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Interactive Comment

Interactive comment on "Pattern dynamics, pattern hierarchies, and forecasting in complex multi-scale earth systems" *by* J. B. Rundle et al.

Anonymous Referee #2

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The paper aims at presenting an integrated view of earth systems through many-degree-of-freedom, multi-scale dynamic system theory. This is an interesting and ambitious objective, on which the paper delivers only in part. One problem in my opinion is the diversity of natural phenomena mentioned in the Introduction. Before one can claim to have at least the framework of a general theory, one should examine in detail each phenomenon and show that indeed various physical systems fit under a single umbrella. For example, I do not see how hurricanes fit the paradigm of threshold phenomena (except for the obvious fact that hurricane flooding occurs when protective systems are overcome).

The treatment of El Nino events is mathematically unclear and lacks depth. While this is understandable in a paper with focus on earthquakes, I wonder why this section is there at all.

The bulk of the paper deals with spatial and temporal patterns of seismicity in California. These patterns are studied using an artificial 10,000-year earthquake catalog generated by a mechanistic model of the region. The principal components of the empirical earthquake rates are analyzed, with interesting insights into the dynamics of regional seismicity. However, it is unclear how either the mechanistic model or the fitted space-time variability in earthquake activity can be used for earthquake prediction (one of the main objectives of the paper), starting from say a 200-year catalog.

The mathematics is not sufficiently explained or justified. For example, the earthquake rate function in Eq. 4 should be better justified. The rate S from that equation appears to be bounded and in the special case of a single **x** point (N = 1) becomes a sinusoidal function of time. Why should the number of eigenfunctions be the same as the number N of discrete spatial locations?

The authors assume that S(**x**,t) = 1 if an earthquake occurs at time t on the fault segment centered at **x**; should it not be that S(**x**,t)$\rightarrow\infty$. Does this mean that the empirical S(**x**,t) function is a (0, 1) function?

In Eq. 6, what is D in the assumed case of “Brownian noise”?

My general recommendation to the authors is that they edit the paper to be focused on a single physical system and treat that system in greater depth.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1045, 2006.

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