Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., referee comment RC2
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Comment on esd-2021-100

Anonymous Referee #2

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-RC2, 2022

General comments

The paper documents intriguing dynamic behaviour of the Greenland ice sheet resulting from the interplay between the melt-elevation feedback and the GIA feedback. The material is generally well presented and easy to follow. By itself the results are very interesting and potentially provide a very novel insight into the longer-term internal dynamics of the coupled climate-ice sheet-bedrock system. At the same time, I am also very puzzled by the results, in particular by the self-sustained quasi-periodic oscillations the authors find for (a rather narrow range) of parameter combinations. Many Greenland ice sheet modelers have performed similar experiments already since the early 1990s by imposing a stepwise warming in very similar model setups involving quasi the same degree-day type of climate forcing and taking into account isostasy with state-of-the-art models, but none of these studies have ever found even a trace of the kind of oscillations described in the paper. This makes me conclude that indeed their oscillatory behaviour may well be an 'artifact' (to cite their own words) of their particular experimental design and parameter choice. In other words, their model behaviour is probably not a very robust type of behaviour, to say the least, and might be very difficult to replicate in other models. My suspicion is that their model behaviour is a result from the particular choice of the Lingle-Clarke isostatic model and will not show up for any other isostatic model, be it of the 'ELRA' type, or of the more sophisticated 'self-gravitating visco-elastic earth model' type.

I think the paper would be a very valuable addition to the literature of Greenland ice sheet dynamics, but first I would like to find out more on the robustness of the results and the specific role played by isostasy. A particular feature of the Lingle-Clarke model, and its implementation by Bueler et al. (2007) is that the relaxation time increases for wavelengths up to a few thousand km (a wavelength corresponding to the Greenland situation), which I believe is unrealistic. Full visco-elastic models show the contrary, the relaxation time decreases for a larger load. My guess is that it is exactly this specific behaviour of the LC model that is causing the oscillations. I suggest the authors make an effort to respond to this criticism by including material (figures and/or discussion) to prove or disprove this point.

Specific comments

page 2, line 5: a reference is needed to substantiate the 65/35% attribution of current ice losses of the Greenland ice sheet. As far as I am aware from comprehensive studies, the ratio is more like 50/50 for both SMB changes and ice calving changes (e.g . IMBIE team, 2020)

page 2, line 22: here, and elsewhere (page 6, line 5) it is stated that 'to our knowledge' their have been no previous studies coupling Greenland ice-sheet dynamics to bedrock dynamics. That is not entirely true. Le Meur and Huybrechts (1998, also in GJI in 2001) have done this for the glacial cycles, also in Zweck and Huybrechts (2005) Greenland ice sheet dynamics was included and was part of the sensitivity study.

page 4, line 11: explain what the 'small ice cap instability' is.

page 4, line 12: To what does 'This' refer?

page 4, line 17: explain why the factor 1/3 is expected.

page 6, section 2.1.2: a critical appraisal of the specific features of the LC model is in order here. A more thorough discussion of the dependence of the relaxation time on the wavelength of the load change and how this compares to other models is required here, as this may well be a crucial issue in this paper.

page 6, section 2.1.3: apparently the precipitation pattern from RACMO does not interact with climate change or ice sheet geometry as it seems to be fixed. Mention this explicitly and mention the shortcomings of such an approach.

page 7, section 2.1.3: is the rain fraction a function of the monthly mean temperature? If so, the transition temperature range between 0 and 2°C seems much too small. One would still expect rainfall during a month with a mean temperature below 0°C and snowfall for a mean temperature above 2°C. Please discuss the limitations of this approach.

page 7, line 16: it is mentioned that ice-ocean interaction is included via PICO. More information is needed here. Where is the ocean forcing coming from? At what resolution? What about water circulation in the fjords? How is oceanic forcing transferred to calving fronts? Does the model have a grounding line and attached ice shelves, and how are they treated? Does it matter to include ocean forcing for the type of experiments described here at all?

page 7, section 2.2.1, and associated figures in the supplement: it is puzzling to me that while the climatic mass balance from the model differs substantially from RACMO (Fig. S2), the simulated ice sheet domain almost exactly matches the observations (Figs. S1 and S3). Almost on view it can be seen that the ice-sheet wide average surface mass balance must be positive over the domains shown, yet there is hardly any advance of the margin for the initial state. How was the initial ice sheet constrained? What is the meaning of the row of black points (low or zero velocity) at the margin in Fig. S3? To me it is hard to believe that the initial state corresponds to a self-sustained steady-state ice sheet with a freely evolving margin, the latter of which is crucial in the experiments.

page 12 and further, section 3.2: A crucial issue is how realistic the bedrock model is. In the model only viscosities are changed to control the relaxation time scale. What about the effect of variations in flexural rigidity of the lithosphere?

page 14, figure 6: The figure is very difficult to read and understand, and should be improved. The colour saturation seems to represent time (but the caption does not say), however the pale parts of the lines are difficult to see. What is the meaning of both crosses? Lower axis: accumumlation-> accumulation. Left axis: are you sure the average level of topography has negative values? Please adapt the figure and the caption to increase readability.

Page 15, lines 18-20: it is impossible to discern on Figure 6 the clockwise or counterclockwise sense of the trajectories. Perhaps an arrow would help.

Page 16, line 31: Petrini et al. (2021) is a crucial reference to prove that the results are not an artifact of the specific experimental design. However, that is an EGU abstract, and cannot be checked. Remove the reference to Petrini et al. (2021).

Technical corrections

Page 3, line 14: solte -> solve

Page 3, line 32: sophisticates -> sophisticated

Page 3, line 33: year of Fettweis et al. publication missing

Page 4, line16: add ‘itself’ between ‘manifest’ and ‘on’

Page 8, table 1: the mantle viscosity value of 1x1**-19 cannot be right.

Page 8, line 12: there is a ‘?’ in the reference list

Page 8, line 22: remove the comma between ‘both’ and ‘the’.

Page 14: line 11: do not start a sentence with a capital after a semi-colon

Page 16, line 31: Hoever -> However