The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-225-RC1, 2018


The Cryosphere Discussions
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# *Interactive comment on* "Eemian Greenland ice sheet simulated with a higher-order model shows strong sensitivity to SMB forcing" *by* Andreas Plach et al.

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Received and published: 27 November 2018

The paper by Plach et al. describes a simulation of the Greenland ice sheet (GrIS) with a higher-order model version of the ISSM ice-sheet model. The paper generally addresses the question to what extend surface mass balance models influence the simulated volume of the GrIS during the last interglacial, which is within the scope of TC. To my understanding this is at least one of the first papers that uses a higher-order model to run ice-sheet models simulations of the last interglacial, in that sense it presents novel concepts. However, I believe the conclusions reached do rely a lot on the experimental design and caveats of using a higher-order model (relative to the



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more common, but less advanced shallow ice/shelf models). I will discuss this below in more detail.

The methods used are well explained, but needs a bit more detail on some parts, see my main remark #3. Results are well presented in nice figures, and support the interpretation and conclusions, referencing is generally well done. The title is clear and concise and grasps the general conclusions of the paper. I think a clear goal (my main comment #1) is not well explained, which should be added at the end of the introduction. The overall presentation and structure, language and figures looks good. References are appropriate.

In general your results are presented well in the figures and especially Table 3, Figure 6 and 10. I do think that one can think of different experiments, specifically in the case of basal sliding or ice flow (e.g. ice-flow parameters) that can have a larger impact on your results as those summarised in figure 6. So the question is, is the suite of experiments you use here enough to draw the conclusion that SMB has the largest influence on the simulated GrIS for the Eemian? Or does it rely (too much) on the experimental design. As already was shown in previous papers (e.g. Van de Berg et al., 2011; Robinson and Goelzer, 2014), Insolation is a dominant forcing that controls the SMB and thus the retreat of the GrIS (since CO2 variations are small..), so in that sense external forcing already controls the retreat in a way. This should be discussed at the end and in the conclusions.

In summary, I would say my revisions are minor in the text, but I would like to see some additional experiments as mentioned in my comments.

ref:

Robinson, A. and Goelzer, H., 2014. The importance of insolation changes for paleo ice sheet modelling, The Cryosphere, 8, 1419–1428.

Main remarks:

1) Clear goal of the paper

Page 2, Line 29: Clear goal of the paper is lacking in the introduction. A (firm) sentence should be added at the end here.

2) Description of SMB methods

In Section 2.1, I would definitely like a bit more explanation on the two SMB methods in this section, since it also largely determines your results. I do prefer to not fully read previous articles (Planch, 2018 or Helsen, 2013). Also add an explanation what the differences are between the 4 time slices you use (CO2, orbits?), and the differences between the two SMB methods. What is ice-sheet topography that is used in the NorESM simulations, etc.?

3) Experimental design

I have a number of remarks on the experimental design, outlined on page 4 and 5. Although you do use a sophisticated model, which might be expensive to run, you also use a (faster) SSA-type setting of ISSM to test basal friction. The initial conditions of your simulation are not tested, but have been shown (e.g. Helsen et al., 2013) that it can influence your results. Although you mentioned in the discussion that it is hard to include a full glacial spin-up for your simulations, I do think it would be good to include an experiment (perhaps using the SSA-type model) to determine the influence of the initial conditions (or pre glacial changes) on your Eemian simulations.

Secondly, in general this section could use a bit more explanation. About the SMB methods, but also about what you do with basal friction. Does it stay constant throughout an experiment, is it spatially varying in both the HO and SSA experiments? See also comment on page 4, lines 9-12.

Thirdly, why you would keep the temperature prescribed at the surface constant (bottom page 4)? Is this for reasons of numerical stability? Please explain. Otherwise you should include these in a simulation on these time scales.

Finally, the same holds for bedrock adjustment. I think that it is necessary to include in paleoclimate simulations. At least use an ELRA model to include this please, I think it is vital that this is included when changes are relatively large (MAR-BESSI experiments in Fig. 1.2).

4) Experiments

Looking at Table 2 and reading the text on page 5,6 it is unclear to me how many experiments you performed and with what model. From the final note on page 6 (line 10.11), I think you did a lot of experiments also with the SSA version of ISSM, but from Table 1, it looks like you only did one. Perhaps number all experiments you did, individually, in Table 1, or make a clearer list, mentioned how many experiments you use in the analysis for this paper exactly. Make clear for which experiments you used the SSA version of ISSM. The 'relaxed' experiments is (sort of) an experiment testing the initial conditions I would say. But considering my previous comments it might be worthwhile to also include additional experiments that include a (glacial) spin-up (using the SSA version) of the GrIS. See also my specific comments in the attached pdf.

5) Discussion of results

I think it is essential, concerning your main results, that you explain what causes the differences between the SEB and BESSI models. An additional paragraph that would concisely explain the differences would be good. Shortly reading through your 2018 paper, the differences do not seem to be that large in terms of SMB, however in terms of final ice volume changes are rather significant. Also discuss which do you consider to be the most realistic, and what factors/processes could play a role in determining the SMB.

Your discussion (starting on page 16, line 28) is in a way good to show what the HO version of ISSM can (and cannot) be used for. Considering you are not using bedrock adjustment, ocean forcing, and keep the boundary of the ice sheet fixed, it makes me wonder if ISSM is a suitable model to be used for forthcoming paleoclimate

(glacial-interglacial) simulations. There are so many other options available nowadays. Nonetheless, I do feel it is a suitable tool to investigate warmer than present climates, but initial conditions and pre-glacial impact on the interglacial (e.g. GIA, ice rheology, relative sea level) are vital to assess the exact changes of the GrIS during the last interglacial.

Comments on the figures (also in the pdf).

Add panel numbers to figures 3,4,5,7, 8 and 9. Use panel numbers when referring to the specific panels (have commented this at some locations, but not all).

Figure 1

add legend inside the figure (e.g. bottom right). I would suggest to put ice volume on the left axis and add sea-level contribution (relative to the modern ice sheet) on the right.

Figure 2

Definitely need a legend in, or next to, this figure. You might want to use a bit darker shade of yellow. Perhaps make all lines a bit thicker too. Same as fig. 1, switch the y-axis and use sea level contribution relative to PD on the right.

Figure 3

Why is SMB still positive in the southwest area in the BESSI experiment for 125 ka? Please discuss this in the text. It looks like it already stems from the beginning of the run (also slightly visible in the 127 ka picture).

Figure 5

I suggest to put the y-axis in meters. The grey lines are not so well visible, would use a colour (blue?).

General comments





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General remarks are provided in the attached pdf.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-225/tc-2018-225-RC1-supplement.pdf

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-225, 2018.