Why is earthquake prediction research not progressing faster?

Article *in* Tectonophysics · August 2001

DOI: 10.1016/S0040-1951(01)00077-4

Max Wyss
International Center for Earth Modeling

ELSEVIER

Tectonophysics 338 (2001) 217–223

TECTONOPHYSICS

www.elsevier.com/locate/tecto

Why is earthquake prediction research not progressing faster?

Max Wyss*

Geophysical Institute, University of Alaska, Fairbanks 99775, USA

Received 21 January 2000; accepted 30 August 2000

Abstract

As a physical phenomenon, earthquakes must be predictable to a certain degree. However, the problem is difficult, because the source volume inside the earth is inaccessible to direct observation and because the most important parameter, the stress level, cannot be measured directly. Also, seismology is such a young science that the cause of earthquakes was discovered in the 1960s only. Advanced seismograph networks as well as modern techniques to measure crustal deformations, such as the Global Positioning System (GPS) and the Synthetic Aperture Radar Interferometry technique (InSAR), have come on line only recently, and only in Japan are they deployed with the densities necessary for significant advances in the understanding of the rupture initiation process. In addition, no real program for earthquake prediction research exists in the United States, largely because funding agencies and peer reviewers shy away from a field in which unprofessional, but motivated individuals are active. Although claims of successful predictions are often not justified, a few correct predictions have been made. Most of these had time-windows of years, but some were accurate to days and allowed preparatory actions. To make significant progress, we must learn how to conduct rigorous science in a field where amateurs cannot be discouraged to venture. Leadership is necessary to raise the funding to an adequate level and to involve the best minds in this promising, potentially extremely rewarding, but controversial research topic.

Keywords: earthquake prediction; earthquake precursors; seismology

1. Why the current dicussion about the predictability of earthquakes?

Earthquakes, the rupture of faults, obey the physics of failure processes. For failure to come about, elastic energy must be accumulated to a level at which friction is overcome. The current generation of seismologists figured out where the energy is coming from: movements of elastic plates at the surface of the Earth participate in cooling the planet by convection.

In physics, the degree to which we understand a phenomenon is often measured by how well we can predict it. Therefore, the question is not whether or not we can predict earthquakes; rather it is how well we can predict them. Geller et al. (1997a) introduced a definition of prediction that was so narrow (extremely short time window) that allowed them to argue that earthquakes cannot be "predicted" because of an element of randomness that can modify the exact time of occurrence. In my opinion, their statement that "earthquakes are not predictable" was misleading, because they are predictable if one uses the generally accepted definition of prediction.

The definition of prediction generally used is the following: Specify the location, size and time of occurrence, all with error windows, and the probability that this event will occur (Allen, 1976; Table 1).

* Fax: +1-907-474-5529.
E-mail address: max@giseis.alaska.edu (M. Wyss).

0040-1951/01/$ - see front matter
PII: S0040-1951(01)00077-4

Table 1
Elements necessary for a valid earthquake prediction

1	Location ± uncertainty
2	Size ± uncertainty
3	Occurrence time ± uncertainty
4	Probability

In addition, an estimate of the probability that the specified event may happen by chance is required to determine that the prediction is not trivial.

I see two reasons why we have this discussion (Wyss, 1997b; Geller et al., 1997b; Nature, web-page, March/April 1999) about the predictability of earthquakes. One is that the ratio of the difficulty of the problem to our knowledge of its physics is unfavorable at this early stage of the research on earthquake predictability; the other is psychological. Investigators trained in rigorous methods of science are dismayed by the slipshod approach of some, who dream of helping mankind quickly by predicting earthquakes.

In my opinion, a discussion about earthquake-prediction research is necessary, but I would prefer it to focus on “how predictable are earthquakes?” and on “what are the minimum requirements of rigor in earthquake prediction research?”

2. Notes on the history of seismology

It was Geller’s (1997) incorrect claim that “earthquake prediction research has been conducted for over 100 years with no obvious successes” that brought me to realize how extraordinarily young seismology is. When the Beatles rose to world fame with their music, we did not even know why quakes happen on our planet. The theory of plate tectonics was in the process of being born. Although some of the key papers were published in the early 1960s (e.g. Hess, 1962; Vine and Matthews, 1963; Wilson, 1965) the idea of the sea floor spreading was viewed with great skepticism by most seismologists, even up to 1967, when the paper by Sykes (1967) showed how seismology could make important contributions to test these strange new ideas. It is obvious that earthquake-prediction research had no chance of advancing significantly before this time, that is, before we understood where the energy for rupture is coming from and what forces produce earthquakes.

