Charles F. Richter – How It Was

Charles F. Richter, professor of seismology emeritus since 1970, has been considered a master interpreter of earthquakes for most of the last 50 years. This may seem somewhat unusual because he graduated from Caltech with a PhD in physics. It was the opening up of a research assistant's job at the newly established Seismological Laboratory in 1927 that deflected him, and one result, in 1935, was his first public enunciation of a way to measure the magnitude of earthquakes. Richter's name has since become a household word in seismology – and grist for the mill of the Oral History program of the Caltech Archives, for which he was interviewed by Ann Underleak Scheid. E&S *presents here an excerpt from the transcript of those interviews that describes his background and some of what led to the development and worldwide use of the "Richter Scale."*

Ann Scheid: Let's start with some of your background, your childhood and early life.

Charles Richter: To begin with, the name Richter is actually my mother's maiden name, which she resumed after a divorce, and I have never been known by any other name. It is, of course, the name of my maternal grandfather, to whom I owe practically everything I am 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 his small son who was then about four years old — my grandfather, Charles Otto Richter. The family was at first in New York, and not long before the Civil War they moved to Richmond, Virginia. In later years my grandfather was with a 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, which then included only my grandfather, mother, and myself with an older sister, moved to California in 1909.

AS: So you went to school in Los Angeles?

CR: Yes, a short time in the public schools and then at the age of 12 I entered the preparatory school for USC, which was at first Southern California Academy and later University High School. Still later it 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.

I should explain that 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.

AS: You went to college?

CR: My first year of college was as a freshman at USC, but from there I transferred and went to Stanford. There, as I mentioned, I took a chemistry course first, and that didn't seem to be satisfactory. Gradually I got into physics, which was more congenial. 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. After some unfortunate experiences, I felt it wasn't for me.

AS: You finished Stanford at quite an early age. How old were you?

CR: Twenty.

AS: And then you came back to Los Angeles?

CR: Well, I did finish my AB in physics, and then I found other things to do, and in particular employment. My first job was as a messenger boy at the Los Angeles County Museum. 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. By 1923 the former Throop Institute of Pasadena was reorganized as Caltech, and Dr. Millikan came to take charge and also to lecture. 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. Eventually I became Paul Epstein's student, and I owe a very great deal to him.

AS: Would you describe Epstein a little bit, as a person, as a lecturer, as a teacher?

CR: 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. He was very much absorbed in his subject anyhow.

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. This was no special difficulty for me because I approved of it heartily, even though it was not always easy. Nevertheless, it was not that he had to push me to try to do things right; I had to push myself to get them right.

AS: When you taught, did you attempt to emulate that?

CR: 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. Later on, when I came to give a course in elementary seismology, things were better. 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. Paul Epstein was by no means the only member of the Institute staff who was capable of maintaining high standards.

Also 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 scale should be defined in terms of energy, but to do that would have involved continuous revisions, both numerical and theoretical. I have always insisted that the magnitude scale represents what we observe, and this may or may not be interpretable in terms of energy.

AS: You did your thesis under Epstein?

CR: Yes, though actually the topic came about through Dr. Millikan. Millikan had received a letter from Paul Ehrenfest, which was in German (which Millikan could read perfectly well), describing the results that G. E. Uhlenbeck and S. A. 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 that had been coming up in atomic theory just at that time. Millikan asked me to look this over, and I 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.

AS: What was your thesis topic?

CR: It was on the hydrogen atom with a spinning electron. 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 Max Born, Werner Heisenberg, and Erwin Schrödinger with quantum mechanics, and there was almost a second thesis dealing with the same subject from that point of view.

AS: Schrödinger came to Caltech at about that time. Do you remember him at all?

CR: Quite vividly. He was a decidedly good lecturer, and he was speaking on a very fresh and new subject. I owe to him one very priceless general remark that 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. The problem was how to generalize it so that it would take into account the special theory of relativity. He was about to outline the procedure he favored at that time; then he stopped, saying, "Now, of course, the generalization is not unique." He stopped again and then said, "Of course not, otherwise it would not be a generalization." I always enjoy repeating that because it is a very profound observation.

Another story I like to tell is about E. T. Bell, the mathematician. He was a highly original and imaginative person, and naturally this was expressed in his work. Also, he had facility in writing which was evident in both his scientific contributions and in his fiction. He was a well-known author of science fiction under the name of John Taine, and he had several worthwhile books dealing with mathematics. My story is illustrative of both the man and the subject. I had come upon a rather general proposition on factorability of expressions that I thought might be interesting to put forward, as can be done in mathematical publications, simply as an outstanding question. I wondered if someone of better ability than mine could make sense of it. So I told Bell about this and 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, . . .'"

