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Earth System Dynamics Discussions

Interactive comment on "Early warning signals of tipping points in periodically forced systems" *by* M. S. Williamson et al.

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The paper under review discusses the case of early-warning signs for systems with periodic forcing. The paper proposes a method for certain forced systems with a periodic orbit based upon measuring phase lag, amplification and finding dominant nonlinear terms near the tipping point. The results are also applied in the context of a time series for arctic sea ice.

Overall, it is certainly useful to think about what happens to early-warning signs for forced and/or periodic systems. The same applies to trying to use the theory in the context of climate time series. However, as far as I can see from the paper, the authors also claim that their methods and mathematical ideas for early-warning signs are novel.

At least, the bulk of the paper is dedicated to this topic and they use "here we find..." and "we show that..." and similar formulations to indicate that their approach is new. In my opinion, the major problem I see with this work is that the authors did not seem to make enough of an effort to link and/or base their results on previously available mathematical techniques. I will give the authors the benefit of the doubt that they simply did not know, or could not find the adequate sources on which their analysis could have been based and/or compared to since it may not be in the climate-science-related journals (and it could very well be common to just argue things are novel if they have not appeared in a certain subsets of journals; in general, this is a view which I disagree with, particularly for such a highly interdisciplinary topic as nonlinear dynamics).

Here are the main steps that probably should be taken before one may claim a new method for early-warning signs for periodic systems:

1) For periodic systems, there is a well-developed theory of return maps which converts the continuous-time periodic orbit into questions about the local fixed point of a return map (see e.g. the books by Kuznetsov or Guckenheimer/Holmes or in fact many other dynamical systems texts). It is really strange that the authors do not even mention this approach to the problem. A very natural approach would be to just to try to re-use results about slowing down and early-warning signs for local bifurcations for periodic systems by looking at a return map. Of course, the change of the lag will not be visible directly in the return map, so it would be reasonable to try to do a comparison why in certain circumstances the lag might be a better or worse warning sign compared to quantities computed directly from the return map.

2) The authors are also apparently not aware that there is already quite a bit of very classical work on early-warning signs for periodic systems. For example, it should be mentioned that warning signs for bifurcations have already appeared for periodic orbits many years ago in the groundbreaking work by Wiesenfeld:

Wiesenfeld, K. (1985). Noisy precursors of nonlinear instabilities. Journal of Statistical Physics, 38(5-6), 1071-1097.

Furthermore, there is also a lot of recent activity on the field as exemplified by the recent work:

Zhu, J., Kuske, R., & Erneux, T. (2014). Tipping points near a delayed saddle node bifurcation with periodic forcing. arXiv preprint arXiv:1410.5101.

I am pretty sure that upon further search one would be able to come up with a rather long list of papers that have studied periodic orbits near instability and their statistical, Fourier-analysis, and phase properties. Then it is a natural question which of these results can be applied directly to the problem of early-warning signs. The authors simply skip this step in their analysis. There is one mention to stochastic resonance, and also in this part of the literature I would expect to find already a lot of readily applicable results. Of course, after this detailed review, one could try to do a direct and/or different calculation, do a comparison and then argue which parts are new/old, better/worse, etc.

3) Since the authors deal with a time-dependent non-autonomous system when using the variational equation around the periodic orbit before averaging out to a mean value, it is also very natural to ask which classical results from Floquet theory and non-autonomous dynamical systems could be applied for finding early-warning signs for tipping points. In this context, there are many different notions for a spectrum if we go beyond classical Floquet theory. For example, what about looking at finite-time Lyapunov exponents, the dichotomy spectrum, etc and simply see what these quantities say as warning signs? At least, things like FTLEs are easily computable via standard packages so there really is very little effort involved in doing these calculations and comparing it to the direct calculations the authors do. I would even guess that from return map data, return times and FTLEs, one should be able to recover identical or very similar warning signs...

4) The authors also spend a long part of the paper on discussing the issue of time scales and relevant limits. This issue has been discussed in a very analogous situation regarding noise-induced and bifurcation-induced transitions. Depending upon the time scale of the noise relative to the parameter drift one either sees noise-induced or bifurcation-induced transitions in certain classes of systems. See for example:

Ashwin, P., Wieczorek, S., Vitolo, R., & Cox, P. (2012). Tipping points in open systems: bifurcation, noise-induced and rate-dependent examples in the climate system. Philosophical Transactions of the Royal Society of London A: Mathematical, Physical and Engineering Sciences, 370(1962), 1166-1184.

Kuehn, C. (2013). A mathematical framework for critical transitions: normal forms, variance and applications. Journal of Nonlinear Science, 23(3), 457-510.

In fact, the issue has appeared in many works implicitly before these works in stochastic multiscale systems. Here the situation is very similar except that there is now instead of the noise-focus a comparison between the forcing scale and parameter drift scale. Therefore, it is actually quite easy to see that there should be two asymptotic regimes and one intermediate regime as for the noise/parameter case also in the forcing/parameter case. In fact, noise terms are frequently just be treated as forcing terms if the noise is smooth enough and maybe one could even transfer previous results via this view.

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My suggestions for the authors would be to take the paper into one of two possible directions for a major revision. Either, (I) one does incorporate and compare a lot more to available techniques and previous results on time-periodic dynamical systems. However, this does seem to be out of the focus of ESD a bit. A second alternative (II) would be to shorten the mathematical part and clearly identify some of the warning signs as the same ones as if one would use return-map methods. With the now available space one could either try to apply techniques to other forced climate models and draw applied conclusions, or look at more time series. These are the stronger parts of this paper and probably more adequate for ESD anyhow. Either way, some re-writing is necessary to embed the problem in a more proper way into previously developed and available techniques. Overall, I think if the authors should pursue a major revision using the second option (II), then I could see the revised paper to be a very solid contribution to ESD.

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