During the decade following the early 1960s, seismologists were busy establishing first basic, then more detailed facts concerning plate tectonics, and scarcely concerned themselves with the problem of how to predict earthquakes. It was also during this period that the first relatively modern seismograph networks came on line. For example, the world-wide-standardized-seismograph-network began to furnish more important information, without which the understanding of the phenomenon of plate tectonics and the reason for earthquakes could not have been developed.

However, in these early seismograph networks, the seismologist’s most important tools, were inadequate. Technological advances brought more sophisticated instruments on the market and expanded networks produced far superior earthquake catalogs, starting in the 1980s. Even in the best-instrumented areas, such as California and Japan, the earthquake catalogs before 1980 cannot be used for most modern studies of seismicity, because they are inadequate. This means that for the study of seismicity patterns of small earthquakes we have not quite two decades of data, yet return periods of large earthquakes typically exceed 100 years.

The information contained in our earthquake catalogs and records falls far short of what is needed and of what could be obtained by a serious effort to understand the physics of earthquakes. The problem of earthquake physics is difficult, because the source, deep in the earth, is not directly accessible for experiments, and because the state of stress cannot be directly observed. The level of funding for earthquake investigations in the US, and other countries, has been far below what is necessary to make significant advances in this difficult problem. Only in recent years has Japan made an adequate effort to deploy the numbers of advanced instruments necessary to monitor earthquakes. The United States has lagged behind.

In addition, great advances have been made during the last couple of decades in techniques to measure crustal deformations. In the 1980s the GPS method was discovered, and it is now refined to the point where positions of many locations on the surface can be determined to millimeter accuracy (Zumberge

Table 2
Elements of the prediction of volcanic eruptions

1	Location	Assumed as known
2	Size	Two classes only
3	Occurrence time	set = days to weeks
4	Probability	Four values only
5	Type	Explosive, effusive summit, flank

et al. (1997) e.g. Zumberge, 1997). During the last few years, the InSAR technique added another spectacularly successful tool to measure and monitor crustal deformations at the sub-centimeter level. InSAR analyses are beginning to document uplift in volcanoes, which have not been suspected of preparing for a new eruption (Lu et al., 1997, 2000). These new methods to measure crustal deformations are bound to have a great impact in the field of earthquake studies also. However, at present the United States investigators have to buy the radar images from the European Space Agency, because the US do not have a satellite in orbit, capable of gathering the data needed for InSAR analysis.

Thus, I conclude that seismology is an extraordinarily young science. Just now, we are at the brink of finally deploying the numbers of high-quality instruments we need, and it will take decades for these instruments to gather the data necessary for advancing to the understanding we seek.

3. Why are the prediction of volcanic eruptions and earthquake-hazard assessments not controversial?

Everybody agrees that volcanic eruptions can be predicted, that many have been predicted correctly, that many have not been predicted, and that false alarms are common. The volcanology community uses the term ‘forecast’ in place of ‘prediction.’ One factor that makes this job easier than earthquake prediction is that almost all eruptions issue from well-known volcanoes; creations of new volcanoes, like Paricutin, are rare. Thus, the location is known in the case of volcanoes. In the case of large earthquakes, we also expect most of them to re-rupture known faults. However, recent examples in California, India and Japan showed that ruptures on unknown faults can be very substantial and not infrequent (M6.5 San Fernando, California 1971; M7.3 Landers, California 1992; M6.7 Northridge, California 1994; M6.1 Latur, 1993 India; M7.0 Hyuganada, Japan 1995).

Table 3
Elements necessary for seismic hazard assessment

1	Location	Known
2	Size of earthquake	Estimated from history
3	Occurrence time	Not required
4	Probability	Calculated
5	Amplitude of ground motion ± uncertainty	Calculated

Another factor that makes it easier to claim success is that the size of the expected eruption is specified in two classes only. The standard practice at volcano observatories is to gauge a particular volcano’s probability to erupt in the near future (days to weeks) by monitoring geophysical data and using a color code that typically has four stages (red = significant eruption in progress or explosion expected (timeframe: hours), orange = in eruption or eruption imminent (timeframe: days), yellow = restless (eruption possible in weeks), green = dormant) (e.g. Hill et al., 1991; Wolfe, 1992).

