More agreement than division

MAX WYSS

As we all agree that we know little about how earthquakes initiate and how to predict them, it follows that we will study the problem and eventually reach a relatively satisfactory solution. The question is, will we do it with the significant financial support and expedience this humanitarian effort deserves, or will we force individual scientists to do it in their non-existent spare time?

Definition of earthquake prediction

The attempt to define earthquake prediction in such a narrow way (a time resolution of a few days) is failing, thus forcing it to be declared as generally impossible. This is a red herring. If I'm not mistaken, nobody in the debate has disagreed with the point, made by many, that there are significant benefits derived from intermediate- and long-term predictions, even if they are made with relatively low probabilities. In which case, let us see if we cannot make progress in formulating rigorous, yet consumer friendly statements describing the time dependent earthquake hazard in some locations. That is, predict earthquakes.

Quality of research

There are two types of influences that degrade the quality of research into earthquake prediction. Firstly, there are two emotional factors. Limelight seekers are attracted to this field and others are electrified into hasty work by the idea of coming to the rescue of the populace. But there is a second problem; lack of financial support. A researcher who explores a hypothesis after regular working hours exclusively, is likely to present the 'best' results only, instead of exploring all the possible, but marginal data sets that may exist.

Thus, our weapons in the struggle for high quality work are threefold:

1. Funding the research at an adequate level such that the most capable scientists are attracted to this field; such that a researcher has the time to penetrate to the greatest depth allowed by a data set; and such that the necessary high quality data are available.
2. Rigorous peer review of research proposals.
3. Stringent reviews of journal articles.

We should use all of these tools to foster high quality earthquake prediction research.

Case histories are often all we have

Very large earthquakes occur too infrequently to test hypotheses on how to predict them with the statistical rigor one would like (for example L. Knopoff's contribution to this debate), and potential data sets for testing are further reduced by the need to separate different tectonic settings (see Z. Wu's contribution to this debate). In addition, most earthquakes occur far from existing dense instrumentation networks, making it impossible to gather data pertinent to most hypotheses.

Thus sets of case histories in which the individual cases may number about a dozen will be all we will have for years to come, whether we like it or not. However, I do not see this as a reason to give up prediction research or rupture initiation studies altogether, as long as we have not exhausted the data available. As it is, we have hardly scratched the surface, because of lack of funding.

Here we go again.

"extensive prediction efforts in several countries in several eras have all failed" (Geller, this debate).

Such exaggerations know no bounds. What "several eras" has the fledgling discipline of seismology seen? I'm utterly unimpressed by the quotes Geller used to support his assertion in week four of this debate, because these quotes stem from the years 1937 and 1945. No wonder satisfactory results concerning the problem of earthquake prediction were not achieved, although this problem was "attacked with every resource at their command," since they had essentially no resources, had not even classified earthquakes as to type and size and had not yet understood the reason for earthquake ruptures on the planet.

The most basic tool of seismologists is the seismograph network. Rudimentary networks first came into existence in a few locations in the 1930s. A world wide network was installed in the mid 1960s, and anyone who wishes to analyze high resolution earthquake catalogues produced by dense networks cannot start their data set before the 1980s, because up to that time the data were so poor. Thus the researchers around 1940, whom Geller quotes, had hardly an opportunity to catch a glimpse of the distribution of earthquakes in space, time and as a function of size. They were in no position to conduct serious prediction research. They did not have even the most basic, let alone sophisticated tools.

In addition, the reason for fault ruptures (earthquakes) on this planet was only discovered in the late 1960s. Only then did it became clear that the planet cools by moving heat, generated in its interior due to radioactive decay, by convection to the surface, where brittle plates are pushed past one another, generating earthquakes. Preoccupied with consolidating this discovery for about a decade, seismologists spent no time on prediction research and plans drawn up for such a program remained largely unimplemented (see A. Michael in this debate).

It is clear that in the US there was never a serious research program for earthquake prediction. There did exist a thorough seismology program to detect and discriminate nuclear explosions in the USSR and China, which was very successful, because it attracted the best workers in the field, since it was well funded.

Separation of sub-disciplines

Geller seems preoccupied by separation of sub-disciplines. Most researchers and educators try to combat the barriers that constantly appear between sub-disciplines but it is difficult to keep the channels of communication open. Of course the problem of earthquake prediction is intimately intertwined with those of the seismic source processes, of tectonics and self organized criticality. In addition, laboratory experiments on rock fracture, computer simulation of faulting, crustal deformation measurements and modelling, as well as studies of ground water properties are all important for, and applicable to, problems in prediction research.

It would be beneficial, if, as Geller suggests, an audience of such wide expertise were in one room at a conference. For one thing Geller himself would then not make such elementary mistakes as to confuse increased seismic moment release with "more small earthquakes." However, as we all know, wide participation in specialist lectures at conferences is unrealistic. People cannot be forced to attend. To foster interactions between sub-disciplines, one must make the special effort of interdisciplinary retreat-meetings.

Thus, I disagree with Geller when he sees a need for organizational changes. The boundaries of sub-disciplines establish themselves and there is no particular evil associated with them. However, I agree that more frequent retreat-meetings with attendance by experts from a wide range of fields is needed to advance earthquake prediction research.

Conclusion

I conclude that the criticism of earthquake prediction research has some worthy targets: the low quality work and the exaggerated claims that exist in this field. I hope we can reduce the originators of these problems to a small minority (they will never completely disappear). However, when the criticism takes on the character of a crusade, which tries to outlaw earthquake prediction research, many of us grow a bit tired of the "debate".

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