I am going to check over it again and make sure nothing is wrong." And sure enough within a few days it turned out there had been a defect somehow in the equipment, so that the promising sensation failed. This didn't faze Eckart, nor anyone else of his character, but again it made a strong impression on me. The feeling that the real research man is never in any haste to give out a new result, particularly if it looks good.

Scheid: Houston you mentioned. Did you know him very well?

Richter: Not very well. I can't say I knew anyone very well. We were all there, associating at lectures and seminars and occasionally discussing research problems. One of the most common topics of conversation in those days was leaks. They might be electrical or very frequently they were vacuum leaks, and one person even had a lot of trouble with water leaks in his apparatus.

Scheid: Who was that--do you remember?

Richter: I am trying to recall--Langer. He was a good man, too.

Scheid: What about Ira Bowen. Did you know him very well?

Richter: Not particularly.

Scheid: He was a bit older perhaps?

Richter: Maybe a few years between us. I recall him quite well because of course he was always around participating in discussions, and I was interested also in the work he was doing, but not to the point of going and bothering him about it, but kept aware of what was going on. He was a very, very nice fellow personally.

Scheid: What was he engaged in?

Richter: When I was there he was doing a great deal of investigation on the atomic spectra for ultraviolet, because it is particularly important in connection with the theory of spectra and atomic constitution which was working out and being organized at that time, work which he had started as a student under Millikan and was continuing.

Scheid: I think Clark Millikan was a student at that time, wasn't he? He got his degree about 1928, I believe.

Richter: Yes, I was together with him in some lectures given by Bateman, and by the way, you asked me whether there was anyone, any other instructor who made a great impression, and I must say I had allowed Bateman to slip my mind. I remember particularly being in the same group with Clark Millikan and Hervey Hicks who was a very brilliant mathematician. I'm afraid he didn't last many years after that. But both of them used to relate an experience they had had with one of Bateman's lectures in which Bateman had proceeded from one expression to another which looked quite different with a minimum of explanation. Clark Millikan and Hervey Hicks got together in one of the rooms in Bridge where there were blackboards all around, and they started at opposite ends and worked together until they finally got expressions which they could compare. This made it possible to verify Bateman's result, and my recollection of the report was that they discovered that Bateman had subtracted an infinite number of imaginary infinities from both sides of his equation. Bateman was an even more picturesque and anecdotal personality than Epstein. I could tell you several Bateman stories.

Scheid: Oh, please do. At least one anyway, for the record.

Richter: Let's see--what would be a good one? A student came to Bateman just before lecture time with a development which he hadn't been able to carry out. It may have been a difficult integration, I don't recall anymore. But Bateman looked at it and said, "Yes, I think that can be done." He got busily to work on the blackboard, almost filling it with his beautiful and small hand--even with chalk it was a joy to see. This went on for a long time, and finally Bateman said, "I don't want to keep the class waiting. But there are six more steps, and this is the result."

Then there was the occasion also when he set forth the contents of, I think it was, a paper by Sezawa. And at the conclusion he said, "Now, I think this is right, but I cannot quite guarantee it because I had to read the article in the original Japanese." Well, he had followed the mathematics, of course.

Scheid: Very interesting. I guess I started on that with Clark Millikan, actually.

Richter: And that set me on to Bateman whom I had momentarily escaped. I saw quite a bit of Clark Millikan. Later on he was one of those attending this group of Judy's.

Scheid: He was involved in the Pasadena Playhouse, too, I think.

Richter: That I wouldn't know, but he very well might have been.

Scheid: You didn't know the other Millikan boys, did you?

Richter: No, I don't think so.

Scheid: It seems that when you were a student there weren't any student activities particularly that you recall. People were pretty serious. . . .

Richter: Well, not that serious. Undoubtedly there was plenty going on, but there was nothing I was participating in, and as I mentioned at first, I was living in Los Angeles and just coming out during the day. It was only after my attachment to the Seismological Laboratory and my marriage that I was based in Pasadena.

Scheid: That was about 1928 or so.

Richter: We moved to Pasadena in 1936.

Scheid: Buwalda had already come. Were you aware of that department at all, the organization of that department?

Richter: Yes, in those years particularly I was often lunching at the Faculty Club, and he was there. Of course, I met him, and I recall having a specific question for him at the first opportunity. At first, of course, I was still in physics. It was only a little later that I got into seismology and so had a little more contact with Buwalda and his group. Then in 1937 when the laboratory program was transferred to Caltech there was much more regular contact.

Scheid: You never did then take a course in it as a graduate student?

Richter: No. I never took courses in geology in my student years. Quite a long time later, after I had settled down in the Seismological Laboratory, I found opportunity to come over and through at least one full term attend the course in petrology, because I merely felt that petrology is fundamental in the geological sciences, and except for the big generalities I hadn't known anything about it. That was a very valuable experience.

Scheid: That was a big part of the division and still is, isn't it?

Richter: It is an important part of any competent geology division, I would say. It's basic. You don't get anywhere without it.

Scheid: In graduate school, were you doing physics just because you loved it or did you think about what you would do afterwards?

Richter: Not very specifically, because people were going out and finding positions. And this was before the depression and I was not yet married, so that I was pretty well free and it didn't concern me. What was in the back of my mind--of course, I had already done some work for the Institute as student assistant, which hadn't turned out too well. I had the feeling that I was in the good graces of the administration, enough so that if I stuck around, probably something would be found, and I might eventually work into a permanent position, because I had demonstrated interest and ability in this very critical field of quantum mechanics. But then quite accidentally the opportunity came up in seismology.

Scheid: And were you interested in that? You were happy that that came up or were you sorry to leave what you had been doing in quantum mechanics?

Richter: I didn't feel that I was leaving anything, because so long as I could stay in or near Pasadena I could keep in touch, which I did to a certain extent. I had indeed some problems in theoretical physics in mind which I wanted to work on and see whether I could get somewhere with them, which proved not to be the case. Nevertheless, I didn't feel I was departing. I did want to keep in touch, and the work at the laboratory I felt at least provided me with the means to stay around here instead of taking a position in some other part of the country. And gradually I settled into the seismological work as my main occupation.

Scheid: So you did want to stay around Caltech because of the stimulating atmosphere?

Richter: Quite so.

Scheid: When you went to the lab it was just the very beginnings of it? How did things look over there? They had that building, I assume, that they were renovating or something.

Richter: It was the new Laboratory building which of course is now the old Laboratory building. It had been completed the previous year. It was occupied by the staff in March of 1927. Or, let's see, I think they got moved in in January already and some of the instruments started recording in March. I didn't make its acquaintance until the fall of the year. Anyway, everything, as you said, was very new.

Scheid: And they had a staff already. Wood was there, wasn't he?

Richter: Yes, he was in charge. Hugo Benioff was on the staff but I didn't see much of him because he was incapacitated by a chronic illness which he really never completely got over. And at that particular time he was around only a very small part of the time. Later on he returned. About 1931 or 1932 he did some of his very best work in connection with the Laboratory.

There was Archie King, technical assistant and Halley Wolfe, who was a young fellow who acted partly as secretary and partly as photographic assistant.

Scheid: Then there was Anderson, or was that later?

Richter: That is Dr. Anderson of the Carnegie Institution and of course he was on the Mt. Wilson Observatory staff. He and Harry Wood were very good friends personally, and they worked together on the development of the torsion seismometer which became known as the Wood-Anderson instrument.

Scheid: So he really wasn't on the staff of the Seismological Lab?

Richter: No. No connection at any time, except I think he--no I don't think there was ever anything official about it. Of course, naturally Wood was in good contact with the whole Observatory staff, since after all this was under the Carnegie Institution of Washington.

Scheid: And then there was Arthur Day. Or was he there later?

Richter: He was never there in that sense. He was the director of the Geophysical Laboratory--so-called--at Washington, which was under the Carnegie Institution. That put him in the position of the head of a division, and Wood was nominally under his department. So far as Wood had any boss it was Arthur L. Day.

Scheid: You say "Geophysical Lab, so-called." That means they really didn't have a lab in Washington?

Richter: No. The name, the Geophysical Laboratory, I think is still there. But it was a standing joke that almost all the work that was being done there was geochemistry. And very good work it was too, but it just so happened.

Scheid: When the Carnegie Institution gave this money for the lab in Pasadena did they have other similar labs elsewhere, or was this their only seismological lab?

Richter: This was the only seismological installation. I don't think at that time they had anything to do with any other operations in that field. Later on, yes, but not at that time.

Scheid: Were there other seismological labs of the same size and caliber in this country at that time?

Richter: For one thing there was the Seismological Laboratory established at Berkeley with a second station at Lick Observatory, quite a number of years earlier. And then one of the chief centers of seismological work in the country was at St. Louis University.

Scheid: And was that it for this country?

Richter: No, there were several others. I am trying to think which location was in operation in the East. There were several good stations. One was at Georgetown, just ouside of Washington. That was a Jesuit station. And another at Fordham University. You will notice that these again are Jesuit institutions. Father Macelwane at St. Louis was an outstanding figure at that time, and he used his connection with the church to further the development of seismology at the various Jesuit institutions. It was said that this got along well because this was a scientific development and no one could see any way that it could come in conflict with the church dogma. Whatever the circumstances, this was so. Then there were instruments in operation at Harvard, and one near Washington, which was associated in some way with the government departments. That was at the observatory at Cheltenham, I believe. I could find a list very easily.

Scheid: Oh, well, this may be not so important. So you were exchanging data with all of these stations at the very beginning?

Richter: More or less. Not very systematically; especially in the first years, we didn't have material in a form to give out, so we were receiving more than we got. Indeed, we were receiving quite a number of bulletins from stations abroad. That was so already when I got connected with the

Laboratory.

Scheid: How was cooperation in seismology in this era? Was that quite extensive? And where were you getting information from, which countries?

Richter: That would be a long list.

Scheid: So there was a good deal of cooperation in the field at this time?

Richter: There was a considerable degree of international organization. It had been interrupted somewhat during the First World War, but was in pretty vigorous recovery. The international headquarters in seismology was, at least nominally, at Strasbourg, France. But the chief organ of publication of data, apart from other research information, was the International Seismological Summary, which was being put out from England. Turner at Oxford was doing the work. Stations all over the world would send their bulletins there, and they would be summarized, digested, tabulated, and put out in the form of the International Seismological Summary, which was consequently rather behind date but nevertheless was progressing.

Scheid: What about the Japanese? Were they participating at this time?

Richter: Yes, they had quite a large number of stations, some of them with very small and minor equipment. A central bulletin was being put out from Tokyo, and in addition some of the individual stations were issuing their own individual bulletins.

Scheid: Other seismically active areas, such as South America, China, were they participating at this time?

Richter: Well, in those early years. . . . South America is an interesting case. There were some established stations, for example, at La Plata in Argentine, but very notably in Bolivia at La Paz, where Father Descotes had got some very sensitive instruments and was reading them in detail under favorable conditions. For many years, his bulletins were the best source of information for earthquakes in that whole part of the world and had a strong bearing on the interpretation of recordings from distant parts of the world. When we got to publishing on the seismicity of the earth, we had to remark that for many years, La Paz was the most important single station in the world. There were of course large groups and networks operating elsewhere, but that one station assumed extraordinary importance because of its location and the care with which it was administered.

Scheid: So again it was a Jesuit priest who was involved here in seismology?

Richter: Yes, Father Descotes, S.J.

Scheid: Was he an American or was he South American?

Richter: He was not from this country. I really don't have the background. I do remember having seen some biographical material on him years ago, but I just don't recall anymore. But I have a feeling that he was native to that country originally, anyhow.

Scheid: So it wasn't just the American Jesuits who were interested in seismology? Were there others all over the world, or was he an isolated case?

Richter: No, he was not an isolated case. Another important installation in the same group was at Riverview College which is just outside of Sydney, Australia. That is a very important and well-equipped station, still is.

Scheid: And there was also a station in China that was a Jesuit station?

Richter: Zi-ka-wei. I am not sure they were Jesuits but they were very competent.

Begin Tape 2, Side 1

Richter: Occasion did arise after the Long Beach earthquake [1933], because then there was considerable effort on the part of some groups to just sweep that under the rug and forget about it. As it occurred at practically the bottom of the depression, the economic situation was also pretty awkward.

Scheid: So these were business groups or people who wanted to boost southern California, who wanted to hide this?

Richter: Yes. There was apprehension that it would slow up business development and particularly damage the tourist trade. There was an effort to give the impression out in other parts of the country: "Yes, we have this earthquake, but now everything is being taken care of, everything will be rebuilt now, and there will be no more danger in the future," which was a complete misrepresentation.

Scheid: Millikan was getting money from a lot of southern Californians for Caltech. Did he ever get any for seismology? That was totally supported by the Carnegie, wasn't it?

Richter: No. The Institute had contributed certain funds to the program and in particular, due to the generosity of Mr. Arthur Fleming, who was one of the Caltech trustees; it was due to his cooperation that the Laboratory building was established and constructed. And thereafter, the Institute took over the maintenance of the building and grounds, etc. So there was always a contribution to that extent.

Scheid: There must have been people here who would have been positive towards seismology in the belief that, knowing the area was active, to learn more about it would be an advantage.

Richter: We always had a certain amount of support. Not always financial, but in other indirect ways from the insurance interests, because they had taken a bad beating at the time of the Santa Barbara earthquake. So the insurance people and the better building organizations were on our side, and between the insurance and engineering groups, they produced the first versions of the Uniform Building Code, which did contain some auxiliary provisions for safe construction against earthquakes. Those were rather carefully detached from the main body of the code. So this was a situation which improved gradually. Of course, one direct and productive result of the Long Beach earthquake was the enactment by the state legislature of the Field Act. But that was only effective for schools and public buildings and only for those of new construction, so that it did not solve the problem of old and unsafe structures.

Scheid: You mentioned the insurance industry. I wondered if publication of fault lines and fault maps was a sensitive issue at any time?

Richter: Well, it was no problem. The Seismological Society published a fault map of the whole state on a large scale in 1922. Harry Wood prepared the southern half of that.

Scheid: But nobody seemed to react to that unfavorably? Or was it just ignored?

Richter: I don't know. It was published. The information was available. I wouldn't know much about that because, of course, that appeared before I was involved in the subject at all. But I don't recall that we ever had any very serious public relations problems, although occasionally some individual or some group would take offense, or some uninformed public figure would sound off in the press, or somebody would write a stupid editorial. But in general we went on pretty well. There was the advantage that for the first ten years, at least, the program was under the Carnegie Institution, which was quite nonpolitical, had its own financial base, and was centered outside of California. So not much could be done. After the program was transferred to Caltech, then we were perhaps a little more open to attempts to put pressure on our operations. But we were never seriously inconvenienced in that way, as far as I know. We were much more inconvenienced by the circumstance that we were still in the process of getting over the depression, so that we were not able to really start any expansion of the seismological program, which was urgently needed, for several years after that.

Scheid: Yet you didn't suffer the kind of cutbacks that Caltech did during that time, did you? Or were you familiar with what happened over at the campus?

Richter: Oh, yes, there was a general cut in salaries, and my personal circumstance was simply that naturally I wasn't drawing very much money from the Carnegie Institution, and after the transfer to Caltech that remained at that same level for many years. In fact, up to the time that Dr. DuBridge took over the administration.

Scheid: There was no raise in salary from 1929 or so till '46?

Richter: Quite right.

Scheid: Oh, my. That is quite different from what goes on nowadays.

Richter: Yes. On the other hand, nowadays the money doesn't mean so much when we get it. As someone at the Institute remarked, "We took a 10% cut in salary but the cost of living went down 20%." It was that sort of situation.

Scheid: You said that expansion was urgently needed. You mean setting up more stations to gather more data, is that it?

Richter: We had had plans naturally. We needed more stations, more instruments. One thing that I was particularly interested in was the development and use of portable installations, and that went on with various accidents and mistakes, but it did progress gradually until finally we always had at least one portable unit we could use in an emergency.

Scheid: You took these out to record a specifically active area?

Richter: The original idea was to take them out particularly after a considerable event and record the aftershocks so that we could trace down the geographical area from which they were originating. That was first done right after the Long Beach earthquake. We were just barely able to put the unit in operation, but it did work and did make a few useful recordings which contributed to our understanding of the event. It was used on a number of subsequent occasions, not with great effect, and then after the Kern County earthquake in 1952, we put several units of different characters into operation, unfortunately for too short a time, but nevertheless it was done. Nowadays, there are several units available in the Laboratory program and they go into operation almost immediately when there is an event of any consequence.

Scheid: The Long Beach earthquake was kind of a watershed in many respects in seismology in southern California?

Richter: At least it settled some matters forever, because we had had individuals ready to claim in public and even in print that there was no real earthquake danger in the Los Angeles area, that it was all San Francisco. And the Long Beach disaster put an end to that. And also, as I mentioned, it produced the first intelligent action on the part of the state, the Field Act, which certainly was a watershed in that sense. Although it left a great many problems unsolved which are still with us, at least it was a major step in the right direction. The provisions of the Field Act were good, and the later school buildings constructed under the Field Act provision performed properly and conspicuously better in comparison with those of earlier construction. So there is no doubt that it was a good and effective measure. It simply didn't go far enough.

Scheid: But did you learn anything seismologically from the Long Beach earthquake that was of any great importance?

Richter: It definitely put beyond question the seismicity of the Inglewood-Newport fault zone which had been under discussion, since naturally there was no very convincing evidence before that time. There had been the occurrence of the damaging Inglewood earthquake of 1920 from which the feature was named, and there was no assurance as to what the true circumstances in terms of seismicity were. The event of 1933 put that pretty much beyond doubt. And naturally there were some individual details, studies of the succession of aftershocks, using the recordings of the Long Beach earthquake to get a little better understanding of the subsurface structure in the entire region, and providing a good argument for the need of further seismological installations and study in the region. But it did happen right at the bottom of the depression--the banks were closed--so the recovery in terms of expanding the program had to wait for quite a few years.

Scheid: Something I didn't ask you about at all was the coming of Beno Gutenberg in 1930. Is that correct?

Richter: Yes, on appointment. In 1929 the Carnegie Institution called a conference at Pasadena to evaluate progress in that program to that point. Dr. Day was there and Dr. Anderson and Harry Fielding Reid of the elastic rebound theory, and two very important visitors from abroad, Harold Jeffreys and Beno Gutenberg. Oh, and I mustn't fail to mention Father Macelwane, who is an outstanding figure in seismology. Also, Perry Byerly was there. The idea was simply to ask, "Well, now, what are you doing, what have you found, and what directions are indicated?" So we took several days to present what we had been finding to the guests and show some of the records, etc. It was commonly understood among the whole Pasadena group that in all probability one of our distinguished foreign visitors would be invited to come to us, either on a temporary or permanent basis. There was some back and forth, and finally it was decided to offer the opportunity to Gutenberg. He accepted and arrived with his family the next year with a professorship at the Institute. But it should be quite clear that Gutenberg did not become a member of the Laboratory staff as such; but as courtesy and for the best of reasons, he was given office space at the Laboratory and spent a good deal of his time working there and familiarizing himself with what was going on and contributing to the program and even doing some research work on the records which were then available.

Scheid: Jeffreys, he was English, he was the other person under consideration?

Richter: Yes, he is very English, he is still with us. He is getting pretty old now, but has published I think the fifth edition of his outstanding manual, The Earth.

Scheid: And Gutenberg at that time, he had been publishing this Handbuch der Geophysik?

Richter: Editing it, yes. And of course he has some of the parts from his own hand, but it was a symposium. It ran into difficulties during the Nazi regime, so that it was never completed in its original proposed form. But many major parts of it were published and in circulation for years.

Scheid: Gutenberg was very eminent at that time? Do you know why he decided to come to Pasadena? Was there no opportunity for him in Germany?

Richter: It was certainly a better position. In Germany, he had the position in Frankfurt of Professor Extraordinarius, which I think had a small stipend but not much of anything. Mrs. Gutenberg would tell you. He was consequently depending for his living and the support of his family on the operation of the family soap factory.

Scheid: I see. You had to be independently wealthy to be an academician there.

Richter: Well, not always, but the position he had was more an honorary than a remunerative one. In addition, he was doing a lot of publishing and editing and particularly that editing of the Handbuch which was a big undertaking, and earned him an appreciable compensation. He had a couple of smaller books in print on the structure of the earth, and so forth. Naturally those sold to some extent and brought in some income. But in general, the offer was attractive to him from an economic point of view, and he had been over here and seen what the situation was in California, and in those years the cost of living was low in this area even compared with the rest of the United States, which of course ceased to be the case but was true at the time. So he was coming to a better position, both in terms of compensation and actual influence. And he had already some indication of the trouble which was then developing in Germany.

Scheid: Oh, he did. Did he ever speak of that? He felt that he didn't have much of a future there?

Richter: Well, after all, he was Jewish, and there were already indications of trouble. After he was over here he went to considerable trouble and expense to help other people to get out of Germany before the storm broke.

Scheid: Do you know specific cases of that?

Richter: No. I could have, but not anymore. I imagine that getting on the good side of Hertha Gutenberg, she could tell you a great deal about that if she is willing.

Scheid: Well, he probably had family there that he helped, but did you know of scientists that he helped?

Richter: Yes, there were instances outside of his family, and again, names do not occur to me. But I think they had something like a small, informally organized group which worked to get people out of Germany, and naturally where possible to find them positions in this country. Quite a number of people were rescued in that way before it became too late.

Scheid: Actually, Einstein came in that period to Caltech in the early thirties. I would like to touch on that here since we're in that period.

Richter: Yes, he arrived, very definitely it must have been 1931 or early 1932.

Scheid: Yes, I think he came three different years for the winter. Do you recall him from that period?

Richter: I recall meeting him, especially when Gutenberg brought him to visit the Laboratory after he had been here for a comparatively short time.

Scheid: Were they friends?

Richter: Well, yes, at least they were on good terms. One of the good stories, which happens to be perfectly true as I heard the details from one of the parties, concerns the Long Beach earthquake. It occurred at 5:54 in the evening. There had been a physics seminar. Gutenberg and Einstein had attended it and were walking across the campus talking, mostly about earthquakes. They didn't notice the occurrence. But Dr. Huse, who had, came up to them and asked, "Well, what do you think of the earthquake?" "What earthquake?" [Laughter]

Scheid: Neither of them had noticed it.

Richter: Now, it has to be said, the earthquake was not that strong in Pasadena that you would notice it walking around outside, and particularly if you were talking. Had they been observant, of course, it was probably getting dark about that time, but it swayed limbs of trees, and power lines, and things of that sort. But there was nothing that would have drawn it to their attention. So this is always appreciated as a good story and it happens to be perfectly true. Later that same evening, Gutenberg, realizing what the excitement was, showed up at the Laboratory and related this circumstance with considerable amusement. But I heard it later from Huse.

Scheid: Did you have any other contact with Einstein, other than that he came over to the Lab? Did you go to his lectures at all?

Richter: No, I attended some seminars in which he participated, but I had very little contact with him.

Scheid: So, although he was here, you didn't see him all that much?

Richter: No. I always felt I wasn't taking advantage of the opportunity

I might have had. It was a regular procedure. The Institute Calendar for the week would come out: "Physics seminar, subject and speaker to be announced." And that was nearly always Einstein, that was understood. They preferred not to put it definitely on the calendar because it tended to attract cranks and curiosity seekers. But I failed to attend most of those. I was pretty well absorbed in my time at the Laboratory and other things going on outside.

Scheid: Those were the years when you were assembling the data that eventually became organized in the scale that is named after you? It was around that time, wasn't it?

