Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esd-2021-100-RC1, 2022

Comment on esd-2021-100

Kristin Poinar (Referee)

Referee comment on "Dynamic regimes of the Greenland Ice Sheet emerging from interacting melt–elevation and glacial isostatic adjustment feedbacks" by Maria Zeitz et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-100-RC1, 2022

Summary and general comments:

This manuscript presents a discovery of unforced, long-term fluctuations in the size of the Greenland Ice Sheet. The fluctuations (which are not really oscillations, as they are not strictly regular or repeating) have periods ~80 - 300 kyr and originate from the interactions between the melt-elevation feedback (a positive feedback) and glacial isostatic adjustment (a negative feedback). This has not been previously studied on long (ice age) timescales in the absence of external triggers (e.g., Heinrich events initiated by ocean heat pulses) for a land-terminating ice sheet. The finding of these emergent cycles could be relevant for "deep future" states of the Greenland Ice Sheet, although it is a challenge to imagine a future without climate forcings that would presumably overshadow the internal variability. Regardless, it is an interesting discovery that merits reporting, and this paper is largely successful. I have only minor suggestions, and although they are somewhat numerous, they are all quite attainable.

Specific comments:

The authors used a "power spectrum analysis" to identify periods in the ice volume time series. These methods should be explained, if only briefly, and some test for significance should be carried out. The authors state that "The oscillation times do not seem to show a clear dependence on the values for warming, lapse rate or mantle viscosity" (P11 L12). This seems troubling -- wouldn't we expect a clear pattern to emerge within the parameter space? If so, the authors should do additional thinking and put forth possible explanations for the scatter. If not, that is interesting too, and the authors should elaborate on the reasons why this system is not governed regularly.

Relatedly, Figure 2 shows that some of the parameter combinations do, apparently, have quite regular periods (especially in Figure 2b), while others do not (such as the higher

lapse rates in Figure 2a). A short presentation of the values of the periods (and which are significant) should be done. The significant period values (kyr) could even simply be written inside the cyan blocks of Figure 4.

As alluded to in my summary, I suggest replacing "oscillation" throughout the manuscript with a similar word that does not imply regularity, such as "fluctuation" or even "variation". This is because the sequence of states does not always have a regular repeat interval.

The first six lines of the Discussion restate the results, as do lines 11-17 on this page. These are redundant to the rest of the manuscript and should be removed. The last three lines of the first paragraph describe one possible extended importance of this study, which is not actually studied or discussed, and therefore would be more appropriate in the Conclusion or Introduction.

Finally, I would suggest a different name than "recovery" for the state where the ice sheet reaches a new equilibrium size significantly smaller than its start. "Recovery" implies, to me, that the ice sheet returns to its initial state. More precise names could be "re-equilibration" or "new steady state".

Technical corrections:

- P1 L5 - "Greenland could become essentially ice-free on the long-term" - I suggest stating the rough number of years found for this, instead of the vague "long-term".
- P1 L13 - "oscillation periods of tens to hundreds of thousand of years" - similarly, I suggest stating the rough number of years here. This is because your minimum period (80 kyr) is not that well described by "tens of thousand of years", so it is unintentionally misleading.
- P4 L4 - add Laurentide Ice Sheet, which is what Bassis et al. (2017) studied.
- P5 L8 - Please include a brief explanation, and/or citation, for why the enhancement factors (1 and 1.5) are different depending on which stress balance is used across the domain.
- P5 Sect 2.1 - The level of description of the ice sheet model (2.1.1) is much more general than the earth deformation model (2.1.2). The classic bending-beam PDE (Eq. 1) is included with all parameters described and values supplied, for instance, but the sliding law and till stress model used in PISM are only described in words, with no parameter values given. These should be enhanced to match the level of 2.1.2.
- P8 L12 - Missing reference (?).
- P11 L3 - specify meters global sea level rise; write 1 \times 10^{19} instead of 1e+19
- P11 L21 - typo "2astern"
- P12 L4 - I have never seen a zero-indexed "o/i/ii/iii" list before. I suggest standardizing to "i/ii/iii/iv".
- Table 1 - Specific values used for \Delta T are listed, which is helpful. Values for \Gamma and \nu, instead of their ranges, should be listed similarly.
- Figure 2 - Title of panel a is missing the "times" sign. X axis labels in kyr would make it

more legible.

- Figure 5 - I suggest you outline or stipple the boxes that you classify as oscillating. As it is, the figure relies on the reader to interpret on their own which boxes show "significant difference" in color.
- Figure 6 - What is the mantle viscosity & climate change forcing (\Delta T) used here? It looks like it might be the same runs shown on Figure 2a, but that is only my guess.