Bulletin of the Seismological Society of America, Vol. 72, No. 6, pp. S331–S335, December 1982

EARTHQUAKE PREDICTION—1982 OVERVIEW

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ABSTRACT

Short-term earthquake prediction represents a more difficult scientific problem than most of us thought 5 yr ago when the National Earthquake Hazards Reduction Program commenced, and our progress has not been as rapid as initially hoped. At this point, reasons can be cited for both encouragement and discouragement. Despite slow progress, the goal of short-term prediction remains realistic, and research should continue vigorously, albeit with some changes in scientific strategy. In contrast, progress in long-term prediction and hazard evaluation has been far more rapid than initially envisaged.

It has now been 5 yr since the passage of the Earthquake Hazards Reduction Act of 1977 and since the formal commencement of the American earthquake-prediction program. This is, therefore, a logical time to review our progress in prediction, and we would, in fact, be negligent if we did not periodically assess our progress and modify our strategy as called for.

In my opinion, we must face up to the fact that our progress during the past 5 yr in short-term earthquake prediction has not been as rapid as we had envisaged when the program started. On the other hand, our progress in long-term prediction and hazard evaluation has been more rapid than expected. Not everyone agrees with this assessment, of course, nor did everyone have the same expectations 5 yr ago. Some researchers vigorously defend our allegedly rapid progress in short-term prediction, but there are also thoughtful scientists who have now concluded that routine short-term prediction represents a goal that is virtually unattainable within the foreseeable future. I seem to stand somewhere in the middle.

Granting that our progress has been somewhat slower than expected, can we blame the shortage of funds? Clearly, we could have done much more if additional funds had been available, and certainly those of us who have served on proposal-review panels can testify to the many very promising proposals that have been left unfunded. Programs in other countries, such as that in Japan, have been far more generously supported. But I think that we cannot blame inadequate funding alone; the short-term prediction problem has simply turned out to be a more difficult problem than many of us visualized 5 yr ago.

One can point out a number of reasons for both encouragement and discouragement. Consider the discouraging aspects first.

1. A number of the proposed prediction techniques that looked so promising 5 yr ago, such as the V_p/V_s method, have not proved to be as effective as hoped. Indeed, a certain euphoria of imminent victory pervaded the earthquake-prediction community 5 yr ago, primarily based on this and other proposed prediction techniques, and this euphoria has by now largely evaporated.
2. It was said 5 yr ago that what we really needed to implement the prediction effort in California was one or two moderate-sized earthquakes in areas of good instrumentation; i.e., we needed to "trap" a significant event. Two such events have in fact recently occurred—the 1979 $M = 5.9$ earthquake near Gilroy, and the 1979 $M = 6.6$ earthquake near El Centro. While neither of these earthquakes

occurred within a network with instrumentation as dense as we would like, certainly the instrumentation in both areas was far higher than we have generally had in the past. Both of the earthquakes are still being intensively studied, but it seems fair to observe that no plethora of precursors has been claimed. Indeed, some of the results have been downright discouraging. For example, prior to the 1966 Parkfield earthquake on the San Andreas fault, obvious creep took place along the fault in the days and weeks just prior to the event, and with this in mind, several continuously recording creepmeters were placed across the Imperial and Brawley faults in 1975. Three of these instruments crossed the fault trace that broke in 1979 and recorded the coseismic displacement, but none of the three creepmeters showed any hint of precursory creep during the minutes, hours, or days preceding the earthquake.

3. One of the significant results of seismological research during the past few years has been the discovery that earthquakes are vastly more different from one to another in their mechanical parameters than we had ever suspected—differences in stress drops, absolute stresses, rupture velocities, rupture patterns, etc. While these discoveries are significant and exciting from the point of view of fundamental seismology, they are not particularly good news from the point of view of earthquake prediction. It seems logical that the more different earthquakes are from one to another, the less likely it is that they will have common precursors.
4. Increasingly, studies of large earthquakes indicate that they typically are multiple events—a series of earthquakes of increasing size which successively trigger one another along a given fault zone. Thus, the prediction problem for such complex events is not so much that of predicting the integrated large earthquake as it is that of predicting which small earthquake will be the trigger that starts the sequence. Which straw will break the camel's back? Inasmuch as it is these largest and most damaging earthquakes that we are primarily interested in predicting, the triggering phenomenon certainly makes the prediction problem a more difficult one.