Richter: The first work was done on a group of earthquakes that occurred in January of 1932. And that was sufficient to arrive at and set up the general picture, and then I considered the technique as more or less under test for several years afterwards, although by the end of the year we were putting out bulletins with numbers on them from the scale. But the details were not published in full discussion until 1935.

Scheid: In you address to the Seismological Society [1978] you mentioned the role of other people in the development of the scale. Also the purpose of the scale actually seems to have been more of a public purpose than anything else, or am I wrong there?

Richter: We needed something which would not be subject to misinterpretation in terms of the size and importance of the events. And also in the process of working with the scale it developed, which we had already suspected, that the statistics on earthquakes in general were in a very bad way because they had been too much influenced by accidental circumstances of local intensity. It seemed desirable to have some objective and instrumentally-founded means of comparing earthquakes with each other. Even within a limited region such as California it had advantages, and when it developed that it could be expanded to cover the entire world, the value of the thing was greatly increased.

Scheid: You got some idea from an article by Wadati who was a Japanese earthquake expert. In other words you did read that literature in English? There were abstracts?

Richter: No, the Geophysical Magazine's (Tokyo) contents were almost exclusively in English. Occasionally they would attach abstracts in Japanese but I didn't read those. Professor Wadati was discussing the larger Japanese earthquakes and using the instrumental indications to compare them with each other, in a way which suggested to me the procedure which eventually led to the magnitude scale.

Scheid: How was it that your name was attached to it? You were instrumental in doing it, but there were other people who were involved.

Richter: Well, the scale as such originated under my hands quite unexpectedly. I had been working with Wood trying out various tentative means of comparing our California earthquakes for the purpose you have just mentioned a short while ago. And we weren't getting anywhere with it. Then I got hold of this Wadati paper and that gave me the idea of plotting up the data which we had in a particular way, and it worked out much better than I had expected and produced this definite numerical scale that practically fell out of the data. So I showed this to Gutenberg and Wood separately, and they liked it, and I went on systematizing it. Wood put a brief mention of it into his annual report to the [Carnegie] Institution.

Scheid: And that is where it got the term. Is that where the term was first used?

Richter: I called it the magnitude scale, and I refrained from attaching my personal name to it for a number of years. And I think it was Professor Byerly who started referring to it as the Richter Scale in public. As I think I said at Sacramento [1978], this somewhat underrates Gutenberg's part in developing it for further use, because after all, he knew a tremendous amount about seismographs and seismograph recording, and his knowledge could be applied to the interpretation of records written all over the world in a way that was coherent with the scale I had set up in California.

Scheid: And he was the one who suggested the logarithmic. . . ?

Richter: Yes. That is a rather elementary suggestion. That is, it was merely a matter of how do you plot the data when the numerical values extend over an unmanageably large range. The common practice in engineering and physics is you use a vertical logarithmic scale which compresses the data. And he had undoubtedly encountered the same procedure in some of his own practice. I had simply gone to him and said, "Here is the problem, what do I do?" He said, "Try plotting them on the logarithmic scale." I did, and then it became evident that it could be used in a manner to set up a definite scale for which again there was some parallel precedent in the astronomical use of the stellar magnitude scale, which is where I got the word, and also of course in the decibel scale for sound intensities, which is logarithmic. There is also a scale used in--oh, dear, what is it called? [The pH scale] It is working with soils and expressing their acidity, which is expressed in a logarithm scale again. It's a rather natural procedure wherever you have to deal with numbers which extend over a very wide range. Which proved to be rather astonishingly wide in the case of the earthquakes. If there was anything you could call an actual discovery that came out of that scale, it was that the biggest earthquakes were ever so much bigger than the little ones.

Scheid: And as your instruments got better, of course you were probably being able to record the smaller and smaller quakes.

Richter: In that direction the progress is relatively recent. Comparatively early in the program, [J.M.] Nordquist and I set up a paper in which we were investigating some groups of comparatively small shocks with the idea of finding out what the minimal recorded earthquakes were in terms of the magnitude scale. We found them ranging down about to the arbitrary logarithmic zero of the scale, not much below. But later investigators with more sensitive instruments have carried it down a good bit below that. And that is astonishing, because these very small events, which they record on highly sensitive seismographs, have about as much energy in them as dropping a rock out of a tree.

Scheid: And they can separate out all the background from that?

Richter: They can, in favorable circumstances. Naturally, the best work on micro-earthquakes has been done taking instruments away from the urban centers off to some undisturbed area. Fortunately, some of our important faults are out in still unpopulated areas. Population density along the San Andreas Fault has been increasing steadily for years.

Charles F. Richter

Session 2

February 17, 1978

Begin Tape 2, Side 2

Scheid: About the family name--as I say, I wanted to backtrack.

Richter: You should understand that Richter is a moderately common name in Germany and Austria, so very frequently I am asked whether I am related to this or that person, and the probability is very small. I don't know of any case of the kind. But it has been a distinguished name. Many years ago, there was the distinguished philosopher Jean Paul Richter. And in this country, Conrad Richter is a well-known author. I think now that the most distinguished Richter in this country is Dr. Burton Richter at Stanford. He has the Nobel Prize in physics. And then I have always rejoiced in the coincidence of names of Sviatoslav Richter, who is a great pianist, and naturally he spells his name in the Russian fashion, but it is the same name. But I have no relatives that I know of using that name. In fact I have very few relatives that I know of at all.

Scheid: None in this country?

Richter: Oh, by now I must have quite an assortment of distant cousins, here and abroad, but I have no idea who they are or where they are. Once in a while someone will write into me and say, "Now this name is in my family, is there a connection?" I usually have to write back, I don't think so, or I can be positive that it isn't so.

Scheid: You said that your grandfather left because of the revolution in 1848. Was he directly involved in anything there?

Richter: My great-grandfather, Erhard Richter. I don't know the details, but there was a party in Germany which was working toward a social revolution. Along with a great many others, my great-grandfather fell afoul of the authorities, and it was get out or else. So he emigrated with his whole family, including the small son who was my grandfather.

Scheid: Did he carry on his brewery business in this country?

Richter: I don't believe so. I don't know precisely what he found to do at first. He was in New York for quite a while, and one of the stories my grandfather used to relate with pleasure was being taken as a small boy out to Staten Island where his father was visiting Garibaldi, who was on Staten Island making his living by making candles at that time.

Scheid: So he was politically interested?

Richter: Very much so oriented. And this led to complexity later on, because for some reason the family went from New York to, of all places, Richmond, Virginia. And they were there at the time that the war broke out. In fact the story was that they got the last train out of Richmond for the north and lost all their possessions.

Scheid: So they were Union sympathizers then?

Richter: Yes, and he and his two sons served in the Union Army.

Scheid: So your grandfather did serve . . . ?

Richter: Yes, he lied about his age.

Scheid: Another thing that I wanted to ask about your grandfather, was he interested in science at all?

Richter: Yes, in a rather inclusive way. He was one of those people who buy encyclopedias and sets of great books. I remember we had a set of the World's Classics. And in particular there was a very old set of volumes called Circle of the Sciences that went in print about that time. It actually was a reprint of pretty old and partly obsolete material. It covered the range of the sciences of the mid-nineteenth century quite well. It was beautifully illustrated, and of course I read quite a bit of it.

Scheid: So that was his contribution to your science education.

Richter: Well, it was part of it, yes.

Scheid: You didn't talk about things like that at home so much?

Richter: Not very much, no. He had a general interest. His interests were generally less in pure science than in technical and engineering matters, partly, of course, conditioned by his business connection. He had practically no mathematics.

Scheid: Then I wanted to jump to the twenties when you were at Caltech and ask a few more questions about faculty members at that time. I wondered if you took any courses from [Richard Chace] Tolman? Do you recall what kind of person he was?

Richter: He was a lovely person. And, yes, I did take one set of lectures, and at the moment I could hardly tell you what they were. I'm not sure it wasn't a seminar set-up. That was more likely. But I was in more or less frequent contact with him. For years I would see and meet him at the faculty table in the Faculty Club, and at seminars and other occasions. I remember very well that he gave the address at the commencement where I received my doctorate degree.

Scheid: So that was an important event in your life.

Richter: To me certainly.

Scheid: Another person I wanted to ask about was A. A. Noyes. I know you weren't in chemistry, but perhaps you encountered him?

Richter: Only casually. I doubt if I ever exchanged two words with him. It just happened that way. But he was about and I would see him, and he would sometimes be present at seminars or meetings. But I had very little contact there. I remember when I was first in the Institute I had gone over and talked to someone else, not Noyes, about registering or at least auditing some courses with the idea of bringing up my deficiency in chemistry. And I was somewhat discouraged from that largely because their laboratory facilities were pretty crowded. They had barely enough to take care of the students they had at that time. So I never did go back and pick that up.

Scheid: So they had plenty of students in those days, even though it was a small institution?

Richter: This is relative. Let's remember also that those were the years where there was a uniform course for all freshmen. So this involved chemistry. So at the very least they got all the freshmen.

Scheid: Another person I wanted to ask about was E. T. Bell. Did you take any courses from him?

Richter: Seminars. And once again we encountered each other frequently around the faculty table. He was an extremely alive and stimulating personality, and I enjoyed his fiction as much as his scientific contribution.

Scheid: What was his fiction like? You mean he told stories or . . . ?

Richter: No. He was a well-known author of science fiction under the name of John Taine.

Scheid: And did he reflect this in his personal behavior, that he was writing these stories?

Richter: No, I wouldn't--I would only say that he was a highly original and imaginative person. Naturally this was expressed in his work. Also, he had the facility in writing which was evident in both contributions. He had several very worthwhile books dealing with mathematics and the progress in the field. There is one little story I like to tell about Bell because it is illustrative of both the man and the subject. I had come upon a rather general proposition on factorability of expressions which I thought might be interesting to put forward, as can be done in the mathematical publications, simply as an outstanding question to see if someone of better ability could make sense of it. So I told him about this and I said, "The only trouble about this is that you have to state it in such a way as to exclude trivial cases." And he said, "Well, that's easy. You just start out by saying, 'Excluding trivial cases.'" [Laughter]

Scheid: What was he like as a person? Was he like Bateman as a lecturer, someone who proceeded very quickly through things, or . . . ?

Richter: Well, they both proceeded very quickly through things, but Bateman was on the whole far more formal and he had everything very closely organized. He had an enormous file of notes and research contributions in mathematics, which, after he passed away, was made the basis of these various volumes of the Bateman project. He was that sort of person. That was not the direction that Bell's genius took.

Scheid: He was less organized, you would say?

Richter: Perhaps, in a way. But he had a very fine sense of humor which occasionally could become quite caustic. You can find some of it in his books.

Scheid: Was he hard on students?

Richter: No, no. I think he would have been pretty hard on any pretentious and ungifted person. We occasionally had a few of those around. But in general, no, he was very generous and easy to get along with.

Scheid: Who do you think was the most demanding professor that you had?

Richter: That could be misinterpreted. More than anyone else I worked with Epstein. His standards were those of sound scientific work of the sort we regard as characteristically German, and he expected himself and others to keep up to those levels of care and precision. And this was no special problem for me, because I personally approved of it heartily and felt the same way about things, even though I found it difficult to keep myself up to that level. Nevertheless, it was not the position that he had to push me to try to do things right, I had to push myself to get them right. I imagine that this point of view might have been less congenial to other students who were more inclined to go off suddenly in an unexpected direction.

Scheid: So he not only gave you a lot of background in physics and mathematics, but he also gave you a standard to strive for?

Richter: Yes. His lectures alone were models of presentation. The subject would be picked up in what I came to regard as the "proper way."

Scheid: When you taught, then, you attempted to emulate that?

Richter: Well, hardly. I pass over my brief experience as a teaching assistant trying to teach mathematics to freshmen. The Institute quite wisely got me out of that pretty promptly. I would say, yes, that later on when I came to give this course in elementary seismology, naturally the principles of organization and presentation were, to the best of my ability, the sort of thing that I had learned from Epstein and others, because Paul Epstein was by no means the only member of the Institute staff who was capable of maintaining high standards.

Scheid: But then perhaps in your presentation of papers or in the way you carried on your scientific work, you were influenced by his principles?

Richter: Yes, and also by the general atmosphere of the subject at the time. The quantum mechanics was developing very rapidly and one of its features, which was a controlling element and was difficult for some observers to adjust to, was the idea of approaching every definition and discussion in terms of known and observable quantities, and to leave out as much as possible of theoretical, or still worse, philosophical implications. This stuck with me and was responsible for a feature of the magnitude scale, namely, that the magnitude is very carefully defined in terms of what can be measured on the seismograms. Frequently there have been suggestions that the magnitude scale should be defined in terms of energy and this was purposely not done, because to do that would have then involved continuous revisions, both numerical and theoretical. So that I have always insisted the magnitude scale represents what we observe and this may not be interpretable in terms of energy. So I instance that as one effect of general principle.

Scheid: I see. I wanted then to also ask you a little bit more about the students. You mentioned Howard P. Robertson and it wasn't quite clear in my mind who he was and what he did. He was a fellow student in physics?

Richter: Yes, he was completing his doctorate--let me see, I think about a year, maybe two, before I received mine. I'm sure there are many other people who recall him vividly. He contributed very brilliantly to the theory of general relativity, particularly on the cosmological side.

Scheid: I see, as a student already?

Richter: At the point of developing his thesis, yes. And he went on--I believe he was at Princeton for some years, and he published a number of important contributions to the subject, and then passed away very untimely, because he was still very productive. It was a great loss.

Scheid: Did you know him personally very well?

Richter: Not very well; but at the level of common association, participation in the same seminars, meeting at the Faculty Club, across the lunch table, this kind of thing. As much as with most of the others who were there at the time. I mentioned the names of Carl Eckart and Will Houston, and of course, Dr. [William R.] Smythe was there and already giving courses.

Scheid: Did you have courses from him? He wasn't a student with you, was he?

Richter: Oh, no. No, he was a little ahead of me, and no, I never went through one of his courses. It would have probably been good for me. His courses were an excellent example of what I was just saying about the severe discipline and requirement for precision which was very congenial to the Institute as a whole.

Scheid: And so he contributed to that kind of rigor that was going on then? You didn't know him otherwise though?

Richter: Naturally we know each other, and once in a while I have, not so long ago, encountered him on the campus. Only said hello. The answer to what you have in mind is mostly no. I was not socialized, and I did not encounter these gentlemen otherwise than in the course of my work and attendance at the Institute. Most of the time I was living in Los Angeles and commuting back and forth, so that if anything went on in the evening I had to make a special occasion of it.

Scheid: You didn't move into Pasadena until 1936, I believe you said.

Richter: That's correct.

Scheid: You mentioned something about going out in the summer on back-packing trips and so on. Did you go with other students, or hadn't you started that at this time?

Richter: That will go back very early. About 1916 I was a member of a small natural history club [of persons] who had various interests in the sciences, but mostly the biological sciences. I had a strong amateur interest in botany, and we would go out in the mountains and hike for a day or more and look at the plants and collect some of them. And that involved backpacking sometimes over several days. The group still persists in connection with the County Museum. It was the Lorquin Natural History Club, but now is the Lorquin Entomological Society.

Scheid: And about how many people were in that?

Richter: Not very many, twelve or fifteen I should say.

Scheid: Who were they? Were they young people mostly, and what were their professions and backgrounds?

Richter: Yes, [they were mostly young people]. I could hardly give any full account of that time. One or two of the older men were in some kind of business, I couldn't tell you what. Some were students. No, the details escape me. So do most of the names.

Scheid: Did you continue going out with them when you were at Caltech?

Richter: Mainly earlier. What happened was that, see, this started in 1916 and we were going out during that year and the following summer, and then the war started. And this dispersed us more or less; one fellow I think actually left the country. So it never did resume. Then afterwards and especially after I had returned from Stanford, I took to going into the mountains with my family, their friends occasionally, and then after I married, my wife and I were often out together.

Scheid: Let's see, you married in 1928? You mentioned that Boris Podolsky was the person who introduced you.

Richter: He was a student at the Institute working toward his doctorate about the same time I was working toward mine. In fact, for a while, we shared an office together.

Scheid: I think we have talked pretty much about your student days. Now

I wanted to go back again though to the beginning of the Seismology Lab a little bit and talk about Harry Wood and his background and how he came to be head of the Lab.

Richter: I think I can tell you a little. By the way, here is this thing. [Copy of paper by Robert Friedel, "Institution and Institute: Seismology in Southern California, 1921-1936." Paper filed in Archives.]

Scheid: Did you find it accurate for the most part?

Richter: For the most part, but there are a large number of small inaccuracies which do no harm but they just aren't right. It rested very heavily on Dr. Day's account of the origination of the program, which he wrote quite a number of years after the fact. And I think there are a few imprecisions there which are due to Dr. Day himself and then others are misreadings by the young man who wrote this up. One or two slight errors, for example, just one, in connection with the establishment of stations in the Owens Valley, he refers to the Los Angeles Water Company. It should be the Department of Water and Power of the City of Los Angeles. So such things as that. But the general picture presented is on the whole quite correct. And I don't think there is much in the way of background on Harry Wood, and by the way I wrote up a memorial on Harry Wood which is published by the Geological Society. I gather you haven't seen it?

Scheid: No, I haven't.

Richter: There is a possibility that I may have a copy here. Just a moment, I'll find out. . . . He was born and brought up in Gardiner, Maine. He went to Bowdoin and from there to Harvard where he took a master's. He never took a doctorate degree anywhere in spite of the fact that people would frequently refer to him as Dr. Wood, which rather annoyed him.

Scheid: Did he feel sensitive about that?

Richter: I don't think so very specially. But I don't know, it just isn't right; and it may also have been due to the very large reputation of Dr. R. W. Wood with whom he was occasionally confused, to their mutual annoyance.

Scheid: Who was R. W. Wood?

Richter: Very famous for contributions to optics and physics generally. One of the outstanding physicists about the turn of the century. And a very colorful character so that there is a published biography on him. I don't think I have that here. But to take up Harry Wood. In 1904 he received an appointment at UC Berkeley, an instructorship in mineralogy and geology. Mineralogy was his special field. He was there at the time of the 1906 earthquake and was a member of the commission which investigated and published on it, and in particular his area was the city of San Francisco, which he went over and investigated thoroughly, and very recently people are going back and referring to that paper on occasion for details.

From Berkeley he went to Hawaii, where he was at the Volcano Observatory for a number of years [1912-1917], and quite a few of his publications were linked to the volcanoes and vulcanism. After the First Word War, he returned to this country and he exercised himself in getting the Carnegie Institution to implement a proposal for a seismological network and installation in Southern California, based on a suggestion in a paper by Andrew Lawson, with whom Wood had been associated while he was at Berkeley and particularly on the Earthquake Commission work. The program was officially set up under the Carnegie Institution of Washington in 1921, with Wood as Research Associate in charge. The first instrumental records under the program were written in 1923, the new Laboratory was constructed in 1926 and occupied in 1927.

Scheid: Where was he physically in those years when there was no laboratory? Was he on the Caltech campus?

Richter: No, he was living in Pasadena, and he had facilities at the Mt. Wilson Observatory office. Of course, he was under the Carnegie

Institution of Washington. And he was working continuously with Dr. J. A. Anderson who was a Carnegie Institution staff member, particularly on the development of the Wood-Anderson torsion seismograph.

Scheid: So the Seismology Laboratory was an offshoot of the Mt. Wilson Observatory in the sense that they collaborated in the early years.

Richter: To that extent, yes. At least there was considerable collaboration--but this was especially Dr. Anderson. Naturally, Wood knew all the staff members quite well.

Scheid: That actually brings me to [George Ellery] Hale. I wonder if you ever knew Hale.

Richter: I never knew him. I'm not sure that I ever spoke to him. Hale was involved to some extent in getting action in the Carnegie Institution to support the seismological program. Wood, in particular, knew Hale quite well and discussed this with him. What I had in mind is gossip and to nobody's discredit, but it might not be right. My difficulty is that I came into the program after it had been underway for a number of years, and I had had no other contact with it, so what I know about the early background and proceedings of the program I have secondhand from conversation with Wood and Benioff, who was with it before I was, and of course from such things as Dr. Day put in print.

Scheid: But Hale was definitely interested in the seismological program?

Richter: Yes, indeed. If I remember right, he was on the committee under the Carnegie Institution which was originally appointed to investigate the possibility of such a program and which finally recommended that it be set up with Wood in charge.

Scheid: Do you think he saw any benefits for the observatory in the seismological research for their precision of measurement there?

Richter: Well, there was a remote bearing, yes.

Scheid: But you don't think there was any thought about that?

Richter: The connection is largely through bearing on geodesy and the location of positions, which is somewhat essential to the astronomers. There was considerable discussion right after the 1906 earthquake. The International Latitude Observatory was at Ukiah--well, it still is there. It was in the area of displacements caused by the 1906 event, and it was hoped at first that the latitude observations, which were very precise, could further confirm those displacements. But it turned out that they were below the accuracy obtainable in that way at that time. There was considerable concern about such ground displacements affecting geodesy, affecting the survey, and eventually having some effect on the precise operations of the astronomical observatories. It was remote, but very definite. Of course, there was later on a very definite earthquake effect on the Mt. Wilson Observatory which no one anticipated but is one of the good stories. I think I have put that in my book, about the business about the effect of the earthquake on the big mirror?

Begin Tape 3, Side 1

Scheid: Well, I think we were at your coming to the Institute, and your saying that you weren't particularly enthusiastic about this situation because of the academic responsibilities.

Richter: That was relatively a minor objection. Of course, otherwise my relations with the Institute were very happy, and I was rather pleased than otherwise to be definitely on the staff. But I had had some previous unfortunate experience with elementary instruction and I didn't look forward to more of the same. But it wasn't quite that bad.

Scheid: You find it difficult to instruct?

Richter: Generally speaking, yes, but something like not quite ten years had intervened between my previous experience and getting back to the Institute, and I had matured somewhat in that time. So it was not particularly bad, and apparently the results I got were on the whole satisfactory.

Scheid: You mean that in the beginning when you were younger you felt less secure when you were standing in front of your class and that sort of thing?

Richter: Not only that, but I was very nervous and badly adjusted psychologically, and that is about the worst situation one can have in trying to talk to a class of freshmen.

Scheid: I think we talked a little bit last time about the ups and downs of student enrollment in seismology, and you mentioned that most of them were intending to go into a career of prospecting.

Richter: Well, at least it was in that direction that most of the opportunities were offering.

Scheid: Did you get any students particularly, though, that you recall who were really interested in your field, and particularly in seismicity?

Richter: Of course, outstandingly, Clarence Allen. That was later on. Then there were--oh, I remember quite a few brilliant people who came in with us in years considerably later than those we are now talking about, including people who were actually visiting research fellows. If we're thinking about the period immediately following the administrational transfer, not so much. After all, within a few years after that, everything was changed by the war effort. That went on until 1945, so that it was after 1945 that we commenced to get in more and more men who had real ability and distinction.

Scheid: Let's talk about the war effort as far as you were concerned. Did it affect your work?

Richter: That separates a little bit, because from 1939 through most of 1941 the war was going on in Europe, but it happened we were not involved. We were keeping up our previous program of even cooperation on the international scale so far as practicable. Naturally, that was getting more and more difficult; it was tapering off. Particularly Gutenberg and I took the opportunity to set up investigations on material already in hand. After Pearl Harbor, like many other people, I thought I should be doing something specific, and I took the matter up with the division and the Institute office, and for a while then, I was attached to the OSRD program, and part of my compensation was coming from that source. And I think I was telling you what we were doing mostly was in the code name Operation Mousetrap, which was use of rockets against enemy submarines.

