PROCEDURES FOR EVALUATION OF EARTHQUAKE PREDICTIONS: EXPERIENCE IN USA

Clarence R. Allen
Seismological Laboratory, California Institute of Technology
Pasadena, California 91125, USA

and

Chairman, National Earthquake Prediction Evaluation Council

1. INTRODUCTION

The potential impact on the public of the prediction of a significant earthquake is so extraordinary that a prediction obviously must be issued with great caution and forethought with regard to its scientific validity. The purpose of this paper is to examine ways in which the public can be assured that predictions are subject to thorough scientific scrutiny and are truly representative of a reasonable scientific consensus.

If earthquake predictions were to come to the attention of the public only through specific authorized channels, such as a national earthquake-prediction agency, the problem would be relatively simple. Before any prediction were publicity issued, it would have opportunity to undergo confidential analysis by authorized evaluation groups, and indeed the prediction would not come to public attention without this formal approval. But in few parts of the world today is such a highly regimented public-information system operative; even in those countries with highly centralized earthquake-prediction efforts, rumours of alleged earthquake predictions often manage to capture public attention. Furthermore, amateur predictions inevitably seem to arise from completely outside the formal system. In fact, in most parts of the world today, the greatest numbers of earthquake predictions that have caused public concern have probably come from amateurs, psychics, soothsayers, and self-proclaimed scientists, rather than through "legitimate" scientific channels. This will undoubtedly continue to be the case until routine earthquake prediction is placed on a solid scientific foundation, and the prediction procedure becomes somewhat analogous to that of systematic weather forecasting. But this ideal situation is far from being realized, and scientists in every part of the world acknowledge that earthquake prediction is still in a research phase. It is significant that no country claims as yet to have a successful and routinely operative earthquake-prediction program.

2. THE NEED FOR EVALUATING GROUPS

Thus, in most seismic countries today, the need exists for formal, government-sponsored evaluation groups to advise government authorities, and ultimately the public, on the judged scientific validity of earthquake predictions that have come to public attention--from whatever sources. In many countries this may be the same group that is formally designated to make official predictions, but one must recognize that unofficial predictions may come from other sources as well. The fact that earthquake prediction is still in a research phase implies that scientists from a wide variety of institutions and agencies may be contributing predictions or elements thereof. Indeed, a significant problem already exists with scientists from one country predicting earthquakes in another country. For example, two American scientists recently predicted a series of potentially disastrous earthquakes in Peru, to be discussed below, and Russian scientists have predicted damaging earthquakes in both Italy and California. Regardless of the scientific or political wisdom of such pronouncements, they represent a problem that must be dealt with promptly and forthrightly within the concerned country, and local authorities may have no control whatsoever over the predictors or the announcements.

3. CREDIBILITY AND OPERATING PROCEDURES

In order for an earthquake-prediction evaluation body to be effective and retain credibility, it must, in my opinion, meet several conditions:

(1) It must be composed of members of respected scientific stature.

(2) It must be representative of a broad spectrum of the local scientific community, so that no single scientific clique dominates it either in perception or in fact.

(3) It must have sufficient periodic turnover in membership to retain scientific vitality, recognizing that the field is one of rapid change.

(4) It must be organized in such as way as to be able to react to a prediction with reasonable promptness.

(5) It must have the ear of appropriate governmental authorities, presumably by formally reporting its findings to a specific governmental executive of high rank.

(6) It must be able to hold scientific hearings and reach its decisions in an atmosphere reasonably devoid of non-scientific considerations.

(7) It must restrict its findings to those of a solely scientific nature; the problem of how to respond to a prediction, as opposed to its scientific evaluation, involves judgments of a social, economic, and political

nature that--however important--are best left to other groups of different makeup.

(8) It must be prepared to be exceedingly careful in handling predictions for earthquakes outside of its country's national borders;.one solution is to make no such evaluations except at the specific request of a foreign government.

(9) In concert with appropriate government authorities, it must make specific plans for relations with the news media and for the announcements of its findings.

