Pure appl. geophys. 149 (1997) 1–2
0033–4553/97/010001–02$1.50 + 0.20/0

Pure and Applied Geophysics

Introduction

Earthquake prediction research has proven to be more difficult than many expected. On occasion a researcher, or a group, makes a statement that prediction may become possible in ten years. Such a period seems like a long time, but then it passes quickly, and little progress has been made because the problem is difficult and because the funding is inadequate. Some have suggested that this discrepancy of optimistic expectations and lack of progress means that earthquakes can never be predicted. We think that is an overly pessimistic view. Most likely there are some earthquakes that are not predictable. For example, it is generally understood that large earthquakes often are multiple events, in which case it is difficult to construct a scenario that predicts the magnitude reliably. On the other hand, it is not disputed that some main shocks have foreshocks and extended foreshock sequences. In these cases it is clear that a preparation process leading up to the main rupture takes place. Thus, there should be no question that some earthquakes are predictable.

We still do not know enough about fundamentals of the rupture process and tectonic processes to develop a clear idea of what measurements could furnish the data for predictions. The recent debate pertaining to the adequacy of the seismic gap hypothesis for forecasting earthquakes (KAGAN and JACKSON, 1991; NISHENKO and SYKES, 1993) demonstrates that some of our cherished simple ideas about the earthquake generation process have still to be examined. Given this lack of understanding of basic processes leading to earthquakes, it is not surprising that we cannot predict them routinely yet. The issues of which claims of correct predictions and which claims of successful methods can be accepted continue to be thorny. Many authors overestimate the power of the method they advocate in the opinion of reviewers and researchers who advocate rigorous methods to measure the significance of claimed results. We think that the pendulum is swinging slowly to the side of quantitative analysis, since we have seen many articles on statistical evaluations of claims recently. However, there are also a few authors who vastly overvalue their accomplishments and exploit what they believe to be their success in predicting earthquakes with news media and the public. The field of earthquake prediction research consequently is tarnished and funding has diminished.

In this issue we report on additional evaluations of prediction methods proposed for the IASPEI (International Association of Seismology and Physics of the Earth's Interior) list of significant precursors. The acceptance of new entries on this list indicates that progress in prediction research is being made. In addition we collected articles regarding the strategy of prediction and particular proposals for precursors. The differences of opinion expressed by some of the authors and reviewers continue to be strong. It seems we have not made much headway in reaching a consensus in the seismological community concerning the questions of how to proceed with earthquake prediction research and what constitute acceptable results. We expect that considerably more exchanges of ideas and special issues on earthquake prediction will be necessary before the wide gap that now exists between optimists and pessimists can be eroded.

REFERENCES

KAGAN, Y. Y., and JACKSON, D. D. (1991), *The Seismic Gap Hypothesis: Ten Years After*, J. Geophys. Res. *96*, 21419–21431.

NISHENKO, S. P., and SYKES, L. R. (1993), Comments on "The Seismic Gap Hypothesis: Ten Years After", by Y. Y. Kagan and D. D. Jackson, J. Geophys. Res. *98*, 9909–9916.

Max Wyss
Geophysical Institute
University of Alaska
Fairbanks, Alaska 99775-0800, U.S.A.

Renata Dmowska
Division of Engineering and Applied Sciences
Harvard University
Pierce Hall, 29 Oxford Street
Cambridge, MA 02138, U.S.A.