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The Cryosphere
Discussions

Interactive comment on "The tipping points and early-warning indicators for Pine Island Glacier, West Antarctica" *by* Sebastian H. R. et al.

Alexander Robel (Referee)

robel@eas.gatech.edu

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This manuscript explores the stability of Pine Island Glacier under forcing from ocean melt, using a high order model of marine ice sheet dynamics. Additionally, this is the first study to bring the concept of Early Warning Signals (EWS) to the stability transition known as the "marine ice sheet instability" (MISI). Though EWS have been explored in a limited degree in some other areas of glaciology, this is an interesting and useful application to the MISI problem, which has recently been a focus of intense study in the glaciology community.

The central concept and design of this study are sufficiently novel and important that it eventually should be published in The Cryosphere, though I think it requires some revision first. In particular, since this is the first application of EWS to the MISI problem, it needs to be clear why exactly EWS are a useful tool for studying marine ice sheet stability. Additionally, the methodological details of EWS, while established in the dynamical systems literature, are not well known in glaciology. If the authors wish other glaciologists to follow their lead in using this approach (which I think should be the case), then they need to do a better job explaining the methods they use and the assumptions inherent to these methods. I lay out these critiques in more detail below through major and minor suggestions:

Major:

1. Here is the question that you should answer in this manuscript: Why/When are EWS a useful tool for understanding MISI at a particular glacier? At the moment, my interpretation (perhaps erroneous) of the purpose of EWS laid out in this manuscript is to show that there is a bifurcation (in fact several) in the model. However, you don't need EWS to show that this is the case when you have a model available, since you have the quasi-steady simulations which show the bifurcation structure of PIG. Rather, the point of EWS is to detect a bifurcation before it occurs. You do so in the context of your model, however, solely within the context of a predictive model, EWS are not strictly necessary, because the model can be run forward to determine whether a bifurcation will occur with continued forcing along some trajectory (this is the point of physical models!). However, what you could argue here is that your study is a proof-of-concept to indicate the circumstances under which we would expect to detect EWS in observations, which would be immensely useful for the community. This is what I find currently

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lacking in the study - any discussion of the implications of your study for observations. For example, you touch on this issue later in the paper about the fact that in the real world, ice sheets do not stay on the stable manifold because forcing is much faster than the response time, but then don't really explore whether this makes EWS useless in practice (I think no, but that isn't my take away from the current way its written). Another issue (which you don't remark on) is the fact that a 300 year averaging window for the EWS indicators is not super useful when the entirety of the observational record is 40 years long (perhaps a bit longer if we include some lower quality historical obs). This is all to say that showing EWS exist in a model is not very insightful in of itself if it doesn't provide some indication for what we should be looking for in observations (since again, we already know that there are bifurcations associated with MISI in models).

2. You haven't necessarily explained why EWS show up in certain types of systems. To me, this is key to then explaining why you are calculating these things (ACF, variance, etc). In a canonical saddle-node bifurcation, we expect the stable eigenvalue of the linearized system state to smoothly decrease towards zero as you approach the bifurcation, which causes weaker damping of noisy forcing back towards the stable manifold. So, do EWS only occur where there is a saddle-node bifurcation? How do we know that MISI at PIG has such a smooth approach to the bifurcation? i.e. if the eigenvalue associated with the stable mode is controlled at first order by bed topography (which it is in the canonical formulation of MISI, see Schoof 2012 and others), then does the topography in the vicinity of the bifurcation need to vary smoothly towards a bedrock peak to produce EWS? Maybe these are questions for another study, but there needs to be some indication that you have grappled with the question of why you expect EWS to occur for PIG.

3. Related to the issue above, more of the detail about how the EWS indicators are calculated would be helpful to bring into the main text, since this is a topic most

TC readers are not familiar with. How do you ensure statistical significance? Could we see EWS away from a bifurcation by chance? Why not?

4. You say that you force your model with variability from a "surrogate model" based on ocean variability in the Amundsen Sea region, but don't provide further details. First, more detail is needed on the surrogate model. Second, presumably this surrogate model produces ocean variability with significant power in the decadal range, as many studies (e.g. from Jenkins and others) have found that such variability is important in this region. However, the typical formulation of EWS (e.g. Lenton et al. 2008, and previous studies) assumes a martingale process for the noise forcing (i.e. white noise) which is not the case here. Can you explain why this doesn't affect your interpretations of EWS indicators? Third, it is unclear whether the steady-state and quasi-steady-state simulations used to make the bifurcation diagrams in Figure 3 include noise forcing. If not, then this is concerning, because it is well known that marine ice sheets have a different steady-state with and without noise in the forcing (e.g. Robel et al. 2018, Hoffman et al. 2019, Mikkelsen et al. 2018 (but for non-marine ice sheets)). This could be quite important in your simulations, since the location of the bifurcation is important to know for calculating EWS.

Minor:
Line 23: please define what you mean by "tipping element" for the uninitiated
Line 29: grounding line flux
Line 47: the 2012 Schoof JFM paper makes more sense to cite here
Line 48: what do you mean by catastrophic?
Line 89: So is accumulation held constant in time or does it have a seasonal cycle? Be more specific here, because its important to know if there is variability in more than just ocean melt.
Line 119: Again, to be clear, it isn't necessarily the case that EWS exist for all tipping points (if we define any bifurcation as a tipping point).

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Line 126: it would help here to explain exactly how you force the simulations to produce the grey dashed and black lines in Figure 3 (also in caption). How fast is the forcing? How large are the step increments to determine the steady-states? How do you determine when there is a tipping point (just a large enough jump in the grounding line?)? What if you have a tipping point that causes the grounding line to retreat only a small amount?
Line 119-129: This whole paragraph is confusing
Line 136: What do you mean by short tem "weather noise"? Isn't this the thing that is detected by EWS? Line 184: What do you mean by "equivalent to a random walk"?
Line 197-198: is this hysteresis related to the domain extent? If you aren't simulating flow from outside the domain, then when the whole domain collapses you won't be able to regrow the glacier for any parameter (because theres no catchment).
Line 203-205: what about much shorter windows? (related to point 1 above related to observations). How short of a window would actually be calculable from observations (a decade? does this start to run into the AC time scale of forcing?)
Line 214-215: this is a confusing sentence which leads me to think that you are saying the EWS are not actually "early". To what extent does this depend on the speed of the trend in forcing? Can you test it for different trend rates?

Figure 3a: If the black line doesn't fall on top of the gray dashed line, then the black line simulation isn't really quasi-steady. Why not call it something else? Also, it is unclear how the grey dashed line is determined, and which parts are stable and unstable?
Figure 3b: please explain the different between "tipping point" and "instability onset". I can guess that the latter has to do with the region in which an unstable manifold exists (i.e. there is hysteresis), but I'm not sure readers will necessarily pick up on this without you explaining it explicitly.
Figure 4: Looking at this (and Fig. B2), it seems clear to me that the length of time ahead of the tipping point needed to detect EWS is directly correlated to the speed of

the forcing. Yet, that isn't really made clear here or in the text.
Figure B2: can you extend this to shorter windows?

Appendix A: Related to some of the issues I raised above, it may be valuable to bring the flowline simulations into the main text to demonstrate, in a very simple system where the exact location of the tipping points are known, how EWS work.

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