

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

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We would like to thank the referee for his detailed and thoughtful comments on our manuscript. We reply below (the referee's original text in italics followed by our response):

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

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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.

We also agree the second alternative would be the best way to take a revised manuscript and this is the direction we have broadly taken in line with the referee's recommendations. We have added discussions of existing techniques and previous literature into the introduction of the revised manuscript including the return map method which we have applied to the examples in the previous manuscript which we agree was lacking from the original. It turns out the return map method is complementary to the phase lag and response amplification in that in one regime one set of indicators is not useful while the other is. The systems we were largely concerned with in this and the previous manuscript are best handled with phase lag and response amplification which was the reason we did not use return maps in the original manuscript. We discuss this further in the reply to the referee's point (1). We have also extensively restructured the manuscript and included sliding window analysis of harmonic amplitude increasing on approach of a local bifurcation (a suggestion of referee #2). Although the manuscript has a slightly revised title, many new figures and has been significantly restructured to include a more thorough discussion, the technical content, main points and conclusions are unchanged from the previous manuscript although they are strengthened.

We would have liked to apply our method to more examples in the Earth system but feel the paper is coherent with an expanded analysis of Arctic sea ice. Plenty of components of the climate are periodically forced for example by the solar insolation and

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have similar time scales to this forcing, however we could not think of any that are conjectured to be approaching a local bifurcation apart from the Arctic sea ice.

We did consider including analysis of a simple vegetation-savannah model which had a local bifurcation due to variations in precipitation. We decided not to include this as the model was a bit simplified and added nothing new to the manuscript, being very similar to the double well example.

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).

The mathematics we use in the manuscript are very simple and clearly not novel. So much so in fact that it becomes hard to cite a relevant source as any student of physics or engineering will very likely have solved the equation for the damped harmonic oscillator forced periodically and found the solution in the overdamped limit has a phase lag and an amplitude depending on the damping parameter. For instance one can look in any undergraduate level text on oscillations and waves and find these solutions. We have cited one such example in the revised manuscript and a discussion of this point.

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However, we have not been able to find any other authors using the phase lag, amplification response and increase in harmonic amplitude as an indicator of the approach of a local bifurcation. Of course, we would not be surprised if this was not the case since the method is very simple, which is the reason incidently, that we like it! Because of this simplicity, we have been careful not to make novelty claims in the previous manuscript but we have eliminated the offending 'we show' in the revised version.

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.

We agree we should have mentioned the return map method and this is indeed another way of looking for early warning signals of local bifurcations. The motivation for the method outlined in the manuscript was that we were looking at the particular case of the conjectured Arctic sea ice bifurcation and for this system the forcing (the annual cycle of insolation) is about the same order as the time scale of the sea ice (order of months, possibly a couple of years).

Recall methods used for looking for local bifurcations are based on detecting a decrease in the stability of the system's steady state by inferring the change in time taken for perturbations away from that steady state to decay. If the steady state is a fixed point, one usually thinks of the noise in the system as the perturbation and infers the

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system time scale by sampling the system's state at some time interval and computing the correlation between successive time intervals resulting from the perturbation's decay. If the steady state is periodic, like the ones considered in this manuscript, one approach is to sample the system once every cycle to obtain a new time series that can be treated as a fixed point steady state, but now the interval between samples of the system state has increased. This is the return map method and one can repeat the fixed point, compute correlations between the now increased, successive time intervals.

For the cases we consider in the manuscript, where the period of the forcing is of the same order as the time scale of the system such as the sea-ice, the return map would take an annual time series with the resolution of a day if desired (essentially a continuous flow) and convert it to a single point per cycle, that is one data point per year (a discrete map), T . There are two problems with this: (i) there are far less data points to analyze in the time series so any trend in the signal becomes harder to detect with statistical indicators as the standard error scales $1/\sqrt{N}$ (N is number of data points in the time series) and (ii) more importantly, even if there was critical slowing down, since the time scale of the system, τ may be smaller or of the same order of the resolution of the return map time series, detection becomes very difficult or impossible i.e. the time taken for a perturbation to decay back to the steady state is less than the interval between data points resulting in little or no correlation between the data points in the return map. One also cannot reliably use autocorrelation, the usual indicator of noisy slowing down of fixed points, to infer time scale as an assumption in the derivation is that T/τ is small which it is not in this case.

