Climate
of the Past

Discussions

Interactive comment on "The sensitivity of the Greenland ice sheet to glacial-interglacial oceanic forcing" *by* Ilaria Tabone et al.

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Review

The paper by Tabone et al. describes a series of experiments over the past 2 glacial cycles performed with an ice-sheet-shelf model for the Greenland ice sheet (GrIS). The authors explore the influence of ocean temperature, through a sub-shelf melt parameter, on the advance and retreat of the Greenland ice sheet. Simulations are performed over 240 kyr, for which atmospheric forcing is prescribed through a glacial index derived from a combination of ice-core data reconstructions. The same index is used to vary oceanic temperatures, although this time variability is also switched off in some experiments.

The paper has a clear and relevant scientific question within the scope of CP, exploring the variations of the GrIS over glacial cycles, using a novel concept (at least for Greenland) using an ice-sheet-shelf model that is capable of grounding line migration and simulation ice on land and afloat. The conclusions are clear but need further exploration, and some additional comments in the text. Methods and results are clearly presented, but I do suggest additional experiments need to added that take into account changes in sea level. Already in the Hubrechts (2002) study, sea level is used as external forcing, so why not do it here? Particularly when running an ice-sheet model over multiple glacial cycles this should be included.

The paper's title is clear and overall presentation and language is good. References are appropriate and the introduction is presented very well. All in all, I think this is a well written and good paper that deserves publication in CP. As said, I do suggest that the authors include additional experiments that include an external sea-level as forcing to further explore the influence of changes in sea level. Besides my four main comments I have attached a pdf with comments included (I hope you can read/find them clearly).

Main remarks:

1. Description of basal melting
As I noted in my comments in the pdf, it is unclear to me if you use spatially varying basal melting and how Bref depends on kappa. As given in Table 1, for the first set of experiments you set kappa to 0, which as I imply from equations (9) and (10) that Basal melting at the grounding line is zero everywhere, all the time. Please clarify this in the paper.

After reading section 2.3, it seems that you use the equations 6-10 as a general description of how basal melting can be calculated. Overall reading this I thought that

all parameters mentioned are used in the model, or not? Please make clear how you actually prescribe basal melting in the model, is it a uniform single value or is it a function of x and y or locations? State explicitly what is used in the model and which variables you mention in equations 6-10 are actually not used. It is rather confusing that you mention these variables and then actually not use them.

2. Influence of Insolation on surface melt

In Section 2.2 the melting scheme is shortly discussed. I do wonder why insolation changes are not included (as I noted in the pdf as well). Why not include the correction as suggested in Robinson and Goelzer (2014)? It is shown by them (and other studies) that particularly for the GrIS insolation changes are important, especially for the last interglacial. I expect the authors to at least discuss this in more detail in the discussion/conclusions.

3. Comparison to Bradley et al.

About the same time a study investigating similar issues has been published in CPD, Bradley et al. I see that the first author has actually reviewed this paper. I want to note that I was not involved in this paper, although it does make use of the same ice-sheet model (although a different version) that I use. It might be good that some comparison would be made with this other paper for example in the discussion and in Figure 11. At least mentioning this paper in the introduction and the discussion.

4. External sea level forcing

As I noted in the pdf, it seems to me that using a constant sea level during the entire simulation time is unrealistic. I would highly suggest to at least include eustatic (i.e. global mean) sea level variations as external forcing in these simulations. (for example, from Grant et al: http://dx.doi.org/10.1038/ncomms6076 or other studies you might find). I would be very interested in how the results will change. It provides you with additional experiments to also show the sensitivity to ocean forcing in terms of sea level.

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General comments are given in the attached pdf.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-127/cp-2017-127-RC1-supplement.pdf