Second Award of the Medal of the Seismological Society of America

The MEDAL OF THE SEISMOLOGICAL SOCIETY OF AMERICA was established as Article XII of the Constitution and Bylaws in the 1975 annual election. The Medal recognizes outstanding contributions in Seismology or Earthquake Engineering. The second award, in 1976, was made to Charles F. Richter. The Medal was presented by President Nuttli on April 5, 1977 at the Society luncheon during the annual meeting in Sacramento, California.

CITATION

The name of Charles Francis Richter is not only familiar to all seismologists, but has become a household word in many areas where earthquakes are discussed. As a result of the good offices of our friends in the news media, with whom Richter has spent many patient and often frustrating hours explaining the nature of earthquakes, the Richter magnitude scale is one of the most widely understood scientific measures of our time. It is characteristic of the man that he has distilled a lifetime of research on seismicity into a single number which expresses simply, clearly, and quantitatively the most important aspect of an earthquake: its size. For this he is well known. Yet, the magnitude scale is merely the most familiar of his many contributions to seismology.

He was born 77 years ago this April in Butler County, Ohio, about 70 miles south of Anna, the most active seismic site in that state. He received his bachelor's degree from Stanford at the precocious age of 20 and his Ph.D. from Caltech in 1928 in Physics. This was at the time when The Carnegie Institution was operating the Pasadena Seismological Laboratory, and Richter was one of the bright young men recruited to the staff of that organization, starting even before his graduation. Later, when Caltech took over the management of the Laboratory, Richter became a Professor at the Institute as well as a staff seismologist.

It was in his role as a professor that I first came to know him. During my first year at Caltech it was his course in Elementary Seismology which introduced me, like so many other generations of Caltech students, to the exciting problems of earthquakes. His lectures were both rich in anecdotes and thorough in their detailing of observational phenomena. His textbook, *Elementary Seismology,* is so good that no one since Richter has attempted to write a competing text.

In his research, he specialized, from the start, in problems of southern California earthquakes. He began the accumulation of systematic, precise, detailed information on the locations of earthquakes of all sizes from the largest to the smallest detectable in southern California. It is difficult to appreciate today, when standardized instruments, sophisticated systems of automation in record interpretation, and computerized epicenter location are common, the long and patient labors which were once required for accurate work. Richter began the major task of detailing the seismicity of southern California. All of us who use such data today are able to conduct our research effectively only because he built systematically and thoroughly, over many years, the comprehensive list of southern California earthquake data which was published in 1973 by the Division of Geological Sciences at Caltech to make it most easily available to everyone. In works such as this, Richter was one of the pioneers in transforming seismology from a descriptive to a quantitative science.

To many persons, his name is most familiar in association with his colleague Beno Gutenberg, with whom he coauthored dozens of papers. Because Gutenberg was both an extremely effective speaker and adept in responding to points raised in discussion, he presented most of the papers before this society and elsewhere. The research was, however, truly a joint effort. Each member of the team brought to the work special qualifications which made their association usually effective. Gutenberg had a remarkable capacity for conceiving of all possible explanations for a set of data. Richter brought to the team a phenomenal memory. He has the most remarkable photographic memory I have ever encountered. As a typical example, I shall describe an occasion which occurred when I was a graduate assistant in the Seismological Laboratory. It was the daily custom for Richter and Gutenberg to examine each day's seismograms as soon as they were dry enough to handle and to discuss interesting events which had been recorded. One day a particularly interesting earthquake was being discussed. Gutenberg was intrigued by certain features of this event and asked Richter if he remembered seeing anything similar before. Richter looked carefully at the seismogram. "Yes,' he replied, "that looks very much like the seismogram of April 17, 1927" (I don't remember the actual date he stated). Richter went to the files and returned a few moments later with the record, which looked, at least to me, to be virtualy identical with the new one. This ability to provide valuable information on instant recall is a remarkable feat and an important part of what made this team of scientists so tremendously productive of new knowledge. In a science where uninhibited sharing of data is the foundation of much of our progress, it is fitting that a man noted for this type of effort be recognized by this award of the Society's medal.

Richter has served also in many other ways: on commissions such as the Los Angeles County Earthquake Commission, as a consultant, for instance for the Los Angeles Department of Water and Power, and as a member of the Board of Directors of this Society, of which he was president in 1959–1960.

It is particularly appropriate that in awarding this medal for only the second time, it is going to a man whose career has been devoted as much to the unselfish gathering of data for others to use as to his own special research interests. Charles Richter, for your lifetime of effective labors on behalf of the science of seismology, for the effective tools such as magnitude you have developed for all of us to use, for your contributions to knowledge both individual and joint, for your inspiring teaching and for your valued friendship we all thank you.