Scheid: How did your work in seismology relate to that?

Richter: Not at all, and practically while that was going on I wasn't doing any seismology. I would get to the Laboratory once in a while and try to keep some of the routine filing and measuring in order. But in general it was not a good time.

Scheid: You stayed here in Pasadena during that time?

Richter: Oh, yes.

Scheid: It was just a project that was here on the campus?

Richter: Yes, at least a good deal of it was headquartered here on the campus. It involved rocketry in part and that development was at JPL.

Scheid: So you were also working up there at JPL?

Richter: No. I was spending almost all my time, when I was doing anything, on the campus trying to solve some entirely theoretical and mathematical problems. And I developed more and more the feeling that I wasn't getting anywhere and was not really significantly contributing to the war effort, and consequently I would do better to get back in seismology and help in developing some of the problems which actually had come to involve the war effort geographically. You see, you might say almost by coincidence, the Japanese took over and for a time held almost all the most heavily seismic areas of the globe, the whole western Pacific. So the Armed Forces were very interested in what we could tell them about the general seismicity in terms of locating installations and the like. So that it was possible therefore to contribute something toward the war effort working in my own subject and I felt that this was fairest, all considered. So I asked to be transferred to the Laboratory work and that was what took place.

Scheid: Installations--do you mean they were interested in pinpointing . . . ?

Richter: They didn't want to go and set up a barracks or a harbor development right on an active fault. It was not a question of what might be going on at the time but what to expect in terms of earthquake geography. It actually so happened that there were a couple of very important and locally destructive earthquakes in the Japanese area--well, one particularly before the end of the war--that raised a great many questions which were not adequately answered until afterwards when we got once more in communication with the Japanese.

Scheid: But you were able to record those quakes here?

Richter: Yes, as a matter of fact we did. Our station and others were able to get together and identify that a very large earthquake had happened in the Japanese area, and this was supported by the fact that broadcasts and other reports from Japan began to taper off rather abruptly immediately afterwards. That information was to some extent of military value. How much it really was I don't know, and it might easily have been more.

Scheid: You mentioned something about isolation. When you were up at the Lab you didn't get down to the campus very often, and when you started coming to the campus that changed. Did that change the acquaintances you made?

Richter: Not very greatly. It helped maintain the contact which I had already established. Of course, for some years I was commuting from Los Angeles, and that would mean generally that I was here during the lunch hour, so I would go to the Athenaeum and have lunch at the tables with some of the most brilliant people on the campus. So I got to know many of those people better than ever before, and they got to know me and would come to me when, rather surprisingly, questions arose on which I had something to suggest. That went on more or less regularly through the war years. Of course, in 1936 I moved to Pasadena, I wasn't commuting back and forth, but I was going to the Laboratory and then lunching on the campus and then coming home. In still later years, however, I decided I needed more continuous time at the Lab, so I gave up lunching on the campus and would take a box lunch to the Lab.

Scheid: Then you just came in to teach your class.

Richter: And so forth. That was a rather later development.

Scheid: Did the war change things around Caltech very much? You were working for a time on a wartime project. Was that true of most people?

Richter: Yes, it was true of most people. Most of what was going on was war-related. We had courses of instruction for prospective officers in several departments, one of which was meteorology which had quite a boom at that time, but eventually they detached themselves from the Institute after the war.

Scheid: Under Krick.

Richter: Yes.

Scheid: Yes, he was doing work trying to help predict weather and situations for bombing raids.

Richter: Yes. The forecasting of weather was very important for military reasons, so that it is not surprising that that flourished under the war conditions.

Scheid: Was the Institute any different at this time under Millikan? Millikan was still head of the Institute, but he was getting quite a bit older.

Richter: That created a situation which is hardly necessary to describe. It had been expected and he had intended to retire earlier, and he did not do so on account of the war emergency. This resulted in the administration, in effect, getting into the hands of others of the staff. No criticism, but naturally you can't change personnel without changing details. And there are people who could tell you much more about what went on on the campus then, because as I indicated, in spite of all this, especially after I got out of OSRD, I was spending most of my time off the campus.

Scheid: What about students? There must have been a big drop in students then.

Richter; I don't think so. That is a matter of statistics. You can verify it. But, you see, we had these people in Officers' Training.

Scheid: So they were essentially in the military.

Richter: The big boom in students everywhere came after the war with the GI students. We did not have so high a proportion at Caltech as at other places.

Scheid: You mentioned that the caliber of the students was better after the war.

Richter: Sometime after the war, that is. By no means immediately after, just for the reason I have suggested. Not that the men who came in under the GI program weren't some of them very good men, especially fundamentally, but they were less well prepared, with the result that quite a number of them found they could not go into the advanced programs in which they were interested, unless they were willing to spend extra time in elementary preparatory courses, some of them even going back to the high school level outside of the Institute to get their mathematics. Right after the war, practically every other student wanted to go into nuclear physics, and of course there were only a limited number of opportunities, and even those were not too favorable.

Scheid: Then the admissions policies were pretty free after the war? People were let in who maybe weren't quite well qualified or hadn't had the preparation, as you said. There were no entrance examinations?

Richter: Not to the same extent. I don't think I can date this exactly, but the office can. There arose a very serious problem which was discussed in special faculty meetings and elsewhere. We were getting far too high a mortality at the end of the freshman year. Proportions were higher, and statistics were unfavorably compared with those of other institutions. And what came out of that was the interview program which still persists. People would go out all over the country and talk to prospective candidates and encourage those whose preparations or personalities seemed likely to do well at Caltech and discourage the opposite. And the effect was noticeable almost immediately. It was good.

Scheid: Beforehand, you admitted people essentially according to their grades, and that was the only thing that you had to go on?

Richter: Very largely. Grades and personal references. But those things are not always satisfactory. The extreme cases of this kind often come with foreign students where we would have their academic records which we really didn't know how to evaluate in terms of our own standards, and references which in brief read, "This is my student, and I think he is wonderful."

Scheid: You were quite involved in the admissions processes, weren't you?

Richter: Later on, not by choice, I served a certain number of years on the Geology Division Committee, which was passing on admissions and also evaluating students already present and deciding on their admission to candidacy for advanced degrees, and so forth. So that I was rather regularly involved in that for quite a while, and consequently I was acquainted with the procedures in our division which in general were pretty much the same as elsewhere in the Institute, though we did have some specific differences which were due to the special character of the subject.

Scheid: Did you begin to get graduate students, too, who were interested in seismology more in an academic way?

Richter: Yes, we got some. The majority of them were graduate students who were actually students in geology and would probably write a thesis in geology, but nevertheless had an interest in seismology, furthered of course by this development on the practical side of geophysics and the prospect of being able to use that experience in the oil companies, for example.

Scheid: Was Caltech the main place in the country to study seismology at that time, or did you have competitors for students?

Richter: Berkeley--very little shade of distinction there. For a number of years, we were ahead of them because we had more and more expensive equipment. Eventually, they found means to fund a larger program, so that at present I would say that there isn't very much shade of difference. Then, St. Louis, which as I mentioned was Father Macelwane's headquarters for the Jesuit Seismological Association. They trained quite a number of men with considerable ability, who contributed to seismology, and some of them are still with us and turning out fine work. Later on, not at first, MIT, which developed rather rapidly in geophysics, especially under Ewing and Press. Of course, Press finally came to us and was in charge of our Laboratory for a while. There is no other one place I would mention on the same level, though there are quite a number of institutions where people have gone and done good work in seismology and geophysics. Then there are special cases like Colorado School of Mines, where they operated a more or less special program in geophysics or geophysical engineering which was very decidedly tailored to those students who were going into commercial work, either for the oil companies or geophysical work with minerals, usually beginning their employment as a party chief and if they were good, working up through the company organizations. No criticism of the Colorado School, they were doing a good job in that field. But sometimes we would get men who had gone there for a limited time, and then came to us, and they found they had a great deal to make up before they could work at the Caltech level.

Scheid: These were people who came in as graduate students?

Richter: Mostly. I think also there were a few who changed during their undergraduate periods, which probably made the transition easier for them. You could verify this from Dr. Allen directly, but I hate to mention him in the same breath as Colorado School; I didn't mean to. Anyway, Clarence Allen originally went to Reed College, which is one of those excellent small institutions which doesn't attempt more than it can do and does very well. So he graduated there in physics. He had no good background in geology, but it occurred to him, quite rightly, that the geophysical field was relatively new and developing fast, he wanted to get into it. So he took a summer course in geology, if I am not mistaken, at the University of Colorado, and from there he came to us as a graduate student in the geology division, with excellent results.

Scheid: So in other words, one doesn't have to have gone through Caltech from the beginning?

Richter: No, very far from it. After all, look where I started.

Scheid: Reed has a formidable reputation in turning out scientists.

Richter: Well, it is understandable. It's certainly one of the best-run places--I remember distinctly that Clarence Allen came up for discussion at an Admissions Committee meeting, the matter of Reed College came up, and it so happened that the rest of the committee were not particularly acquainted with the reputation. Not that they wouldn't have found out on checking, but I was able to speak up. I had had some previous contact, I forget now what, but anyway, I said, "You'll find it's a very fine small institution. He's probably thoroughly well prepared."

Scheid: I wanted to get into something about the donors to seismology. Once you got into Caltech you had to have some money from donors. The Mudd family is the obvious one . . . .

Richter: You'll have to go back before that to the Balches. They donated a very considerable amount to the Institute. They provided the Athenaeum, and also they did give money specifically to this field, so that for a number of years we were publishing and heading everything as the Balch Graduate School of the Geological Sciences. This was kept up, along with a certain number of social activities, in the hope that they would leave more, which they didn't. It went to USC, unless I am mistaken.

[Interview interrupted]

Scheid: You mentioned certain social activities with a sort of a smile.

Richter: This was about the time when a great deal was very formal in the way of dinners, etcetera. It was customary at the Institute that one had to put on full dress so that the ladies could show off their evening gowns. This was very unpleasant and contrary to my feelings, so I was relieved when it finally petered out.

Scheid: Maybe your wife enjoyed it though?

Richter: Not particularly.

Scheid: It wasn't your type of thing?

Richter: No, it certainly wasn't.

Scheid: You did mention the Mudds. They certainly began to give money.

Richter: Yes; now, that is later, when I was not very much in touch anymore with what was going on at the campus, so that background you would have to get from someone else.

Scheid: You just met the Balches on these social occasions, rather stiff social occasions. You didn't really know them in any way?

Richter: It was a little more than ordinary social occasions. About once a year, I think sometimes oftener, we organized a sort of report, so that there would be a huge banquet, and then the various members of the division would give short reports on what had been going on in the department, allegedly for the benefit of the Balches, because it was the Balch Graduate School. And all this effort wasted! Although if they got a kick out of it, they were certainly entitled to it, whether they contributed in future it didn't matter, they had already contributed so much that they were entitled to some return.

Scheid: Were you ever called upon to lecture for the Associates?

Richter: No, I don't recall ever talking for that group. I was occasionally dragged in to give one of the Monday Evening Lectures, but that was quite a separate arrangement.

Scheid: So your special field and special expertise weren't being used to raise funds particularly? I would think that people would have been interested in it.

Richter: A high point in that regard was in 1952, when the Kern County earthquake occurred, and the Laboratory was able to go into it quite extensively, and it resulted in a great deal of publicity, and yes, a special program was set up for the Associates. This was a larger occasion in which we all contributed. That was made a vehicle for furthering the program and getting more funds into it.

Scheid: This brings me to your public contacts, with the press, and so on. When did that start?

Richter: Rather early, because it often happened that I had to handle phone calls to the Laboratory when Wood wasn't there or wasn't available for some other reason, and got gradually accustomed to giving out reports to the press and dealing with them, and that went on rather regularly throughout my connection with the Laboratory.

Scheid: What was the attitude in those days toward giving out information?

Richter: No problem from the Institute side, provided that nothing was said or given out that might be needlessly sensational. But it was felt that otherwise giving out the information and the resultant minor publicity was good for the Institute. An earthquake would be reported somewhere, and the first information they would have about it would very often be a report from the Laboratory. And we had some competitors who would try to beat us to it.

Scheid: Who was that?

Richter: Berkeley occasionally, and one or two institutions in the East. This was, I must say, certainly friendly competition. We'd joke about it and very frequently we were following along behind the people at Fordham because they often had three hours' jump on us.

Scheid: It seems as though you did pretty well, because I remember always hearing that it was Caltech who got the information. As a member of the public, that was my impression.

Richter: Well, that would naturally be so around here, but I'm not sure just exactly what impression you might get on the other side of the country.

Scheid: What about the public responsibility? Did you feel a certain public responsibility to keep the public informed?

Richter: Yes, to a certain degree, particularly as misinformation was often siezed upon and twisted in a way that was contrary to the public interest. We were very much in favor of earthquake-resistant construction, and normal safety measures, and a consciousness of the general population as to the possibility of earthquakes and earthquake risk. This was from the very first. It was particularly accentuated by the circumstances of the Long Beach earthquake and by some of the wild, panicky rumors which got out at that time. We felt that this was, after all, only one side of the general responsibility of Caltech toward the public, namely, to give out correct information on any matter which might be of public concern. And this was a particularly critical one because it is subject to a great deal of honest misunderstanding as well as misrepresentation.

Scheid: Do you think things have changed in that regard?

Richter: Well, I don't know. Except for the World War II period, there has been a steady improvement in the quality of reporting and handling of scientific material of all types, and notably in my own field, in regard to the press. In the first years, I often had to deal with reporters who were not in the least interested in the facts, but only in getting something that would produce a headline, and would occasionally deliberately misquote me. Less and less of that sort of thing has happened, and now on the whole we are dealing with responsible reporting of events. This doesn't change the effect that when there is something spectacular or disastrous, then immediately the reports originate from unauthorized persons that are picked up by inexperienced reporters, so that quite a lot of nonsense still gets into print. But the general situation is very, very much improved.

Begin Tape 3, Side 2

Scheid: Something I wanted to get into was that your field, of all the sciences, seems to be bedeviled by cranks and crank predictions.

Richter: Well, it is perhaps a little more conspicuous in the earthquake field, but cranks we always have with us, and particularly if anything is getting a little extra publicity, they come out of the woodwork.

Scheid: But you didn't feel bound to refute them necessarily, just to give the facts.

Richter: So far as possible. There is one element of experience, and that is not to get involved in controversy in the columns of the newspapers with incompetent persons. It is best simply ignored.

Scheid: You mentioned that the students changed to a considerable degree after the war. What about the faculty here? There was quite a bit of growth, wasn't there?

Richter: Yes. I think we had a good deal of expansion. The major change since the war is relatively recent, and I regard it as a fundamental change in Institute policy, namely, to abandon the restriction to an institute of technology and expand into an inclusive university, particularly in the direction of the humanities, but elsewhere as well. That decision was reached after a great deal of careful consideration and discussion, but I cannot say that I personally like it.

Scheid: I don't think it has really happened, though, has it?

Richter: Well, it is certainly a matter of explicitly stated intention, and I think there have been changes in that direction. I believe there are more funds available for scholarships, fellowships, etcetera, in the humanities proportionately than were formerly the case. But there again, I am only stating my personal impression. It's hardly an important matter for this particular record. But you were suggesting changes in the Institute. Naturally, the bringing in of women is an important change, but it hasn't proved nearly as revolutionary as was hoped on one side or feared on the other. I don't think it has created much of a change, especially in regard to women students. We do have an unresolved and serious problem about the participation of women on the staff and faculty.

Scheid: But you haven't been actually overwhelmed with women students either, have you?

Richter: Well, that was scarcely to be expected. After all, we had some idea what to expect because, for example, MIT has never had any barrier. So we had their experience to go on, and they have never been swamped, and they have never had any really serious problems. Naturally, odd things would come up with individuals, but on the whole I think it has gone on there very well, and there is no reason why it shouldn't here.

Scheid: After the war, was there more connection with industry as far as the faculty was concerned?

Richter: I think that there is a trend in that direction which would be hard to evaluate. I associate it with the very considerable increase in the number of trustees, a large proportion of whom have been drawn from the industrial side. Quite a number of them are alumni of Caltech or equivalent institutes, they're not small-town tailors and hardware dealers; but naturally they are in business and industry, naturally they see matters from that point of view, and naturally it results in the Institute being encouraged in those directions which promise immediate practical applications. But I do not think it is excessive, because practically every high-placed corporation executive is well aware of the fact that some of the most productive research is that which is not at first obviously with practical applications. You go looking for practical results and maybe you get them. You go looking for what is there, you find much more valuable things that you didn't expect. That is the whole history of the matter, and I think that Caltech is operated more or less in that way, and what I just said would probably be assented to by most of the administration and trustees.

Scheid: You don't feel that these ties with business have diminished the fundamental research programs?

Richter: I don't think so. There is, of course, some effect on the degree of publicity and availability. If a large corporation funds a piece of research they want first break on the results. And this may theoretically delay making the results of the investigation generally available. And that of course, from the academic point of view, is not desirable.

Scheid: And that does happen?

Richter: I think it happens to a limited degree, and I think both the administration and their associates are aware of it and exert themselves to minimize the undesirable effects. Naturally, we are still getting a large amount of funding from government sources, and that, as you are aware, has all sorts of strings tied to it, and I don't need to beat that matter because it has been done very much in public.

Scheid: Does that same sort of thing happen in government research, then, that the results are held back?

Richter: Anything which the military can get interested in is apt to be classified and retained. But that has been less of a problem in late years. Immediately after the war, war psychology continued and there was a somewhat panicky attitude. That has decreased, but of course, still those problems of national security do come up, and we still have inclusive laws which could be applied to limit or suppress anything if it were deemed by authority to involve national security. With the kind of world we live in, I don't see how this could be helped.

Scheid: Were you personally, other than the war work, involved in any classified research?

Richter: In general, I avoided it. However, we were unavoidably continuously recording the atomic weapon testing, and consequently, we were often embarrassed by not feeling that we could give out information about what we were recording, particularly to the press. Gradually, that difficulty decreased until in recent years, when a large test is about to occur, it is practically always announced by the authorities in advance, so the fact that we have recorded it is not a very serious problem. But at first, there was considerable difficulty about the whole program, and some people, such as the late and lamented Professor Bullen, found means to get hold of published data, which related to an atomic test, and put them to very good use in investigating the interior of the earth.

Scheid: The use of your instruments for that has been an important factor in international relations in the last two decades.

Richter: Yes, indeed. There was a great deal of discussion and even controversy, because, of course, international negotiations came to hinge in large part on what the authorities believed was possible in the way of detecting tests.

Scheid: When you took data on an event, could you tell immediately whether it was a natural event or an artificial one?

Richter: Not invariably, and this indeed was one of the very critical points, particularly because there was often a dispute between those who insisted you always could, and those who were perfectly aware that you couldn't always.

Scheid: In other words, you couldn't release information because you sometimes didn't know right away whether it was a natural event or a test? Was that part of the problem?

Richter: Well, that might have occurred. What happened was when we were in the process of issuing regular bulletins, if we got one of those things, we would merely include it without a remark, unless the information had already been released through official channels. No very serious problem ever arose. We never got into anything that could cause trouble. We greatly distressed and angered a few offices by putting out some information which we were afraid might be suppressed and was of too much value to be lost, but that was maybe one or two cases. On the whole, no, it was a persistent annoyance, because we always had this hanging over our heads that we might suddenly be put under some undesirable kind of restriction. But it never actually eventuated, and now, of course, the general atmosphere is pretty much relaxed and there is no real problem of that kind.

Scheid: So you are still recording these events for the government?

Richter: Well, I'm afraid you'll have to ask the people here in the Laboratory what's happening in routine, but I think probably they're just being picked up regularly. Instruments are not prejudiced; they'll pick up a shot at the Nevada test site with complete indifference--whether it is that or a natural earthquake in Nevada doesn't matter at all. The seismograms get measured, and the data entered, and then it is perfectly obvious that these are test shots and they usually get so marked in the files.

Scheid: We were talking about government funding and the fact that you were involved in detecting atomic tests because of the nature of your instruments. I also wondered if you remembered the McCarthy era, if that made any impression here at Caltech. I haven't really heard much of that. I don't know if you were interviewed or if other people were interviewed?

Richter: We would occasionally get inquiries from official sources and sometimes an agent would appear locally when someone made application for a position which might involve classified material, then we would be asked what is this person's background, and do you think he is loyal to the government and so forth. I never felt that it was an extreme expression of McCarthyism because, especially in the fields we were dealing with, security provisions were almost unavoidable. It was only a question of how far they carried them. I do remember one former graduate student of Russian and Jewish background who got into some considerable trouble, and it took him a long time to disentangle himself, and there wasn't much anybody could do for him.

Scheid: Was that during the fifties?

Richter: Yes, I believe so. For some reason I am not clear about it.

Scheid: Was he suspected of something?

Richter: Apparently so. Apparently they thought he was acting as an agent or a spy. He might have been, but I don't think so.

Scheid: While he was a student here?

Richter: He had been, yes.

Scheid: And so you were asked about him.

Richter: Yes.

Scheid: I think those job interview investigations are fairly routine even now for many jobs, but I wondered if there was any pressure on any faculty members here at that time as there was in some other universities because of their political views?

Richter: I imagine a few people made some stir. Of course we have the conspicuous case of Linus Pauling. He was big enough so that they couldn't pull him down.

Scheid: Did someone come around and talk to you about him ever?

Richter: No.

Scheid: I guess they knew everything they needed to know.

Richter: Yes, probably so. Anyway, I got very few of these contacts myself. Others may have had more. It would probably depend on the particular field. Most of them were cases when the man had been at the

Laboratory or been a student in one of my classes and had given my name as a reference. There weren't too many of those.

Scheid: No one asked you about your contacts with Oppenheimer?

Richter: No.

Scheid: You didn't feel that there were any pressures here [at Caltech]?

Richter: Well, I simply didn't come in contact with it to that extent. This one case I mentioned, that I won't mention the name, was a pretty bad one. It was pretty severe, and was so felt by other people at the Institute. But in my experience, that is one single instance. Nothing else comparable ever came to my attention.

Scheid: Did this person manage to rehabilitate himself?

Richter: More or less. At least he is still around and not in jail.

Scheid: He is not at the Institute anymore, though?

Richter: No.

Scheid: In regard to the Pauling case, was there talk here of doing anything about that? Was the Institute concerned about it?

Richter: I think the Institute was no doubt concerned, especially about some of the things that Pauling said. I recall with considerable amusement the occasion when Pauling earned his second Nobel Prize, the Peace Prize. And there was a large banquet affair to celebrate, and I think it was DuBridge who was in the unenviable position of having to get up and say, "Now we are all very happy for Dr. Pauling in this connection, but we don't agree with him one hundred percent." I am paraphrasing; of course it was said much more gently than that, but that was the content.

Scheid: That brings me to DuBridge. Did you have much contact with him?

Richter: Not very much, but very pleasant, and I thought, on the whole, he was very good for the Institute, and his arrival was marked by an outstanding feat of generalship. He looked at the situation and decided, "Now what do we need; what are the priorities?" Priority number one was getting Tournament Park for the Institute. Otherwise, everything else would have been distorted. Priority number two was better salaries for the staff. So naturally this was appreciated.

Scheid: So there was a marked change between his regime and Millikan's?

Richter: Yes, because the previous administration had been more or less marking time until Millikan finally could actually let go of the reins, and they had to find somebody to generally pick it up. The kind of situation we are in right now.

Scheid: But Millikan stayed on though anyway, even after DuBridge came, didn't he?