In addition, for the case of the sea-ice, the opportunity of having such an easy to spot, deterministic system response to the annual forcing (which one can think of as a very predictable perturbation) to exploit to infer system time scale without having to do any detailed manipulation of the data motivated our approach.

We therefore realized very early on in the investigation that a return map method would

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very likely not be useful and is not useful for the cases the phase lag and response amplification are most useful. This gave the resulting 'ignore-return-map-tunnel-vision' in the manuscript which on reflection we should have reviewed and critiqued. We have rectified this in the revised manuscript. The cases where the phase lag and response amplification work well ($\omega\tau \sim 1$) are not well suited to return map analysis. Conversely when the interval between return map data points is smaller than the system time scale, $\tau/T \geq 1$ (equivalent to $\omega\tau \geq 2\pi$), a reasonable condition for return map analysis to work well, phase lag and response amplification tend to asymptote and are not so useful. The two methods therefore have some complementarity.

We have included figures illustrating this complementarity in the revised manuscript. Figure 1 is essentially the same as figure 2 in our manuscript except we have varied D_m over 100 cycles instead of 25. This is because we need extra data points to calculate the autocorrelation of the return maps with any reliability. We have also added Gaussian white noise to \dot{x} of standard deviation 0.01 as the return map method needs small perturbations to work. In figure 2 we have plotted all the early warning indicators for this system including the return map calculated with a sliding window of 25 cycles. The black lines are the theoretical curves and the coloured lines are the estimated curves. The key point is the theory and estimated autocorrelations do not show anything in this regime ($\omega\tau \sim 1$). In figures 3 and 4 we have plotted the same quantities but with decreased period of forcing ($T = 1/4$ so $\omega\tau \sim 4\pi$). This is a regime in which phase lag and response amplitude start to asymptote and are therefore not so useful to infer changing system time scale. However, autocorrelation of the return map now becomes useful as can be seen in the figure. This system is going in the $\omega\tau \gg 1$ regime which we have previously discussed in the manuscript.

From the sea-ice time scale of 6 months estimated using phase lag ($\omega\tau \sim \pi$) we did not expect the return map method to be useful. However, these estimates are uncertain so we also calculated the return map for completeness. The results confirm return map analysis is not useful for this case. Specifically we show autocorrelation in a sliding

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window of the return map time series is very uncertain and/or small.

We have added discussion of these points in similar or more detail in the revised manuscript.

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., and 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.

We have added more context and review of previous literature in a revised manuscript, some quoted directly below:

'Abrupt change in a system can occur due to a bifurcation - that is, a small smooth change in parameter values can result in a sudden or topological change in the sys-

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tem's attractors. Extreme sensitivity of systems close to criticality is familiar from studies of critical phenomena in statistical mechanics ? and stability analysis in nonlinear dynamical systems ?.'

We have also briefly reviewed Wiesenfeld's work and mentioned how it differs from ours:

'In an elegant study Wiesenfeld⁸⁵ computed the Fourier spectra of noisy perturbations in systems with periodic attractors. Very close to a local bifurcation, the dominant system time scale asymptotes towards infinity causing the dynamics of the noisy perturbations away from the attractor to be dependent only on the type of bifurcation and not on the details of the system's specific equations. This observation allowed the author to classify all codimension 1 bifurcations in an arbitrary periodic system by the harmonics in the spectra of residuals. He called these early warning signals noisy precursors.'

And when describing using the harmonics in the response:

'With a similar motivation Wiesenfeld⁸⁵ and WiesenfeldMcNamara⁸⁶ calculated the Fourier spectra of the perturbations, rather than the response, away from periodic attractors very close to local bifurcations with noisy and weak periodic modulation respectively.'

We have mentioned work on stochastic resonance where appropriate. The simplest systems this community studies are essentially our conceptual model, that is a periodically driven double well potential, but with the added complication of additive Gaussian noise. They have studied phase response, amplitude and Fourier spectra in this context. However, they are interested in hopping between the wells with some barrier height (the 'stochastic resonance') rather than bifurcations (barrier height goes to zero) as in our study.