AS: In graduate school were you doing physics just for the love of it, or were you thinking about what you would do afterwards?

CR: Not very specifically. This was before the Depression, and I was not yet married, so I was pretty well free. What was in the back of my mind, of course, was that if I stuck around, probably something would be found for me. When a position as research assistant at the Seismological Laboratory became available, Dr. Millikan recommended me.

AS: Were you sorry to leave what you had been doing in quantum mechanics?

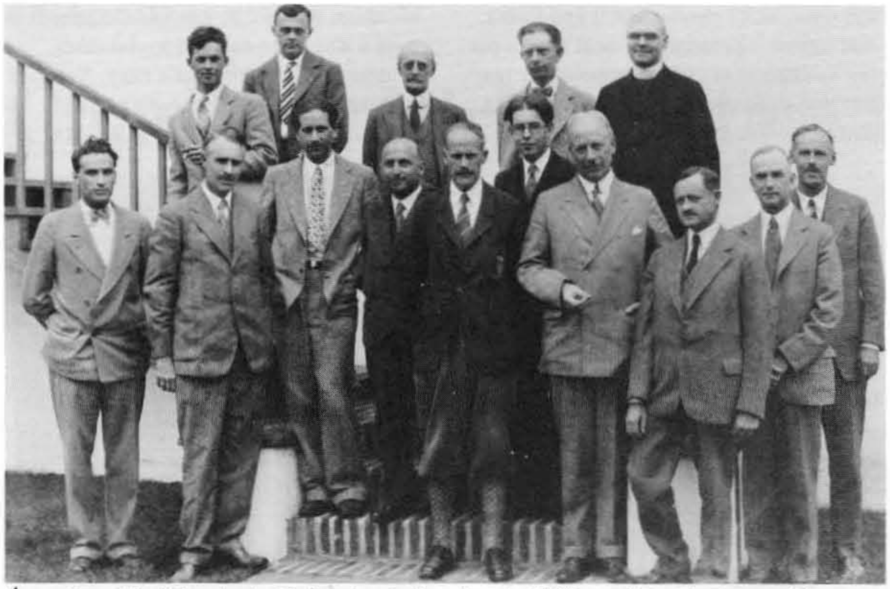
CR: I didn't feel that I was leaving anything, because so long as I could stay in or near Pasadena, I could keep in touch, which I did to a certain extent. The opportunity to work at the Seismological Laboratory provided me with the means to stay around here. And gradually I settled into the seismological work as my main occupation.

AS: You joined the lab at the very beginning. They had a building, I assume, that they were renovating or something.

CR: It was the "new" laboratory building then, and it's the "old" one now. It had been completed in 1926 and was occupied by staff in January of 1927. I made its acquaintance in the fall.

AS: There was already a staff there?

CR: Yes. Harry Wood was in charge, and it is largely due to his personal persistence and initiative that the Seismological Laboratory was established. He was a petrologist with an appointment at UC Berkeley at the time of the 1906 San Francisco earthquake, and he was a member of the commission that investigated that event and published on it. People still refer to that paper on occasion for details. From Berkeley, Wood went to Hawaii where he was at the Volcano Observatory for a number of years. After the First World War, he returned to this country and exercised himself in getting the Carnegie Institution to implement a proposal for a seismological network and installation in southern California. The program was officially set up in 1921 with Wood as research associate in charge.



A meeting at the Seismological Laboratory in Pasadena in 1929 brought together many of the world's leading authorities in earthquake research. It also led to an invitation to Beno Gutenberg to come to Caltech. In the front row (left to right) are Archie King, L. H. Adams, Hugo Benioff, Beno Gutenberg, Harold Jeffreys, Charles Richter, Arthur L. Day, Harry Wood, Ralph Arnold, and John Buwalda. At the back are Alden C. White, Perry Byerly, Harry Reid, John Anderson, and Father J. P. Macelwane.

Hugo Benioff was on the staff when I came to the lab, but I didn't see much of him because he was incapacitated by a chronic illness. Later on he returned, and about 1931 or 1932 he did some of his very best work in connection with the laboratory. Archie King was the technical assistant, and Halley Wolfe was a young fellow who acted partly as secretary and partly as photographic assistant. John Anderson, who was on the Mt.Wilson Observatory staff, was a very good personal friend of Harry Wood, and they worked together on the development of the torsion seismometer, which became known as the Wood-Anderson instrument.

AS: Millikan was getting money from a lot of southern Californians for Caltech. Did he ever get any for seismology, or was that totally supported by the Carnegie Institution?