Richter: Yes, he was here in an advisory capacity, and he had a title, and I think they called him chancellor. I don't remember clearly. Yes, there was something of that sort, but I don't think there was any problem, and I still feel that DuBridge was good for the Institute, and it certainly prospered after he came in.

Scheid: What were his particular talents do you feel?

Richter: He was a first-rate physicist.

Scheid: So he had sympathy for scientific fields?

Richter: Oh, of course.

Scheid: He also was interested in humanities, too, wasn't he?

Richter: Yes, I think that was personal. It was not in his professional background.

Scheid: And he was also very good at raising money?

Richter: Adequately, yes.

Scheid: After the war, as you note in that article you wrote, there was a taking up again of international cooperation. You went off to Japan for a year. Was that an important trip for you as far as your field was concerned?

Richter: Yes, I think I learned quite a bit from that.

Scheid: Did you find it interesting in other ways?

Richter: So much so that I am afraid I didn't devote as much time to research as I could. I found the country and the people and the social circumstances much too attractive.

Scheid: In what way? What did you particularly enjoy there?

Richter: That is hard to say. There is a great deal in Japan which is very beautiful, and the Japanese themselves have a strong sense of it and tend on the whole to preserve it. They have the Oriental tradition of courtesy which makes any contact with the older generation rather pleasant, whereas contact with the younger generation was apt to be less so, because they were more or less in revolt. We had a great many student and other disturbances going on in Tokyo at the time I was there.

Scheid: Yes, you mentioned that in your article. You felt that was very detrimental to the university.

Richter: Yes, it was precipitated by the coming up for renewal of the Mutual Security Treaty, and they were demonstrating against that.

Scheid: What about the seismologists that you knew there? Had you known them before? Do they tend to come here?

Richter: Dr. Tsuboi, to whom I was indebted for most of the invitations and for most of the arrangements, had been with us for the better part of a year, some time before the war, and again a few months even more immediately before it. I know Dr. Tsuboi as well as I ever expect to know any Japanese.

Scheid: It always remains on very formal terms, is that what you mean?

Richter: Well, hardly that, no. Of course, he could be dignified and formal when necessary. No, our relations were more or less informal. He was over here quite long enough to get adjusted. I remember one of my first assignments when he arrived with us for the first time was to take him to a shoe shop and get him to get some shoes which would fit. Somebody had wished some on him which he could hardly walk on.

Scheid: Was he here as a visiting scholar?

Richter: Yes.

Scheid: Was he the only Japanese here?

Richter: No, several others have been here mostly for shorter intervals. In fact, they have been here on and off, and some have been here and come back and stayed like Dr. Kanamori.

Scheid: Was there quite a large number of stations in Japan?

Richter: Yes, even before the war there were over 100. Of course, with equipment largely of older date, which is now no longer the case. They have been expanding their instrumental facilities considerably.

Scheid: You went out to see some of these stations?

Richter: Yes, to a few of them and to other points of interest in the country.

Scheid: Did you go to the parks?

Richter: One or two. One of the larger parks is actually within the city of Tokyo, which compares with Los Angeles in geographcial extent. It covers a lot of area, and yes, I used to get on the public transportation and run out there and get a walk in the hills.

Scheid: You had no trouble finding your way around?

Richter: Eventually, no, because I had good maps, and I got so that I could read the Japanese characters enough to read the names of stations, and that kind of thing, so I never got lost.

Scheid: And if you did, did you know a few words?

Richter: Yes, if necessary.

Scheid: Were there any other foreign visitors to Caltech other than the Japanese from important centers?

Richter: Yes, I remember the late Dr. Stoneley was here from Britain; that was some years ago. I remember having an inquiry from someone who was writing his memorial and wanted the information. And we had with us working at the Laboratory for over a year a very genial Indian by the name of Chakrabarty.

Scheid: And he went back?

Richter: Yes, he went back to find that the rug had been pulled out from under him in his absence. I don't know quite how he made out, I haven't heard from him now in a good many years. Let me see, oh, we had some brilliant Chinese students. There were two or three of them; they're back there now.

Scheid: Yes, that brings me to the Chinese progress in seismological research. Do you feel that all the press reports reflect important progress?

Richter: They have been expanding over a number of years and were doing very well, and they have this more or less political pressure on them to concentrate on prediction, which they managed to deliver on in some respects, but it is very difficult to evaluate the work in that direction. The men most involved are intelligent, highly competent and sincere, but they have this problem of keeping on the good side of the administration.

Scheid: Are some of these men that came to Caltech now involved in the Chinese earthquake program?

Richter: Oh, yes.

Charles F. Richter
Session 3
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Begin Tape 4, Side 1

Richter: I find the questions I'm most often asked have to do with the magnitude scale, or with prediction, or with the safety of tall buildings. Those are the most common. Now I have pretty well put on record the origins and nature of the magnitude scale. I don't think there is much necessity of going back over that. And naturally the safety of tall buildings is not particularly my specialty--I'm no engineer. But we did get very much concerned about the matter some years ago, as there were some very unfortunate occurrences abroad, and this was just at the time when the height limit on tall buildings in Los Angeles was removed, rather to my personal disgust because, well, I don't like tall buildings, a matter not having much to do with seismic risk or anything of that sort. But the fact about the height limit in Los Angeles, its background, doesn't seem to be generally known. It was not established as a precaution against earthquakes. The city fathers in Los Angeles years ago realized that they had rather narrow streets and they were averse to having tall buildings constructed overshadowing these narrow streets and producing a canyon effect like that very well known in New York, for example. They liked their city and their landscape much as it then was, and for this reason the height limit was imposed. It was only removed when strong pressures were brought to bear later, because the erection of tall buildings means ultimately larger return to the owner for a given piece of property. So it is purely a commercial matter in Los Angeles. The shooting up of tall buildings is no peculiarity to this particular city; it happens everywhere. At present, we can say that tall buildings, at least in this area, are being intelligently engineered, so that they probably will constitute no element of catastrophe in case of a large earthquake, though I would anticipate, in the case of a really large earthquake, a very considerable amount of expensive damage, interior as well as structural, and very difficult to compensate. But that is a matter for the engineers and the future building owners. Some points, which should have been obvious from the first, received proper attention only after the San Fernando earthquake. One of those is the safety of elevators and the associated installation, on which a great deal of work has been done, and particularly the latest of the tall buildings have been engineered with that in mind. But I used to say that buildings up to twenty stories were about as safe as any other, provided they met the normal standards of safe construction which would be applied anywhere, but that above that they constituted special problems and demanded first-class engineering to make them suitable for this situation. I think by and large those requirements have been met, with exceptions. I am told by those who know that perhaps a more serious situation exists in San Francisco than in Los Angeles, because of the rapid erection immediately after the last war of a number of buildings in the general twenty-story range which hardly conform to standards which were supposed to be enforced at that time, and many San Francisco engineers are really concerned about the fate of those structures in a potential future strong earthquake. Because of the nature of damage which I have been seeing since I first got into this field, I am very much aware of the danger constituted by the obsolete, old structures, mainly of commercial type, some in use as apartment houses, and the like, which exist not only in Los Angeles but in every business center in the state. Most of the loss of life in past earthquakes have been due to the failure of buildings in that class. The majority of them are brick masonry of poor quality--not quite all, but that is the chief source of trouble. And it is very difficult, even in the face of clear presentation of the facts and risks, to get any real action by the authorities in that direction. A great deal has been done in the direction of improving the safety of school buildings. That is not completely removed, but at least we have an improvement. But in the case of these older structures in private use, and some of them erected rather hastily in the boom period at the end of the last century, almost nothing whatever has been accomplished and they remain with us as a permanent source of danger.

Scheid: Can they be reinforced or do they have to be demolished?

Richter: I think there is a range of possibilities. In many instances, I am informed that reinforcement would be possible or at least constitute an improvement. But many of them have demonstrated so much weakness in comparatively moderate shaking that I doubt whether anything but demolishing and replacing would be in the true interest of public safety.

Scheid: So that is a good section of downtown Pasadena.

Richter: There are a number of such structures in Pasadena, yes. And there were quite a few which showed conspicuous damage in the San Fernando earthquake, which was not, after all, very strong in this locality. And in Los Angeles and the surrounding district there are thousands of such buildings. The city building department has put that fact on record repeatedly in various proceedings, and not very much has been accomplished in the direction of taking them out of use.

Scheid: Is it possible to determine which piece of property will be more dangerous than another?

Richter: Well, there is some variation due to ground, and even that gets to be controversial. And there is quite a difference in the type of construction, and some of that is pretty obvious on inspection. Some of the older brick structures were put up in haste with none of the ordinary precautions common to good masonry. That is fairly obvious on inspection. All of these matters have been gone over pretty well by the pertinent building departments, and the circumstances are quite well known and on record. But it is very, very difficult to get anything done about it.

Scheid: You mentioned in some talk or article, I'm not sure, that one of the chief dangers of high buildings was the concentration of people, the difficulty in evacuation.

Richter: That certainly adds to the risk to life and limb. In the Los Angeles area, the regulations under which the tall buildings are constructed are geared to a limitation on the degree of occupation of the structures. So that this is one reason why a number of the larger structures have been located in outlying areas where they could be placed in the center of relatively open space. So that this means consequently that, not only in case of earthquake but also other emergencies, we don't have the phenomena of crowds of people trying to get out.

Scheid: In the earthquake in Rumania it was the newer buildings that were most damaged. Does that indicate that they didn't have such careful building codes?

Richter: I don't have the details in mind. Apparently the worst damage in that particular event was in a comparatively small area in Bucharest where there had been construction going on since the last big earthquake in 1940. It was notable in the very destructive earthquake in Guatemala that the buildings that had been put up after the previous destructive shock in the area survived comparatively well. The greatest damage and loss of life was due to old construction, much of it the unstable native type of construction and not modern building at all. But very often the engineers and other experts go out and look at the results of this damage and this or that earthquake and come back saying, "Well, we haven't learned anything new." But in the improved engineering standards, at least in this area, I think definite progress can be marked.

Scheid: Maybe we should go into the subject of prediction, on which you have voiced your opinion, I think, several times.

Richter: It is a critical matter and hard to handle because the subject is so open to misunderstanding, misrepresentation, and downright fakery--always has been. There is a great deal of serious and respectable work going on now which is at least nominally directed toward the prediction of earthquakes. And whether it does result in anticipation of a serious event by a useful interval of time, whether it does that or not, is not so much important to me as the fact that we are getting more and more accurate information as to the occurrence of earthquakes now and in the immediate past, and the location and characteristics of active faults. All of these matters are being investigated on a larger scale more systematically than ever before. And we get some elements into the investigation and some enthusiastic propaganda which can be well spared. I used to be particularly unfavorable to discussion of prediction because it tended to divert attention, discussion, funding and personnel away from fields in which a great deal was to be obtained by careful study, and we did find out in those years a great deal more about the geography and nature of earthquakes, and not by going out and trying to find a means of prediction. But in consequence largely of what was done thirty or forty years ago, we have arrived at a better understanding of the nature of earthquakes and of the tectonics of the earth on a large scale, so perhaps now it is a little more appropriate to try to investigate the details and see if we cannot usefully anticipate future events. The occurrence of a few apparently successful predictions which are to be attributed in part to coincidence or plain luck have a tendency to pass over the cases when predictions have not been successful, and easily leads to a strictly unscientific approach to the subject, so this is not desirable.

Scheid: Do you think that the new sources of information are going to be helpful in prediction, other than the standard seismograph?

Richter: We get new and significant information occasionally in rather unexpected ways, such as the recent work done by Dr. Kerry Sieh on the San Andreas fault where he has uncovered datable evidence of past large earthquakes on the fault back over a period of centuries, and this gives us a much better perspective on the processes which are going on and also on the probabilities affecting our ideas of events in the immediate future. So here is decidedly new information. Earthquakes keep on happening, and the recording equipment is more or less regularly improved and expanded so that we have better and better registration of the events which are taking place under our hands, and this naturally improves our evaluation of attempted prediction and tends to at least prevent some of the most common misapprehensions from getting too well into circulation. There are two natural and important types of misapprehension which have been with us more or less ever since; they were certainly current when I first came into the subject. One is the idea that from the registration of small events we can identify the active faults and get some idea of the activity and perhaps in that way arrive at prediction of larger events. That is a hope that has been held out at the beginning of every seismological program, including the one in Southern California. It has not been well realized, because actually the occurrence of the smaller events in general seems to have no very close correlation with the larger processes, and consequently with the occurrence of larger earthquakes. And in particular, the larger earthquakes occur on the major large faults, the small earthquakes occur about everywhere, but particularly on the smaller faults, so that if we start generalizing too rapidly from the small events to the large events, we get a distorted picture. The other chief source of misapprehension is the very natural idea that of course a large earthquake is the culmination of a process which has been a long time in preparation, so that there is the notion of recording and observing the buildup of strain toward the fracture which takes place in the earthquakes. This is a very nice idea, but it so happens that, in the majority of cases, the significant events occur with little or no obvious immediately preceding buildup. There is a small percentage of occurrences, and some of them are important, where there is a buildup of that kind, and one of those occurred in China in 1975, which led to the very famous and celebrated Chinese prediction. They did have an instance in which there were a great many preliminary signs that strain was building up, and they were able to follow it, and finally issued a warning which proved to be in time. But in the following year, they had an even worse event and an outstanding catastrophe, for which they had little, if any, prior indication, for which no warning was issued. And the Chinese have been going on predicting earthquakes for quite a while and claim various successes since. But they never have given any systematic reports on their numerous failures of both kinds, namely, predicting events which didn't happen and failing to predict those which did. In this respect, their practice has been no better than that of certain amateurs and downright cranks, who have been coming out now and then. There is one of them that comes out of the woodwork every few years, with the claim to have predicted in the past, which usually can't be substantiated, and a loudly publicized prediction for the future, which usually doesn't occur either, but is distorted to include whatever may chance to happen at about the indicated time. Well, it is rather astonishing how regular this pattern of self-deceiving misrepresentation recurs, and once in a while the news media get hold of someone like that and give him a tremendous amount of undeserved publicity, which results in wasting the time of a great many people who are trying to do serious work in the field.

Scheid: You mentioned that the Chinese saw some preliminary signs in the buildup of strain. What were those and how did they detect them?

Richter: First there were preliminary earthquakes and there were such things as changes in water in wells. There were cracks appearing in the ground, and there was evidence of uneasiness on the part of domestic animals which one would attribute naturally to reaction to small earthquakes and vibrations in the ground, which were perhaps more disturbing to animals than people, but of course represents no kind of mysterious foreknowledge by the animals that an earthquake was about to take place. I emphasize that because this kind of thing crops up from time to time, and it is fundamentally unscientific.

Scheid: You don't feel that there is any way to try to determine what is affecting the animals?

Richter: There is, and in a relatively undeveloped region like China--undeveloped in the modern sense--it makes sense to ask all the farmers to report uneasiness in their animals, changes in the level of wells, and so forth. One gets in this way a great body of information, individually of secondary value only, but possibly usable for a large synthesis where so many observations are involved. But in a country like this one, where we are able to install sensitive instruments which are far more reliable and far more delicate than any such crude indications may be, it is a disservice to science to suggest that we should go out and watch the behavior of cockroaches. Not all of the evidences for disturbance of animals are attributable directly to things going on which might have been observed by ordinary seismographs. There were some observations recently which indicated, for example, there were electrostatic changes before earthquakes in central

California particularly. And it is understandable that those might affect the behavior of animals, so these things are not utterly nonsensical, but they are rather a subject for study on the part of the biologist than a contribution to our means of forecasting earthquakes.

Scheid: The Russians and Chinese have measured radon gas in wells, and they also used tiltmeters, I believe. Is that something that you feel might be useful?

Richter: The radon gas evidence is difficult to evaluate. There seem to be well-established incidences of it, but like the occurrence of preliminary earthquakes, it's unreliable. The tilting of the ground, if carefully observed and not due to non-seismic causes, is of course perfectly valuable information; essentially, it's the kind of data we are getting here in connection with the so-called Palmdale bulge. So such observations are entirely legitimate for instrumental purposes, directed toward forecasting earthquakes. Obviously, we get first the installation of seismographs, then we get local strainmeters and tiltmeters, which are a form of determining the kind of strain going on. Then very probably gravity measurements, and then observations of magnetism and electrostatic conditions, the bearing of which we aren't sure. Now these are legitimate, geophysical methods of trying to investigate a geophysical process, but trying to use impressions of animals or people on the level with accurate work of these kinds is only confusing the issue.

Scheid: Do you feel that the installation of many more instruments would be a great help in this?

Richter: I don't see how it could fail to be, and indeed such installations are underway in critical areas. There has been, I think, in the past, with the situation now improving, a little too much concentration of effort along the San Andreas fault, which is the main active feature of the region, and the one which from the point of view of public interest and risk is the most important, but we know definitely that we have periods of considerable relative quiet along the San Andreas fault, and therefore there is always the question of how much we can find out at this juncture by geophysical observation. We have other active faults scattered over an entire area; they are coming more and more under careful observation, and in this way probably we will get a more inclusive picture of what is really going on in the area, which may or may not lead us to some definite conclusions before the occurrence of the large event which is obviously in preparation, as everyone has known for fifty years.

Scheid: On the San Andreas fault you mean?

Richter: Yes.

Scheid: How do you feel about the role of amateurs in reading instruments? Do you think that would be a fruitful thing to do?

Richter: If you are going to have a very large increase in the number of installations, then you are going to have a considerable number of persons operating who are in the amateur class, and if they are properly instructed and capable enough, while the percentage of error is higher than in the case of interpretation by a specialist, it's not that high, and organizations of amateur observers, if well directed, have been useful in other fields. Amateur astronomy is a very live and productive field and always has been, so why not amateur seismology? But of course in the past we have been plagued by individual enthusiastic amateurs, who rejoiced in giving out misinformation to the press. One or two of them were persistent headaches.

Scheid: The reason I brought that up is that I noticed in the paper the other day that they were thinking of asking the Girl Scouts to do it. It didn't mention the Boy Scouts.

Richter: That is a little . . . Maybe because they had other jobs in mind for them. That is a bit on the ridiculous side.

Scheid: At Caltech there was a prediction about a year ago. That seems to be a definite break with Caltech policy. Do you know the background on that, how it came about?

Richter: You are referring to Dr. Whitcomb's prediction? That was definitely in line with what was going on at that time. The original break was made by the Russians, who had come out with observations of considerable changes in the speed of seismic waves in areas in which moderate-to-large earthquakes followed, and they felt they had it systematized to the point that they could estimate not only the magnitude of the expectable event but its approximate time. There was apparent confirmation of the methods and observation relating to a rather small earthquake in, of all places, central New York State. Naturally there was interest here, and Dr. Whitcomb collected and correlated the data to see whether any progressive changes in velocity of that kind could be found. The first result of the kind was out to the east of this area here in the vicinity of Yucaipa which isn't far from the San Andreas or San Jacinto fault, and it was expected that there would be a moderate-sized event, magnitude 5 or so, in that area. Well, actually there was eventually a small earthquake in the indicated area, but there have been so many small occurrences in that same area since the program was set up, over fifty years, that it was hard to say that this was seriously indicative of anything. But then, more or less coincidentally, about the time the furor about the Palmdale bulge started, and there was no connection other than coincidence, Dr. Whitcomb did find evidences of progressive changes of wave speed in this area northwest of us, in that direction, and an earthquake of a magnitude of 5 or possibly larger was indicated as due within the following year or so. About the same time that the prediction came out there was a smaller event, a magnitude about 4.7 in the area. But this was smaller and sooner than expected, and yet it is entirely possible that this had a real connection with the observations which led to the prediction. So that the procedure is by no means discredited; it only means that we do not know in detail just when and in what way to apply it.

Scheid: There has also been some attempt at prediction using the swarm earthquake phenomena. Do you know about that at Caltech?

Richter: I think that is perhaps a matter of a use of terms. Are you talking about some of Dr. Kanamori's recent results?

Scheid: I was thinking of Karen McNally, but I am not sure. Maybe Dr. Kanamori has . . . .

Richter: It is essentially this same idea, that if we do find an unexpected increase in small events in a given active area, there is some reason for interpreting them as preliminaries to large events. The difficulty about that is that it is by no means invariable, and quite frequently we do have important occurrences prior to which there has been no detectable prelude. So that I would say that this is a very interesting subject, but it needs a great deal more investigation. Perhaps in the course of time, with more geophysical data, we may be able to distinguish between those cases in which the occurrence of an earthquake swarm is indicative of something to follow, and those in which it seems to be an event which runs its course. There has been, of course, practically no earthquake of any consequence in the area in my experience, with one exception I could mention, which was not preceded by the recording of smaller events in the same immediate vicinity. The exception I was thinking of was the Manix earthquake of 1947 out in the Mojave desert, and we cannot there say anything definite about the possible occurrence of very small events before that. There would be a lower limit to what we could have detected, which lower limit certainly wasn't exceeded in the preceding twenty years of observation. This is a minor example of what I did mention as an area of misapprehension. The idea being almost an obstinate conviction that there must be some detectable prelude to a considerable event. This may be a . . .

Begin Tape 4, Side 2

Scheid: This theory of Wood's was that there was a very short time before . . . ?

Richter: In Hawaii, they have been accustomed for many years to forecast eruptive outbreaks by the occurrence of swarms of small volcanic earthquakes which often follow a more or less predictable course in increasing in number, size, and depth below the surface. But they have had one or two instances in which the whole detectable process started suddenly and built up toward eruption in a very few hours, and this is the same sort of problem I have in view as possibly an obstacle to the use of swarms of earthquake events to forecast a larger one. Moreover, there are of course numerous cases of evidence of swarms or at least numerous sequences of earthquakes in a given area which are not followed by anything of great consequence. In the Imperial Valley, particularly in that region, swarm earthquakes with or without the association of a larger individual event have been the rule rather than the exception ever since we started working on it.

Scheid: So maybe each fault has its own pattern?

Richter: That may well be the case, and that is one of the things on which the increasing accumulation of data should shed some light. One of the questions which has arisen and is still not completely resolved is the relation of the creep events to the larger fracturing events along the same fault or on different parts of the fault. In particular, where there is creep going on, there are often also various frequent small earthquakes sometimes classifiable as swarms. This has now been pretty well established by the amount of detailed observation which has been going on on the central stretch of the San Andreas fault since the occurrence of the Parkfield earthquake. For years--in fact, I would say, at least since the occurrence of the 1906 event--we've had this picture of dividing the San Andreas fault into three principal segments--the central one characterized by the relatively frequent occurrences of small-to-moderate earthquakes, and, as we now know, the occurrence of creep; and on the two sides of that, the segments which accompanied the great earthquakes of 1857 and 1906, where apparently comparative quiet exists between larger events, so that those parts of the faults are, as is usual to say, locked until finally the locking is released, and the major event breaks through the structure. Now, it would be very nice to say we have then certain faults or parts of faults which are characterized by creep and swarm earthquakes and others which are characterized mainly by large events with a minimum of smaller ones, but we have instances such as that of the Hayward fault in which both types of events are known to have taken place, in different epochs to be sure, but apparently then the separation is not purely geographical, and we don't know at present exactly what to make of it. The Menlo Park group have been doing a great deal of work detecting and following the consequences of the creep on all the faults in central California.

Scheid: It seems that some very small things can set off an event. I am thinking of the water that was put down into the earth in Colorado that they believe in retrospect set off small earthquakes.

