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RESPONSIBILITIES IN EARTHQUAKE PREDICTION

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As recently as only a year or two ago, seismologists were still debating among themselves the question of how we should interact with the public when the day finally arrived that scientific predictions of damaging earthquakes would be forthcoming. Whether we're ready or not, that day has suddenly arrived, and some of us are already a bit bloodied from our first encounters. In this address, I wish to discuss some of the responsibilities that seismologists must assume as we enter this new era of what might be called "fishbowl science," and also ways in which we can perhaps encourage the public, the news media, and public agencies to respond to predictions in the most effective and constructive ways.

Most of us, as scientists, are not comfortable working in the public eye and under the intense scrutiny of the news media; certainly this is not the deliberative science to which we are accustomed. Nevertheless, if we are to claim that earthquake predictions will have significant social benefits—which I am firmly convinced is the case—and if we are to use this as an argument for taxpayer support of our research, then we must necessarily be willing to interface with the public and with public agencies in ways quite different from those we have been accustomed to in the past. Somehow we must find ways of doing this that are both scientifically honest and fair, and at the same time socially responsible. Those of us who are currently a bit battle-scarred will be the first to testify that this is far more easily said than done!

What constitutes an earthquake prediction? Certainly the scientists and the public must see relatively eye to eye on this question before any meaningful interaction or dialogue is possible.

I would argue that no prediction should be considered valid unless the time, magnitude, and area of occurrence are specified, in writing, within sufficiently well-defined limits so that the ultimate success or the failure of the prediction can readily be judged. I am, of course, by no means the first to ask for such specifications; Harry Wood and Beno Gutenberg, in their 1935 paper on earthquake prediction, specifically stated that "to have any useful meaning the prediction of an earthquake must indicate accurately, within narrow limits, the region or district where and the time when it will occur. . ." Many of Wood and Gutenberg's observations in this rather surprising paper are, incidentally, as valid today as they were in 1935, although I doubt that their suggestion of withholding certain kinds of forecasts confidential from the press is at all possible or even desirable in much of the world today.

It is clear that the uncertainties in the specifications of time, magnitude, and area will necessarily be much greater for some predictions than for others. Very generalized time predictions, such as "within 25 years," can have significant value in encouraging permanent social responses such as the development of realistic building codes and in land-use planning. Nevertheless, the word "prediction" is more commonly interpreted by the public to imply a much smaller time uncertainty, and I would urge seismologists to attempt to use other words to describe the long-term predictions that involve permanent social responses. Of primary concern to the average citizen are those short-term predictions with a sufficiently narrow time window and immediacy of occurrence to encourage temporary responses such as the alerting of emergency forces and the temporary evacuation of recognized hazardous structures.

A prediction of locality without any specification of time may be very valuable in studies such as the construction of seismic zoning maps, but I would not consider it a prediction in the sense that the word is normally understood by the public.

In my opinion, it is also incumbent on the author of a prediction to give some sort of indication of the confidence that he or she places in the prediction. Particularly during the develop-

mental stage of our earthquake-prediction capability, some events will be predicted with very great uncertainties and low confidence levels, but they certainly cannot be held confidential for this reason alone. We must make every effort somehow to reflect this sense of confidence to the public. There is some difference of opinion as to how best to do this, although I would prefer a percentage probability such as the public has become used to in weather forecasts. I recognize that the author of the prediction may have no very firm basis for a probability assignment (nor often, it seems, does the meteorologist!), but there is a clear social obligation to make *some* statement of probability, however expressed and however phrased. Although the basis for such a statement admittedly may be weak, no one is in a better position to make an estimate than is the person predicting the event—least of all the public official who is morally and legally obligated to respond to the prediction. For the predictor to say simply “I don’t know the probability of the predicted event” may be completely honest, but the public which has become concerned deserves more than that. Some judgment, however frail, must be offered. In fact, I think I would go so far as to say that, if the author can make absolutely no meaningful statement of probability or confidence, it should not be offered as a prediction, or at least the word “prediction” should not be applied to it.

Another area in which the author of a prediction has an obligation is in giving some feeling for the chances that the predicted event would occur anyway! That is, as emphasized by my colleague, James Whitcomb, what is the probability that the prediction might be fulfilled by randomly occurring events? Although the public might not be overly concerned with this question, certainly the scientific community, in its evaluation, will be.

I would argue, therefore, that a valid earthquake prediction should ideally have six attributes:

1. It must specify a time window.
2. It must specify a space window.
3. It must specify a magnitude window.
4. It must give some sort of indication of the author’s confidence in the reliability of the prediction.
5. It must give some sort of indication of the chances of the earthquake occurring anyway, as a random event.
6. It must be written and presented in some accessible form so that data on failures are as easily obtained as data on successes.

