Anonymous Referee #2

In this manuscript the authors seek to detect the onset of marine ice sheet instability (MISI) in model simulations of Pine Island Glacier, using techniques that have previously been applied to other complex systems. The novelty of this study lies in the application of critical slowing indicators to confirm MISI events. It provides an interesting framework for evaluating vulnerabilities in ice sheets that will be of interest to the TC scientific community. However, particularly due to its novelty within glaciology, some aspects of the paper need improving to aid the clarity, and it would benefit from further exploration/discussion of the usefulness of the techniques beyond the modelling example provided here. I have outlined these below, followed by line-by-line comments.

Many thanks for the positive evaluation of our study and the helpful comments. We replied to all comments and hope that our replies and changes made to the manuscript will address all issues raised.

The paper includes a nice explanation and accompanying schematics of hysteresis; however, the critical slowing description and explanation of the indicators is less intuitive. This may partly be due to the structure – Appendix A offers a useful demonstration of how critical slowing manifests in a carefully controlled simple experiment. I think it is safe to assume most TC readers will not be familiar with these concepts, and therefore I do not think this example should necessarily be tucked away in an appendix. A diagram of critical slowing in a similar vein to Figure 1 would be helpful, or some additional annotation to Figure A1. There is a disconnect between the flowline example in Appendix A, and the methods used for determining the onset of a tipping point in the main set of experiments. Could you show how critical slowing in the flowline experiment can be demonstrated with the various indicators you use in your main experiments? This would help show how these indicators are related to the increased recovery time from a stepwise perturbation as the tipping point is approached.

The other reviewer also highlighted that various elements currently in the appendix could be moved into the main body of text and we will do so. We will also give a more detailed explanation of critical slowing since it is true that many TC readers will not be familiar with this concept and will produce a new figure in the introduction to hopefully aid this explanation. Our two sets of experiments, one using the MISMIP setup and the other using the PIG geometry, currently identify critical slowing in two different ways. This is intentional as these have different goals and we do not think it would be useful to directly compare them. The benefit of the MISMIP experiment is that it is simple and the way we extract the change in response time directly is very easy to understand. It is also an experiment that many readers of TC and the wider glaciological community are very familiar with. However, since the bed is smooth and the perturbations to the model are stepwise, we can not detect EWS in the flowline setup using the same approach as was used for the PIG experiment. Since we will move the MISMIP experiment into the main body of text, we will try to emphasise the differences and unique goals of each set of experiments more clearly in a revised version of the paper. We will add a figure to explain how EWS show critical slowing in a system driven by a forcing with superimposed natural variability.

My other main comment is about the usefulness of this method in detecting EWS in reality. Would the 300-year optimal window size apply to other catchments? What kind of observational datasets are required to implement this analysis, in a way that would act as a useful EWS for MISI? The measurement used in this study (grounding line flux) does not exist (at least not at the quality/resolution required here) prior to the satellite era, so what is the alternative, given the 300-year window size? In your model simulations the forcing is applied gradually in order to avoid "one tipping point cascading into the next and result in three individual tipping points being misinterpreted as only one event" (L168). What are the implications of this for detecting tipping points in observations, where the system is not necessarily able to return to a quasi-steady-state with changing forcings? Do you have a sense of whether the indicators would hold up if the forcing is more rapid? Further discussion of these issues would strengthen the paper.

Many of these points are raised by reviewer 1 and we have addressed them at length in that reply. To summarise our response: we will add more discussion around the implications of our results for finding EWS in observational records. We do not believe the 300-year window size to be a lower bound and did not try to reduce this systematically, but this will also be discussed in a revised version of the paper. Since the length of record necessary for EWS as well as the variable used to detect EWS are critical parts of what observations can or cannot be used, this is a very important research question but one that requires considerable work to address and is beyond the scope of this initial study. Regarding the last point about how cascading tipping points and a rapid forcing might affect our ability to detect critical slowing, this is known to influence the ability to detect EWS (Dakos et al., 2015). For example in ecosystems, interacting regime shifts can muffle or magnify variance near critical transitions (Brook and Carpenter, 2010). In this study, as explained in detail in the response to reviewer 1, we first of all want to make the nontrivial case, that ESW – that exist theoretically for MISI – can actually be detected in a realistic geometry that is of great interest with respect to MISI at the moment. We would argue that this issue raised by the reviewer is very important and an interesting research questions but beyond the scope of this study.

All minor comments will be addressed in a revised version of the manuscript and where appropriate we have added a response to specific questions. Any suggestions/comments left unanswered here will simply be directly incorporated into a revised manuscript.

Other comments:

L13: "Self-amplifying retreat" this could be considered an overstatement. Selfsustaining retreat would be more accurate (and more in keeping with language further on in the manuscript).

L18: "early warning indicators robustly detect critical slowing for the marine ice sheet instability". It might be worth removing the term "critical slowing" from the abstract, and instead using a less jargon-y alternative, e.g. "robustly detect the onset of MISI"?

L31-32: "a complex range of factors can either cause or suppress the MISI" – such as? The two papers cited refer to buttressing, what about local sea level, GIA etc? Haseloff, 2018, should be Haseloff and Sergienko, 2018.

L68-70: "Our results reveal the existence of multiple smaller tipping points that when crossed could easily be mis-identified as simply periods of rapid retreat, with the irreversible and the self-sustained aspect of the retreat being missed": this seems to contradict your results and conclusions. The two smaller tipping points are not irreversible, as the system can return to previous state through stronger perturbations in the opposite direction – shown by the hysteresis loops in Figure 3.

Unhelpfully, the word irreversible is used by various authors to mean different things in this context and we have tried to be consistent throughout the paper and we clarify our meaning in lines 53-54. We are not aware of a word that uniquely describes one or other type of irreversibility but this is an important point to clarify.

L106: Basal melt rates: it is not clear from this paragraph whether basal melt occurs under grounded ice. How do you treat partially grounded elements? This has been shown to be important in modelling grounding line retreat (e.g. Seroussi and Morlighem, 2018, doi: 10.5194/tc-12-3085-2018).

Melting is only ever applied to fully floating elements.

L153-159: Unlike the other paragraphs in this section, this paragraph does not contain an outcome of your decision-making process – which of the criteria will you use?

We find that indicators do seem to reach critical values ahead of a tipping point and but this is a result of our study rather than a decision made during the experiment design.

L180-181: "Furthermore, the indicator reaches a critical value relatively close in time to when the MISI event gets underway". Clarify that the critical value is 1.

L183-185: "For this early warning indicator. . .". I don't understand this sentence and it seems like it would be better suited (with added detail) to the methods. I thought both indicators have a critical value of 1 (section 2.2), so why does scaling the DFA help with comparison to ACF?

The DFA indicator does not have a critical value of 1, hence why scaling it makes sense.

L188-189: "although variance cannot be used directly to predict when that threshold will be crossed" – perhaps this is obvious, but why not? Because there is no critical value? But crossing the critical value doesn't seem like a robust way of detecting the exact onset of MISI either, considering some of the trend lines in Fig. 4 cross x=0 before they reach the critical value?

Yes, because variance has no critical value. It is true that our trend lines do not cross critical values exactly at the tipping point, that would be remarkable given the complexity of the model, however we do find our indicators reach critical values very close to a tipping point, making this a useful EWS.

L275: What do you mean by "i.e. a record length of 600 years" – how does that relate to the window size? Is that the minimum record length required?

This point is addressed at length elsewhere in our response.

L258: Basin of attraction has not been defined/explained.

Figure A1: Panel A, grounding line position in km: clarify the direction of retreat.