Richter: The first good evidence of this kind came from the earthquakes which were observed in the Lake Mead area after they began to fill the reservoir, and those were later shown with pretty fair definiteness to have a correlation not so much with the level of the water in the reservoir but with rapid changes in the level that would alter the strain conditions. Then the instance you have in mind occurred when the Armed Forces decided to dispose of liquid waste into a very deep well in the vicinity of Denver. That triggered earthquakes, some of which were strong enough to cause light damage in Denver itself, and which were shown to show a fairly definite relation to the pumping or lack of pumping with a certain understandable time lag. After which, consequently, the pumping was discontinued. It is commonly stated that the earthquakes then ceased. That isn't precisely so. Something had been started which had to stop itself, and these events continued for a period of months, among them one of the largest of the whole series. I believe that has now diminshed, but at least it practically demonstrated the possibility of the artificial triggering of earthquakes, and in addition to the Lake Mead instance, there are three other well-known instances of occurrence of earthquakes associated with reservoirs which indicated triggering--instances in India, in Greece, and in East Africa. Then there was some experimental work. The Ridgeley Oil Field in Colorado in which the injection of water was shown to show correlation with small, superficial earthquakes, so that they could apparently trigger them and not trigger them as they chose. Now this is all very interesting and has led legitimately on the one side to considerable concern of possible effects of reservoirs in producing risk of this kind, and indeed the point has been raised and is still not finally settled. The Oroville earthquakes of 1975 of course occurred in the vicinity of one of the largest reservoirs in the area. But the correlation isn't completely established and the main effect has been to establish the existence of potentially active faults in the foothill region of the Sierras, something which had only been suspected previously and which has had a very critical bearing on the late discussion about the safety of the projected Auburn Dam.

Anyway, this problem of triggering of earthquakes, relatively shallow usually in connection with the reservoirs, is an important matter, especially from the point of view of public safety, but there has been another development which I feel is a little out on the fringe of the scientific--the idea of using or pumping water or similar means to try to relieve the strain along a major fault and so perhaps postpone the occurrence of an earthquake, and this has been made the basis of a great deal of propaganda, I should say. It has been much publicized far beyond its actual merits. And certain groups have been pushing for experimental work of this kind along the active faults. And all I can say is I don't envy them if they should be conducting such an experiment, and we should get the major earthquake.

Scheid: Quite a bit of liability there.

Richter: Yes. I think some people would be well advised to leave the country in a hurry in such circumstances.

Scheid: Is it understood how these reservoirs affect the ground? Is there assumed to be weight or seepage or both?

Richter: My feeling is in the case of the reservoirs it is primarily a matter of weight, and in particular in the variations of weight. As I pointed out, in the first good instance, Lake Mead, there was shown to be distinct correlation with the changes in level--not with the actual stand of the water, but the rate at which it was varying. There is a possibility of effect due to seepage and that point has been raised, for example, in connection with the Oroville earthquake, where the center of the event was very decidedly not under the reservoir. And this would apply to the explanation of the Denver earthquake. So these are possibilities.

Scheid: So it may be two things at work here, really.

Richter: Or maybe three or four.

Scheid: You mentioned Dr. Kanamori's recent studies.

Richter: I don't want to be speaking for Dr. Kanamori, who is amply well able to speak for himself, but he has had two very interesting groups of results of this kind. One, the occurrence of quite a series of small earthquake events in the area of what we are now calling the Palmdale bulge. And the other, the finding on the records, where apparently it had been overlooked, of a series of small events preceding the San Fernando earthquake in the area of its epicenter. So that that latter has been of particular interest to me, because the main San Fernando earthquake was one of those in which there was definitely no immediate foreshock, not down to a very low level of recording. It simply came altogether without warning at the time. Now the fact that these prior small shocks have been shown to have occurred in the vicinity is interesting. It has to be pointed out, however, that we do get events of that size peppered almost uniformly over the entire area, so that as we discovered, to our disappointment, very early in the program, the occurrence of small events shows very little relation to active faults or to the continuing seismicity--with one very conspicuous exception which I always have to name in that connection, and that is the San Jacinto fault, which has been, throughout its known length, a frequent source of earthquakes of all sizes down to the very smallest and running up occasionally to the lower level of major events. So the San Jacinto fault in that sense has behaved more in the way that a naive or inexperienced investigator would expect to find in approaching it, but in this area, at least, it is the exception and not the rule.

Scheid: What about the study of faults in other areas of the world?

Richter: We have done the very best we can. There are few areas in the world where we have an instrumental program which is comparable, and in some of them there is a further complication of one sort or another. For example, there is a great volume of seismological data in Japan, has been for many years. Also there the local tectonics are of an extremely complicated nature, and I would say that even now, with the advances provided by the plate tectonics and the interpretation associated with it, the picture in Japan is still very far from being elucidated. So that there is not much likely to be found in Japan that can be applied without further consideration to other areas such as our own. The overall activity is very much higher than ours. Many years ago it was established, and I think it still holds up, that the annual number of earthquakes perceptible to persons in the city of Tokyo is about fifty, compared with something like three or four in an ordinary year in Los Angeles.

So there is a higher level of activity and a greater degree of complexity due to the geographical intersection of different structures and the extension of the Pacific structure down to great depth in the same area. Now there is a very favorable area for investigation in the Soviet Union, and particularly on its southern border in the region of Tajikistan and its common border with India, and particularly the Garm district which is characterized by a large number of frequent, small earthquakes and which the Russian group have provided with an extremely numerous and well-instrumented network. It was out of that area that this first new suggestion toward prediction in the sense of detecting change in seismic waves came. And also in investigating instrumental records and historical information in the Soviet Union so far as we could, the conclusion was also reached that one can identify faults or areas which are more or less continuously active and others in which there are long periods of quiet interrupted by major events, aftershocks that subside and then another major event, so that to that extent that area tends to confirm pretty well what we have. New Zealand, which is a country I am very fond of, has also supplied a very interesting parallel study to California. There are many faults and structures in the New Zealand region, particularly in the South Island, comparable with those in California and in particular with a large element of strike-slips which is characteristic of our California faults. On the other hand, in the north, the New Zealand region impinges on a typical Pacific arc structure, and there what one finds is different from anything we find in California, including a much higher level of volcanic activity. So the comparison of the two regions is extremely interesting and instructive and has been a matter of exciting interest to the people of both groups. There is a very live and effective seismological group in New Zealand.

Scheid: Has there ever been any consideration at Caltech of going into lesser-developed countries and setting up instruments there? Was that ever considered?

Richter: It has been considered at times, but Caltech itself has generally not initiated that because that is a program which to some extent was going on when the Coast and Geodetic Survey was running the national program, and is being supported to some degree still through the Geological Survey. It is something that comes up more or less at practically every international conference. Generally, what has been accomplished has been the provision of funds to install local networks, either temporarily or permanently, and we get reports of such work from time to time. Also the Carnegie Institution has carried out some expeditionary work of that sort, particularly in South America. So it's not a matter which is overlooked at all, but it is not on a very widely systematized scale.

Scheid: Has there been any attempt to involve UNESCO or such an organization?

Richter: UNESCO has some tie-in with the International Seismological Association, and so some approaches and contacts have been made through there and particularly some publications have been financed through UNESCO.

Scheid: I wondered what you thought about Dr. Allen's remarks of a few days ago in which he said prediction in ten years.

Richter: I'll wait until I can talk to him about that. In the first place, I want to know what he said. I don't trust the newspapers.

Scheid: Well, of course that made a big headline. Also I was going to go back for a minute to the Palmdale bulge, which was detected by surveyors and not by any seismologists. Was there no watching of that area?

Richter: Well, that was the original evidence, and it was turned up quite some time ago, and there were efforts made to have that matter followed up and further investigated. There were problems of funding and problems of inertia, and this was the time when the Coast and Geodetic Survey was getting involved under the general administration of NOAA, and matters were in a confused and not too well organized state for quite a number of years. Then in the 1960s there commenced to be the push toward take-over by the Geological Survey, and that naturally made it still more difficult to get anything organized and carried out. Some of this is guesswork, but politics are the same whatever bureaus you are dealing with.

Scheid: That is something I would like to get clear actually. You said the Coast and Geodetic Survey was in charge of all the earthquake or seismology recordings?

Richter: All right, I had better set that forth, so there is no doubt about the main outline. At the time of the 1906 earthquake, the only government bureau which was authorized to do anything with or about earthquakes was the Weather Bureau. Weather observers were instructed to report anything unusual including earthquakes, and indeed there were quite a number of useful reports on earthquakes to be found in the reports collected and issued by the Weather Bureau. The Geological Survey, up to that time, had shown no considerable interest in earthquakes, although there had been some significant reports issued, notably the papers dealing with the New Madrid earthquakes and the Charleston earthquake, which are still more or less classics of seismology. But in general, the Geological Survey was operating like geological organizations in most other countries, paying comparatively little attention to the study of earthquakes as such. They would study such things as faulting where it affected the stratigraphy of an area which was being studied, and a little attention would be given to evidence of fault activity, but nothing satisfactory or systematic from the seismological point of view, and the United States Geological Survey was not participating in any earthquake recording program.

After the earthquake of 1906, there was a feeling on the part of people like Lawson and Reid that it would be a good idea to detach this thing from the Weather Bureau and get it into more expert hands. The Geological Survey was considered, but apparently they were not interested. On the other hand, it was possible to interest the Coast and Geodetic Survey, because they had been faced with the fact that their monuments and surveys were affected by the displacements in the 1906 earthquake, and, indeed, they had some of the most important data available for interpretation, which were a large part of the foundation for Reid's theories. Very competent people, like Dr. Bowie, who were in the Coast and Geodetic Survey were interested, so that finally proper political strings were pulled, and action was put through Congress which officially transferred the responsibility for earthquakes from the Weather Bureau to the Coast and Geodetic Survey, where it remained up until about 1965. And quite a lot of work on earthquakes was then done by the staff and under the auspices of the Coast and Geodetic Survey. Among other things, seismological stations were set up, operated, and maintained, and their recordings reported and digested. Reports of damaging earthquakes were collected and published in a series of reports on United States earthquakes, and more general historical documents were collected, among them the publications by Harry Wood and Maxwell Allen on the destructive and near-destructive earthquakes in the California and Nevada region. And very important for scientific purposes was the setting up of the program of strong motion stations of recordings. It got started in the early 1930s--just in time, very fortunately, for some of the earlier records refer to the Long Beach earthquake. Those seismograms are still being studied. The strong motion program was actually a revival of the original purpose for which the true seismograph was invented, because the first real seismographs were set up by the British group in Japan of the 1880s. They wanted to know in detail what was going on for engineering and constructional purposes. It happened that within a few years it was discovered that seismographs were capable of recording earthquakes all over the world, and that focussed the interest of seismologists on this fascinating new field which had a tremendous bearing on our understanding of the earth and its past history. And I would say it culminated in the formulation of plate tectonics and what people have called a revolution in geology, all of which took some fifty-odd years.

And in that general time interval, then, there was less attention focussed on what came to be called strong motion recording, and it actually was revived by the Coast and Geodetic Survey on the encouragement of seismologists, engineers, and to some extent insurance people. This was one of the most important contributions of the Coast and Geodetic Survey. Then, naturally, under the survey there went on the continuous work of triangulation and levelling and the consequent detection of ongoing motions in the earth's crust and this was all part of the Coast and Geodetic Survey program. In the meantime, the Geological Survey was doing nothing in these general directions. There was no reason for overlap; it merely meant that the Geological Survey was concentrating on its other previous interests. And all this persisted more or less in the same way with ups and downs connected with changes of personnel. There was one chief in the Coast and Geodetic Survey for a number of years who was tremendously interested in magnetism, with practically no interest at all in earthquakes, except in publishing some of his own wrong ideas about them.

The next major change in the picture came following the Alaskan earthquake of 1964, when there commenced to be tremendous clamour for earthquake prediction. And this, I regret to state, was in part aided and abetted by some of my own colleagues.

Scheid: At Caltech you mean, or elsewhere?

Richter: Yes, [at Caltech]. Anyway, an ambitious program for a systematic attack on the problem of prediction was proposed and a large budget was suggested for it, none of which ever got funded at that time. But less ambitious possibilities of funding began to be available. The lines for research laid out in the committee report were taken up and carried on either completely independently, as by Caltech, or by other institutions, who got grants to work on specific problems from the National Science Foundation or other sources, so that the effort in planning was by no means wasted. But after a while it became apparent that this was going on enough so that large funds were going to be available. And then suddenly the Geological Survey got interested, put in projects which would call for funding, and finally succeeded in a very complicated way in practically elbowing the Coast and Geodetic Survey out of the picture so that at present the Geological Survey is the main steering source and continuing to some degree the programs, including the strong motion program originally set up by the Coast and Geodetic Survey.

I've been only loosely in touch with the people most involved. There are people at Caltech who can give you much more detailed and accurate information, with all the appropriate gestures.

Scheid: This caused some disruption in the field. Do you feel that it wasn't particularly beneficial?

Richter: Naturally the expansion, the increase in the number of personnel, the amount of equipment applied, is all to the good, and some very good work has been done under the auspices of the Geological Survey. But there have been undesirable results, both from the older and the younger personnel in the Survey, because some of the geologists in or connected with the Survey with very well deserved high reputations in their field, now felt qualified to make very pontifical assertions about highly critical points in seismology, and in some cases that caused a great deal of unnecessary expenditure of public funds. On the other hand, the increase of personnel has brought in a number of bright young men without previous experience with earthquakes. Some of them have done quite a bit of work with artificial explosions and so forth in geophysical prospecting, but a good many of them had to learn their seismology from the ground up, and so when they get out in the field or the seismological laboratory, they tend to make some of the familiar mistakes which we thought we had got rid of. This is aggravated by certain policies which are of long standing in the Geological Survey, namely of publishing rather late but allowing material to get out as open file for study before it is finally revised. And some of these open file reports coming from relatively inexperienced persons have been, well, unfortunate.

Begin Tape 5, Side 1

Scheid: You mentioned that the USGS investigated the Charleston quake, and that it was one of the classics of seismology. Is that because they made such very good observations?

Richter: It is just one of the pioneer investigations of an earthquake on the ground. After all, seismology was fairly young in those days, and that report of Dutton's is a valuable, permanent document and still frequently referred to. It gains extra importance from the comparative infrequence of large earthquakes in the eastern United States.

Scheid: But his observations were very exhaustive?

Richter: Yes, they were practically everything that could be expected without instrumental aid. Such instruments that existed at that time were relatively crude.

Scheid: Another thing you mentioned was the role of the insurance people in instituting the study of strong motion. What exactly was their role?

Richter: I wish I could give you a little more detail on that, because it has been rather regularized, formulized, and I would hate to not give adequate credit to the insurance associations who maintained a considerable interest and occasionally contributed funds to the programs and had also sent out some of their own staff into the field to prepare reports which are of considerable value. This is an ongoing cooperation which rather improves with time. It dates back especially to about the period of the Santa Barbara earthquake. Actually, it was beginning to get underway before that occurrence, but was enormously accelerated by it, because the Santa Barbara earthquake was a great shock to the insurance industry by and large. Earthquake insurance had been written very extensively with little regard to the actuarial soundness, with the result that some of the companies suffered disproportionately great losses in claims following that comparatively moderate event. This scared them no end. The first reaction was panicky and was gradually overcome by a more rational approach to the problem, and by the influence of such people as John R. Freeman who had collected a lot of data on earthquakes and wrote a considerable volume on earthquake risk and earthquake insurance which was widely circulated. It contains some misinformation, but on the whole was a very productive piece of work.

Scheid: Did they have scientific personnel, then, that they hired, and instruments? The insurance companies, I mean.

Richter: I think on the instrumental side their contribution has always been in the direction of funding or otherwise supporting operations which were already going on.

[Interview interrupted]

Scheid: You were saying that they mainly supported the work with funding.

Richter: Yes. For one thing, their organizations took corporate memberships in the Seismological Society, which meant making funds available for investigation of earthquakes. On the whole it has been a pretty satisfactory setup, and I think the insurance industry has contributed significantly to the progress of seismology, sometimes to the alarm of public figures, because the insurance people have a financial interest in sound earthquake-resistant construction.

Scheid: And perhaps in prediction as well.

Richter: Yes. If there really were a very solid and established basis for prediction, you would find the insurance people backing it wholeheartedly. And I think their attitude toward the efforts that we're making at the present are favorable, but naturally it is a business proposition. They've got to see some definite promise, before they could justify anything on a large scale.

Scheid: So they don't have any scientific personnel to do their own investigating?

Richter: Well, yes. The people who have done the investigation fieldwork specifically for the insurance people have been engineers, mainly structural engineers, but many of them, like Steinbrugge, have been out in the field and observed the geological effects so frequently that they are better by far, probably, by this time on that subject than I am. Quite a number of important reports on the effect of given earthquakes has been published from the engineering side, and it is due to the studies published by engineers, and particularly those connected with the insurance organizations, that I first came to realize the enormous effect of type and soundness of construction on the damage and consequently the apparent effects of a given earthquake. I might say that that was one of the various motivations in setting up the magnitude scale, because it was very clear that we were getting earthquakes in some parts of the world which were rather alarming in the amount of damage and even loss of life, and yet simply weren't writing large records on the seismographs. In fact, that very circumstance over many years led to what really was overestimation of the degree of seismic activity in the Mediterranean and the Near East because of the prevalence in that part of the world of traditional types of construction which were very far from earthquake resistant. One of the latest very horrifying examples was the catastrophe in Morocco at Agadir in 1960, with a loss of 12,000 lives in an earthquake of a magnitude of something like 5.75.

Scheid: Did the insurance people ever give any funds directly to Caltech or to the Seismological Laboratory?

Richter: The insurance groups have memberships in the Earthquake Research Associates.

Scheid: Which is a special group within the Associates?

Richter: No, it's a special group which has that title. It has a connection with the Institute Associates, but it is a self-contained and self-operating organization.

Scheid: Was that started during the period when the Seismological Laboratory was separate from Caltech?

Richter: No, that has been a Caltech development and comparatively recent. I don't have dates clearly in mind and might easily give you misinformation, but it is something that grew up particularly after the Kern County earthquakes in 1952.

Sheid: Have real estate interests attempted to influence your work in any way? They must not be for prediction, I would think.

Richter: In general that is so. And of course I don't know what individual remarks have been made, individual strings pulled, but I know such pressure in that direction has usually been unfavorable, particularly because as we get more and more urban development, it tends to expand out onto less desirable areas which may be regions of greater risk, and not in relation to Caltech particularly, but certainly in relation to regulation by government authorities there has been considerable difficulty. For example, there was at one time in process a Los Angeles County regulation which would limit considerably prospective developments along the San Andreas fault, and in the vicinity of the San Andreas fault, that passed through the County. That area was one in which there was quite a bit of ongoing development, particularly for residences. Yes, there was a good deal of opposition, and some modifications were made in the law. But that's part of the general problem and not a specifically Caltech one.

Scheid: I thought I would skip back to something that we talked about on Friday. You mentioned Tolman, whom you knew, and you said that he was a lovely person and that his address at your commencement had made an impression on you. Could you elaborate on that a little bit?

Richter: I don't remember enough in detail anymore. This may be completely out of line, because after all it is recollection after a great many years, but it had to do to some extent with a consideration of admission requirements, and whether there would be something to be said for the reduction of the admission level so as to open the facilities and opportunities at Caltech to a greater number of students. This is at least a rational argument which has been heard many times since. His position, as I recall it, was to the effect that there were already sufficient opportunities for moderately qualified students elsewhere, and that no particular advantage to the Institute would result from numerical increase, obviously. I remember some remark to the effect that after all we have to have some means to attach recognition and awarding of degrees on the basis of proper preparation. It is one of those indications of a man's competence; otherwise, you might as well start issuing Ph.D's along with the birth certificate. This was the trend, and as I say, this is a vague recollection after fifty years, so it may not be too accurate. The chances are that the address or something about it is somewhere in the Archives anyhow.

Scheid: This was a proposal in the twenties then, you believe, about admitting more students--or perhaps later?

Richter: I think it's been recurrent. I think I mentioned to you at some time the problem which existed while we were having an undesirably high freshman mortality, and what came out of that was the interview system which greatly reduced the problem.

Scheid: In that connection, you mentioned that you were on a committee for admissions for the division, and I wondered if you were on any other faculty committees at all at Caltech?

Richter: No, that was nearly the extent. No, I have not served on any other staff committee in the Institute. Even that--that was something in the nature of an assignment. It was decided within the division that everybody on the principal staff should serve for a year or two on that committee. There was a rotation.

Scheid: Another thing you mentioned several times was the lunch table at the Faculty Club, at the Athenaeum. Was there an earlier Faculty Club than the Athenaeum?

Richter: Oh, yes. It was situated in a wooden--I was going to say barracks-like building, but it was a little better than that. It was an old house, I think. About on the same grounds.

Scheid: So that was a long tradition then, going back many years.

Richter: Oh, yes. I imagine it probably dated back to the Throop period.

Scheid: You mentioned several times that you met people there, spoke to people there. In fact, you said some of the most brilliant people on the campus, and that some of them came to you later sometimes to talk, or to discuss things that they were concerned with that you might know about. I wonder if you remember a specific instance?

Richter: No. I can remember one or two specific occasions when there were current exciting developments, and somebody would come in. I remember someone coming in who had just returned from one of the meetings at which there had been a first report of Schrödinger's results, which was the decisive reformulation of the quantum mechanics. It was described with considerable astonishment, because the connection with what had already been obtained was not obvious, and it was not until later when we had got more familiar both with Schrödinger's work and with some of the implications of the quantum mechanics in existence at that point that we realized that this would form a part of a connected whole. But I do recall the arrival of that information rather suddenly and unexpectedly and a rather lively discussion. And I remember also something coming in with the announcement of an event which I always thought of as one of the most important watersheds in science, which is rarely mentioned to the extent it deserves: Stanley's crystallization of the tobacco virus. Because to my way of thinking, at that time that was the breaking down of the barrier between physics and biology. I was just thinking of that the other day and looking up the date, which seems to have been 1935.

Scheid: Do you remember who that was who brought that information?

Richter: I really don't remember who came in with it. Again, a matter of somebody coming back from a meeting with the news.

Scheid: Do you remember who brought the information about Schrödinger? That must have come in a letter or something.

Richter: I think it was a preliminary publication, something of that kind.

Scheid: There was a meeting in Pasadena in 1931 of the American Association of the Advancement of Science. I don't know if you recall that at all; that was a rather important meeting in the sense that it was here for the first time, I think, on the West Coast. Quite a number of important people came, but you probably attended, I would have thought.

Richter: AAAS--I don't know very much about it. Now, I do remember meetings of the [American] Physical Society, and of the Seismological Society on the campus and elsewhere in that case.

Scheid: I think they also used the facilities at the Huntington at that time because there are photographs of people there.

Richter: No, that is just not in my memory now.

Scheid: There were some other visitors in the thirties. There was Niels Bohr and [P.A.M.] Dirac. I don't know if you recall seeing them or their lectures perhaps.

Richter: Very briefly, I believe, in those cases. I remember more clearly, as I have already mentioned to you, some of the people who remained and gave series of lectures. Quite a few of those. Somehow I don't recall Dirac being on the campus. Born I remember quite clearly.

Scheid: Well, Niels Bohr, though.

Richter: I do not remember. I am not sure that I ever saw him.

Scheid: Another person who came, in fact, in conjunction with Epstein, was Kármán. He came in 1930 permanently; he had been before, I understand.

Richter: Yes, I remember when I was still a student I did a little job of translating, getting one of his lighter papers out of German into English.

Scheid: That wasn't the 1912 paper was it?

Richter: No, no, no, this was some small thing about that date which would have been--oh, about 1927.

Scheid: But did you ever know him personally?