How should an earthquake prediction be released? This is a question of a fair amount of recent soul searching. The time will probably come when virtually all predictions are systematically released by government agencies specifically charged with that responsibility, but during the research and development stage of achieving a prediction capability, such centralized authority is probably neither possible nor desirable. The valuable and necessary wide ranges of viewpoints, imaginative thinking, testing of hypotheses, and individual efforts are simply not compatible with centralized authority, and such authority would, in fact, be looked upon as infringement on scholarly independence and free speech. I agree with the recent NRC Turner report on “Earthquake Prediction and Public Policy” in its conclusion that “. . . an important consequence of trying to establish a single agency for the release of predictions at the present time would be to postpone the day when scientists can agree on the criteria for a valid prediction.”

I see no alternative to the situation whereby, for the next few years, we will be seeing numerous predictions of varying types by various individuals and various groups. It goes without saying that those groups, agencies, and individuals who show the greatest responsibility and the greatest success will be those ultimately gaining the greatest credit. And vice versa.

On the other hand, public officials who are legally obligated to respond to warnings and predictions, even at the present time, must have some way of evaluating predictions—whatever the source. How are the city fathers of Pasadena, for example, to distinguish between the ravings of a soothsayer and a recognized scientific prediction? In all frankness, we must admit that the differences are often not all that obvious, and in any event, there are many shades of gray in between.

It seems clear that the time has very suddenly arrived when public agencies must establish formal scientific advisory bodies on earthquake prediction not for the purpose of censoring or

restricting individuals in the making of predictions, and not for the purpose of making predictions themselves but, instead, to serve as filters between those who issue predictions and those who are obliged to react to them.

A prototype for such groups might be the California Earthquake Prediction Evaluation Council, which for some time has advised the State Office of Emergency Services regarding predictions which have come to its attention—predictions ranging thus far over the entire gamut from reasonably credible to utterly incredible forecasts. The Office of Emergency Services in turn notifies concerned public agencies, such as law enforcement bodies, of the judged validity of such predictions. It is the local agencies that, on the basis of this advice, are faced with the responsibility of deciding what kind of a public warning or statement, if any, to issue. Considerable discussion is now taking place in the United States concerning the desirability of forming a somewhat similar evaluating body at the Federal level.

Any scientist is naive who believes either that the public in earthquake-prone areas is not vitally concerned with even the vaguest hint of an earthquake prediction, or that public agencies and officials will not demand complete, open, and prompt scrutiny of the scientific basis for any prediction. Those who do not live in seismically active areas perhaps cannot appreciate the intensity of public interest, the sense of responsibility of public agencies, and the uncanny skill and persistence of the news media in ferreting-out even the most obscure hint of a prediction or forecast. There is no question, moreover, but that the scientist working in the field of earthquake prediction does have an overriding obligation to the public interest; lives, property, and public well-being are directly at stake, whether we like it or not.

The total effect of this state of affairs, in my opinion, is that a scientist who is not willing to see his or her work subject to prompt and intense public criticism and evaluation—in ways quite different from those we are accustomed to—should not be in the field of earthquake prediction. There is no question but that this exposure in the public arena will have the effect of leaving some scientists—unfortunately perhaps the young ones in particular—somewhat embittered and bloodied after their work has been subject to open criticism, widely displayed in the news media—admittedly sometimes unfairly so. But if one is unhappy with this environment and prefers the sequestered criticism by colleagues in smoke-filled rooms of the traditional scientific meetings, he or she should choose a different research area. This is necessarily part of the price we pay for relevance, and we should not forget Harry Truman's admonition to get out of the kitchen if the heat becomes too great!

I don't mean this observation in any sense as a criticism of those scientists who have already entered the fray. On the contrary, one has to admire the courage of those who are willing to put their reputations on the line and expose themselves to this very unusual form of criticism. Only by offering hypotheses and data for testing, the very basis of the scientific method, will we ever achieve the ultimate goal of routine and consistent earthquake prediction. But lest the unwary be surprised, let them be fully forewarned that the scrutiny to which their work will be subject will be quite unlike anything they have known in the past.

This intense scientific and public scrutiny will, I think, have both good and bad effects. On the one hand, scientists will tend to be much more careful in making statements about earthquake predictions than they might in other research fields, and I feel that this is only appropriate in view of the great potential social impact and even danger of such statements. Great care and great caution in making predictions are certainly fully warranted. As was emphasized in the Turner report, "The principal constraint on both premature or scientifically unwarranted release and undue delay or suppression is peer pressure."

