debates

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Earthquake prediction is difficult but not impossible

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For a prediction to be successful, the probability of occurrence in a time interval and a space domain must be specified in advance, as must the lower magnitude. There are two important additional constraints: a utilitarian constraint demands that the lower magnitude bound be appropriate to societal needs; in other words, we are especially interested in strong destructive earthquakes.

The time intervals for societal needs in the developing countries are of the order of days, but in the developed countries the windows can be broader, even of the order of years, because the response can be one of marshalling resources to improve construction, for example. A second constraint is that we must guard against self-indulgence: if the time or space windows are made too broad, or the magnitude threshold is made too low, then we can increase the probability of success up to 100% without any serious effort on our part (as, equally, will a Poisson random process). To avoid this problem we must specify how our probability estimate for the window compares with the poissonian estimate.

Despite our assertions about the desirability of probabilistic estimates the problem is not statistical. There have been too few large enough events in any small sufficiently area in the past century to be able to define probabilities of the largest events sufficiently accurately.

Cyclic inferences

There are two ways in which to proceed. One is to study the time intervals between earthquakes in the region in this magnitude scale. If earthquakes are periodic, the problem is solved. Current estimates of interval times through measurements by global positioning by satellite (GPS) of rates of slip, coupled with geological estimates of slips in great earthquakes, give only average values of interval times. However, from palaeoseismicity, we find that the interval times for the strongest earthquakes at one site on the San Andreas fault have large variability[1]. The statistical distribution of these interval times is poorly identified even in this, the best of cases. And a long duration since the last occurrence is no guarantee that the next event is imminent; the next event could be farther in the future[2], as Ian Main has also noted. The conclusion depends on the actual interval time distribution, which is unknown.

The failure of the Parkfield prediction is a case in point: extrapolation from a brief set of interval times was insufficient to provide adequate information about the distribution of interval times. The variability of interval times is due to the influence of earthquakes on nearby faults; the earthquakes on a given fault cannot be taken as occurring as though they were independent of the activity on the other faults in the neighbourhood. Without information about the distribution of interval times, an earthquake prediction programme based only on GPS and short runs of palaeoseismicity must fail; the average values of slips and slip rates alone are not sufficient to solve the problem, but they comprise one of several pieces of information important to the prediction problem. Indeed, it is only on some faults that we have information about the date of the most recent sizable earthquake. What is lacking in this version of the programme is a theoretical effort to understand the distribution of interval times in one subarea due to earthquakes on an inhomogeneous network of inhomogeneous faults and subfaults, a modelling problem of considerable difficulty.

De novo prediction

The second and more attractive approach is to search for the immediate precursors of strong earthquakes. Here there have been many culs-de-sac: searches for foreshocks, tilts, radon, electrical precursors and variations in velocity ratios of P-waves to S-waves have either failed or are at best unproven. In general, these efforts (*a*) failed to restrict the problem to the study of large earthquakes and (*b*) failed to evaluate seriously the success in units of poissonian behaviour. In many cases the invalid assumption was made that one could use the prediction of small earthquakes as a proxy for the prediction of large ones.

Part of the blame for the use of the assumption can be put at a misinterpretation of the Gutenberg-Richter magnitude frequency distribution. The illusion of the G-R distribution is that there are no characteristic scale sizes except for the largest-magnitude events that a region can support. We now know that there are at least three subscales in the Southern California distribution: the central trapped-mode core of the fracture in the largest earthquakes has a dimension of the order of 100-200 m (ref. 3); the dimension of the zone of aftershocks astride a large fracture is of the order of 1-3 km; and the thickness of the brittle seismogenic layer is of the order of 15 km. (Space limitations do not allow me to discuss the cause of the apparent log-linearity of the G-R distribution in the presence of characteristic length scales[4].)

Because of the wealth of scales, the 'prediction' of earthquakes at a smaller scale to understand larger ones cannot be valid. The assumption that we can amplify our data set by a study of large earthquakes worldwide is also not tenable, because of the large variability of the faulting environment for the largest earthquakes from region to region.