Richter: Only casually. I would encounter him, of course, at the Faculty Club and so forth. But no, the closest to anything personal I ever came was that particular job. I remember during the war during one of these programs we were giving for prospective officers, I had one group in elementary mathematical physics and was using a textbook which was part Kármán.

Scheid: But your paths didn't cross professionally at all?

Richter: No.

Scheid: Maybe we could talk a little bit about the main people over at the Seismological Laboratory in its early years. You talked about Harry Wood, but there was also Gutenberg who you said was chosen after having been brought here together with Jeffreys, and you mentioned that it was a difficult decision to decide between the two of them.

Richter: I imagine it was. I wasn't on the inside of it, I was just a young assistant. I am not sure, but I think Wood did ask me whether I had a personal reaction, to which I naturally said, "Well, I appreciate Harold Jeffreys very much but, of course, I like Gutenberg. In some ways I have more in common with him." But I didn't want my reaction to have anything to do with an important decision.

Scheid: You mentioned that Gutenberg had a very good sense of humor. Do you think that his personality may have been one of the factors that caused them to choose him?

Richter: I am inclined to think so. And also there was the feeling that his publications and the work he had been doing dovetailed better with the program of the Laboratory as it was set up and was going on. Jeffreys is a high genius, but he has an extremely abstract and theoretical point of view. As a matter of fact, as far as we could find out at that time, he had done very little work with actual seismograms and had seen relatively few of them. Whereas Gutenberg had been involved in the detailed interpretation of seismograms for years and years and years.

Scheid: What sort of work had Jeffreys been doing then, actually? Just mathematical . . . ?

Richter: To a large part, yes, as in his volume, which does summarize and refer to most of his other published work, as well as that done by other people. That is the fourth edition [of The Earth]. There is one newer edition since. I haven't acquired it because from everything I hear, I would find it disappointing. It's one of these cases when someone of high reputation simply refuses to accept a new development, and he has set his head against plate tectonics. And that has badly colored the fifth edition and retarded the reception.

Scheid: But he has used data here.

Richter: Yes, and particularly he was working with Bullen, also a very brilliant fellow, and Jeffreys tried earnestly to emphasize, "Now, Bullen is not my assistant," and he did everything to further Bullen's reputation and position. Bullen was an extremely fine fellow, and he is a great loss. But they did work together, particularly on the travel times of seismic waves in the interior of the earth, and so on, just about the same time that Gutenberg and I were working together on the same general problem. We were exchanging conclusions and data, and what made it particularly fruitful was that they were working almost exclusively with reported readings as contained in the International Seismological Summary or in the bulletins of the individual stations. And they were doing very, very little reading of individual seismograms themselves, they were accepting the data. But because there was an enormous mass of it, by handling it with good judgment and with the proper use of statistics, as against the kind of use of statistics that some people make, they were coming out with essentially correct results which were then found to be in quite close agreement with those which Gutenberg and I and some others were deriving from careful study and revision of a small group of specific seismograms. So that Jeffreys' work at Cambridge and our work at Pasadena tended to complement each other, and there was a very cordial exchange of data, and we would get inquiries from Jeffreys about particular points, and on the whole, he was, what shall I say, very open and cooperative on the acceptance of use of data. I remember there was one particular seismic wave, which had not been particularly well studied or reported on. It was an obvious theoretical possibility, PCPP'. It happened to be particularly conspicuous on some of the seismograms we had had here and on the auxiliary stations on an unusual group of earthquakes in the Indian Ocean. So we published on that, and Jeffreys' wrote about it, among other things, that he particularly wanted to know where we got these data. So we sent him the readings and also some copies of the seismograms, and he was most favorably impressed, and he wrote an appreciative note about it. I recall some ideas that Jeffreys had which were well known and indeed quite current at the time I came into the subject, about the recording of local earthquakes which were simply out of line from what we know now, but all right--great men make mistakes. He was, I would say, never over-positive, so that this uncompromising position he seems to have taken in recent years about the latest important development is painful but unfortunately not unexampled; other people have done similar things.

Scheid: Was there a certain rivalry between Gutenberg and Jeffreys?

Richter: Only in the most friendly way. They had some definite differences of opinions on critical points, but just the usual thing. You have different types of evidence which indicate different conclusions, and so there is room for perfectly reasonable difference of opinion, and that's the way you get on. But it was always entirely cordial.

Scheid: Well, actually we came on to this because of Gutenberg and his work in the Seismological Lab--was he there most of the time when he was at Caltech?

Richter: He spent a large part of his time at the Laboratory. He was giving courses on the campus. Yes, he was effectively one of the staff, and he was also ex officio on what was supposed to be the steering committee for the Laboratory, so that if there was a conference on that, he was involved.

Scheid: Did you regard him as your boss, so to speak, or was that Harry Wood?

Richter: My boss was Harry Wood, but at Gutenberg's invitation and with Wood's consent, I picked up a certain amount of work in collaboration with Gutenberg, and I owe a very great deal to him and came to regard him with almost a filial affection. I was very, very fond of that man.

Scheid: He had a good sense of humor . . . .

Richter: Oh, yes. Rather difficult to quote an instance offhand, though I could quote this one which is a little sour but still very good. Let me see, in the early 1940s we were still getting publications from Germany, and in particular the illustrated, popular publications. Some of these would feature Nazi Jugend and members of the Hitler regime, and I remember one full-page picture of Hitler at the opening of some new roadway, something of that sort, and shoveling the first spadeful. Gutenberg looked at it and said, "That's what he ought to be doing all the time." [Laughter]

Scheid: Gutenberg was a very small man, physically?

Richter: Yes, I think he was 5'1", something of that order.

Scheid: Do you think that affected his personality?

Richter: Oh, somewhat. He had that kind of vivacity which sometimes goes with short people. I think I have already mentioned to you the entertainment that he and Vening Meinesz got out of being photographed together because Vening Meinesz was over 6'. [Laughter]

Scheid: And Gutenberg was very small.

Richter: Yes, the usual joke about the long and the short of geophysics.

Scheid: What do you regard as the most important thing that came out of your collaboration with Gutenberg? What was the most stimulus that he gave you? There were many of them, perhaps, but what you consider significant.

Richter: Well, that sounds like two different questions.

Begin Tape 5, Side 2

Richter: You were raising the question about the most significant contributions. Of course, Gutenberg did a great deal of work in a variety of geophysical fields while he was here, and much of it was of considerable interest and importance. But for a number of years a large part of his time went into our collaborations. There were two major projects, one of which was the four papers on seismic waves, which were a rather inclusive recension of the travel time tables for seismic waves and concomitant discussion of the interior of the earth and excursions into such problems as the structure of the crust and the extension of the magnitude scale. So those four papers taken together are a very considerable project. Then the other large project was that represented by the papers on the seismicity of the earth. Those started originally as investigations only in the field of deep focus earthquakes and then we found that the occurrence of deep shocks had so confused the general picture that the entire seismicity of the earth was ripe for reinvestigation. So that was a very large project which engaged us for quite a while and proved to be of significance in directions which we had not originally intended, because it helped to lay the foundations for the plate tectonics.

Scheid: On those papers which were collaborations, would you care to elaborate on your respective roles?

Richter: Well, like most collaborations, it would be very difficult to disentangle separate parts. In the work on seismic waves the theory and procedures of a large part were those which Gutenberg had established years ago and was familiar with, and so it was a matter of applying them, without much transition, to a new set of data. And naturally the material bearing on the magnitude scale, well, based on my beginning with the local earthquakes, was capable of extension largely through Gutenberg's familiarity with what we should expect from the propagation of surface waves over the earth. In regard to the seismicity of the earth, we were largely working with the same group of bulletins and individual records for the same regions of the earth, but also a great deal of bibliographical work went into that. Some of the materials were found in Gutenberg's library, but others were in sources with which I was more familiar, so that I contributed a good deal to the basic material which is included in that volume.

Scheid: How did the writing come about? Did you write separate parts and then read the other's contributions?

Richter: It varied. The best procedure usually was to discuss a matter, and I would write up a rough draft, and we would go over and revise that. But it didn't work out always too well to Gutenberg's satisfaction.

Scheid: You wrote up most of the rough drafts, then?

Richter: Yes, especially in the earlier years. Naturally my English was somewhat more facile than Gutenberg's, in spite of his very good foundation in the language.

Scheid: He felt less confident in English then?

Richter: I felt less confident in letting his English get by. There were very curious and unexpected things. I think maybe I should tell two or three of those stories which mainly date back to his early acquaintance with our ways when he still had quite a bit of the Germanic clinging to him. I remember in the discussions at the 1929 conference, which I attended to some degree, I found it advisable to take Wood to one side one day and say, "I think there is some misunderstanding arising from what Gutenberg says when he would say, 'Well, I think we must not do this,' when what he meant was, 'We need not do this,'" which led to some conflicts. And there would be odd things such as, he would come to the office with something to mail and say to the secretary, "Will you please take this to the post office occasionally?"

Scheid: Did he retain that throughout his life--the occasional misuse of words?

Richter: It decreased, and there were some slips he became conscious of, but would occasionally make and then correct. But naturally as he got to working and writing almost exclusively in English, in fact he complained one day that he was beginning to lose his grip on German.

Scheid: He never returned to Germany, did he?

Richter: No, I guess not. There was no occasion to. I don't think so.

Scheid: There was also Benioff working at the Lab, and I believe he was in charge of quite different responsibilities.

Richter: In a rough way that is so. These lines were never sharply drawn. On the instrumental side, Hugo Benioff was a genius, and a large share of his contributions were on that side, and as the years went by, that came to worry him more and more. He didn't want to be classified as an instrument man. And, indeed, he did have some very good general theoretical ideas, of which I think the most successful was the recognition of the non-elastic part in the mechanism of earthquakes. Reid's theory considered the rebound theory exclusively from the point of view of elastic theory. And this was very justifiable as a first approach, but Benioff's contribution was quite significant, and I think it still remains a definite part of the subject. He had some other theoretical ideas which were less happy, but he was a man of broad interests and abilities. He had a slight weakness from the procedural point of view, and if he once got an idea or development in hand, it was very difficult for him afterwards to see any faults or weakness with it. And he would sometimes become very combative on a point like that, and then gradually, after a while, he would come around to a modified point of view. But it was difficult and, as I think I may have said before, some of his temperamental characteristics may have been due to the fact that he was never really a well man in all the time I knew him.

Scheid: It's getting rather late. Maybe we should stop for today.

Charles F. Richter
Session 4
February 24, 1978

Begin Tape 6, Side 1

Richter: The chairman of the resulting [Geology] Board was Dr. Ian Campbell, who is no longer with us, I regret.

Scheid: He was professor of geology here?

Richter: He was for some years chairman of the division, and he left to take the position of State Geologist, which he retained I think for three or four years. After that he retired into writing papers and some consultation, and then he got involved with this Geology Board. The original act provided registration and licensing of geologists, a major problem. The Geologist Act went on and was administered as such for a couple of years, and then further legislation was introduced which expanded it to the Geologist and Geophysicist Act, so we now have the State Board for Registration of Geologists and Geophysicists. And I came in under that as the lone representative of geophysics on the board.

Scheid: Does everyone have to be certified, or only if they are going to prepare these reports?

Richter: Licensing is required for the practice of geology or geophysics "for others" in the words of the statute. So that would mean for example that if somebody is working employed in a company or even in a university and is turning out reports which are in-house and not published and put out for circulation, the Act does not apply. But if any such material gets out, it then requires at least approval by someone who is certified.

Scheid: And what are the standards of certification?

Richter: Most briefly, a bachelor's degree or equivalent in geology, geophysics, or some subject sufficiently related that it can be accepted.

Then in estimating experience, graduate work is allowed to the same numerical extent. Seven years of professional experience are required before licensing is possible and that requires examination. We had, which is now expired in both sections, a grandfather clause type of arrangement in which anyone establishing fourteen years of professional experience and the academic equivalent could be registered without examination, so naturally a very large proportion of those practicing are registered under that provision. That is now expired so that everyone who comes up now in geology or geophysics has to pass an examination or be registered under a very special provision in two sections of the Act which provide for registration of persons with fourteen years experience and without examination if, in the judgment of the Board, they have the equivalent knowledge of the subject. I was originally registered as a geologist under that provision, with the idea that I knew enough about geology to pass. I didn't think so, but the board did.

Scheid: That would have been embarrassing not to have passed the test.

Richter: Well, I probably couldn't have passed the geology examination as it is now given. It involves too much specific and special information of a sort which I recognize as valuable but don't have in my mind. I could probably get in and swat up a lot; that is what the young candidates do. So I am now registered as geologist and as geophysicist. Now, of course, we have had a great many deliberations going on on the Board, and we have new members coming in, and we have not always very happy relations with the administration of the Consumer Affairs Department under which this board is set up. Some of these papers you saw had to do with both of those situations, the terms of registration and also their relations with the general administration.

Scheid: I wondered about your present work, your consulting work in this connection. You had to be registered to be doing that, didn't you?

Richter: Yes. Of course, I could always have got by by making sure that anything I prepared was approved, and so to speak, sponsored by someone who was licensed, but that was not true. For a couple of years, in the first epoch of the Act, I was emphatically claiming that I was a geophysicist, not a geologist, and I was consulting only on that side. This, at least, kept me out of the range of technical violation. After the Act was extended, there was no longer any serious problem.

Scheid: What is the kind of work that you have been doing lately?

Richter: Well, we have this consulting firm, let's see if I have the professional card here. Lindvall, Richter, and Associates. Lindvall is Fred Lindvall, who was for some years chairman of our engineering division here. He left the Institute to take an executive position with the John Deere manufacturing firm in Minnesota, and now he has retired from that and is back here, living down here on Allen Avenue, and participating in the operations of this consulting firm.

Scheid: What are you mostly involved in and how does his background tie in?

Richter: We are covering the field of consultation in geology, geophysics and engineering. His contribution is on the engineering side. Of the Institute group, we also have with us Dr. Ron Scott and Dr. Jahns, who is now at Stanford University.

Scheid: What kind of jobs have you gotten?

Richter: A good majority of them have been in relation to dams and reservoirs. This came about in part due to the fact that since 1964 I had been working with the Los Angeles City Department of Water and Power on their consulting board on the safety of dams and reservoirs. I was the seismological representative on that board. And after our firm was set up, this became a very natural association of the firm with the Department of Water and Power, and we have an ongoing more or less open contract with them. And we have taken up one consultation after another. We have also carried out consultation in the same field for the County Flood Control and for the Metropolitan Water District, and we have been involved in two or three problems which involved the safety of buildings, rather than of dams and reservoirs.

Scheid: Have you done many dams? You do one after the other, is that how it works?

Richter: More or less one after another. There is this circumstance, that since the occurrence of the San Fernando earthquake and the near failure of an important dam on that occasion, the state Department of Water Resources initiated a compulsory program of revising the data on the safety and condition of dams throughout the state, whether under public or private control. And quite a number of these are under the Department of Water and Power, and they have been taken up more or less in order of priority as agreed on between the DWP and the state authorities.

Scheid: You said you were on the Board of the Department of Water and Power. I wonder if they had always had a seismological consultant or not?

Richter: In the design and initial reports on most of the dams, they have usually had some seismological information collected. This was generally done by geologists either on their own staff or as consultants.

Scheid: So they haven't been negligent in that area?

Richter: No, not at all.

Scheid: Are there many dams left that haven't been looked at?

Richter: Very few that haven't been looked at at all, but still quite a number that are waiting for thorough reinvestigation and report. Also, this is an ongoing thing. It is felt that because of possible changes of all sorts, the matter has to be returned to after a lapse of years. For instance, when I was first working with the department, one of the dams considered and reported on to the board was at Encino, in the San Fernando Valley, and that is one of the installations which is now undergoing a further examination.

Scheid: There is some doubt about that one?

Richter: Not particularly, not specially, no, but for technical reasons it is on the state department's list of installations requiring current report.

Scheid: Have you had any other types of consulting jobs other than this dam survey?

Richter: We had a very interesting problem about the safety of a large building in Manila, Philippines. That involved us in various international complications. One of our men had to go over there and look the matter over, and while he was there, a very considerable earthquake shook the city, so he had an enlightening experience.

Scheid: How was the building that you were investigating?

Richter: The building did not suffer appreciably, but it already had quite a bit of suspicious cracking which did not contribute to our opinion of its safety.

Scheid: You mentioned other buildings in the Los Angeles area that you investigated?

Richter: Yes. Of course, the amount of work involved varies. For example, we had a case involving a tall building in Century City.

Scheid: Yes, those are rather new.

Richter: And we were consulted on the safety of a prospective new building extending the County Hospital.

Scheid: I see. Any other types of work other than buildings and dams?

Richter: Those are the principal items that--well, yes, nuclear installations. We had a considerable problem involving hearings and court appearances concerning a nuclear power plant on the Hudson near New York. There is at present quite a bit still going on with reference to nuclear installations in central California. Those come up from time to time.

Scheid: Oh, so you are working on a California one then?

Richter: Yes, we have been involved in at least one of those in a touch-and-go sort of fashion. We have been consulting for consultants.

Scheid: So that is about the extent of your work?

Richter: Those are the major lines of operation, yes.

Scheid: Do you personally do a lot of the work?

Richter: My contribution to the reports has usually been in connection with the earthquake history, both instrumental and non-instrumental as bearing on the circumstances at a particular location. And in the course of that, I have often been in the position of getting old papers and references out of my file which have been applicable to the particular instance.

Scheid: I thought that would be interesting because it is hard to get information about that kind of activity from any other source. I wanted to go back--I guess we stopped last time with a couple of items, and I mentioned Chester Stock, and I wondered if you would like to speak about him. You said you knew him very well.

Richter: Well, I wouldn't say very well, but we were together in the division for quite a while and he was chairman for some years. And he was a very agreeable person and it was always interesting to talk to him, especially when we had a group meeting, because he was approaching the subject from a different angle from most of us.

Scheid: He was in a very outside kind of subject in the present structure of the department.

Richter: Yes. See, there was the rather considerable expansion in the direction of paleontology particularly, notably vertebrate paleontology, under his administration and that has not continued on the same scale.

Scheid: So that area has been pushed aside a bit, you would say?

Richter: I wouldn't want to be responsible for saying that, because the proportion and the organization of the division is a matter depending on the best judgment of the current staff, and certainly that has not been altogether extinguished, and I believe that the very valuable collections made at that time are still available and in use sometimes by visiting research people.

Scheid: Is the Caltech collection especially good?

Richter: I would say it is certainly good; now, whether it is especially good I don't know, but naturally it is special in containing some materials hardly to be found anywhere else. It is one of the best local collections. I believe there have been sometimes negotiations with other institutions to house some of that collection elsewhere and so on, but you'd better talk to some of the people who know more about it than I do.

Scheid: You mentioned that when Buwalda first came here you asked him about the question of continental drift and then you related his reply. I wonder if you could go over that again.

Richter: Well, that was a very brief conversation. It was almost my first meeting with Dr. Buwalda, and I believe I had been introduced to him a short time before. And this matter was very much on my mind for its geophysical implications, so that I took the first pleasant opportunity to ask him what seemed to be the present status of that matter, and I got from him I think the answer I would have got from anyone else at the time. Not long before, there had been a large symposium under government auspices which came out in published form and in which the matter was discussed back and forth by proponents and opponents and the general feeling was--well, certainly the impression one would get from reading that book, was "not proven." I think essentially that was what I got from Dr. Buwalda at that time. Naturally the situation changed over the years and changed radically with the new data that came out from the magnetic surveys over the oceans.

Scheid: But this was in the late twenties that you discussed this with him?

Richter: Almost certainly 1926.

Scheid: He came to change his mind, or did he?

Richter: I really don't know. Dr. Buwalda was always open-minded on critical questions, and the problem which concerned practically everyone in those years was how good is the evidence. And he was taken from us rather suddenly in 1952 which was just about the time when some of the new developments were getting started.

Scheid: I wanted to move on more generally to Caltech a little bit.

Richter: There is a little anecdote I'd like to provide you with which you could probably eventually insert where it may seem to fit in better. But we were speaking of Harry Bateman the other day, and there are undoubtedly any number of good stories about Professor Bateman, but this one I remember personally, and it chanced that I was reminded of it again this morning.

I forget what the lecture course was, but he would introduce almost any interesting subject which happened to be on his mind if he felt like it. So one day he came into class and commenced to discuss the problem of pursuit curves. These are rather a wide class of curves defined in various ways but in rather general terms. The simplest one which you find in most of the textbooks runs like this: "A man is walking in a straight line with uniform speed. Off to his side is a dog also approaching him at uniform speed. Now the dog always runs directly toward where he sees the man. So the problem is, what is the mathematical description of the curve described by the dog's path?" Naturally you can extend this problem a great deal by modifying the speeds or having the man walk in a circle instead of a straight line, whatever.

So after more or less this kind of an introduction, Dr. Bateman then said, "This could even be extended to three dimensions." So then he presented the problem of a man walking in a circle on the ground and a bird flying down from above directly toward him. This seemed entertaining, and then it occurred to me that just at that time the blackbirds were nesting in the trees here on California Street, and they were particularly aggressive and pesky [laughter]. So I think I could recognize the source of the inspiration. This one is too good to lose.

Very well, but what you were about to ask is probably something of a different character.

Scheid: Well, that was a good digression. What I was going to talk about was a couple of things you mentioned earlier. Millikan had this goal that Caltech was to be a pure research institution, I think, and you mentioned that that had perhaps changed a bit.

Richter: Well, I don't know precisely. Of course, the original vision of the Institute was as a research center, and this was organized largely by Millikan, Hale, and Noyes. I don't know just how to describe it in formal terms, but anyway, Hale had had the idea that the existing Throop Institute could be developed into an institute of technology, and this would be a very happy situation with the [Mt. Wilson] Observatory and its technical facilities and interests close at hand. And no one overlooked the cultural influence of the Huntington Library and Art Gallery, although that was not really drawn into the Institute orbit until a number of years later. So I would say the original creator of the idea was Hale, and, of course, with the background of the Carnegie Institution, his primary interest was in research, but it was obvious that the Institute would provide a center for training promising young men to take up research. And if you look into the earlier prospectuses of the Institute, the things that got into the bulletins of information of students at that time, you will find that kind of ideal set forth. Now, the Institute from the first had its trustees, and a number of them, for good economic reasons, were businessmen able to support the Institute, and naturally they had the businessman's concern for research which they could see would lead to practical results. So this was a natural slant, and it resulted in later years in modifications, which I still regard as somewhat unfortunate, in which economics was expanded and wished on the humanities and rather put the pressure on the humanities instruction and operations, which had not been part originally of the Institute program. I don't want to get too far into that, but I will mention that it is something I have never liked. But you suggested this by indicating that the original purpose was research. With this Millikan was very heartily in agreement, and a great deal of his public efforts was directed toward selling pure research to the practical-headed citizens. And with a considerable amount of success, I don't need to emphasize. But nevertheless, the results of this were plain. I remember some years before the Second World War the Institute was having some difficulty placing physicists with the corporations, because they would meet this reaction, "Oh, we don't want physicists, those fellows are only interested in atoms, that's no use." So they would go out and get engineers or people with rather divergent training from what they would receive from Caltech.

Scheid: Placing physicists is a problem again, I guess.

Richter: Well, I suppose so.

Scheid: That brings me to this idea of the expansion of Caltech into a university, into a more diverse institution. You mentioned that you didn't think that that was a very good idea.

