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Sociological aspects of the prediction debate

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The question at the heart of this debate appears to be whether earthquake prediction should be recognised as a distinct and independent research field, or whether it is just one possible research topic in the general field of study of the earthquake source process. As there are no known grounds for optimism that reliable and accurate earthquake prediction (as defined in my first article) can be realized in the foreseeable future, the case for the latter position appears clear-cut. As a consequence there is no obvious need for specialised organisations for prediction research. Besides the benefits that always accrue from pruning deadwood, abolition of such organisations would force prediction proponents and critics to confront each other in common forums, thereby speeding the resolution of the controversy.

Paradigms Lost?

A specialized community of scientists, which has its own journals, meetings, and paradigms, is the arbiter of what is acceptable in its own field[1]. Viewed in sociological terms, such groups strive for recognition of their authority from the broader scientific community. In the long-run this recognition is dependent on whether a community's methods and theories can successfully explain experiments or observations.

In the short- and intermediate-term however, subjective and sociological factors can lead to recognition being accorded to scientific communities whose paradigms are lacking in merit, or to the needless prolonging of controversies. Some revisionist historians of science have recently called attention to these sociological aspects of scientific research (in discussions commonly referred to as 'science wars').

While physical theories are certainly more than arbitrary social conventions, working scientists must admit that there may be room for improvement of present methods for resolving scientific disputes. The earthquake prediction debate provides an example of how sociological factors can impede the resolution of a scientific controversy.

Cold fusion: Case Closed

Cold fusion is a case where current methods for resolving controversies worked reasonably well[2]. Cold fusion proponents attempted to set up all the trappings of a genuine research field (specialized research institutes, conferences, journals, funding programs), but once the underlying experiments were shown to be unreliable, the cold fusion enterprise quickly collapsed.

This episode was basically a success story for science, although relatively large costs were incurred in the evaluation process before cold fusion was rejected[2]. One reason the controversy could be efficiently resolved was that much of the debate was carried out in the open, for example at meetings of scientific societies or in scientific journals. Consequently the largely positive conclusions reached by cold fusion 'believers' at their own specialized conferences were not accorded credence by the scientific community as a whole.

Ten years after the first public cold fusion claims, a small band of cold fusion proponents continues to hold out (*New York Times*, 23 March 1999). Until fairly recently international cold fusion conferences were still being held[3]. Nevertheless, the cold fusion community has clearly failed to convince the scientific community as a whole of the legitimacy of its claims and methods.

Cold fusion is typical, rather than unique. In all episodes of 'pathological science' there are some credentialed scientists who hold out indefinitely in support of generally discredited theories[4]. Debates are resolved when the mainstream scientific community decides that one side or the other has nothing new to say and treats the discussion as effectively closed, barring truly new data. Perhaps it is time to consider whether the prediction debate has reached this point.

Chronic Problems in Geoscience

Geoscience is an observational field and controversies are harder to resolve than in more experimental disciplines. For example, Wegener's early 20th century evidence for continental drift was widely disregarded because of objections to the proposed driving mechanism[5]. It was not until 1967-68 that evidence from paleomagnetism, marine geophysics and seismology became so clear-cut that the geoscience community generally embraced plate tectonics, of which continental drift is one consequence.

The dramatic turnabout that led to the acceptance of continental drift has perhaps made geoscientists wary of resolving other controversies, lest they later be proven wrong. But all such decisions are inherently made on an interim basis, and controversies can always be reopened if new data are obtained. Allowing controversies such as the earthquake prediction debate to remain open indefinitely wastes time and energy, thereby slowing scientific progress.

Ironically, the advent of plate tectonics was viewed in the late 1960s and early 1970s as reason for optimism about earthquake prediction[6]. This was not wholly unreasonable, as plate tectonics explains why large earthquakes are concentrated along plate boundaries, and also the direction of earthquake slip. Unfortunately, we now know, as noted by Jackson that plate tectonics does not allow either short-term or long-term prediction with success beyond random chance (although some controversy still lingers; see Scholz and Jackson).

Deconstructing the debate

On the surface the central question in *Nature's* current prediction debate has been how much funding should be allocated to 'prediction research'. At the extremes Wyss says as much as is now given to research in astrophysics while I say none, except through the normal peer-review process; the other debaters hold positions between these.

Wyss and I reach diametrically opposite conclusions despite our agreement that there are no immediate prospects for reliable and accurate prediction. The reason appears to be that Wyss's implicit starting point is that earthquake prediction is a legitimate scientific research field, and should be funded as such. On the other hand, I argue that prediction research is in principle a perfectly legitimate research topic within the field of study of the earthquake source process (although much prediction research is of too low a quality to warrant funding), but that it is not a legitimate research field in its own right. One hallmark of a research field is the existence of a widely recognised journal. It is interesting to note that the journal *Earthquake Prediction Research* ceased publication in 1986 after only 4 volumes.

Resolving the debate

My point of view leads to a number of specific conclusions. One is that discussion of `prediction research' at scientific meetings should be held together with all other talks on the earthquake source process, rather than off in its own room, attended only by prediction 'believers'. This might make life unpleasant for everyone in the short run, as it would force prediction proponents and critics into head-on confrontations, but in the long run such discussions, although sometimes painful for all concerned, would be invaluable for resolving the prediction controversy. Holding prediction and earthquake source sessions in the same room at the same time would also encourage the development of common terminology, and would lead to more rapid dissemination of new research results.

The major international body for seismology is the International Association of Seismology and Physics of the Earth's Interior (IASPEI). One of the working groups under the IASPEI is the 'Subcommission on Earthquake Prediction'. This and similar bodies were founded 20 or 30 years ago at a time when there was more optimism about prospects for prediction than exists at present[6]. The need for such bodies should be re-examined in light of current knowledge of the difficulties besetting prediction research. Even if such bodies were not abolished, their terms of reference ought to be redefined to reflect current scientific knowledge.

I emphasise that I have no intention of criticising the officers or individual members of the IASPEI Subcommission (although I don't share some of their scientific views). Rather my point is that the very existence of a separate body for 'prediction research' is an impediment to scientific progress, as it tends to cleave 'prediction research' apart from work on the seismic source in general.

There are many other prediction organisations whose continued existence might usefully be reviewed. Among these are the various bodies associated with the earthquake prediction program in Japan (see section 5.3 of ref. 6), and the US National Earthquake Prediction Evaluation Council, which endorsed the unsuccessful Parkfield prediction (see section 6 of ref. 6). The European Seismological Commission's Subcommission for Earthquake Prediction Research is another organisation that might merit review.

Just as war is too important to be left to the military, earthquake prediction should not be left only to prediction proponents and ignored by the rest of the seismological community. Unfortunately this is a generally accurate, albeit somewhat oversimplified, description of the present situation.

I feel that if special organisations for earthquake prediction were abolished, thereby forcing the prediction debate into the open, it would be possible to achieve some resolution relatively soon. However, unless this is done, the earthquake prediction debate appears doomed to linger in its present form almost indefinitely. Anyone comparing my articles in this debate to that of Macelwane[7] in 1946 will be struck by how little has changed. Let us hope that seismologists in 2049 will not be making similar comments.

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