The other reference the referee mentions, Zhu¹⁵, seems of limited relevance. These authors look at the well known phenomenon of delayed bifurcation when the control

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parameter is slowly varied compared to the static case. The control parameter in their study, instead of linearly increasing with time is now periodic with changes in amplitude and frequency. At the end of the paper they use a simple sea ice model as an example of this. We have therefore chosen not to include this work.

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...

These ideas may be potentially useful lines of future investigation. However, we are not familiar with all the techniques the referee mentions or how they could be applied to time series analysis in climate applications where the dynamical equations are not known and one's control on the system for repeatable experiments is limited or non-existent. We would be interested to hear the referee's opinion on this.

Although we are interested to hear more about this, the comment is more in line with the first, rather than the referee recommended and author chosen second direction to take a revised manuscript.

*4) The authors also spend a long part of the paper on discussing the
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*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., and 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.*

We are in agreement with the referee. This is the central issue in applying early warning techniques and this is why we spend some time discussing it.

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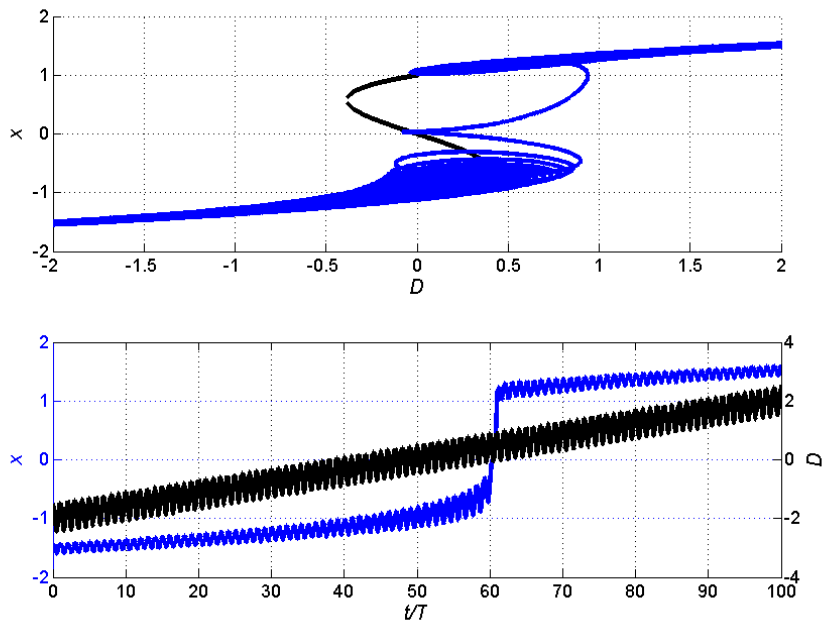


Fig. 1.

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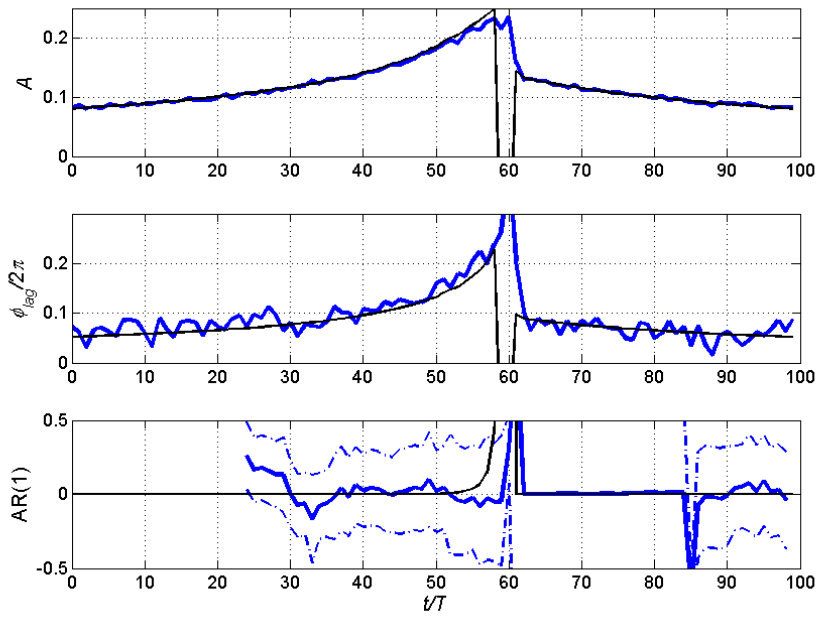


Fig. 2.