On the other hand, fear of massive and unfair criticism may have the effect of discouraging competent scientists from offering those tests which are essential to the development of a prediction capability, inasmuch as these tests obviously cannot be carried on in secret. This is going to be a very difficult task for us during the next 10 years: How do we encourage scientists, particularly young ones, to offer critical tests that sooner or later must be made, and some of which will necessarily have negative results, in this difficult environment of science in a fishbowl?

Some concern has been expressed that public release of earthquake predictions may prejudice the acceptance of the supporting documentation by scientific journals. It has been suggested that

the journal itself might wish to hold the rights to make a public release at the same time the article is finally published. Certainly it is unlikely, and probably undesirable as well, that critical information of this type could or should be suppressed from public exposure for the length of time involved in publication, and I think that this will be recognized by editors of journals. The recent article in *Science* by Castle and others on the Palmdale uplift was no less interesting or less scientifically significant just because it had already been the subject of nation-wide discussion, news releases, and wide distribution of preprints.

In any event, the primary obligation of a scientist who is making a significant prediction should be to the public and not to a scientific journal or to the scientific community alone. It would be indefensible to withhold a significant prediction from public release and public scrutiny solely because of publication delays in the scientific journals.

I do not mean to imply, however, that a proposed earthquake prediction should not be thoroughly and confidentially reviewed by a scientist's own colleagues before it is released in any public form; certainly internal review is one of the procedures a scientist may wish to make use of in assuring himself or herself that a contribution is ready for wider exposure. But earthquake predictions constitute a very difficult and highly unusual situation in this regard because of the problem of "leaks." Even an unannounced seminar for colleagues will almost assuredly result in rumors and in subsequent inquiries from the news media. And tight security, even if it were possible, is somewhat of an anathema to most scientists, perhaps particularly those in academic institutions.

Nevertheless, it seems to me that one of our primary challenges and obligations is to establish review mechanisms for earthquake predictions that are both confidential and prompt, so that the author can obtain significant scientific feedback prior to the time the prediction is first discussed at seminars or scientific meetings, and prior to the time that it is submitted for publication or for review by formal evaluating committees.

The problem of dealing with the news media concerning rumors of alleged predictions is a particularly difficult one, and it is for this reason that confidentiality is so important in the early review process. If one says "yes" to an inquiry concerning a rumored prediction, certainly the roof will then fall in, and literal inundation by the press will result within hours if not minutes; if the answer is said to be "no," and that answer subsequently turns out to be somewhat less than honest—or shall we say, "inoperative"—relations with the news media will quickly deteriorate and encourage valid charges of a scientific credibility gap. And if one responds "maybe," "no comment," or "call back next week," the results are not much more satisfactory.

It may seem that I am overemphasizing the problem of relations with the news media, and this may be a valid observation for regions of low public interest in earthquakes, but be assured that this is by no means a minor problem for those of us who live in areas where rumors of a predicted earthquake can very quickly drive even the most flamboyant stories out of the headlines. And I reiterate that the interest of the news media is a perfectly valid one; to scorn or deceive the news media is an essentially irresponsible act if we are to claim at the same time that earthquake prediction is really a significant endeavor in the public's direct interest.

Mentioned earlier was the California Earthquake Prediction Evaluation Council, which is made up of nine seismologists and geologists from a variety of institutions in the state. In this Council's recent deliberations on events in southern California, another public-policy issue arose that scientists should be aware of: should meetings of a governmentally sponsored body, even if of a scientific evaluative nature, be open or closed to the public and to the press? The tendency in recent years, at least in the United States, has been to try to make governmental operations more and more open, and the latest meeting of the California Council was in fact carried out entirely in the public eye and with the press in attendance throughout, even though it involved the scientific evaluation of one scientist's work by a group of his professional colleagues. Whether scientific justice is furthered or hindered by this kind of on-the-spot public scrutiny is a debatable question, but one that is bound to arise again and again. Certainly, as scientists, we are used to criticizing our colleagues primarily behind closed doors, or at least within the cloistered recesses of our own scientific cliques, but, when public safety is directly

involved, I think one should carefully consider the question of what things we would not be willing to say publicly that we *would* be willing to say behind closed doors. What are we trying to hide? If the author of the prediction is asked to stick out his neck and risk his reputation in making the public aware of the prediction, should not the reviewers of his work also be asked to make their criticisms in the public eye? I don't know the answer to this question, and we will be feeling our way cautiously over the next few years, but my tendency is to opt for the side of relatively full public disclosure. I appreciate that this is a delicate issue that is not entirely irrelevant to the question of the confidential review of manuscripts submitted for publication and proposals submitted for funding. But earthquake prediction is indeed a bit special; the public has both a vital and an immediate interest. The more that is done behind closed doors, the more the public will be unconvinced that scientists are truly operating in the public's best interest.