Richter: Well, put it in the terms of personal preference, and as an objection I have always felt that this was organizing rather unnecessary competition, because we had several other good institutions of learning in the area which were well equipped in the liberal arts and humanities, whereas Caltech was in a comparatively unique position. So that expanding Caltech into an inclusive university seemed a little superfluous to me, because it was clear that it would require additional equipment, funding, and personnel--all of which has been provided. I of course realize that all of these things were very seriously discussed, and the decision to expand was not taken lightly by any means, but I never personally adjusted to it very well.

Scheid: You yourself went to Stanford as an undergraduate rather than to an institution like Caltech. Do you feel that that was personally more rewarding for you as an undergraduate than a Caltech-type education?

Richter: I may have had it a little easier at Stanford in some respects, which meant that when I came here I had rather more to catch up and fill in than would have been the case had I worked myself through the Caltech undergraduate school. But fortunately I had had very good basic training which started all the way back at USC, and this stood me in stead so that I was able to absorb the kind of background I needed, very largely from the lectures of Professor Epstein.

Scheid: Well, I guess the thing I was getting at more was the training of scientists and whether they just need science or if they should also have other subjects?

Richter: My answer to that is yes, and that was the answer of the earlier Institute administration. Indeed, to come back to something I touched on before, the Institute in those early years had a uniform freshman year. Everybody took the same courses no matter what they were going to do the next year. And among those courses was a certain proportion of humanities and there was a general humanities requirement which had to be fulfilled before completion of any course. That is something which in intention and in execution at that time I approved of highly. Now, one of the things that happened, as I have suggested, owing to certain pressures, economics was imposed on the humanities from above, over considerable objection from the humanities people themselves, the objective being to allow students to take economics as part of their humanities requirement. Well, if I ever expressed that rather fully it probably would burn out your recorder. [laughter]

Scheid: I guess we have covered that subject. I was going to ask another question which was more specific. You mentioned that during the war there was a certain change in the administration which wasn't really official, and there were certain changes in the way the Institute was run.

Richter: I can't tell you too much about it, because I didn't know too much about it. I knew in general what was going on. What occurred of course was that Dr. Millikan had expected to retire before that time, and continued in view of the war emergency, but he continued by delegating more of his responsibilities than had previously been the case. So there were, I think, a great many smaller changes, and the war conditions necessarily involved a considerable number of other changes in atmosphere, the setting up of special courses for military personnel, the war-oriented projects, the initiation of JPL--in fact, anyone who knows the facts could go on indefinitely, so that like every other institution in the country, Caltech presented a different aspect from 1942 to 1945. But after the war there was pretty satisfactory general return to more or less the previous line of development. There were adjustments due to the fact that we had a considerable number of students under the GI provisions, and naturally they were cared for specially; it was felt they deserved it. And such things as rocketry and atomic energy had come to the fore so that for a while every other new student came into Caltech expecting to go into atomic research.

Scheid: But I meant changes in the way the Institute was run particularly. I thought that you had meant that.

Richter: I would say that one might say there was rather a lack of change. Except for restoration as far as practicable to the pre-war situation, there was very little progress. I can very painfully mention that there was no increase of the salaries of the faculty until Dr. DuBridge arrived on the scene. I think I told you before he had number one and number two priorities--number one being Tournament Park and number two, faculty salaries.

Begin Tape 6, Side 2

Scheid: The question that we talked about last time a little bit was the controversy about the admission of women at Caltech. I wonder if you could talk about that. Did it ever come up in the twenties or thirties?

Richter: Not to my knowledge. The big stir came along much later, after the war. There were some people on the staff who were definitely against it in public and emphatically against it in private. I don't think I need repeat the kind of arguments that were used, they are very familiar. And I think I told you that one of the things that was brought up in faculty discussion was the rather favorable report we got from MIT and their experience, which was very encouraging. And I think I also mentioned that in spite of all this discussion the thing that finally made a crack was the arrival of a new staff member in chemistry who insisted on bringing his assistant with him or else he wasn't going to come.

Scheid: Was this a graduate student that he had?

Richter: Yes, a woman graduate assistant who had been practically indispensable to him in his research, and he just wasn't going to break that off.

Scheid: Who was that?

Richter: I am sorry, I can't give you names.* But I'm sure that there are plenty of people here could tell you precisely with very likely much more detail. So this took some special action on the part of the faculty and the trustees to make that possible even in this individual case, and while they were about it, they made it inclusive so that women of special qualifications could be brought in as graduate students and assistants. So there were a gradually increasing but small number of those. But the big excitement came about when it was proposed to bring women as undergraduates, and there were the usual arguments about dormitories and facilities on the campus and all of this kind of thing. But I don't think I need to go into detail on that; I was not directly in touch with it. Of course, we got a good deal of background on what was going on from Imra Buwalda who was very much interested in forwarding it. And there was the general idea approached with pleasant anticipation by one group and apprehension by the other, that having the women would change the character of the place, which doesn't seem

* John Roberts was the professor; Dorothy Semenow was the graduate student. [Ed.]

to have happened at all.

Scheid: Was there consideration that the standards would be lowered?

Richter: Not as you put it. I think there was pretty firm determination on all sides that that should not happen.

Scheid: You mentioned the role of Imra Buwalda. Were there other faculty wives who were for the idea?

Richter: Yes, several, but I can't give you names now. But there were not only wives of staff but also wives of some of the trustees.

Scheid: You mentioned that the graduate students were admitted earlier. When was that, about? Since the war?

Richter: Oh, yes, quite, quite. But I am sorry, I don't have the date. Except that there were quite a few years in between that step and the final one of bringing in women undergraduates, which is comparatively recent.

Scheid: I have looked in the bulletins of the thirties, and I found some kind of research positions in the biology department that were filled by women, and I wondered if you knew anything about that?

Richter: Other than the fact, no.

Scheid: Most of them had Ph.D.'s and were very well qualified.

Richter: They would have to be. I would suppose that those were mostly people working with Morgan and his group.

Scheid: Do you know of any instances where women have made special contributions at Caltech in the research fields? Not recently, but perhaps earlier?

Richter: No, I really don't. Let us see, we have had only one or two, I believe, in our own division, and they were good students and so forth, but I don't have much background.

Scheid: But there were never any women working in the Seismological Laboratory, in a research capacity?

Richter: Well, almost, yes. Not with that kind of appointment, but for many years two of the mainstays of the routine operations of the Laboratory were Gertrude Killeen, who did most of the photographic work, and Violet Taylor, who became my assistant in the measuring department, and continued there after I had left until she was retired just a very short time ago.

Scheid: But these women didn't have advanced degrees? They were technical staff?

Richter: Right. Violet had just ordinary high school background, she had been doing very ordinary work, I believe, in the Buildings and Maintenance Department, and this position opened at the Seismo Lab, the girl we had had was leaving, so she came in, we interviewed, we looked at each other and said, "Well, at least we can try."

Scheid: You didn't have such high hopes then?

Richter: No, and I know she didn't, she was scared pink. But it worked out very well. Vi is a very agreeable, intelligent, and alert person, and the place was never run so well. One very good qualification was she knew how to handle the young men around the place--they didn't get away with anything.

Scheid: I think maybe we'll have to explore that question with some people in biology, because perhaps there were more . . . . Well, there was of course Olga Taussky Todd on the faculty, but I believe she was the only faculty member.

Richter: Well, I am simply out of touch with that side. If you are going to get into the general problem of women at the Institute, I am afraid I am not the best source, because after all during most of this time, I was off the campus. So I can tell you about these particular people and what went on in the division; we had very few. As you probably know, from the time the Laboratory became part of the Institute, for a great many years the division was practically run by Miss Reno. Of course, she was Dr. Buwalda's secretary, and as one expects from a good secretary, a great many of the minor decisions and operations were actually in her hands.

Scheid: But there were no women in paleontology?

Richter: Except perhaps as temporary assistants whom I don't recall. See, I am not too much an authority on the division, and my most regular contacts were in connection with the admissions committee, and that had to deal with only a certain aspect of the division problems. Occasionally, we would have a division meeting where other matters would come up.

Scheid: Besides the controversy about the admission of women, I wondered whether the opposition came from the trustees or from the faculty, or whether it was equally distributed?

Richter: I just can't speak for the trustees at all. I do know that certain of the faculty were very vociferous on the matter.

Scheid: That was one controversy. I would like to ask you a little more about another controversy and that was the controversy about Linus Pauling. It seems to me, I'm not sure about this, that there was some movement to ask him to leave, and I don't know whether you know about that at all?

Richter: Well, I have heard the same sort of rumors, but I don't know what the details were at all. I think I mentioned to you the joyous occasion when he was awarded the second Nobel Prize, the Peace Prize, and they were in a very equivocal position.

Scheid: Did you observe any difference in the way the faculty treated him other than on that particular occasion?

Richter: No, I don't think so.

Scheid: Did you see him at the Athenaeum, when he was talking with certain people?

Richter: I don't remember seeing him there frequently. In fact, not frequently at all. Of course, there were periods about which I don't recall clearly when I was not on the campus at all.

Scheid: So you didn't really know him.

Richter: If you want background on this you should talk to somebody else who was on the campus and knew a little more about what was going on. Please remember that so much of my time was being put in off campus, even in those years when I managed to get over regularly to lunch and come to occasional seminars, even so, most of my time was not here.

Scheid: Well, another thing I wanted to ask you about was, when the A-tests were going on, and you were sometimes unsure about publishing information and you did publish information that you sometimes feared might be touchy or that might have to be suppressed. Do you remember any specific instance?

Richter: I had in mind a very specific instance. This had to do with the second test shot at Bikini, Test Baker, which was fired underwater, which is a very efficient way of transmitting energy into the earth, and this recorded small amplitudes definitely on a number of instruments in our network and on some of the other instruments of the same type in this part of the United States, so we collected these data. We felt that the elapsed times from the shot, which of course was very accurately timed, to the arrival of the seismic waves at these stations were perhaps the single most important observations made in instrumental seismology up to that date, because they were the first to give us an absolutely positive control over the speeds of propagation of seismic waves to large distances. So this material was written up and more or less smuggled through to publication in the Transactions of the Geophysical Union, to the great annoyance of certain offices which wanted to keep everything that had anything to do with an atomic test under wraps. This we realized, so that we went to considerable effort to make sure that this would not be lost. There were other things coming out at that time which simply were effectively suppressed. I can compare the situation with which Dr. Bullen dealt many years later when the large tests were going on in the Pacific and being recorded all over the world, and the times were reported in the bulletins of various stations, many of whom did not realize that these were atomic tests at all. So Bullen was able to collect and correlate those and publish valuable results bearing on the propagation of seismic waves and the interior of the earth.

Scheid: Do you know what the mechanism of suppression was? Who would say, "No, we can't publish this," or "No, it shouldn't be published?"

Richter: Oh, some second-rate clerk or undersecretary had only to pick up a rubber stamp and mark the material as classified, and it wouldn't be able to get out at all.

Scheid: I see. Well, where would that be done though? You had the information here and the Geophysical Union was presumably independent.

Richter: Well, we had to have it published through the offices of the Geophysical Union, which of course were in Washington.

Scheid: I see. I didn't understand that. If you'd published in the [Bulletin of the] Seismological Society?

Richter: It might have been, but you raised the point about this particular paper. Actually, some time later Gutenberg published an account in the Bulletin of the Seismological Society of the recordings of the first Los Alamos Test.

Scheid: Were you forewarned about the test or did you understand that they were tests from the data?

Richter: Let's go back just a moment to the Bikini Test B. That was announced in advance, and we were aware it was coming off. Now, indeed, even the first test, Test Able, got quite a bit of publicity, but that, being fired in the air, produced almost no effects at a distance, and I recall, if I'm not mistaken, that we had representatives of the press in the Laboratory looking for effects from that which didn't occur.

Scheid: There were other stations you mentioned that reported these tests, but certainly some of them must have recognized that they were artificial?

Richter: Well, let's not confuse two matters. In the case of the Bikini test--that is, the Test Baker--practically all stations that recorded anything legible were in the western United States and were operating Benioff-type instruments. So that we knew pretty well where to find them, and we borrowed copies of some of the recordings to make sure. The other circumstance I mentioned is a good many years later, about 1954, when as I said, recordings of these Pacific tests would come out and appear in the bulletins of seismological stations all over the world, most of whom were not aware of the nature of the test. In the first place we had contacts and in the second place, even without that, our own recordings were sufficient to identify the event after we had seen one or two of them and recognized their characteristics.

Scheid: Well, then, other stations outside the U.S. could publish these things without any worry?

Richter: Well, that was the advantage Bullen had, of course. He was publishing outside the United States, he had gathered this information from bulletins, and he could not be classified out of existence.

Scheid: So the English government was not so hard on this question? There was no cooperation?

Richter: Bullen's headquarters were in Australia. No, it was our security organization here which was very nervous about letting out anything that had to do with the subject.

Charles F. Richter

Session 5

September 1, 1978

Begin Tape 7, Side 1

Scheid: I was thinking maybe of your particular experience when you first went there to the Seismological Laboratory. Who was head of the Laboratory?

Richter: Harry Wood.

Scheid: Was he your boss then?

Richter: Yes, he was in charge of the Laboratory under the Carnegie Institution of Washington.

Scheid: What were your duties when you first went there?

Richter: I was engaged in effect as Research Assistant, and I was told to begin with that I would not be expected to occupy myself to any considerable extent with the routine work of measuring and interpreting seismograms. There was another young man on the staff there who was supposed to be doing most of that at the time--his name was M. D. Shappell. I very soon found out that I could accomplish nothing whatever without doing a good bit of routine work myself and gradually we reorganized the cataloging and filing system and down to my last days at the Laboratory I was still doing a good bit of routine work.

Scheid: When you were hired as a research assistant what was your major project? Were you given a project when you first came there?

Richter: The object of the Laboratory program, so far as it could be tied down to anything limited, was of course to investigate the occurrence of earthquakes in the Southern California region--to catalog them, determine their epicenters as well as their times of occurrence, and investigate their relationship to the known fault system. As in many other places, the program of local investigation was set up with the idea that the recording and study of small earthquakes would bring to light all the significant active faults, and that was an essentially disappointed expectation; something which developed not only in this area but also elsewhere where such work is going on. Major earthquakes often occur only on a few principal faults, and there may be long intervals of quiet between them during which the recording of minor earthquakes sheds very little light on the major activity. As I stated, the immediate objective, regardless of what the ultimate result might have been, was to find out in considerable detail what was going on. It was bound to eventuate into a sort of a cataloging procedure, and it was in connection with that objective that the need for something of the kind of the magnitude scale later developed, because it was felt that in addition to the timing and location of the earthquake occurrences, some indication as to their magnitude, in the sense which we came to apply it, was highly desirable.

Scheid: So you became involved in this cataloging operation just to gather information.

Richter: Yes, we were very much interested to find out in as much detail as possible what was actually going on, and we got a considerable number of results at a rather early stage which were rather significant. The general distribution of small earthquake events over the entire area without any obvious correlation with a major fault structure--that was in a sense a new and unexpected result. At the same time we did find that it was very rarely that an earthquake event of consequence had not been preceded by some smaller event in the same or nearly the same area.

Scheid: Were you the only person working on this at that time?

Richter: I think I can say I was the only person assisting Wood in a program of investigation. The other man whose name I mentioned was doing the work as a routine assignment. Eventually he drifted away from the Laboratory to some other employment. Hugo Benioff was in an equivalent position so far as responsibility is concerned, but he was, especially at that time, primarily occupied with the problems of instrumentation, which were very important and to which he contributed a great deal, because it had developed that any really precise work in locating earthquakes occurring at short distance was going to depend on a higher degree of timing precision than was ordinarily available. It was quite clear that we needed to work with timing to a tenth of a second, and that is a severe demand on any clock and recording system. Benioff came up with a fairly satisfactory practical solution, which eventually was applied throughout the system. Naturally, like all new things, it had some bugs in it which had to be taken out. But, nevertheless, it was a major piece of work, and he went on refining it during the subsequent years.

Scheid: Wood was directing your research then? Was that true?

Richter: It doesn't give the right picture. I was working with Wood. We would get together and discuss the results. I was constructing small maps month by month of what we seemed to be recording. Not long after I became attached to the program, Dr. Day and the Carnegie Institution set up a plan for a conference at the Laboratory inviting distinguished guests and visitors to review and evaluate what had been done and to suggest procedures for the future. So as soon as that became evident, naturally both Wood and myself were more concerned than before with preparing material for exhibit, summarizing the results and lists and maps and so forth for presentation at this forthcoming conference.

Scheid: But you were more or less on an equal footing with Wood as far as carrying out the research was concerned?

Richter: Well, that would be an overstatement. One of the things that happened was that Wood had an excellent fundamental background in physics, but had not concerned himself to any great extent with any very exact mathematical and geometrical operations, and even on the simplest assumptions which we started out by making about the interpretation of our recordings there was quite a lot of room available for developing procedures using a map and compass to locate the earthquake epicenters on our map and assign coordinates to them, and I did quite a bit of purely geometrical work. I was following at first some procedures which had been worked out and published in Japan.

Scheid: Was Wood involved in other projects besides your particular project when you first went there?

Richter: Naturally, he was in a supervisory position, so he was discussing the instrumental developments with Benioff and also the problems with respect to timing. He had a great deal of correspondence with various people in the seismological field, and at that particular time I don't think he was preparing anything in the way of a paper for publication, though he did so later on. There were two projects that I'm hung up a little about on the matter of date. They were not going on at the time I first got into the Laboratory, but they did develop within two or three years. One was the history of earthquakes in the California region, on which Wood had published a very considerable paper much earlier--about 1916. He was engaged particularly with Maxwell Allen, who was a very brilliant and active amateur who had been interested in the history and had gone to great trouble to visit libraries and ferret out old documents bearing on the matter. This bore fruit finally in the form of a publication which was issued through the Coast and Geodetic Survey dealing with destructive or near-destructive earthquakes of the California region.

Scheid: I would like to ask about Maxwell Allen--was he around when you were there working at the Laboratory?

Richter: No, in fact I didn't meet him for a couple of years. He was living at the time somewhere upstate--let me see, at Sanger I believe, in the San Joaquin valley. Of course there was a good deal of writing back and forth between him and Wood.

Scheid: But he was an amateur, you say?

Richter: He had very little, if any, academic background, but he was a very alert person and a good thinker; he published a couple of papers later on which appeared in the Bulletin of the Seismological Society.

Scheid: This history of earthquakes in the Southern California region, was that just within recorded time?

Richter: Well, yes; naturally for the earlier years it had to be based on non-instrumental reports. The first definitely known California earthquake took place in 1769--it was experienced by the Portola party which was the first Spanish land exploration in California--so that ordinarily appears at the head of lists of California earthquakes. And anything back of that, of course, is inference.

Scheid: Were there attempts at that time to look at faults in the ground and try to determine when those quakes had occurred, or when the movements had occurred? Was there any systematic attempt?

Richter: Certainly not systematic. There was a certain amount of studying of faulting in the area by various geologists, and Wood collected a good deal of data of that sort from various sources. In 1922, the Seismological Society had published a fault map of the state, which was divided north and south between Bailey Willis and Harry Wood. So this was of course a matter on which Wood maintained interest and kept up with. The Carnegie Institution had a system of regular reports to which Wood contributed annually--an account of what was going on in the Laboratory program and any preliminary results which could be stated.

Scheid: Do you know to what extent the Carnegie Institution really directed your research?

Richter: Very little actually. What had happened was that through Dr. Day, Wood had presented a program for what might be done. That had been evaluated by a committee under the Carnegie Institution, and the conclusion of the committee was yes, the program should be activated and Wood should be put in charge of it, so that was the administrative situation for quite a number of years.

Scheid: Were the decisions about the kind of research to be carried out made by Wood, then?

Richter: Primarily, yes. Of course, as I mentioned, the conference of 1929 was called in order to evaluate and discuss what had been accomplished. Naturally, after that there was a redirection--Wood was still in charge, but he was also following to a considerable extent the recommendations and suggestions from the steering committee.

Scheid: And who was on that steering committee, do you remember?

Richter: I'm afraid I can't give you a membership. It's on record if you could find Arthur Day's paper on the founding of the Laboratory and so on. Let me see, originally Ralph Arnold was on the committee, a well-known geologist, and Bailey Willis, who had an even better known international reputation. Oh yes, and Dr. John Anderson.

Scheid: From Mount Wilson.

Richter: Yes.

Scheid: Well, what was your presentation at that conference?

Richter: In general, showing maps of where we thought we had located the earthquakes and quite a number of characteristic seismograms to show how we were interpreting them and what problems we were running into in doing so.

Scheid: What was the general result of the conference?

Richter: I think the sense of the conference was: well, this is fine, but we should now get in some additional member of the staff--how shall I put it?--who could raise the operations to a higher research level. And you see, it was clearly with that in mind that both Jeffreys and Gutenberg were invited to that conference. They were the two outstanding people at that time whose names would have occurred to anyone anywhere, and it was fortunate that the Institution swung enough influence to be able to get them both with us. Of course, undoubtedly, good results would have followed if the choice had fallen on Jeffreys, but I can't help feeling that for just the particular special circumstances, Gutenberg worked out better.

Scheid: Wood still remained as the director of the Laboratory?

Richter: Actually, the title of director was not used until very many years later when it initially was attached to Gutenberg. Wood, if I remember right was Research Associate in charge. I think there was some objection or feeling within the [Carnegie] Institution to the use of the title of director unless it was really strongly called for.

Scheid: Did this mean that you began to work more under Gutenberg rather than under Wood?

Richter: Well, at least, with Gutenberg. Of course, Gutenberg came in with a great deal of force--some fresh ideas and considerable curiosity as to the material we had already assembled. At his invitation and with Wood's permission, I started to work with him part of my time on certain publications. The very earliest one was one for which Gutenberg had been getting together the data and had it fairly well on its way to publishable form, but it was to be published in English so I had the job of writing the text.

Scheid: He didn't publish in German after he came to this country?

Richter: Only occasionally.

Scheid: Was that also because the main activity in this field was in this country?

Richter: Well, it was primarily I think due to the political situation in Germany. He retained his editorship of the Handbuch der Geophysik for quite a few years, until finally under the Nazi regime they managed to pull the rug out from under him and someone else took over the editorship. He would write various short reviews and notes, which might get published in some of the German publications, but he had obviously definitely made up his mind that this was where he was going to stay, this was where he was going to work, and he was consequently working and thinking in English, all of which was to the good.

Scheid: What did Wood carry on with then after Gutenberg had come? What sort of projects was he involved in?

Richter: Now, I am finally reminded of the second major project which I meant to mention earlier, the date of which is very definite--1931. This is the modified Mercalli intensity scale on which he was working in collaboration with Frank Neumann who was then in terrestrial magnetism actually, but he was under the Institutiton. And this was a recension of a later form of the Mercalli scale with the idea of making it more suitable to application in this country. It was ultimately published as a collaboration between the two, and of course, I was involved with it to a considerable degree going through the material with Wood and discussing critical points and the like.

Scheid: What was the nature of the modification?

Richter: Partly just wording. First, Wood was working from a version of the scale which had been prepared by Sieberg which was in German and, in some ways, very Germanic. Not only translation but shifting in emphasis was necessary, and there were some fundamental misapprehensions in the setting up of the scale and the transfer to this side of the Atlantic, which weren't completely cleared up even in the work of Wood and Neumann. I suppose I should be explicit--the grades of the intensity scale are described in large part by their effects on ordinary works of construction. Of course, ordinary works of construction differ very much in their nature and solidness and particularly resistance to earthquake shaking in different parts of the world, simply because different materials and methods are used. So that to transfer conclusions which had originally been more or less drawn for central Europe without more ado to the California region, resulted in some very curious misunderstandings, some of which still persist, I'm sorry to say. I think that's about as briefly as I can respond to your inquiry as to the nature of the revision, but this is pretty well covered in my textbook in the chapter on intensitities and in an appendix.