Comparing the requirements for a satisfactory earthquake prediction (Table 1) with that of a volcanic eruption (Table 2), one sees that the latter is much easier. In some aspects, the prediction of volcanic eruptions is more similar to ‘seismic hazard assessment’ (Table 3) than to ‘earthquake prediction,’ although earthquake hazard assessments focus on estimating the amplitude of ground motion expected, during an assumed exposure time.

In some ways, the form of ‘prediction’ in earthquake hazard estimates (Table 3) is the most advanced, namely the probability of a given size earthquake has to be estimated for many faults (source zones) surrounding the location of interest, and then the combined probability of a certain ground motion amplitude is calculated using a model for attenuation of seismic waves, as they propagate to the site in question. However, in one aspect the seismic hazard assessment is more primitive than the other prediction methods: it assumes that earthquakes occur at random as a function of time. The element of predicting the time is, of course, at the heart of earthquake-prediction attempts. The hope is that initial earthquake predictions with uncertainties of years to decades can be refined to uncertainties of months, and in exceptional cases to days (e.g. Wu et al., 1991; Yamaoka et al., 1999; Zhang et al., 1999).

There exists a remarkable contrast in the level of acceptance of earthquake-hazard assessments and earthquake prediction. The method of earthquake-hazard assessment is very well accepted, even entrenched, to the point that hazard maps of the entire globe are being prepared in large scale international projects, in spite of the fact that the basic assumption (that the occurrence rate of large earthquakes can be estimated by using the a-value and extrapolating the b-value of the frequency-magnitude relation $N =$ a-bM) is clearly not valid for many regions (for example the rupture segments of the great historic earthquakes along the San Andreas fault). In addition, almost nobody believes that large earthquakes occur at random intervals, but, in the absence of a known recurrence time, the assumption of a stationary Poisson process is used.

Testing requirements of the hypotheses used in seismic hazard assessment and earthquake prediction methods have completely different standards. In earthquake prediction, the seismological community does not accept methods, unless they have been relatively rigorously tested. In the business of hazard assessment, no formal tests have been conducted, to my knowledge, to see how well probabilistic maps of peak accelerations specify the observed values for the period since the maps had been constructed. The only test of the probabilistic mapping of peak acceleration is that of precarious rocks (Brune, 1999), which shows that in the western US the expected accelerations are substantially overestimated.

Thus, it seems to me that in the business of earthquake prediction the burden of proof that a method works is on the proponent, and not the critique of the method, whereas in the hazard-assessment work the burden of proof that there are flaws in it, or that the method does not work, is on the critic, not the proponent of the method.

If there are such basic uncertainties, even flaws, in the seismic-hazard assessment method, why is it so widely accepted, whereas earthquake prediction research is controversial? I believe the answer is: seismic-hazard assessments are done by a rigorous scientific method, although sometimes under invalid assumptions (a- plus b-value) and a clearly incorrect assumption (random occurrence of large earthquakes), whereas earthquake-prediction work is often not rigorous, or may be even demonstrably wrong.

4. Transients in the Earth

It is widely expected that major ruptures of the earth's crust are associated with transients in their vicinity. In this respect 'transient' means a local, temporary change in some physical parameter. However, the same type of transients may also occur at times when no large earthquake develops. That is, we expect precursory anomalies and also false alarms in changing crustal parameters.

In predicting volcanic eruptions, false alarms are common and accepted as an unavoidable, but not fatal problem. They are acceptable to investigators, because they are understood. The surfaces of volcanoes buckle up and earthquake swarms are generated during intrusions of magma, even if the latter does not reach the surface for an eruption. In the case of proposed earthquake precursors, some critics see false alarms as evidence that disproves the proposed phenomenon. However, it is reasonable to suppose that transients may occur without leading to a major rupture, but causing transient anomalies. Also, I do not think that the absence of precursors to some main shocks disproves their existence in other cases, just as the absence of pre-eruption swarms in some cases does not negate them as a useful tool for prediction of other eruptions. The difference is that precursors to earthquakes are poorly understood, but intrusions beneath volcanoes are an obvious phenomenon.