(10) It must give careful forethought to the conditions under which the evaluating body will be willing to undertake a formal evaluation. Can anyone request an evaluation of his or her prediction ? Will written documentation be required before an evaluation is commenced ? Will long-term as well as short-term predictions be considered ? How will amateur scientific predictions be handled ? How will it respond, if at all, to predictions by psychics and soothsayers that cause considerable public concern ?

(11) It must be prepared for its findings to be the subject of both scientific and public controversy.

Non-scientists may not appreciate the difficulties and dilemmas faced by scientists in making the kind of judgments called for by the quasi-legal evaluation procedures. Research scientists normally submit new and controversial ideas in the form of manuscripts for publication in scientific journals. These manuscripts are then subject to paintstaking and often lengthy review by anonymous peers selected internationally by the journal's editor, with acceptance, rejection, or suggested modification as the final result. In the case of a truly innovative submission, such as is likely to be the case in a field as exotic as that of earthquake prediction, only a few scientists in the entire world may be fully qualified to offer fair peer judgment in the particular specialty of the author. Thus a given earthquake-prediction evaluation body--however prestigious--may feel very uneasy in the court-like proceedings in which it must make judgments under very tight time pressures and in fields which may be somewhat outside of the members'own areas of expertise. It is little wonder that service on an evaluation council may be looked upon as a tedious and somewhat thankless chore by most scientists.

Each country clearly has different ways in which an evaluation body can most effectively be set up and operated, depending on the local organization of scientific research, locally accepted governmental responsibilities, and the nature of local public-information and news-media systems. It is obvious that no one format would be uniformly applicable to all parts of the world, and it is not the purpose of this paper to suggest one. As an example, however, let me discuss the development of earthquake-evaluation procedures in the United-States.

4. THE CALIFORNIA COUNCIL

Starting in the early 1970's, it became clear in California that earthquake predictions were becoming a "problem," both because of the increasing number of amateur predictions and because of the apparent imminence of scientific predictions. Indeed, it was not always clear to governmental authorities who was an amateur and who was not; even the scientists could not always agree within their own ranks, and a number of predictors with seemingly reasonable scientific credentials were looked upon askance by other members of the scientific community. Thus in 1974 the Advisory Group on Earthquake Prediction was formed, subsequently renamed the California Earthquake Prediction Evaluation Council, reporting to the Director of the Office of Emergency Services, who is a member of the Governor's staff. It consisted of 9 scientists in the fields of geology, seismology, and geophysics, appointed by the OES Director. By charter, the California State Geologist (Chief of the Division of Mines and Geology) is Chairman of the Council, ex officio. Initially, the 8 remaining members of the Council comprised 2 scientists from the University of California system, 4 from other universities, and one each from the U.S. Geological Survey and the State Division of Mines and Geology. Of the 9 members, 6 could be considered to be seismologists-geophysicists and 3 to be geologists.

5. THE WHITCOMB PREDICTION

Although in its first years, several earthquake predictions were brought to the attention of the California Council that were deemed non-scientific and were therefore not formally evaluated, one scientific prediction was considered. This was the 1976 prediction by Dr. James Whitcomb of the California Institute of Technology for an event of about magnitude 6 to occur during the forthcoming year in an area northwest of Los Angeles, based on alleged systematic V_p/V_s anomalies. The forecast was sufficiently generalized that Dr. Whitcomb preferred to refer to it as a "hypothesis test" rather than as a true prediction, but

it nevertheless drew sufficient public attention and concern that the Council held a formal one-day hearing and offered its evaluation. The Council's judgment was basically negative, and Dr. Whitcomb himself withdrew the prediction somewhat later because the anomalous geophysical pattern did not persist. But several important policy questions were emphasized by the episode. For example, the Council's charter provided that the hearings were to be open to the public, but the presence of newspaper reporters and television cameras were admittedly disturbing to the participants, and Dr. Whitcomb, in particular, felt that the atmosphere was far from conducive to deliberative scientific decision-making. He and others have subsequently argued that, with professional reputations literally on-the-line, young scientists will simply not be willing to subject themselves to these kinds of court-like proceedings and will instead turn their research efforts in other directions; we thus stand in danger of stifling the very kinds of innovative research and hypothesis-testing that we are attempting to encourage in the earthquake-prediction effort. Others have argued, to the contrary, that the potential impact of earthquake predictions is so great that their scientific validity must be argued in a public forum, since it is the public's safety that is directly at stake. If the hearings are to be held behind closed doors, what is it that the scientists are trying to hide ?