Statistics of rare events

The small number of events means that again we need a physics-based theory of the precursory process to amplify the meager data. In the area of physics, another blind alley was followed. The beguiling attractiveness of the illusion of scale-independence of the G-R law suggested that the model of self-organized criticality (SOC), which also yielded scale-independent distributions, might be appropriate. (The logic is evidently faulty: if mammals have four legs, and tables have four legs, it does not follow that tables are mammals, or the reverse.) The model of SOC permits a hierarchical development of large events out of the nonlinear interaction of smaller events, at rates in relation to their sizes, and culminating in the largest event. However, there are several important arguments against the applicability of SOC to the earthquake problem.

1. Faults and fault systems are inhomogeneous: we have already noted the presence of several scale sizes.
2. Seismicity at almost all scales is absent from most faults, before any large earthquake on that fault; the San Andreas Fault in Southern California is remarkably somnolent at all magnitudes on the section that tore in the 1857 earthquake.
3. There is no evidence for long-range correlations of the stress field before large earthquakes.

I do not see that the salient properties of SOC that are requisites for its application are reproduced in the earthquake data.

It is now time to develop a sound physics-based theory of the precursory process that takes us away from simplistic models. Such a theory should study the organization of seismicity on the complex geometry of faults and fault systems, and should bring to bear the properties of rocks under high deformational stress and under realistic loading and unloading rates. It is impossible to anticipate catastrophic failure on a purely elastic-loading/brittle-fracture model of rupture. As it has been for nearly 60 years[5], the detection of non-elastic deformation under high stress before fracture is the most promising avenue for the detection and identification of precursors. The nucleation of the largest earthquakes on inhomogeneous faults will take place at sites of greatest compressional strength, which are of geometrical origin[6]. These localized sites are those most likely to display precursory accelerated slip. The tasks of identifying these sites in advance and of measuring the deformation at them are not easy, even for impending large earthquakes. The task of identifying faults and measuring slip on them before remote small earthquakes, such as the recent Armenia, Colombia, event, does not seem to be possible at present.

In my opinion, fluctuations in seismicity are not active agents that participate in a process of self-organization toward large events. Rather, they serve as qualitative stress gauges to indicate that regions of the Earth's crust are in a state of high stress or otherwise. We have used fluctuations in the rates of occurrence of intermediate-magnitude earthquakes to construct hindsight predictive techniques[7] that are successful at about the 80% level (with large error bars) and represent an improvement over poissonian estimates of the order of 3:1 for a region the size of Southern California, with time constants of the order of 10 years, and with a magnitude threshold around 6.8. This is not much progress, but it is a step in the right direction.

Challenges not insolubles

The recent paper by Geller *et al*.[8] is in error on two counts. First, it states that the model of SOC shows that earthquakes are unpredictable. In fact, SOC 'predicts' stresses more readily than do chaotic systems. I have indicated above that the model of SOC is inapplicable to earthquakes on several counts: the data fail to show scale independence, the data fail to show long-range correlations in the stress field, and individual faults are remarkably inactive before large earthquakes.

Second, the paper[8] states that the problem is too difficult, and we should therefore give up trying. I believe the opposite. The community has indeed tried the seemingly easy methods, and they have failed. For 25 years the leadership of our national programmes in prediction have been making the assumption that the problem is simple and will therefore have a simple prescriptive solution.

We have been guilty of jumping on bandwagons without asking the basic questions, "What is an earthquake? What determines its size, and why is it likely to occur where and when it does?" These are physics questions; they are not likely to be solved by statistically unsubstantiable means. We have so far been unsuccessful at prediction because laboratory and theoretical studies of the physics of deformation and fracture have been largely unsupported. The problem is not simple; however, that does not mean it is insoluble. As I have indicated, there are weak solutions at present for large space-time windows. The short-term problem is much more difficult.

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