

**Review of “A Theory of Earthquake Prediction” by Jeen-Hwa Wang.
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In this paper, Dr. Wang makes the useful observation that interpretation of possible earthquake precursory signals requires a model framework. In other words, if a potential precursory signal is observed, a modeling framework is needed in order to make use of that observation to estimate the time, magnitude, and/or location of a future earthquake. He collects a large number of findings from previous theoretical studies—most of them from the 1970’s to 1990’s--and places them into a common framework. For example, he shows that the estimated time T to a future earthquake can be expressed as a function of that future earthquake’s magnitude M , in the form $\log(T) = C + AM$, where C and A are parameters that may vary from place to place. He explores this framework for potential precursory signals in strain, electro-magnetism, and geochemistry.

I fully agree that purported precursors need to be evaluated in light of a model that has some basis in rock and earthquake physics. However, my admiration for the paper stops there, as I find it to have several shortcomings. I summarize my major points below. Additional comments can be found in a marked copy of the manuscript, in which yellow-highlighted passages are accompanied by marginal comments. (The blue-highlighted passages can be ignored—those markings were for my own assistance in understanding the paper.)

My summary evaluation of the paper is that it could be a valuable contribution, but requires **substantial revision** before it should be considered for publication in NPG.

Major areas of comment:

Comment on theoretical framework:

Much of the paper reads as if it were written in the 1980’s. At that time, there was widespread belief among seismologists and geophysicists that earthquakes acted like large versions of simple cracks. Based on that view, there was an expectation that the overall energetics of the earthquake could be related to energetics of the crack (fault) tip. This view led to several hypotheses (some of which Wang summarizes or mentions tangentially): that there should be accelerating levels of stress and strain in the volume of rock surrounding the eventual fault rupture; that the dimensions of that volume should scale with the future earthquake M ; that those accelerating strains should trigger changes in geochemistry (e.g., Radon emission) and movements of fluid that cause changes in the local EM field; and that there is accelerating slip on the eventual earthquake fault plane.

Wang offers numerous references to studies (most from before 2000) that employed that view in presenting purported anomalies that were interpreted as diagnostic precursors.

What the paper fails to say is that those early hypotheses led to numerous, elaborate, rigorous, and often very expensive and protracted observational programs and scientific analyses, nearly all of which either (a) failed to observe any of the hypothesized signals, or (b) demonstrated that they are either absent or too small to be observed with modern instrumental networks. For example, the United States invested tens of millions of dollars in the Parkfield Prediction Experiment, with near-field recordings of strain, fault slip, EM field, etc. Dozens of papers (none of which are cited here) analyzed those results, including in the years-to-seconds preceding a magnitude 6 earthquake. There were no precursory signals detected on any of those networks. A major conclusion of that experiment is that, if there is a precursory process or pre-seismic accelerated fault slip, it was too small and too localized to be detected. For example, it was ruled out that precursory strain near the future rupture reached the nano-strain level. Another example is the modern geodetic network in Japan, installed at extreme cost in part to hunt for earthquake precursors, which has failed to find any precursory deformation before numerous high-M earthquakes. Similarly, geochemical observation networks (e.g., in Italy) have failed to find any systematic signals before moderate-M earthquakes.

Although precursor studies continue (e.g., those analyzing ionospheric data), the majority view of seismologists is that either (a) there is no precursory process at all, or (b) that precursory processes are related to the hypocenter, where there may be localized accelerated strain or accelerating fault slip that leads to inertial failure. On the basis of careful studies of earthquake nucleation both in nature and in the lab, most seismologists believe that the nucleation and propagation processes are somewhat separate: earthquakes nucleate due to physical conditions local to the hypocenter; they propagate depending on the conditions of the broader fault (its continuity, roughness, stress state, frictional conditions, etc.). To state this view in too-simple terms, all earthquakes start similarly, and some propagate to become large.

(Strangely, the section on Radon anomalies focuses on a hypocentral process (see lines 630-632) instead of a process that scales with fault length.)

It is too early to know which of these world views will turn out to be correct. But what is certainly true is that readers of a paper should be presented with a fair and balanced summary of the state of the science. The current draft presents (and even over-states) the hypothesis that precursors can be observed, cherry-picks the literature to provide examples that support that view, and fails to provide any balance.

This is not a fatal flaw for the paper. It would be perfectly fair for the author to state upfront that he is *assuming* that there is a precursory process involving the entire fault, while admitting that this is a controversial hypothesis, that the observational data are mixed, and that many studies have failed to support that case. Despite these issues, it is useful for him to provide a consolidated, theoretical framework in which to analyze potential precursors.

If the author revises the paper in this way, he should avoid bold statements like those on lines 31-34 and 86-90. I predict that many scientists will stop reading after that first sentence, because it suggests that the author is presenting a minority view of earthquake prediction. This would be unfortunate, because the paper can be revised to provide a useful framework for future work.

Testing these ideas:

It's been three to four decades since the foundational work of Aki, Main, etc., upon which this paper is based. Surely there have been studies since that time that have tested those important theoretic constructs and hypotheses. Providing some information about the evolution of faulting theory since 1998 would be helpful to the reader, and would help make this paper a more useful reference for earthquake scientists.

Complexity:

In multiple places the author admits that fault complexity may make it challenging to observe precursory signals. In the past 30 years, it has become increasingly clear that earthquake faults are extremely complex, that the failure propagation process is extremely complex, and that earthquakes occur within a complex shallow crust. It would be helpful for the author to explore this point further, rather than just stating it and moving on with what appears to be blind faith. Given the theoretical framework presented and parameter values estimated, are there any precursory processes that should be observable, given what we now know about complexity? If so, which ones are likely to be observable? Are there particular settings in which they are more likely to be observed? Does the author have advice on how best to observe them?

Amplitude of precursory signals:

The author makes statements about which precursory signals should and should not be visible, but does not provide the reasoning or calculations behind those statements. For example, line 551-552 says that geoelectric signals should be visible after time t_c "because the signals are strong enough"--how is that determined, or is that the author's intuition?

Helping the reader:

The paper presents many equations and parameters, but provides very little help in understanding them. For example, there's almost no mention of units, and little help in understanding the physical meaning of parameters that the author claims depend on geologic setting. What are the units of important parameters A , C , a and α ? What are the expected ranges of A and C ?

It would also be useful to provide example calculations to explore the theory. For example, based on the development in section 3, what are the amplitudes and timing of precursory strains that are expected before earthquakes of various M ? How do those compare with the various observations in the cited papers?

It would be useful to add a section to the Discussion that explores how scientists could use the theoretical framework to study potential precursors, or to create testable prediction hypotheses. See my comment on page 22 of the marked manuscript. Given the theoretical constraints on precursor amplitude and timing, what network configuration is required to observe such precursors? Or, conversely, given a network configuration, should it be possible to observe precursors, given the amplitudes and timing that can be calculated using this theoretical framework?

See many such comments in my marked manuscript, suggesting additional help that can be provided to readers.

Figures:

Figures 1, 2 and 4 are not helpful. Please change the X scale, or present on log scales, in order to show the shape of the curves and the differences between multiple curves.

Language:

The writing style is generally quite good, clear and easy to follow. There are numerous small errors of syntax and grammar, and some missing words, so I suggest that a revised draft be proofread by a native English speaker so that those small issues are not distracting to the reader.