On the other hand, the past few years have also given us some reasons for encouragement.

1. Some earthquakes have been successfully predicted on scientific bases, and these successes obviously give cause for optimism. Perhaps the most widely recognized of these is the Haicheng, China, earthquake of 1975, and although a certain amount of good luck may have been involved, most independent observers have concluded that a real scientific basis existed for the prediction. If we can succeed on some earthquakes, we should be able to succeed on others.
2. Convincing physical precursors to many earthquakes have, in fact, been observed, and although these precursory phenomena have not always led to predictions, they breed optimism that prediction is a realistic scientific objective. Examples might include: the fault creep prior to the 1966 Parkfield, California, earthquake; the distinct seismicity pattern preceding the 1978 Oaxaca, Mexico, earthquake; and the many credible reports of ground-level and well-water changes prior to historic Japanese earthquakes. Perhaps a bit more debatable are the reported radon anomalies prior to the 1976 Gazli, USSR, earthquake, and the dilatometer-recorded strain changes prior to the 1978 Izu-Oshima, Japan, earthquake. Furthermore, of course, foreshocks are a very real precursory phenomenon, whether or not we can as yet always recognize them as such.

3. Distinct temporal changes in physical parameters that must be related to the earthquake process are, in fact, taking place and can increasingly be measured, regardless of whether we currently understand their significance. Examples include the recent major changes in the level of seismicity and the concomitant changes in regional strain patterns in southern California. The fact that such changes are demonstrably taking place and can be documented gives encouragement that we shall eventually be able to understand their relationship to the mechanical processes preceding earthquake rupture.
4. The seismic gap concept has by now been verified time and again, and it represents a very major contribution to our understanding of the strain buildup process preceding earthquakes. While primarily utilized thus far in long-term predictions, this phenomenon allows us to concentrate our short-term prediction efforts in promising areas, and gives hope that short-term precursors will be found in seismicity patterns within gaps.

In summary, I would have to characterize our present attitude toward short-term earthquake prediction as one of very guarded optimism, as compared to the out-and-out euphoria of a few years ago. But in sampling scientific opinion, one must be careful to include the viewpoints of seismologists who are not directly involved in the program as well as those who are, and who, to a significant degree, have a vested interest in its continued funding.

If progress has been slower than expected in short-term prediction, certainly it has been faster than expected in hazard evaluation and long-term prediction. Particularly significant in this area has been the development of techniques for establishing earthquake recurrence intervals based on geological field relationships, as exemplified by the paleoseismicity studies of the southern San Andreas fault in California and of the Wasatch fault zone in Utah. Such studies are aiding considerably in establishing firm probabilistic approaches to hazard evaluation. For long-term planning, engineering design, and the development of realistic building codes, these results may in fact be far more important than the development of a short-term prediction capability.

It is clear, nevertheless, that "earthquake prediction" implies to most people *short*-term prediction, and that is the focus of the present discussion. It was the possibility of short-term prediction that primarily caught the attention of Congress in the 1977 legislation. There are, of course, all gradations between short- and long-term prediction, and one hears an increasing call to eliminate the semantic distinction between the two—partly to capitalize on the success of long-term prediction and thus to make the overall program now more sellable. It is my opinion, however, that some real changes in strategy are necessary in the effort to achieve a short-term prediction capability, and merely changing the semantic umbrella is not enough. That is, some fundamental changes in the scientific game plan are called for, not merely a redefinition of what we mean by "prediction." Other contributors to this symposium will be addressing this specific problem. My one observation, based on our experience in California thus far, would be that we should deemphasize the widespread placing of instruments all over the state in the hopes of somehow trapping a meaningful anomaly, when we do not fully understand either the instruments or what it is we are trying to measure. Our efforts in strain, radon, and tilt to some degree illustrate this problem. If we are ever to predict earthquakes routinely, a better fundamental understanding of many of these phenomena is necessary, and I am convinced, albeit in retrospect, that there must be a larger basic-research element to our overall earthquake-prediction effort.