This problem of the confidentiality of the scientific hearings is perhaps the most persistent and difficult policy question that has plagued the American earthquake-prediction evaluation councils. The present tendency in California seems to be opt for a somewhat more confidential evaluation procedure than was the case for the 1976 episode. On the contrary, this author has continued to argue that, at least in the American social system, the more open the process can be, the more the public will have confidence in it, and the more effective it will be in the long run. Secrecy tends to breed suspicion, and sometimes with good reason. While it is true that individual scientific reputations are at stake, the impact of a prediction is so great that the public's interest must come first; a scientist must indeed be exceedingly careful in the promulgation of a prediction, and he or she must be fully prepared to defend it openly. It is particularly significant that in the cases of both the Whitcomb prediction and the Brady-Spence prediction, to be discussed below, those newspaper and television reporters who sat through the entire hearings were the ones who

presented stories most sympathetic to the evaluation councils and to the trauma they went through in attempting to reach wise decisions. It would be the first to agree, however, that in both of these cases the presence of the news media-- and television cameras in particular--were unduly disturbing to all of the participants, and some modifications in procedures are clearly called for.

6. THE U.S. NATIONAL EVALUATION COUNCIL

Partly as a result of the generally successful experience in California, the federal government decided to form its own earthquake-prediction evaluation group in 1979. Although it was recognized that the most likely locale for an earthquake prediction in the United States was in fact in California, which already had a functioning evaluation council, the federal government felt obliged to have machinery for evaluating predictions elsewhere (e.g., in the even-more seismically active state of Alaska). Furthermore, for reasons of political reality, it evidently wished some degree of independence from state government in advising federal agencies, and the President, of judged earthquake potential. Particularly as a result of the Mt. St. Helens volcanic eruption in 1980, the federal government became more concerned about its disaster-related responsibilities.

The charter of the National Earthquake Prediction Evaluation Council provides that its members shall be appointed by, and shall report to, the Director of the United States Geological Survey (USGS). The Council shall consist of not less than 8 nor more than 12 members, appointed for staggered 3-year terms. At least one-half of the members, including the Chairman, shall be non-USGS employees. The membership currently consists of 5 USGS scientists, 5 university scientists, one state-government scientist, and one scientist of another federal agency. The large USGS membership partly reflects that fact that it has the greatest number of scientists in the country working on earthquake-prediction problems, and it is the government's "lead agency" in the national earthquake-prediction program. Indeed if a credible earthquake prediction were to be issued tomorrow, the USGS is the most likely source of that prediction, and if earthquake prediction eventually becomes a routine operation such as weather prediction, the USGS will probably be the agency assigned the duty. It should be noted that the USGS already has its own internal review system to evaluate predictions that may arise from its own research efforts, although predictions so "approved" would presumably be evaluated subsequently by the National Council as well.

As will be noted below, however, predictions involving USGS scientists that do not have internal approval may still come to public attention.

It is not intended that the California and National evaluation councils be in competition with one another, and coordination plans have recently been formulated for predictions within the state of California. In particular, the two groups will probably hold joint hearings under such a circumstance.

The National Earthquake Prediction Evaluation Council has recently come under some pressure to be, even now, the designated national body to make predictions as well as to evaluate them. This pressure has arisen particularly from governmental groups charged with responsibilities for earthquake-disaster planning and for earthquake-prediction response, who perhaps overestimate the scientists' current prediction capabilities. It is generally the feeling of scientists in the United States that the earthquake-prediction research effort is in such a state of infancy that centralization at this early stage would be premature and would do more to hinder the research effort than to help it. Admittedly, this attitude reflects the very diverse and somewhat unique organization of American scientific research, with major and to-some-degree competitive components in universities, government laboratories, and private industries.