The example of foreshocks has relevance to the problem of false alarms, as well as to that of missed main shocks (no measured precursory transient). Foreshocks are measured in 10%–30% of large main shocks (e.g. Jones and Molnar, 1976; Jones, 1966) and all agree that they are part of the failure process that culminates in the main rupture. I conclude three points from this observation. (1) Some large earthquakes are preceded by a precursory process (foreshocks are one expression of this) and we could probably measure additional changes in a number of crustal parameters in addition to foreshocks. (2) The fact that not all main shocks are preceded by foreshocks (and not all eruptions are preceded by earthquake swarms) does not negate the fact that a precursory process is operative for some earthquakes and eruptions. (3) The fact that false alarms occur (earthquake clusters and swarms that do not culminate in a major rupture or an eruption) does not negate the fact that precursory processes occur and can be measured; it only makes prediction of earthquakes (as well as eruptions) less certain.

Many precursory phenomena have been proposed and a few are being tested quantitatively (e.g. Evison and Rhoades, 1993; Kossobokov et al., 1997). However, these hypotheses are viewed with much skepticism by the scientific community, in part because physical processes that produce them are either not understood or their explanations are often complicated ad hoc schemes. Thus, it seems that our job is to measure carefully and quantitatively the properties of transients in the Earth's crust, to construct models to understand them, and to rigorously test hypotheses of how transients could be related to the initiation of major ruptures. This is not a small undertaking, especially because we must expect substantial differences in the behavior of the crust in different tectonic settings (thrust environments and normal faulting regimes having opposite orientations and different levels of the stress tensor are not likely to produce the same phenomena). Therefore, and because of our rudimentary understanding of earthquake physics, substantial funds and manpower are needed to make progress in the understanding of the role of transients in the initiation process of earthquake ruptures.

5. The element of randomness

An element of randomness that influences the exact time of a major earthquake or an eruption undoubtedly exists. In the case of earthquakes, many little ruptures may occur in a volume that contains enough strain energy for a large rupture, without triggering it, yet every one of these has the potential to grow into a large rupture that is bound to occur eventually (e.g. Brune, 1979). In my view, this dulls the sharpness of a prediction attempt, but it does not make it impossible. If a crustal volume that is in a critical state and may rupture at any time within approximately a one-year window can be identified, I would call it a useful prediction.

An example of this is the difference in occurrence time of two major earthquakes, both triggered by the Landers, M7.3 earthquake of 1992 in California. The Big Bear, M6.5, earthquake occurred about three hours after Landers, but the Hector Mine, M7.1, earthquake waited for 7 years, before it ruptured, yet both were located in volumes where the Landers main shock had advanced the probability of failure by the Coulomb fracture criterion (e.g. Stein et al., 1992). The difference was that the volume of the Big Bear earthquake had been in a critical stage already two years before the Landers rupture, whereas the Hector Mine volume was not. In a study after the fact, Wiemer and Wyss (1994) showed that not only the Landers main shock was preceded by seismic quiescence for four years in parts of its source volume, but the entire source volume of the Big Bear earthquake had turned anomalously quiet two years before these two events occurred on the same day. Our interpretation is that the quiescence in the Big Bear area indicated that this volume was in a critical stage, and that is why Landers was able to trigger this major event on the same day. We suggest that, if the Landers earthquake had not occurred and triggered the Big Bear event that same day, a small earthquake within the Big Bear volume could have grown into the Big Bear earthquake any day within the period 1992/1993, approximately.

In recent years, much discussion has focused on the application of self-organized criticality to the problem of earthquake generation. Some investigators have used this concept to argue that earthquakes are not predictable. However, recent studies have shown that on the contrary, some properties of self-organized critical models can be used to predict major earthquakes (e.g. Huang et al., 1998; Sornette, 1999; Hainzl et al., 2000). I expect that in the future, as these models become more realistic, they may help in designing prediction scenarios that may become effective.

In any case, we have to expect to issue predictions in a probabilistic framework, with time windows of increased probability of years, as it is done in the current testing of the M8 algorithm (e.g. Kossobokov et al., 1997).

6. What needs to be done to get earthquake-prediction research into a productive mode?

It seems to me, that earthquake-prediction research is currently not progressing significantly in the US. However, the research field is promising, although difficult. In addition, the benefits of even a partial solution of the problem of prediction is so enormous that we simply must pursue it, and we must do it vigorously, otherwise the effort is wasted.