Scientists would, of course, be naive to neglect the possible legal complications resulting from earthquake predictions, and this is another area in which we will be feeling our way carefully over the next few years. The recent "hypothesis test" by Whitcomb in southern California evoked the threat of a lawsuit against him and Caltech by the City of Los Angeles for the alleged lowering of property values in the San Fernando Valley, and we will be following this situation with more than academic concern.

It is interesting that at the same time seismologists were being criticized by some for saying too much, a State Assemblyman accused Caltech of *withholding* information on the alleged prediction, claiming that we really knew to a far great precision than Whitcomb had indicated exactly when the alleged earthquake was going to happen. It seems to me that the only way to reduce such absurd charges to the minimum is to be as honest and forthright as possible in interacting with the press and the public, and to work continually to make sure that a major credibility gap does not develop. Again, this is an argument against undue secrecy in the meetings of formal boards and committees. This area of public relations is one in which most scientists do not feel at all comfortable and one in which we have not had much experience, but we must recognize that it represents a responsibility from which we cannot flee if we choose to attempt earthquake prediction. I do feel that we stand a better chance of convincing the public of our sincerity and of our honesty if we face them ourselves, and not turn over the job to so-called public-relations experts or to lawyers, although we will certainly be needing legal advice.

Another area of earthquake prediction in which scientists have responsibilities is in the fair and unbiased evaluation of the success of our cumulative efforts. Are we really making scientific headway or aren't we? Only by full and complete analysis of our failures as well as our successes can we hope to reach an answer to that question, and this is, of course, the very problem that has left many of us who have visited China recently with a rather uneasy feeling. How does one evaluate the significance of successes in the absence of full information on failures?

But the documentation of failures (and I include false alarms with failures) is important not only for judging the success of our total effort, but also for learning from our failures how to do better in the future. Which hypotheses and which tests look more promising than others? Whitcomb has argued recently, and I agree, that we should establish a formal international mechanism for tabulating and disseminating all scientific earthquake predictions, before the fact, so that statistical data on successes versus failures, as well as at least limited scientific backup to individual predictions, will be available to all interested investigators. Hopefully, peer pressures would be such that no prediction would subsequently be considered valid unless it had been formally submitted to this body. If we could indeed achieve wide international participation, which is probably a naive hope, we would be making a significant step forward in establishing a firm basis for evaluating our scientific progress in the prediction of earthquakes.

I foresee that the next 10 years are going to be exceedingly rough ones for us. As we gradually achieve a true earthquake-prediction capability—and I am reasonably optimistic that this day will come—we are assuredly going to stumble sufficiently often so that public support will be difficult to maintain. And in this area, of all areas, we critically need public support and confidence. The impressive and even mind-boggling earthquake-prediction program in China is, in their own words, experiencing a "high proportion" of failures and false alarms, perhaps so

high as to be unacceptable in most western countries in terms of maintaining public support. Almost assuredly, we too are going to have a number of very significant failures and false alarms, and it's going to be a real challenge to convince the public that the scientific method, with its testing of hypotheses, formulation of theories and models, successful and unsuccessful experiments, is really operating to their eventual benefit. And we must continually ask that question even to ourselves. The one thing we cannot afford to do is to tell the public that we are working on a scientific effort in their behalf if we are not in fact sincerely convinced that this is true! 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? This is certainly a possible scenario.

Nevertheless, I personally remain optimistic, and I believe that it is true that we are working in the public's interest, but we are only going to maintain public confidence if we are frank and completely honest in interacting with them and if we go to some effort to avoid the salesmanship and public-relations images that the public has grown to distrust. We are, of course, to some degree at the mercy of the news media in this regard, but our recent experience in southern California is surprisingly encouraging in suggesting that both the media and the public, with some notable exceptions, react sensibly and responsibly when *we* are effective in convincing them of the true absence of a credibility gap.

Nevertheless, I repeat that, in my opinion, the next 10 years are going to be rough ones for those of us involved in the earthquake-prediction effort, and seismologists are going to need all the wisdom, all the statesmanship, and all the talent that they can muster. Hopefully the results will be worth the effort.

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