Earth Syst. Dynam. Discuss., 6, C1224–C1228, 2016
www.earth-syst-dynam-discuss.net/6/C1224/2016/

Earth System Dynamics Discussions

Interactive comment on "Early warning signals in complex ecosystems" *by* I. S. Weaver and J. G. Dyke

Anonymous Referee #1

Received and published: 4 March 2016

General comments

The manuscript "Early warning signals in complex ecosystems" by I. S. Weaver and J. G. Dyke addresses the important and interesting topic of bifurcations in natural systems, and the possibility to understand and predict system stability using statistical indicators.

The manuscript generally shows skilled and clear writing and a relatively thorough literature background. It rightfully points out that "early warning signals" have mostly been applied to very simple models, and that evidence on their use is lacking in more complex systems. To this extent, the paper identifies a very promising research direction.

The fundamental problem is that I do not see in what respect the manuscript constitutes

progess in this direction. Whereas it is often stated that early warning signals are not necessarily applicable in complex systems, the paper does not seem to take this statement seriously in practice. Instead, it seems to be assumed that the model the authors apply would be representative of a huge class of ecological systems. In fact, the purpose to understand the relation between biota and its environment is rather a description of the whole field of ecology, rather than a specific research question. I therefore wonder what the analysis of such an unspecific model approach can tell us about reality.

It is of course well possible that there is something in the model that can be learned about certain systems that I am overlooking. But in this case I suggest that the authors work out this aspect in much more detail, and fundamentally revise their manuscript. I basically see two possibilities: either focus on certain properties of real ecosystems, or focus on a certain mathematical aspect of the model that is relevant for the application of statistical indicators in complex systems in general.

As far as I can see, the main message that the current results offer is the notion that the signal strength of early warning signals is not related to the degree of irreversibility or the distance from the lost equilibrium to the new equilibrium. This is of course a true statement, because statistical indicators can only yield information about the local flow, but not on the global bifurcation structure of a system. However, this is not a new discovery that would merit a publication, and it does not require the application of such a complex model.

Given the interesting topic, I do hope that the authors can come up with a sharpened message. As the authors have spent much more time and thought on their model, there may be a merit in it that I do not see right now. Nonetheless, I am convinced that a potential revision of the paper should consist in a fundamental change and offer more specific and novel results. This would also help the authors to reach other scientists who look into specific processes or specific mathematical problems, who would otherwise ignore the paper.

Specific comments

Title Very vague and general, as the rest of the paper

Introduction p. 2509, line 10-15. Drawing general and far-reaching conclusions from such an idealised model is a very big promise that is not possible to keep, in my opinion. The main problem is, that one cannot have both, complexity and generality. If there is such a relation it is specific to a certain aspect of reality, and it normally emerges from many studies, not just one. This shows that the scope of the paper is too broad.

Sect. 2 It is indicated that "critical transitions“ naturally emerge in complex systems and that they would occur "in almost all real systems“. I do not believe this. For example, the climate is a very complex system, and there is indeed evidence of abrupt changes (as the authors remind us). But such transitions are the exception rather than the norm, and multiple states are not so common in comprehensive models. I think that the notion that such phenomena can occur would be more appropriate than saying that they are everywhere.

Sect. 3 It should be much better described what the purpose of the model is. The popular and adequate notion that "all models are wrong, but some are useful“ and that "there is no model OF something, but a model is made FOR something“ nicely summarise what I perceive as the problem of this paper. It could also be that the structure of the model already poses an interesting setup (e.g. due to the many dimensions, the niche approach and the possibility of non-autonomous forcing. But then there should be experiments that systematically explore what this structure specifically means for the applicability of early warnings. By the way, the model seems to be complex enough to show all kinds of complex behaviour such as chaos. Yet, only stable states seem to occur. I wonder why the solutions of the model are so simple despite its complexity.

Sect. 4 p. 2519, line 19-20. The possibility of hidden equilibria that can only be realised by changing another parameter or by perturbing the state directly, is a very interesting idea. This fact alone is not anything new yet, but might be another aspect to explore

in more detail in some way. At least I am not aware of publications that focus on this aspect.

p. 2521 The fact that the vector field F(E) is not conservative is another interesting aspect that could be chosen as a focus. If the authors should decide to explore this in more detail, they should connect to other literature on early warning signals in high-dimensional systems such as ecology and climate. Examples are (besides the cited Held and Kleinen, 2004), Hastings and Wysham (2010), Dakos et al. (2010, 2011), Bathiany et al. (2013), Mheen et al. (2013), and Williamson and Lenton (2015).

The sections where actual results are shown are very short in comparison with the rest of the paper. This should be better balanced.

Sect. 5 The discussion about skewness and why it behaves that way in the model is unclear to me, and I see no experiments or calculations that would reveal the reason.

Figures Although they are often beautiful, I think the figures are too many as they hardly show any new results.

References

Bathiany, S., Claussen, M., and Fraedrich, K.: Detecting hotspots of atmosphere–vegetation interaction via slowing down – Part 1: A stochastic approach, Earth Syst. Dynam., 4, 63–78, doi:10.5194/esd-4-63-2013, 2013.

Dakos, V., van Nes, E. H., Donangelo, R., Fort, H., and Scheffer, M.: Spatial correlation as leading indicator of catastrophic shifts, Theor. Ecol., 3, 163–174, doi:10.1007/s12080-009-0060-6, 2010.

Dakos, V., Kefi, S., Rietkerk, M., van Nes, E. H., and Scheffer, M.: Slowing down in spatially patterned ecosystems at the brink of collapse, Am. Nat., 177, E153–E166, doi:10.1086/659945, 2011.

Hastings, A. and Wysham, D. B.: Regime shifts in ecological systems can occur with no warning, Ecol. Lett., 13, 464–472, doi:10.1111/j.1461-0248.2010.01439.x, 2010.

van der Mheen, M., H. A. Dijkstra, A. Gozolchiani, M. den Toom, Q. Feng, J. Kurths, and E. Hernandez-Garcia: Interaction network based early warning indicators for the Atlantic MOC collapse, Geophys. Res. Lett., 40, 2714–2719, doi:10.1002/grl.50515, 2013.

Williamson, M. S., and Lenton, T. M.: Detection of bifurcations in noisy coupled systems from multiple time series, Chaos, 25, 036407, doi:10.1063/1.4908603, 2015.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2507, 2015.