To break out of the current state of derisive indifference toward earthquake-prediction research by peers who do not practice it, we need earthquake-prediction researchers to get their own house in order. Usually, the review of submitted manuscripts filters out non-rigorous and wrong work. Weak articles that do get published, generally receive the silent treatment and are forgotten quickly. However, the dream of discovering how to predict earthquakes attracts individuals who put enormous energy into promoting unfounded ideas with the public and policy makers. Unfortunately, it takes a great deal of effort to show the flaws in highly advertised claims of success in earthquake prediction, and not all are able to understand the reasons for which the work is invalid. Nevertheless, the scientific community has to do the job of identifying the flaws in exaggerated claims, as we have an obligation to participate in the review process (e.g. Geller, 1996).

The IASPEI (International Association of Seismology and the Physics of the Earth's Interior) has made an effort to increase the standard of quality in earthquake-prediction research by a campaign of reviewing voluntarily submitted proposals for earthquake precursors and prediction methods (Wyss, 1991, 1997a; Wyss and Booth, 1997). This search for valid earthquake precursors has led to a list containing five cases that were accepted, a list of four undecided cases and 30 proposals which were not accepted. Most of the criticisms by anonymous reviewers and by the review panels addressed the lack of detail and rigor.

In addition to these efforts of cleaning house by dedicated individual researchers, new initiatives must be launched, if we want to advance this field of research. That is, leadership from funding agencies and scientific organizations must rejuvenate the specialty.

Disappointments of the past must be put behind us. It is true that the euphoria concerning the solution of the earthquake-prediction problem that followed the imaginative paper by Scholz et al. (1973) and that was fueled by the self-confidence instilled by the successful development of the theory of plate tectonics, led to exaggerated hopes as well as exaggerated claims. That is, however, no rational reason to lapse into the current equally unrealistic pessimism about the predictability of earthquakes. As rational scientists, we must put these emotional factors aside, face the problem with realistic expectations and work toward its solution.

Without leadership, this will not happen, chiefly because the funding is inadequate and the best scientists are not working on the problem. In my observation, even many good and outstanding scientists fear damaging their reputations if they should get involved in a project like earthquake-prediction research, which appears tainted because incompetent individuals dabble in it. To overcome this obstacle, leadership and funds are required. We must simply learn how to conduct rigorous research in a field where untrained people interfere with their unreasonable opinions and we must set and insist on high standards for useful research.

7. Conclusions

Earthquake prediction is difficult, but not impossible. Most short-term attempts will not be successful because of randomness in rupture initiation, except for some fortunate cases (Wu et al., 1991; Yamaoka et al., 1999; Zhang et al., 1999). We must exercise patience and not expect spectacular success quickly. First, we have to learn what the facts are. For this we must quantitatively study case histories of transients. Then we must learn how to rigorously test the hypothesis that some of them may be connected to the initiation process of earthquake ruptures, that is that they may be precursors. However, none of this will happen, unless the field of prediction research is reformed and well funded. Earthquake prediction will remain controversial, but become more common.

Acknowledgements

I benefited greatly from discussions with S. McNutt and I thank J. Freymueller as well as H. N. Srivastava for helpful comments. This study was supported by the Wadati foundation at the Geophysical Institute of the University of Alaska, Fairbanks.

References

Allen, C.R., 1976. Responsibilities in earthquake prediction. Bull. Seismol. Soc. Am. 66, 2069–2074.

Brune, J.N., 1979. Implications of earthquake triggering and rupture propagation for earthquake prediction based on premonitory phenomena. J. Geophys. Res. 84, 2195–2198.

Brune, J.N., 1999. Precarious rocks along the Mojave section of the San Andreas Fault, California: constraints on ground motion from great earthquakes . Seismol. Res. Lett. 70, 29–33.

Evison, F.F., Rhoades, D.A., 1993. The precursory swarm in New Zealand: hypothesis test. New Zealand J. Geol. Geophys. 36, 51–60.

Geller, R.J., 1996. Debate on evaluation of the VAN method: Editor's introduction. Geophys. Res. Lett. 23, 1291–1294.

Geller, R.J., 1997. Earthquake prediction: A critical review. Geophys. J. Int. 131, 425–450.

Geller, R.J., Jackson, D.D., Kagan, Y.Y., Mulargia, F., 1997a. Earthquakes cannot be predicted. Science 275, 1616–1617.

Geller, R.J., Jackson, D.D, Kagan, Y.Y., Mulargia, F., 1997b. Cannot earthquakes be predicted? Science 278, 488–490.