Quite aside from our degree of success, or lack thereof, in earthquake prediction, we have now had some experience in the *evaluation* of scientific predictions, and this has been a traumatic experience for almost everyone involved. I am convinced that such a governmental evaluation procedure is necessary, in view of the great public impact of prediction statements, but it is certainly unlike the kinds of scientific evaluations most of us are used to in normal research activities—those involving publication procedures and the peer-review process inherent therein. In my 1976 Presidential Address to the Seismological Society of America, I warned that the next 10 yr were going to be difficult ones for us, with many "messy" predictions to deal with as we gradually developed a prediction capability. Certainly this has proved to be the case, with many of the most difficult situations arising from predictions by amateurs or self-proclaimed scientists who nevertheless gained public credibility through the news media. But truly scientific predictions themselves have also already caused serious problems, as evidenced by the so-called Brady-Spence prediction for a series of great earthquakes in Peru during the summer of 1981—a prediction that fortunately turned out to be a false alarm.

The National Earthquake Prediction Evaluation Council formally repudiated the Brady-Spence prediction some 5 months before the first predicted event, but, as Chairman of that group, I am left with the uneasy feeling that the scientific community did not handle this episode as well as we should be able to in the future. It clearly represented a very delicate situation, involving freedom of scientific expression and the willingness of the scientific "establishment" seriously to consider seemingly aberrant points of view, but also involving the economic and social well-being of literally millions of people. Was the public (in this case, the citizenry of Peru) well served by the scientific community? Should the scientific evaluation groups have acted sooner and more positively to renounce the prediction? Were the predictors given adequate and fair opportunities to defend the scientific basis of their prediction? Was an open hearing, with the TV cameras rolling, the fairest and most effective forum for the scientific evaluation of the prediction? Should the Evaluation Council have refused to evaluate the prediction until it was published, or at least written in some sort of formal statement? What role should the professional societies play in such a circumstance, and, in particular, should they be more vigorous in formulating a prediction "code of ethics"? Should the employers of the predictors, in this case the U.S. Bureau of Mines and the U.S. Geological Survey, have been more active in "controlling" the announcements of their employees? In our efforts to be professionally fair to the predictors, were we being equally fair to the people of Peru? And in this particular case, could not the U.S. State Department have played a more stabilizing role in its handling of the prediction and its publicity? There are not questions with easy answers, but surely somewhat similar situations will arise again, and hopefully we can face them with less overall trauma than with the recent Peruvian false-alarm prediction.

In my 1976 Presidential Address, I also asked: "Will we have the courage to admit it to ourselves and to our funding agencies if, after another 2 or 3 years of intensive effort, it turns out that our initial enthusiasm was unwarranted and that there really isn't much hope of routinely predicting earthquakes within the foreseeable future?" Our initial enthusiasm may indeed have been a bit naive, but I remain convinced that the objective is realistic. It would make no more sense to abandon the earthquake-prediction effort now, particularly with much of our critical instrumentation just getting into place, than it would be to abandon the "war on cancer" just because the ultimate objective has not yet been reached. But let us continue to be honest with our funding agencies, Congress, and the public. To some degree, we in the seismological community have been guilty of allowing the public to conclude that short-term earthquake prediction is more imminent than most of us really believe.

There is no firm guarantee that we will ever routinely predict damaging earthquakes on a short-term basis, and the scientific difficulties are clearly formidable. Everyone acknowledges this. But, in my opinion, the chances of success are high enough that we must vigorously continue the program—albeit with possible changes in strategy as the effort moves forward. The potential payoff—both scientifically and socially—is still very great.

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Manuscript received 28 June 1982