Response to Reviewer Comments

Journal: Earth System Dynamics
Title: **Dynamic regimes of the Greenland Ice Sheet emerging from interacting melt-elevation and glacial isostatic adjustment feedbacks**
Authors: Maria Zeitz, Jan Haacker, Jonathan Donges, Torsten Albrecht and Ricarda Winkelmann
MS No: esd-2021-100
MS Type: Research article

Once again, we would like to thank the editor Michel Crucifix and the reviewer, Kristin Poinar for her helpful comments! In our second minor revision of the manuscript we have addressed the suggestions made by the reviewer (point by point answers below). In addition we have reworked the color schemes of the paper to fit the Copernicus standard for accessibility concerning color blindness and have added the revised version of Figure 6, which should have included a visual help to highlight the oscillation region in parameter space already in the last revision (which does not change the data of the figure).
We provide detailed answers to all comments below. The reviewers' comments are given in black and the authors' in blue. The changes made to the manuscript can be found in the track-changes file (created with latexdiff).

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Kristin Poinar (Referee)

Review of revised manuscript "Dynamic regimes of the Greenland Ice Sheet emerging from interacting melt-elevation and glacial isostatic adjustment feedbacks"
by Maria Zeitz et al.
June 7, 2022

The manuscript has been improved and enhanced from the first submission with the addition of a robustness analysis (section 4.3) on the oscillations across six different modeling choices (e.g., bedrock uplift model, spin up state, lithosphere thickness). An appendix is also added that illustrates the dependence of the relaxation time of the Lingle-Clarke bedrock uplift model on the ice sheet length scale. Thirdly, the method for measuring the oscillation timescale is now explained (I believe it is simpler than the method used in the first submission), and the uncertainties on these timescales quantified at 1 - 2.5%.

The new figure that illustrates the ratio of the recovery time to the plateau time (new Figure 5) is a good addition. This shows the result that, for most parameter combinations, an oscillating ice sheet spends more time near its "big" state than around its "collapsed" state (these are termed the "plateau" and "recovery" states, respectively, per page 15). It took me a little time to find my bearings in Figure 5; I think this was due to the use of percentages in the text (e.g. "10%" on page 15) versus whole numbers without a % sign on the figure

(e.g. "14" for the corresponding 10% point on Figure 5, with the "%" appearing in the colorbar), and the use of ratios in the caption (i.e. "1" instead of "100%"). These should be made consistent in some way. (To be clear, I don't have any problem with calling 10% ~ 14%.)
Many thanks for this comment. We have harmonized the notation in the Figure, the caption and the corresponding paragraph. In addition we have decided to be more precise in the text as well and have changed from 10% to 14%.

The new Figure 5 and the new section 3.2.2 go toward addressing my question about why there is no clear pattern between parameter combinations and oscillation timescales. With their new definition of oscillation timescale, the authors have now found that an ice sheet spends more time in the collapsed / recovery state in model runs with higher temperatures, higher mantle viscosity, or higher lapse rate (page 15). If I think through, myself, about which direction I would expect these parameters to work in and why, I think I can agree with the results -- but more ideally, the authors would explain these findings themselves in the discussion. I did not find this in the manuscript. A short additional subsection in the discussion would help readers make more sense of this central finding.
We have added a subsection in the discussion, 4.2.1., explicitly addressing the interpretation of the findings following from Figure 5. In particular we explicitly state that a high fraction of “recovery time” / “plateau time” seems to indicate a loss of stability of the Greenland Ice Sheet in these simulations.

I also like the change to Figure 1. It now more clearly shows the signs of both feedbacks by highlighting the signs of each process, showing twin arrows along the common pathway, and the processes are now named more clearly.
Thank you.

I was not able to assess the other reviewer's comment, and the authors' response, regarding whether the wavelength-dependence of relaxation time in the Lingle-Clarke model was realistic. I do agree with the reviewer that if this is an artifact of Lingle-Clarke, this paper would be better framed as "Watch out for this behavior in your ISM if you're using Lingle-Clark" rather than "This is a true feature of the GrIS on long timecales". The authors responded to this query by testing other bedrock models, which produced similar results as Lingle-Clark, but I lack the expertise to assess whether those models have similar weaknesses to Lingle-Clarke, versus the "full visco-elastic" models requested by the reviewer.
Thank you for the effort to assess the reviewer’s concerns.
Indeed, the interactive coupling of the Parallel Ice Sheet Model (PISM) to full visco-elastic GIA models is partly ongoing work. Torsten Albrecht, one of the authors of this paper, is working on interactively coupling PISM to the VIscoelastic Lithosphere and MAntle model (VILMA) (Klemann et al., 2008; Martinec et al., 2018) and focusing particularly on the Antarctic Ice Sheet. First results for the Greenland Ice Sheet show that indeed, the self-sustained oscillations are indeed reproducible in this setup, and we hint on that in the “robustness” section. However, a proper analysis of the dynamic regimes in the interactively coupled setup would go beyond the scope of the present manuscript: a proper introduction and validation of the coupling scheme are still missing and part of forthcoming work.

Regardless of the outcome of the bedrock uplift model exercises, I find that the authors have improved the manuscript from its original version to make a stronger paper overall. With small changes to the Figure 5 description and a thorough discussion of what the Figure 5 findings mean, the concerns I had will all be addressed.

Cheers,
Kristin Poinar