Hainzl, S., Zoller, G., Kurths, J., Zschau, J., 2000. Seismic quiescence as an indicator for large earthquakes in a system of self-organized criticallity. Geophys. Res. Lett. 27, 597–600.

Hess, H.H., 1962. History of ocean basins, in Petrologic Studies — A Volume in Honor of A. F. Buddington. Geological Society of America, New York, pp. 599–620.

Hill, D.P., et al., 1991. Response plans for volcanic hazards in the Long Valley caldera and Mono Craters area, California. Open-File Report. U. S. Geol. Survey, Menlo Park, California, 64 pp.

Huang, Y., Saleur, H., Sammis, C., Sornette, D., 1998. Precursors, aftershocks, criticality and self-organized criticality. Europhys. Lett. 41, 43–48.

Jones, L., Molnar, P., 1976. Frequency of foreshocks. Nature 262, 677–679.

Jones, L.M., 1984. Foreshocks (1966–1980) in the San Andreas system, California. Bull. Seismol. Soc. Am. 74, 1361–1380.

Kossobokov, V.G., Healy, J.H., Dewey, J.W., 1997. Testing an earthquake prediction algorithm. Pure Appl. Geophys. 149, 219–248.

Lu, Z., et al., 1997. Deformation of New Trident volcano measured by ERS-1 SAR interferometry, Katmai National Park, Alaska. Geophys. Res. Lett. 24, 695–698.

Lu, Z., et al., 2000. Aseismic inflation of Westdahl volcano, Alaska, revealed by satellite radar interferometry. Geophys. Res. Lett. 27, 1567–1570.

Scholz, C.H., Sykes, L.R., Agarwal, Y.P., 1973. Earthquake prediction: a physical basis. Science 181, 803–810.

Sornette, D., 1999. Towards a truly interdisciplinary approach to earthquake prediction. Nature, webpage: debate.

Stein, R.S., King, G.C.P., Lin, J., 1992. Change in failure stress on the San Andreas and surrounding faults caused by the 1992 $M = 7.4$ Landers earthquake. Science 258, 1328–1332.

Sykes, L.R., 1967. Mechanism of earthquakes and nature of faulting on the mid-oceanic ridges. J. Geophys. Res. 72, 5–27.

Vine, F.J., Matthews, D.H., 1963. Magnetic anomalies over oceanic ridges. Nature 199, 947–949.

Wiemer, S., Wyss, M., 1994. Seismic quiescence before the Landers ($M = 7.5$) and Big Bear ($M = 6.5$) 1992 earthquakes. Bull. Seismol. Soc. Am. 84, 900–916.

Wilson, J.T., 1965. A new class of faults and their bearing on continental drift. Nature 207, 343–347.

Wolfe, E.W., 1992. The 1991 eruptions of Mount Pinatubo, Philippines . Earthquakes Volcanoes 23, 5–37.

Wu, K.-T., Yue, M.-S., Wu, H.-Y., Chao, S.-L., Chen, H.-T., Huang, W.-O., Tien, K.-Y., Lu, S.-D., 1991. Certain characteristics of the Haicheng earthquake ($M = 7.3$) sequence. In: Wyss, M. (Ed.), Evaluations of Proposed Earthquake Precursors. AGU Monograph, Washington D.C., pp. 12–14.

Wyss, M., 1991. Evaluation of Proposed Earthquake Precursors. AGU Monograph, Washington D.C., 94pp.

Wyss, M., 1997a. Second round of evaluations of proposed earthquake precursors. Pure Appl. Geophys. 149, 3–16.

Wyss, M., 1997b. Cannot earthquakes be predicted? Science 278, 487–488.

Wyss, M., Booth, D.C., 1997. The IASPEI procedure for the evaluation of earthquake precursors. Geophys. J. Int. 131, 423–424.

Yamaoka, K., Ooida, T., Ueda, Y., 1999. Detailed distribution of accelerating foreshocks before a M 5.1 earthquake in Japan. Pure Appl. Geophys. 155, 335–354.

Zhang, G., et al., 1999. Predictions of the 1997 strong earthquakes in Jiashi, Xinjiang, China. Bull. Seismol. Soc. Am. 89, 1171–1183.

Zumberge, J.F., Heflin, M.B., Jefferson, D.C., Watkins, M.M., Webb, F.H., 1997. Precise point positioning for the efficient and robust analysis of GPS data from large networks. J. Geophys. Res. 102, 5005–5018.