The National Council is also under some pressure to evaluate long-term as well as short-term predictions. But most long-term predictions amount to little more than probabilistic statements of local seismic risk, however valid and well documented, and the routine evaluation of the multitude of such statements could easily become a full-time job. Over some unhappy objections, both the California and National Councils have thus far concentrated their attention primarily on short-term predictions—those involving specific and narrow time windows that would permit temporary rather than permanent countermeasures.

7. THE BRADY/SPENCE PREDICTION

In the three years during which the National Earthquake Prediction Evaluation Council has been in existence, only one serious scientific prediction has come to its attention, although it has been kept abreast of developing situations in parts of Alaska and California where predictions may eventually be forthcoming.

The one serious prediction was that of Drs. Brian Brady and William Spence for a series of three major earthquakes off the coast of Peru in 1982— a prediction that fortunately turned out to be a false alarm, and the details of which are discussed in greater detail elsewhere. The prediction was especially note-worthy for several reasons : (1) It was a prediction by citizens of one country for a series of potentially disastrous earthquakes in another country; (2) the largest of the three predicted earthquakes would have been of higher magnitude than any earthquake yet recorded; (3) The two predictors were United States government employees with reasonable scientific credentials, although the prediction was in no sense endorsed by their respective agencies (the U .S. Bureau of Mines and the USGS); (4) the scientific methodology of the prediction was unconventional and has never been described and made available in detailed written form; and (5) the prediction led to considerable economic and social disruption in the affected area.

Although the National Earthquake Prediction Evaluation Council had agreed at its initial organizational meeting in 1980 to generally avoid commenting on foreign predictions, in the light of an earlier embarrassing incident with Mexico, it undertook evaluation of the Peruvian prediction at the specific request of the President of Peru, to whom it reported its findings prior to release in the United States. Because the predictors were American citizens, and because the impact of the prediction was so great, the Council felt an obligation to undertake this task—which turned out to be a traumatic experience for almost everyone involved. After a highly publicized two-day hearing, some 5 months before the first predicted event, the Council formally repudiated the prediction, but one is left with the feeling that we should be able to handle episodes of this type better 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 National Evaluation Council have acted sooner and more positively to renounce the prediction ? Should the Peruvian scientific community have acted more positively, or perhaps have sought outside advice sooner, in repudiating the prediction ? Were the predictors given adequate and fair opportunities to defend the scientific basis of their prediction ? Was an open hearing, with the television cameras

rolling, the fairest and most effective forum for the scientific evaluation of the prediction ? In our efforts to be professionally fair to the predictors, were we being equally fair to the people of Peru ? Should the National Evaluation Council have refused to evaluate the prediction until it was published, or at least until it was written in some sort of formal scientific statement ? Should the employers of the predictors—in this case, agencies of the United States government—have been more active in "controlling" the announcements of their employees ? 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" ? Did the U.S. State Department, in its somewhat overzealous effort to help the Peruvians prepare for the possible earthquakes, add undue credence to the prediction ? These 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.

The Peruvian episode also points up the need for careful planning as to how the report of an evaluating group should best be written. Because of time pressures from the Peruvian government, the U.S. State Department, and the news media, the Council's report was necessarily composed within a very few hours following the close of the formal hearings, and later commentators have pointed out how some elements of the report might have been more effectively expressed. It is interesting to note, however, that the most convincing argument to people in Peru was reportedly not the scientific rationale for rejection of the prediction, but instead the statement that no members of the Council would have serious reservations about being in Lima on the date of the first predicted large event. Partly as a result of this statement, one member of the Council was in fact invited to Peru on that date, and his presence was duly noted by the Peruvian press—evidently with some calming effects.