CR: The Institute had contributed certain funds to the program and in particular it was due to the generosity of one of the Caltech trustees, Arthur Fleming, that the laboratory building was established and constructed. Thereafter, the Institute took over the maintenance of the building and grounds.

AS: Since the area was seismologically active, there must have been people who would have been positive toward seismology in the belief that to learn more about it would be of an advantage.

CR: We always had a certain amount of support. It was 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. I wish I could give you a little more detail on that because it has been rather regularized, formalized, since then, and I would hate not to give adequate credit to the insurance associations who maintained a considerable interest in the programs. They also sent out some members of their own staffs into the field to prepare reports that are valuable.

This is an ongoing cooperation that rather improved with time. Actually, it was beginning to get under way before the 1925 Santa Barbara earthquake, but that event enormously accelerated it. Prior to that, earthquake insurance had been written very extensively in southern California with little regard to the actuarial soundness, and that event was a great shock with some companies suffering losses in claims that were disproportionate for a comparatively moderate event.

AS: Did insurance companies have scientific personnel and instruments?

CR: I think on the instrumental side their contribution has always been in the direction of funding or otherwise supporting operations that were ongoing. For one thing, their organizations took corporate memberships in the Seismological Society, which made 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.

AS: And perhaps in prediction as well.

CR: Yes. If there really were a very solid and established basis for prediction, you would find the insurance people backing it wholeheartedly. I think their attitude toward the efforts we're making at present is favorable, but naturally they have to see some definite promise before they can justify anything on a large scale. The people who have done investigation field work specifically for the insurance people have been engineers, but many of them 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 effects of a given earthquake have been published from the engineering side, and it is due to the studies published by engineers, and particularly those connected with the insurance organization, 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. That was one of the motivations in setting up the magnitude scale, because it was very clear that we were getting earthquakes in some parts of the world that were alarming in the amount of damage and even loss of life, and yet they simply weren't writing large records on the seismographs. In fact, that very circumstance over many years led to what really was over-estimation 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 that were far from earthquake resistant. One of the horrifying examples was the catastrophe in Morocco in 1960 with a loss of 12,000 lives in an earthquake with a magnitude of only something like 5.75.

So the insurance people and the better building organizations were on our side, and between them 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. One direct and productive result of the 1933 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 did not solve the problem of old and unsafe structures.

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

CR: No problem from the Institute side, provided that nothing was said that might be needlessly sensational. We felt a certain responsibility to keep the public informed, particularly as misinformation was often seized 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 in the general population as to the possibility of earthquakes and earthquake risk. This was from the very first. It was accentuated by the circumstances of the Long Beach earthquake, and by some of the wild, panicky rumors that got out at that time. We felt that it was the responsibility of Caltech toward the public to give out correct information on any matter of public concern. Earthquakes were a particularly critical area because they are subject to a great deal of honest misunderstanding as well as misrepresentation.

AS: I wonder if publication of fault lines and fault maps was a sensitive issue at any time.

CR: I don't recall that we ever had any very serious public relations problems, although occasionally some individual or group would take offense, or some uninformed public figure would sound off in the press — or somebody would write a stupid editorial. 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 we were not able to really start any expansion of the seismological program, which was urgently needed.

AS: You said that expansion was urgently needed. You mean setting up more stations to gather more data?

CR: We had had plans for more stations, more instruments. One thing I was particularly interested in was the development in use of portable installations. That went on with various accidents and mistakes, but it did progress gradually until finally we always had at least one portable unit that we could use in an emergency. The original idea was to take a portable unit 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 into operation, but it did work and did make a few useful recordings that contributed to our understanding of the event.

AS: The Long Beach earthquake was kind of a watershed in seismology in southern California it seems.

CR: 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. The Long Beach disaster put an end to that. And also, as I mentioned, it produced the first permanent action on the part of the state, the Field Act. The provisions of the Field Act were good, and the later school buildings constructed under them 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.

AS: Something I haven't asked you about yet is the coming of Beno Gutenberg. In 1930, wasn't it?

CR: Yes, on appointment. In 1929 the Carnegie Institution called a conference at Pasadena to evaluate progress to that point. Two very important visitors from abroad were there, Harold Jeffreys and Beno Gutenberg. 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 a 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. He had a professorship at the Institute, but he did not become a member of the laboratory staff as such, except as a courtesy. 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, contributing to the program, and even doing some research work on the records that were then available.

AS: 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?