B. F. HOWELL, JR.

To President Nuttli:

If I were to use many words to voice my appreciation on this occasion, expression would still be inadequate. Please allow me to make a few words stand for more, by saying that I am deeply moved by the honor here conferred on me, and especially by the kind and considerate spirit in which this is done. For this, I am very grateful to the officers and membership of the Society.

I take this as encouragement to continue in active contact with the seismological profession, in the hope of drawing constructively on the experiences of many years. Seismology now differs in some conspicuous ways from what it was 50 years ago; but on other points, especially those concerning which misunderstandings most often arise, one must observe that the more it changes, the more it remains the same.

It seems proper at this time to turn to certain reminiscences, which date back before the memories of most of those who are here.

Repeatedly I am approached by journalists and interviewers for an account of the origins of the instrumental earthquake magnitude scale. Some, but not all, of the facts are in print.

Seismology owes a largely unacknowledged debt to the persistent efforts of Harry O. Wood, who brought about the seismological program in southern California. From 1921 through 1936 it was under the Carnegie Institution of Washington, with certain special contributions by the California Institute of Technology. In 1937 the whole program was transferred to the Institute, under the Division of Geological Sciences.

In 1930, Dr. Beno Gutenberg came to Pasadena, with a professorship at the Institute. As a courtesy, and in the best interest of science, he was given office space at the Seismological Laboratory, where he spent much of his time.

I joined the Laboratory staff in 1927, as an assistant to Mr. Wood. In 1937 I received an assistant professorship at the Institute.

At first I was largely engaged in continuing and organizing the routine work of measuring seismograms and locating earthquakes, so as to set up a catalog of epicenters and times of occurrence. In 1931 we were in a position to begin issuing regular bulletins.

Mr. Wood was then collaborating with Maxwell Allen on a review of historical material on earthquakes in the California region, from the earliest years down to date. Their publication on destructive and near-destructive earthquakes went through a series of editions.

Allen wrote to Wood that he thought the list should include earthquakes which seismograms indicated to have been large enough to have caused local damage, even when there were no available reports of such effects. Wood felt this would introduce a speculative element and might lead to misunderstandings.

We were then recording at seven widely spread stations, all with Wood-Anderson torsion seismographs. I suggested that we might compare earthquakes in terms of the measured amplitudes recorded at these stations, with some applied correction for distance from epicenter to station. Wood and I worked together on the latest registered events, but found that no one of several assumptions for attenuation with distance yielded satisfactory results. Recordings at the nearer stations were often alarmingly larger than those at more distant ones.

I found a paper by Professor K. Wadati, in the *Geophysical Magazine (Japan)*, in which he compared several large earthquakes by plotting the maximum ground motions recorded at Japanese stations against epicentral distance. I tried a similar procedure for the maximum amplitudes registered by our torsion seismographs. However, the range between the largest and smallest amplitudes seemed unmanageably great. Here I turned to Dr. Gutenberg, who made the natural suggestion to plot amplitudes logarithmically. This improved the situation, at least in appearance. I was lucky, because logarithmic plots are a device of the devil. I saw that I could now range the earthquakes relatively, one above another, and also, quite unexpectedly, that the attenuation curves were roughly parallel on the plot, so that by moving them vertically a representative mean curve could be formed. Individual events then were characterized by individual logarithmic differences from the standard curve; so that one was in effect derived from another by a constant multiplying factor. The set of logarithmic differences thus became numbers on a new instrumental scale. Mr. Wood, very perceptively, insisted that this new quantity should be given a distinctive name to contrast it with the intensity scale. My amateur interest in astronomy brought out the term magnitude. Stellar magnitudes are assigned on a scale which is logarithmic, but more complex than that introduced for earthquakes.

The initial work was done on earthquakes recorded in January, 1932. It was used in bulletins, and preliminary announcements appeared in annual reports. Full details were not published until 1935. During this probationary period, the magnitude scale was refined by applying it to some of the larger earthquakes in the area, for which Dr. Gutenberg had worked out the epicenters.

The usual designation of the magnitude scale by my name, though perhaps convenient, does less than justice to the great part which Dr. Gutenberg played in extending the scale beyond its initially local range, to apply to earthquakes in all parts of the world. This was based on the registration of surface waves with periods near 20 sec.

