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FORUM

Earthquake Precursors: Banished Forever?

"When a rupture starts, how can it know where will it stop?" [*Gregory Podyapolsky*, 1973]

Podyapolsky, a brilliant theorist, made this comment many years ago, upon perceiving that the earthquake prediction bandwagon in the Soviet Union was gaining speed. Twenty years later, we can observe just how far the bandwagon has gone. The report on the Assessment of Schemes for Earthquake Prediction Symposium [*Geller*, 1997a], given by one of its key figures, provides some insight. I hazard to sum up the key points of this report as follows:

1. Earthquake prediction as a means of making reliable short-term forecasts is currently as far out of reach as it was 30, or even 60, years ago.

2. An earthquake is a nonlinear phenomenon in a system whose evolution is fundamentally unpredictable. Hence, the entire earthquake prediction problem is ill-posed: its subject simply does not exist.

3. The results of earthquake prediction research are mostly or entirely fictitious; earthquake prediction research as a whole is not good science.

With 30 years of experience in earthquake prediction research, I have comments on these points:

1. It is true that there is no socially valuable prediction technology in sight. All of the more or less convincing results, such as demonstrations of a statistically significant precursory phenomenon, or of unusual anomalies observed in advance of certain events, are, at best, of academic value. People who hinted at or openly promised the ability to develop prediction methods of practical importance made understandable human errors, no less and no more. However, in retrospect, one can only lament the excessive money and effort wasted; previous experience cannot definitively answer the question of whether or not earthquake prediction is a fundamentally sound scientific endeavor.

When discussing this point, it is important to distinguish between precursory phenomena and prediction methodology proper (actual or potential). Assume one has established a statistically significant correlation between a feature of a geophysical parameter and future earthquakes. This correlation may well be of high scientific interest, but at the same time it may not be possible to change noticeably the anticipated probability rate of future large events, resulting in a useless practical prediction tool.

I believe that during the last 30 years there has not been sufficient evidence that would enable the design of an efficient earthquake prediction methodology. For this reason, only the precursory phenomena deserve serious discussion at present. They must be solidly studied, first of all empirically, and if possible, phenomenologically or theoretically. Until this is done, any attempt at prediction methodology remains doomed.

I see a general cause for the present earthquake prediction failure. Intrinsically, the problem is posed in the geological timescale, while it is attacked within the human—or even the project—timescale. It is normally part of human nature to respect things, such as redwood trees and pyramids, that are characterized by large timescales. However, in the earthquake prediction case, strong societal interests for viable prediction have pressed for a phenomena with a 100- or 1,000-year timescale to be tamed by a short-term effort. Good prototypes for earthquake-prediction research must be sought instead in the history of studies of Earth's magnetic field or floods of the Nile River. Such a slow style, involving steady accumulation of observational data, may appear old-fashioned, but I see it as critical for any true progress.

2. The earthquake unpredictability concept is now mature and rather energetically formulated with self-organized criticality [*Geller*, 1997b]. On the other hand, I fully agree that simplistic prediction schemes based either on direct deterministic modeling or on assumedly regular precursory patterns must be rejected. I perceive tectonic processes as multiscaled, highly nonlinear, often random in appearance, and generally unmanageable in a deterministic way. However, to be hopelessly unpredictable, a system must lack memory and be isolated; both conditions are hardly valid for an earthquake fault.

The notion of unpredictability is evidently becoming a true new paradigm, namely "everybody knows earthquakes are unpredictable." This conventional wisdom is now suppressing the previous thought that "earthquake prediction is just a corner or two ahead." It is important to realize, however, that the present paradigm shift results from human disillusion and experimental failures. No serious theoretical proof of unpredictability exists.

Until the next paradigm shift, which I optimistically expect to be caused by convincing confirmation of precursors by high-quality, long-term experiments, the thinking of researchers and reviewers is likely to be shaped increasingly by the unpredictability paradigm. This would have two results. It would prevent carrying out many useless and useful observations and analyses, which is evidently a mixed blessing. Secondly, it would establish a prejudice (or a ban?) against a class of possible experimental Earth science results, which is clearly an unwanted outcome. If earthquake prediction researchers would be as fortunate as people who deal with volcanic eruption precursors, and have revealed precursors that despite not being completely reliable, theoretically founded, or statistically significant, are still workable, nobody would object. And the theorists would eventually explain why it must be just so.