One has the uneasy feeling that the Peruvian prediction episode may not be completely over. Seismologists have pointed out for a number of years that, on the basis of the generally-accepted seismic gap concept, parts of coastal Peru and northern Chile represent perhaps the most likely locality anywhere in the world for a truly great earthquake, such as last occured there in 1868. If this event occurs within the next few years, which is certainly possible, it may well be viewed as a vindication of the Brady-Spence prediction, regardless of the fact that the very specific time

window prescribed by that prediction has long-since passed, and the prediction was based on a quite different alleged scientific methodology. The very real possibility of a randomly occuring earthquake satisfying--or being perceived to satisfy--a rejected prediction is a scenario that continues to haunt all earthquake prediction evaluation groups and tends to make their jobs more thankless.

8. CONCLUSIONS

In considering procedures for the evaluation of earthquake predictions, one must recognize that both the scientific and political organizational frameworks are so different from one country to another that no one procedure can be held as the ideal example. Each country has its own peculiar set of circumstances. But there are some common elements : the need for scientific integrity and independence in the technical evaluation of predictions; the necessity for careful coordination with other governmental entities in the announcement and response to predictions; and the seemingly ubiquitous problems of dealing with rumors, non-scientific predictions, and "unofficial" predictions. The broadly representative earthquake-prediction evaluation councils in the United States have been generally successful in gaining both public and scientific credibility but have by no means solved all problems. Confidentiality of the hearings remains the most debated policy question. As the scientific ability to predict earthquakes gradually increases, we will undoubtedly have more predictions to evaluate, and many of these will continue to present difficult social and well as scientific dilemmas. The prediction of earthquakes in one country by scientists in another country represents a particularly troublesome problem, and this may be an area in which worldwide scientific cooperation can assist in forming an internationally accepted "code of ethics" for earthquake predictors. Service by scientists on earthquake-prediction evaluation bodies can be a tedious and thankless task, but it is a necessary obligation if scientists are to continue to claim that earthquake prediction is a socially valuable endeavour.

DISCUSSION

Prof. Nersesov judged it inappropriate to compare the Soviet predictions for the USA and Italy, with Brady's prediction for Peru. Brady's prediction was unsuccessful, while both of the Soviet predictions were successful. He noted that the predictions for the USA and Italy were made in co-operation with scientists from those countries.

Dr. Tazieff requested that the question of establishing a code of ethics for dealing with predictable natural catastrophies (including volcanic eruptions) be discussed by the group.

Prof. Roberts commented that although Prof. Allen's paper said that NEPEC should restrict itself to evaluating short-term predictions, it appears that the only predictions that have been evaluated to date are Whitcomb's by CEPEC, and Brady's by NEPEC. Dr. Tomblin noted that it is important to distinguish between short-term (i.e. imminent) predictions and those with short time-windows (i.e. to occur at a very specific but possibly distant future date).

Prof. Lomnitz suggested that a code of ethics may not be necessary, but a committee of ethics may be important to restrain scientists from issuing "premature" predictions.

Prof. Roberts raised two questions regarding evaluation bodies: (1) Is there any obligation for scientists to present their predictions for review? (2) What rules should apply to the types of work that should be reviewed?

Ing. Giesecke raised an additional question concerning the obligation of scientists to have their predictions for another country reviewed by an evaluation body of that country.

Ing. Giesecke disagreed with two of Prof. Allen's points: (1) the Peruvian scientific community did act early in the prediction process, but it took 2½ years for a formal evaluation to be conducted; (2) the Brady prediction episode is ended, even though the earthquake threat continues.

In response to a request to hear the full text of the NEPEC evaluation of Brady's prediction, Prof. Roberts read a copy of that evaluation.

Ing. Giesecke commented that contrary to the impression conveyed by Allen's paper, the Brady evaluation was not a totally open meeting. The hearing was open, but the deliberations of the Council were conducted "behind closed doors".

It was noted that Prof. Allen in his paper had suggested that it would be useful to discuss the type of criteria that evaluating bodies use to decide whether they will actually review a prediction.

Prof. Lomnitz raised the question as to what type of evidence would have been sufficient for NEPEC to uphold Brady's prediction. Several participants agreed that the criteria to evaluate predictions should be further specified.

N.B.

Prof. Allen was unable to attend the seminar. The above paper was read in his absence and the discussion held without his participation.