Gutenberg later worked out a magnitude scale based on body waves, and wished it to supplant the surface-wave scale. Theoretically, this is better; but it introduces an additional element of hypothesis that experience has shown leads readily to misunderstandings and to oversimplified misapplications, so that many wrong magnitudes, especially for large earthquakes, have been published and are constantly repeated. A good reason for working with body waves was to assign magnitudes to deep-focus earthquakes; but it was later shown that Gutenberg's results were distorted by the comparatively large loss of energy in body waves from shallow earthquakes, when they pass out of and back into the crust. The effect is to relatively exaggerate the magnitudes of deep-focus events.

Magnitude work was only part of a long and fruitful collaboration between Gutenberg and myself: we were engaged in exploring the internal structure of the Earth and the behavior of seismic waves therein, establishing the seismic characteristics of deep-focus earthquakes and cataloging them, and ultimately revising the entire seismic geography of the Earth, since we found that the occurrence of deep-focus events and other sources of systematic error had gravely distorted the usually expounded picture. All this, be it remembered, was only a small part of Gutenberg's enormous contribution to seismology and to general geophysics.

Sometimes I am asked why magnitude was not originally defined in terms of energy. Quite simply: that would have introduced a theoretical element into the practical use of the magnitude scale—an unstable element, moreover, subject to unforeseeable revisions. It was best to define directly in terms of observed quantities and avoid time-consuming calculations when thousands of earthquakes were to be studied.

Although Gutenberg and I published proposed relations between magnitude and energy, I am more and more convinced that there is no one simple correspondence between them. Magnitude is probably more nearly related to power (in kilowatts or whatever). Moreover, different groups of data will result in deviating forms of the scale, as Dr. Kanamori's latest development shows. More important is the concept of magnitude as distinct from intensity, without which it is impossible to set up any sound statistics of earthquakes, or to avoid confused thinking in comparing events with each other.

I am often asked about prediction. Since my first attachment to seismology, I have had a horror of predictions and of predictors. Journalists and the general public rush to any suggestion of earthquake prediction like hogs toward a full trough. It is a parallel to the obsession with a cure for cancer, or with the question of life on other worlds.

There is nothing wrong with aiming toward prediction, if that is done with common sense, proper use of correct information, and an understanding of the inherent difficulties. Otherwise, the subject provides a happy hunting ground for amateurs, cranks, and outright publicity-seeking fakers. The vaporings of such people are from time to time seized upon by the news media, who then encroach on the time of men who are occupied in serious research.

In recent years, there has been much vigorous campaign for funds to support earthquake research, on the representation that it will lead to useful prediction. I must remark that it is not good science to promise the results of research in advance, particularly not with a time limit. However, the means thus acquired are largely put to fruitful use. More and better stations are operated; crustal structures and the velocities of seismic waves are being investigated effectively along lines which were initiated many years ago with interesting but necessarily preliminary results. Moreover, search for the identification of active faults is being intensified. This acts in part to counter the excessive public and professional preoccupation with the San Andreas fault. It should be more generally realized that since 1906 there have been about 15 disastrous earthquakes in California, none of which originated on the San Andreas fault.

I have less sympathy with the work which is going on with relation to animal behavior. In my judgment, this offers no reasonable prospect of contributing to earthquake forecasting, or to seismology in general, except that in rural districts, as in China, the populace may use their domestic animals as crude indicators. Whether horses, chimpanzees, dogs or cockroaches react to minor foreshocks, or more speculatively to magnetic effects, is of no consequence where it is possible to install sensitive recording instruments. The suggestion that animals or persons have some mysterious supernatural power of forecasting earthquakes or other events, is a mere revival of superstition which has no place in scientific discussion.

In these 50 years I have witnessed great and reassuring progress in earthquake engineering. There is good cause to expect that buildings now being designed and constructed will perform better in strong earthquakes than comparable ones built in earlier years. However, from the point of view of risk to the public, new construction is not our most serious concern.

I am no engineer; but merely from what I have seen, it is easy to join my voice to those of engineers and building experts, emphasizing that in the past, and expectably in the immediate future, most loss of life and property has been and will be due to the collapse of numerous antiquated and unsafe structures, mostly inferior brick masonry, such as disfigure all the older business centers in California. You all know what frustrating opposition is encountered by every earnest effort to get rid of these risks. We would have less reason to ask for prediction if they could be eliminated.

I was, once, more concerned about the safety of tall buildings than I am now. After the alarming collapses of multistory structures in other countries, and after the unsettling experience with the San Fernando earthquake of 1971, the techniques used are going over from the simplified static methods based on assumed peak acceleration alone to the more realistic dynamic methods exemplified by the preparation and application of design earthquakes. Like many engineering assignments, all this involves a lot of educated guesswork; but in my judgment the guesswork is mostly good, and the general improvement is great.

I thank you all for your kindness, patience, and attention.

Respectfully,
C. F. Richter