3. I agree that a significant fraction of earthquake-prediction results convince only those willing to be convinced. In regular scientific cases, three independent confirmations of a result normally confer its acceptance. In certain "quick-success" cases, especially those for which there is important practical application (cold fusion or earthquake prediction), the powerful mechanism of wishful thinking comes into play, and even tens of confirmations may prove nothing. Thus, it is not at all unexpected that a large percentage of earthquake prediction research will prove to be bad science. The most powerful mechanism

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for producing bogus results is to perform data analysis without sufficient experience in applied statistics. Successful application of statistics requires experience and judgement as well as education. One possible measure against self-deception is Albert Prozorov's proposal to require a significance level of the order of 10^{-5} to 10^{-6} when the existence of a precursory phenomenon is proclaimed in a retrospective study.

However, a considerable percentage of earthquake-prediction results, including rare successful real-time forecasts, simply cannot be judged statistically and are labeled "anecdotal." Are all such results of zero value? I do not think so. The path of any science begins with anecdotes, then systematics, phenomenology, and only then, quantitative analysis; thus, some fraction of anecdotes may well have something to tell us. The problem is that different steps along this path usually need researchers with different mentalities and even different languages. I would partly describe this problem as a scientific subculture clash between a naturalist's ("soft") and a theorist's ("hard") science.

On the soft end of the scale, a researcher may make all efforts to measure something accurately, to exclude data gaps, etc. If a researcher observes some feature that presents a rare anomaly according to his criteria, and it precedes a rare earthquake, this is a reasonable case for suspecting the feature to be precursory. On the hard end of the scale, the single observation is seen as anecdotal evidence, not worth the money spent even when it is not falsified. However, given the long return periods of strong earthquakes at a certain location, statistically significant evidence is simply impossible to collect during a 10- to 30-year period. And to stack results from various locations is, in essence, to view these cases as a sample from a statistical ensemble—an assumption that may well be incorrect. Therefore, if one requires a hard empirical proof of predictability, failure is virtually guaranteed. In my view, every result that can be considered accurate, unusual, and interesting deserves publication, but the review criteria must be strict. The result need not be accompanied by any theoretical explanation, which would very likely be premature.

I also believe that a considerable fraction of evidence collected to date reflects real precursory phenomena. I will give three examples:

- Foreshocks: the only solidly established earthquake precursor.
- The IASPEI Subcommission for Earthquake Prediction chaired by Max Wyss dealt just with evaluating possible precursors [*Wyss*, 1990]. In addition to foreshocks, it has put two more applications on the "preliminary precursor" list. Looked at as single papers, these applications are quite convincing. One of them (Izu peninsula anomalies) is a rather impressive case story ("anecdotal"). Another (quiescence within an aftershock series) has been confirmed twice and thus can be viewed at as a scientific fact. The comment [*Geller*, 1997a] that the panels ranking the candidate precursors consisted of earthquake prediction people hints to a biased judgement. Anyone can read rather harsh and instructive discussion sections in the volume that put the two new applications forward and will find no permissive bias there.
- From 1967 to 1971, I made a retrospective earthquake prediction study aimed at time forecasting of M>5.5 events on Kamchatka [*Fedotov a.o.*, 1972]. I constructed a nonadaptive pattern-recognition algorithm based on various statistics of small earthquakes. In the examination experiment, I obtained a result with a significance of 0.2%. To imitate real-time study, the examination data set had never been analyzed before, and no adjustments were made to the procedure when applied to the examination set. With modern tools (adaptive neural networks, jack-knife, etc.), the result might be more impressive still.

To conclude, earthquake-prediction research, though clearly not ready to produce a workable prediction methodology, remains a hopeful and intrinsically sound field worthy of further effort. On the other hand, weak research should not be encouraged, and there is indeed good reason to apply a specific, particularly strict, review procedure to earthquake prediction-relevant projects and results. However, such a procedure must not simply be a ban on all funding and publication of a "proven non-science." One should not forget that a couple of centuries ago this was already done with meteoroid research, because "as everybody knows, there are no stones in the sky."—*Alexander Gusev, Institute of Volcanic Geology and Geochemistry, Russian Academy of Sciences, Petropavlovsk-Kamchatskii, Russia, e-mail: gusev@omsp.kamchatka.su*

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