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Earthquake prediction: is this debate necessary?

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Because large earthquakes release huge amounts of energy, many researchers have thought that there ought to be some precursory phenomena that could be consistently observed and identified, and used as the basis for making reliable and accurate predictions. Over the past 100 years, and particularly since 1960, great efforts, all unsuccessful, have been made to find such hypothetical precursors. For further details see my review[1], which includes eight pages of references (in 6-point type, to save space) to this vast body of work.

The public, media, and government regard an 'earthquake prediction' as an alarm of an imminent large earthquake, with enough accuracy and reliability to take measures such as the evacuation of cities. 'Prediction' is used exclusively in the above sense here; in other words, longer-term forecasts of seismic hazards or statistical forecasts of aftershock probabilities are not classified as predictions.

Three obvious questions arise:

1. What pitfalls have undermined prediction research?
2. Why are earthquakes so difficult to predict?
3. Why is prediction still being discussed?

These questions are answered below.

Most earthquake prediction research is empirical, featuring the 'case-study' approach. After a large earthquake, data of all sorts are examined retrospectively in the hope of finding a precursory signal. Workers reporting candidate precursors frequently set up observatories to look for similar signals before future earthquakes.

Empiricism should not necessarily be dismissed out of hand, as it has led to many important scientific discoveries. However, as noted by E.B. Wilson, without proper controls the empirical approach can lead to absurd conclusions, for example that the beating of tom-tom drums will restore the Sun after an eclipse. Lack of controls is one of the main problems that has dogged the search for precursors.

Another chronic problem is attributing 'anomalous' signals to earthquakes before considering more plausible explanations. One research group has repeatedly claimed to be observing electrical precursors of earthquakes (and even managed to get relatively favourable publicity in Nature's news columns[2,3]), but it now seems likely that the signals are noise due to nearby digital radio-telecommunications transmitters, and are unrelated to earthquakes[4].

Rigorous statistical analyses are rarely performed by prediction researchers, leading to a plethora of marginal claims. There are two main problems. First, most precursor claims involve retrospective studies, and it is easy to 'tune' parameters after the fact to produce apparently significant correlations that are actually bogus[5]. Second, earthquakes are clustered in space and time, and spuriously high levels of statistical significance can easily be obtained unless appropriate null hypotheses are used[6,7].

Why is prediction so difficult? This question cannot be answered conclusively, as we do not yet have a definitive theory of the seismic source. The Earth's crust (where almost all earthquakes occur) is highly heterogeneous, as is the distribution of strength and stored elastic strain energy. The earthquake source process seems to be extremely sensitive to small variations in the initial conditions (as are fracture and failure processes in general). There is complex and highly nonlinear interaction between faults in the crust, making prediction yet more difficult. In short, there is no good reason to think that earthquakes ought to be predictable in the first place. A few laboratory failure experiments might seem to suggest otherwise, but they are conducted on a limited scale and do not replicate the complex and heterogeneous conditions of the problem in situ.

If reliable and accurate prediction is impossible now and for the foreseeable future, why is it being debated on Nature's web site? The answer seems to be sociological rather than scientific. Certain research topics are fatally attractive to both scientists and the general public, owing to the combination of their extreme difficulty and great potential reward. No less a scientist than Sir Isaac Newton regarded alchemy (the transmutation of elements by chemical reactions) as his primary research field. His continued failures drove him to despair, and led him to give up science for a sinecure as Master of the Mint. Sir Isaac's failures notwithstanding, alchemy continued to attract the fruitless efforts of talented scientists for another 100 years. Earthquake prediction seems to be the alchemy of our times.

The examples of alchemy and perpetual motion machines show that the only way to 'prove' something is impossible is by developing a satisfactory theory of the underlying phenomenon (nuclear physics and thermodynamics, respectively). No satisfactory theory of the earthquake source process exists at present. Further work should be encouraged, but it will probably lead to a better understanding of why prediction is effectively impossible rather than to effective methods for prediction.

Governments in many countries have awarded lavish funding for work on earthquake prediction[1]. Such funding frequently greatly exceeds what is available through normal peer-reviewed channels for even highly meritorious work. It is regrettable that this disparity sometimes induces reputable scientists to label their work as 'earthquake prediction research' to get a share of such funding.

In view of the bleak prospects, there is no obvious need for specialized organizations and research programmes for prediction. Researchers in this area should seek funding through normal peer-reviewed channels (such as the NSF in the USA), in competition with all other research in earthquake science. This would probably lead to an almost complete phasing out of prediction research, not because of censorship but rather owing to the poor quality of most present work in this field. Of course meritorious prediction proposals (if any exist) should be funded.

More importantly, meritorious work on estimating long-term seismic hazards, real-time seismology and improving design standards for earthquake-resistant construction should be funded, along with basic research and the operation of observational networks, as the key components in an integrated programme of seismological research.

Now that prediction research is under pressure in many countries, including Japan[8], some prediction proponents might seek to reposition their work as one component of such an integrated research programme for the reduction of seismic hazards. However, in view of the goals and methods of prediction research, this seems unwarranted.

Under the ground rules of this debate, participants are not allowed to see other contributions before publication. However, unlike earthquakes themselves, the arguments used by prediction proponents are eminently predictable. See box for a rebuttal of some of the arguments that are likely to be used by the other side in this debate.

The sad history of earthquake prediction research teaches us a lesson that we should already have learned from cold fusion, polywater and similar debacles.

Namely, the potential importance of a particular research topic should not induce a lowering of scientific standards. In the long run (and in the short run too), science progresses when rigorous research methodology is followed.

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