CR: It was certainly a better position. In Germany, he had the position in Frankfurt of Professor Extraordinarius, which I think had a small stipend only. He was consequently depending for his living and the support of his family on the operation of the family soap factory. You didn't have to be independently wealthy to be an academician there, but the position he had was more an honorary one than a remunerative one. In addition, he was doing a lot of publishing and editing, which brought in some income. But, in general, the offer was attractive to him from an economic point of view, 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.

AS: He felt that he didn't have much of a future there?

CR: 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 a number of other people to get out of Germany before the storm broke.

At Gutenberg's invitation, I picked up a certain amount of work in collaboration with him, and we wrote one book together. I owe a very great deal to him and came to regard him with almost filial affection.

AS: These were the years when you were assembling the data that eventually became organized in the scale that is named for you?

CR: 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. 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.

AS: The purpose of the scale actually seems to have been more of a public one than anything else.

CR: 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 of 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 scale was greatly increased.

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

CR: 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, and we weren't getting anywhere with it. Then I got hold of a paper by a Japanese seismologist, Professor Wadati, and that gave me the idea of plotting up our data in a particular way. It worked out much better than I had expected and produced this definite numerical scale that practically fell out of the data. I showed this to Gutenberg and Wood separately, and they both liked it, and I went on systematizing it. Wood put a brief mention of it in his annual report to the Carnegie Institution.

AS: Is that where the term "Richter Scale" was first used?

CR: I called it the magnitude scale, and I refrained from attaching my personal name to it for a number of years. I think it was Professor Perry Byerly of UC Berkeley who started referring to it as the Richter Scale in public. This name somewhat underrates Gutenberg's part in developing it for further use, because after all he knew a tremendous amount about seismographs and seismograph recordings, 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.

AS: Was he the one who suggested using a logarithmic scale?

CR: Yes. The common practice in engineering and physics is to use a vertical logarithmic scale that compresses the data. And Gutenberg had undoubtedly encountered that procedure in some of his own practice. When I went to him and pointed out my problem with the numerical values in the data, he said, "Try plotting the data on a logarithmic scale." I did, and it then became evident that it could be used in a manner to set up a definite scale.

The logarithmic scale is rather a natural procedure wherever you have to deal with numbers that extend over a very wide range, and the range proved to be rather astonishingly wide in the case of earthquakes. In fact, if there was anything you could call an actual discovery that came out of that scale, it was that the biggest earthquakes were enormously bigger than the little ones.

AS: Would you expand on that a little? I suspect there is still some misunderstanding of exactly what the scale is and what it measures.

Charles Richter stands by the Seismological Lab's recording drums, on which the records of incidence, location, and magnitude of earthquakes are read.

CR: First, let me point out that the scale is not an instrument but a series of charts and tables, and it measures magnitude, not intensity. An intensity scale has arbitrary grades, say from I to XII, that are applied by experienced investigators to describe or rate the shaking produced by an earthquake at a given point. On the Modified Mercalli scale, for example, Roman numeral I indicates that the earthquake was in general not felt at a reported place, IV that it was strong enough to rattle windows, and XII (a degree of shaking that is rarely observed) that it was sufficient to cause total destruction to buildings.

The magnitude scale, on the other hand, represents measurements (expressed in ordinary numerals and decimals) of the deflection indicated on a seismogram during an earthquake. It compares earthquakes in terms of the amount they disturb the ground at a fixed distance from the earthquake's epicenter. If we compare local intensity on the Mercalli scale to the signal strength on a radio receiver at a given locality, magnitude is comparable to the power output in kilowatts of a broadcasting station.

Now there is no upper limit to the possible magnitude of an earthquake; that is, earthquake magnitudes are not measured on a fixed scale of, say, one to ten. The highest magnitudes assigned so far to actual earthquakes are about 9, but that is an observed fact, not a ceiling — a limitation in the earth, not in the scale. The scale is, as we said, logarithmic, so a step up of one unit in magnitude implies a tenfold increase in ground motion. An earthquake of magnitude 8, which is a great earthquake, causes ten times as much ground motion as one of magnitude 7, and 100 times as much as one of magnitude 6. If we assign the number 1 as the deflection for a magnitude 3 earthquake, the San Francisco earthquake of 1906, which was of magnitude 8.3, showed a deflection 100,000 times as large. That is what I meant when I said that the biggest earthquakes are enormously bigger than the little ones.

Let me add that this concept of the earthquake magnitude scale is not a final one. Paul Jennings [professor of civil engineering and applied mechanics] and Hiroo Kanamori [professor of geophysics] are particularly active in research here at Caltech in revising its formulations and applications. So the last word on the Richter Scale has by no means been said. □