Scheid: Was Wood then going out and studying damaged buildings?

Richter: On occasion he did so, yes. Of course a good part of any seismologist's experience is to go out and see what happens in various earthquakes. Shortly before I became attached to the Laboratory, there had been the Santa Barbara earthquake, which of course he had gone out and inspected, and which had been the subject of a great deal of investigation, and here I should mention also that he had an enviable degree of prior acquaintance with earthquake effects, because he had investigated the consequences of the earthquake of 1906 in San Francisco. In fact, it was he who carried out the investigation and prepared the mapping for the city of San Francisco, which appears in the Earthquake Commission Report on the 1906 earthquake. So the answer to your question as to whether Wood was experienced with earthquake effects is very decidedly yes.

Scheid: But when he was down here, did he go out of the Laboratory and make these investigations regularly?

Richter: He had on the occasion of the Santa Barbara earthquake. Thereafter there was nothing in this area which urgently called for that kind of attention, and I do not remember his going out. I remember going out myself for the experience and checking around the field for effects of several small earthquakes in our immediate region, not expecting to find anything critical but simply for my own benefit. There was the Whittier earthquake of 1929, which had some interesting features, and after the event I remember Wood went around with me and looked at some of the things I had checked up on in the field. Then in 1933, there was the catastrophe of the Long Beach earthquake--that constituted almost a project by itself.

Scheid: For the Whittier earthquake, did you just investigate buildings or did you also make other observations?

Richter: Well, there were very few other observations to be made since there were no obvious effects on the ground.

Scheid: At the same time, Gutenberg was publishing too, or doing research. In contrast to Wood, what kind of thing was he concentrating on?

Richter: One of his first principal projects was actually not in contrast to Wood. He gathered together the seismograms and data for the larger or better-recorded local earthquakes since the program had been underway--which was only of course three or four years--and used those then to set up a paper on the earthquake wave times and the corresponding structures in Southern California and incidentally made the locations of the epicenters of this series of shocks.

Scheid: When was this idea of locating epicenters fully developed--or was that even much more recently?

Richter: It's a matter of gradual progress. At first we were operating with only a few stations and with very simplified assumptions, which sometimes got rather peculiar results, but on the whole rather better than we would expect considered in light of later events. Naturally, as we got more stations and better timing, the procedures modified and to some extent improved.

Scheid: So it was a matter of timing? Was that the important factor?

Richter: Well, it's also a matter of interpretation. The recording of many small local earthquakes at short distances is rather a simple type of recording consisting of two principal impulses, the P and S--the principal waves of the P and S group. But as soon as you get even 100 kilometers away you begin to get complications, and a real analysis of the data had to await the installation of more stations and the refinement of the procedure. One matter which was a long time getting settled was the general depth of origination of the earthquakes, because that is very difficult to make precise unless you have several stations at a very short distance from the epicenter, and of course especially in the initial part of our program with few stations, that wasn't happening very often. So it was natural that our assumptions as to depth and our procedures for approaching that underwent several changes and several levels of refinement. Of course with a degree of instrumentation available now today, such problems are no longer very serious. But in the early stages, they were very difficult indeed.

Scheid: Did Gutenberg continue along this line of taking the instrumental data? Was that the basis for his work?

Richter: Yes. The particular paper I spoke of was for quite a while his principal contribution to the local earthquake problem. Naturally, he had a global interest in seismology, and we were getting some very exciting new recordings of distant earthquakes largely due to the characteristics of some of the new instruments which Benioff had put into operation. So that started a very extensive program of studies on seismic waves--introduced a great many new facts and ideas--in which I was participating with Gutenberg as co-author.

Scheid: Wood was not using the instrumental data as much. Is that correct?

Richter: Yes, I think we could say that.

Scheid: He wasn't interested in that side of it, or he didn't feel that was fruitful?

Richter: Oh no, to the contrary, I think he was very much interested, but he was also interested in the degree of progress which was being made, and he had these other projects on hand which I mentioned--the intensity scale and the history of earthquakes--and, of course, within a few years he was attacked by this very incapacitating illness or he would undoubtedly have been more productive on the whole.

Scheid: I see. When was that, do you remember?

Richter: I'm just trying to remember; I wish I had the date in hand. It came upon him very suddenly. It was a spinal infection, and it kept him out of activity, first for a couple of years, and then in returning to rather incomplete activity, and then put him completely out of the Laboratory and of course, finally, it killed him.

Scheid: This was in the thirties.

Richter: Yes.

Begin Tape 7, Side 2

Scheid: Did Wood have any other assistant in the Laboratory at this time other than Benioff?

Richter: Yes. There was some coming and going, but when I arrived to work there, there was Archie King, who was in charge of the shop and such instrumental developments as were going on, and Halley Wolf, who was doing some of the photographic work and daily changing of records and also acting as secretary in the office.

Scheid: Photographic work--you mean photographing of the seismograms?

Richter: Developing. First, of course, we were running quite a number of photographic recording instruments at the Laboratory, and those had to be put through the photographic process daily. In addition, the seismograms recorded at the outside stations were being mailed in in light-tight containers so that they were developed and fixed when they arrived up in the Laboratory.

Scheid: You were assisting Gutenberg, but was there anyone assisting Wood in the same way, on the same level?

Richter: Well, I would say that very definitely for a number of years I was Wood's assistant. I was working with him and finding time to work with Gutenberg as that was available. Of course, a considerable amount of my time was going into the purely routine work of going through the recorded seismograms, cataloging the results, and finding out where the earthquakes were happening. Incidental to that was the setting up of the magnitude scale which actually, once underway didn't consume a very great deal of my time.

Scheid: There was a collaboration with Mount Wilson, with the people up there, to some extent. Could you comment on that?

Richter: Well, I don't have many details. Of course, especially earlier, Wood was working very closely with Dr. Anderson, and they developed the torsion seismometer between them, I might say, and a paper was published, joint authorship, describing the theory and application of the torsion seismometer. The very first tests of the new instruments were run at the Observatory offices. Then later on things were moved, first temporarily to the Caltech campus, then finally everything went to the new Laboratory.

Scheid: By the Observatory offices, you mean the buildings up on Santa Barbara street?

Richter: Yes.

Scheid: Was that where Dr. Anderson worked most of the time?

Richter: Yes.

Scheid: Was there any other collaboration going on that you remember other than on that particular project?

Richter: I think some of the development work on the seismometer and so forth was done in the shops at the Observatory, but I just don't have the details in mind. There was a minor but not completely unimportant contribution, in that a substation was eventually set up on Mount Wilson itself. Of course, naturally, that was with the cooperation of the Observatory administration and was no very great matter--it involved shelter, and somebody to change records once a day.

Scheid: Did the fact that you were both being funded by the Carnegie have anything to do with the collaboration?

Richter: Originally, yes, because I would say it was a matter partly of Wood's personal acquaintance with the Observatory staff, not only Dr. Anderson. Of course, Wood knew Dr. Hale very well, and Hale was involved in the procedure for getting the Laboratory set up under the Carnegie Institution.

Scheid: Yes. Was it at Hale's instigation that the Laboratory was set up there?

Richter: No, I think that was acting largely on Wood's proposal. See, the administrative procedure was that the program got assigned under the Geophysical Laboratory at Washington, of which Dr. Day was the head, so that throughout the whole process of development, Day was Wood's immediate superior.

Scheid: So it was set up in Pasadena where they already had the Carnegie installation at Mt. Wilson.

Richter: Yes. Well, there was a fairly obvious choice, and indeed, Pasadena or somewhere else in the Los Angeles area had been suggested years before as a possible center for such a program.

Scheid: I see. Did Millikan also play a role in urging them to have the Seismological Laboratory here? Do you know at all about that?

Richter: No, he couldn't have. Because, you see, the program was set up and funded in 1921, so that it was a going concern by the time Millikan got to Pasadena.

Scheid: Perhaps it was an extra activity which attracted him--I don't know.

Richter: I don't think, at first, Millikan was very much directly interested in it. Not that he had anything against it; but, as I suggested just now, he found that a going concern when he arrived here, and naturally there were other things for him to do. It was under the Institution, and he would consequently not have considered anything more than ordinary cooperation with it, as he would with the Observatory.

Scheid: Yet, when the funding began to get low in the thirties, he and Caltech took over the funding for the Seismological Laboratory.

Richter: Yes; well, that was a matter of long negotiation, and there was a certain amount of transition. Some of the funds continued to come from the Institution for specific support but that was tapered off by arrangement.

Scheid: You mentioned that Hugo Benioff was mostly involved with instrumentation and his major contribution to that was what?

Richter: First, of course, he had other interests and produced some good results mostly later on, but certainly his chief abilities were in that direction. As I mentioned, he set up the first accurate time control system for the laboratory work, which was a major contribution. Then he had developed the new form of seismometer, the Benioff instrument. Along with that went a number of minor auxiliary developments, all of which were of considerable interest. Years later, when he was working in the Laboratory, he got very much interested in the positive mechanism of earthquakes and developed some ideas, which have I think pretty well consolidated with the subject.

Scheid: So they haven't been superseded by anything?

Richter: They cannot be, because Benioff was I think the first to strongly emphasize the necessity for considering non-elastic effects in connection with the cause and occurrence of earthquakes. The previous theories, notably that of Harry Fielding Reid, had been set up on the assumption of an ideally elastic process, which is a good first approximation to the facts, but it doesn't cover everything. It was Benioff's distinct contribution to show how that could be taken into account in a more satisfactory theory.

Scheid: So he wasn't confined to instruments then, as he had feared.

Richter: No, he always had a certain complex about that, which I think drove him a little farther than necessary in certain directions.

Scheid: You mentioned the Long Beach earthquake. Did that have an immediate effect on your activities there, in the long term?

Richter: Oh yes, it had a very critical effect. Of course, naturally, it threw our routine procedures into confusion, because we were swamped both with earthquakes and with other demands on our time. From the instrumental point of view, it was a very fortunate opportunity. We got into the field almost at once with portable instruments and established some of the major facts about that earthquake as an event, which held up pretty well in the light of later occurrences and investigation. And, of course, it was a very serious occurrence for the public. It would have been in any case, but coming as it did right at the bottom of the economic depression, it was a very serious matter indeed. Naturally, there were two resulting major contributions to public safety: it finally scared the state legislature into enacting the Field Act which contributed toward the safety of school buildings, acting on the object lesson of what happened to school structures in that earthquake; and it set to rest the idea which had been put forward by a certain number of people to the effect that the California earthquake risk is all in the San Francisco area, and we don't need to worry about Los Angeles.

Scheid: You said you went out with portable instruments. Exactly where did you go and what did you do?

Richter: Oh, half a dozen different places scattered along the course of the active fault. I remember the first evening we went out somewhere in the vicinity of Laguna, and during the aftershock period we took an instrument over to Catalina Island and ran it there for a day or so.

Scheid: But the first evening--it did occur in the evening didn't it?

Richter: 5:54.

Scheid: Yes, and you were already out that evening?

Richter: No, we were out the next morning.

Scheid: I see, the next morning. Were there a number of you who went? Who actually participated in going out, do you remember?

Richter: Three, I think--I really can't tell you who was with us.

Scheid: You set up your instruments and then waited?

Richter: Well, there wasn't long to wait. Earthquakes were occurring continuously. Naturally, as one might expect on that first outing, the instrument had had no prior field test so that it developed quite a number of deficiencies. But we got something out of it, even on that first expedition.

Scheid: Did you do other field investigations in this period in the early years of the Laboratory? You mentioned Whittier.

Richter: Yes, that was earlier. I may have been out on one or two minor occasions, but the next one of principal consequence was in 1940, the Imperial Valley earthquakes.

Scheid: I see. Were you out there for quite some time?

Richter: A couple of days initially, and then on short visits later on to take further observations.

Scheid: Did that extend over a long period of time?

Richter: Of course, we had instruments set up which were recording the events at our permanent stations. We didn't accomplish very much with portable equipment on the 1940 earthquakes.

Scheid: You mentioned also that you had been on a trip to New Zealand at some time, or a number of trips to New Zealand. I wondered what was the purpose of those trips.

Richter: The first occasion was in 1949. It was one of the Pacific Science Congresses, so it was an opportunity, of course, to meet with other people and go out on excursions and get acquainted with the region, and I felt it was an opportunity not to be missed. What had happened was that Gutenberg had been specifically invited, originally, and pulled strings and managed to get me to go at the same time. So I learned a very great deal indeed, not to mention falling in love with New Zealand.

Scheid: Who were the people who were there; where were they coming from?

Richter: All over the world, practically. Naturally, the people who were working there locally were in evidence, but, yes, there were visitors from the entire region. It was after all, the Pacific Science Congress.

Scheid: It was a general congress then--not merely devoted to seismology?

Richter: Oh, very definitely. In fact, seismology and geology were only a fraction of what went on, but there was plenty in those fields to occupy them.

Scheid: Was this your first contact with Japanese seismologists after the war?

Richter: I think we had had Japanese visitors in Pasadena briefly, but I don't remember any considerable contact with them. It was some years after that when Dr. Tsuboi arrived and stayed here with us in Pasadena, and, of course, that was a very satisfactory and productive event.

Scheid: What was the purpose of the congress do you think at that time, in '49?

Richter: Well, I think there is no individual purpose in these congresses. They are fairly regular biannual affairs.

Scheid: You mentioned you liked New Zealand very much. What was the particular fascination of that country?

Richter: It's a very beautiful country, and the people are wonderful. Even now, it's getting more populated, but it's still relatively unspoiled and has a great many of the attractions that California had when I was a boy.

Scheid: I see. You've been back then since then?

Richter: Twice.

Scheid: In what years?

Richter: 1970 and 1977.

Scheid: Is New Zealand particularly interesting to you, seismologically?

Richter: Very much so. Of course that was what drew me there in the first place. From what I knew of it, it was quite clear that here was a country with the seismological conditions and problems in many respects very closely parallel to those in California, and, in some other respects, decidedly different in the most interesting fashion. So when I came to write my textbook, I devoted a large part of the discussion to correlating what was to be observed and concluded in the two regions.

Scheid: Was it also interesting to you for other reasons? You mentioned that you were interested in plants and botany as well.

Richter: There are many interesting trees and plants in New Zealand, and some of the commonest are introduced exotics, which they rather swear at. And, of course, especially in the South Island, the scenery is very beautiful.

Scheid: It's Alpine, isn't it?

Richter: Well, yes, it's the New Zealand Alps! Southern Alps, I believe is the proper term.

Scheid: It is reminiscent of the Sierras?

Richter: To some extent, yes. There are some geological differences and the elevation is not quite so high, but high enough for all practical purposes.

Scheid: You spent a good many days hiking there--is that one of your main activities when you go?

Richter: No, opportunities did not offer very much of that even on the earlier occasion. I remember one day when we did have quite a distance to cover on foot, and I acquitted myself of that very well, but in general, we were moving around mainly by bus, occasionally and less commonly by air, and walking comparatively short distances.

Scheid: When you went in 1970, was that also to a conference?

Richter: Yes, that was more specifically in my field. It had to do with--I may not have the exact title--recent tectonic movements. Of course, they are to be seen there in New Zealand and also in other parts of the world.

Scheid: You also traveled to Japan.

Richter: Oh yes, I was there as a Fulbright Scholar.

Scheid: Were you involved in any kind of teaching or lecturing there?

Richter: No, except occasional short talks for a seminar, something of that kind of thing. No, I was mainly discussing things individually or trying to read scientific papers in Japanese and getting around and getting some experience in the country.

Scheid: Well, it must have been interesting seismologically, too.

Richter: Oh, yes, extremely interesting, and in many respects, very different from my previous experience.

Scheid: In what way?

Richter: Well, there is relatively little proportion of the kind of tectonics we have in California and New Zealand, with these conspicuous active faults, although they are not unrepresented in Japan. The volcanic element and activity is more significant in the Japanese region. The large-scale plate tectonics in relation to the Pacific basin is distinctly different, so that there is a whole range of quite separate problems arising in the Japanese region which do not arise here or in New Zealand.

Scheid: Do you mean by that that the Japanese region is situated differently in relationship to the plates than New Zealand and California?

Richter: Very decidedly different from California, because Japan is one of the regions of the Pacific active arcuate structures. Here in California we are in an intermediate region between the arcuate structures of Alaska to the north and Mexico to the south; and in New Zealand the North Island is on the southern edge of an arcuate structure trending down through the Pacific, whereas the South Island has structure and tectonics more reminiscent of

California.

Scheid: I see. They are quite separate then on the two islands.

Richter: There's no sharp line and there is an overlapping and graduation between the two types of structure, which I feel is not as yet completely unravelled, though there's a lot of very good work going on toward it.

Scheid: And the Japanese situation is more complex?

Richter: Very much more so--highly complex. It's considerably to the credit of the Japanese that as much progress has been made with it as actually is the case.

Scheid: When you were in Japan, did you go out to a number of stations and look around or did you work with data in the laboratory or . . . ?

Richter: I got out to several other locations. For example, I was a short time at Sendai, where Dr. Honda and some other people were doing a lot of very good work and visited the station at Kyoto where there is also an active group. Finally, we were taken on an excursion to Matsushiro, which is a station underground, under the spine of the main island of Japan--a very interesting location. Some years afterward, it was distinguished by the occurrence of one of the most remarkable swarms of local earthquakes on record.

Scheid: It is right directly on a very active area?

Richter: Active area is a good statement, because again it's not one of these cases of a major fault. Apparently, it is a response to what are fundamentally rather volcanic processes, tectonic in the narrow sense, but the result is a great many earthquakes.

Scheid: Isn't the volcanic process related to the plate arrangement?

Richter: Oh yes, but it's a little complicated. I would illustrate that relationship by drawing a cross-section through the thrust plate and show you just what the circumstances are. This is all in the books. You see, throughout the world, generally speaking, if you take a very small-scale map of the world and look where the active volcanoes and where the major earthquake epicenters are, they look to be rather closely related. But as soon as you go into any active region and examine matters on a larger scale, you'll find a separation. Just as here in California our principal earthquake activity is in the coastal region, associated largely with the San Andreas fault. But our volcanoes are inland running from Shasta north.

Scheid: Otherwise in Japan, did you go to the university? Is that where you were stationed?

Richter: Yes, at the University Earthquake Research Institute which is right on the campus.

Scheid: Is it a similar situation to here where the buildings are all together in one place?

Richter: Yes. It's a rather good-sized, rambling campus. There are others.

Scheid: And were your main contacts there were with other seismologists?

Richter: Yes, with a number of people, mostly younger, and most of them with not quite enough English for good communication, though good for cordial relations. My best contact was always with Dr. Tsuboi whose English was perfect.

Scheid: Did you meet other Japanese people that were outside of seismology?

Richter: Occasionally, yes, particularly socially, because I was with the Fulbright group. We would be going around, and of course my wife was there, and she had her social contacts, and finally she was even teaching English to a small group of Japanese.

Scheid: She was interested in art I believe, was she not?

Richter: Well, not in a productive way, if that's what you have in mind. No, her background was almost exclusively teaching. She was interested.

Scheid: But you mentioned before that when you had your house designed by Neutra that she had known him or known someone that had known him, and that's why I asked the question.

Richter: Well, that was true. That is, she was definitely attracted by the idea, and then there was a second-hand personal acquaintance, so that was set up on a rather cordial and effective basis.

Scheid: So she was aware of the movements in architecture in Europe, particularly, and its exponent in Los Angeles.

Richter: Well, yes. I can't speak for her. I didn't know much about it myself. In this matter I was pretty well led, because it didn't matter as much to me as it did to her. After all, it was her house.

Scheid: Right. Did you attend any cultural events in Japan that made any particular impression on you or meet anyone that was not in science?

Richter: Well, as I said, there were casual contacts. Naturally we went about and saw the things that tourists see and attended one performance of the Kabuki theater and that kind of thing.

Scheid: But you were quite interested in the language, primarily.

Richter: Oh yes. I must say it is a fascinating language. Some of its characteristics are unbelievable.

Scheid: You mean the structure of the language?

Richter: The structure of the language, and, for God's sake, the way they write it.

Scheid: Well that's the hard part, I have a feeling. Did you actually practice that to some extent--the calligraphy?

Richter: Oh, no. No. If I had to write something I could always take a paper and pencil and make it recognizable. It might not be just right, but a Japanese could make out what I was trying to do.

Scheid: I see. Did you study it before you went?

Richter: Yes, we got hold of elementary books and developed some acquaintance.

Scheid: So you weren't entirely cold, when you got there.

Richter: No, not to that extent.

Scheid: You've never been back to Japan?

Richter: No, I haven't.

Scheid: It doesn't hold the fascination that New Zealand does?

Richter: I suppose that's true. Of course I probably would enjoy going back to Japan as a tourist with adequate funds, and I would probably want a different relation between the yen and the dollar.

Scheid: Right now I don't think it's a good time.

Richter: Too expensive. But there is no hospitality like that of a characteristic Japanese inn. You're being treated as the spoiled child of a well-to-do family.

Scheid: I see. You did go out into the inns fairly frequently?

Richter: Well, on occasion, and that gave me opportunities to see a number of places in which I had professional as well as casual interest.

Scheid: Well, what exactly happens when you come to a Japanese inn?

Richter: Well, I don't think my recollection is very clear, but of course, you are at once greeted by the manager and shown to a room. Then, they will proceed to get you to shed your clothes and put on comfortable robes, and probably there will be a bath coming up.

Scheid: The Japanese baths are quite famous, I guess.

Richter: Yes.

Scheid: Very hot?

Richter: Well at least they like them that way. They have a certain pride in going in very hot water. I never got that far in my experience.

Scheid: How did the food agree with you?

Richter: Generally I had no particular trouble. Of course, on the whole it is and can be very good. In Tokyo, we were living in this apartment, and we had this very capable maid, and she made a point of being able to cook what she claimed was any one of three styles. Western, Chinese or Japanese, so we didn't lack for variety.

Scheid: Well, I think maybe that's about all we have to talk about unless you can think of some other things. Would you like to talk?

Richter: Oh well, of course, I could go on chattering by the hour but it would be more or less at random, and I doubt whether there's anything worth putting in time on. I think somewhere in those tapes which already exist I've probably expressed myself sufficiently on the need of getting rid of the dangerous buildings in this area. That's the one thing about which I do feel a certain degree of responsibility. Although for this purpose--after all this is for the Archives--it doesn't matter so much but nevertheless, I never get up to speak on a public occasion without finding some means of underlining that critical situation.

Scheid: Yes. You have a name that's very well known, you could probably say this more often--or do you think people are getting tired of hearing this?

Richter: I don't know whether they're getting tired, but naturally I have said this on every available occasion, and so have other people who have the public ear. Here for example, I just happened to pick this up. This is just out, an interview with Dr. Allen, and somewhere in here is made that very same point. Naturally, as usual they've got him talking mainly on the subject of prediction, but nevertheless he has managed to make this point. I used to get very emphatic about this sort of thing. I used to say "What in the world do you want prediction for? Just get rid of those old buildings and nobody will be killed."

Scheid: Yes, right. It's the popular press, though, that needs to have that kind of information, don't you think?

Richter: Well, all I can say is that it has appeared again and again and again, and it will come to the front. Even this damaging little earthquake the other day in the Santa Barbara area produced some general statements to that effect from people who were in a good position to make them. So I don't know what more one can ask from the news media, if they put reliable information into circulation, and refrain from cheap sensationalism.