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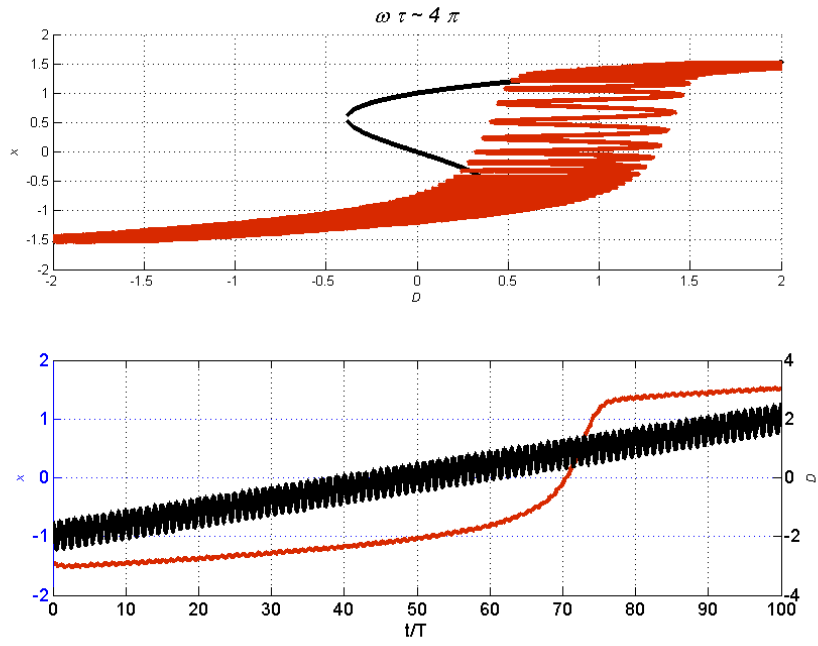


Fig. 3.

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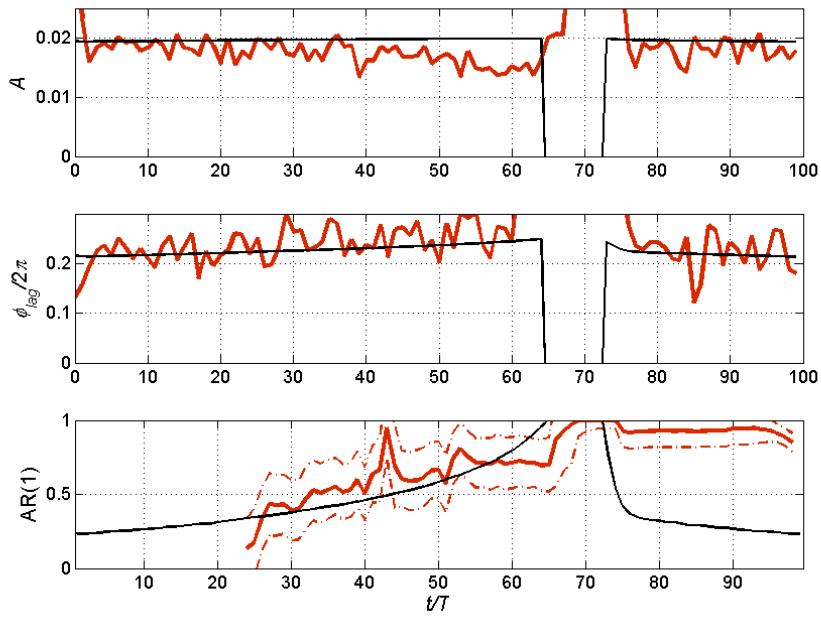


Fig. 4.

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