We would very much like to thank Bas de Boer for reviewing our study and for his constructive comments. Please find below the referee's comments in black font and the author's responses in blue font.

Responses to Bas de Boer (Referee #1)

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 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.
Van de Berg, W. J., van den Broeke, M., Ettema, J., van Meijgaard, E., and Kaspar, F., 2011. Significant contribution of insolation to Eemian melting of the Greenland ice sheet, Nat. Geosci., 4, 679–683.

We thank you for these constructive and positive comments and will address each of your comments in detail in the following.

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.

We will modify the last sentence of the introduction (page 2, line 28-29) in the revised manuscript to make the goals of our paper clearer.

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 (Plach, 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.?

Yes, we acknowledge that additional details on the two SMB methods is useful here and will add this in the revised manuscript. The four time slices only differ in terms of GHG concentrations and orbital insolation. All climate simulations, both NorESM and MAR, use the present-day ice sheet geometry. We will add more information on this and the climate model simulations in Section 2.1 SMB forcing.

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.

We agree that it would be desirable to perform sensitivity experiments of the initial conditions. We present a very simple test of the influence of the initial state with our *relaxed* experiment, where we use an ice sheet which is spun-up for 10 kyr with a pre-industrial SMB, i.e., climate and ice sheet are in equilibrium. However, more comprehensive initial sensitivity tests would introduce many additional simulation choices (i.e., more unknowns) and a spin-up with our ice sheet model would be very costly. Furthermore, we lack a climate forcing older than 130 ka. Our climate and SMB simulations (taken from Plach et al. 2018) only cover the period 130 to 115 ka. Greenland ice cores also do not provide any information about the climate for the penultimate climate period. Additionally, the missing GIA model in the transient mode of ISSM is a significant shortcoming for further spin-up experiments (also see your final remark). Our experiments are designed to show the influence of two SMB models on the ice sheet evolution during a warm climate period which is characterized by melting. We do not aim to provide an accurate estimate of the Eemian ice sheet minimum, but we rather want to illustrate the influence of the choice of SMB model. For a more comprehensive picture, of both the Eemian and the penultimate glacial period it is necessary to use more than one climate (model) forcing. All climate and SMB simulations in this study and the preceding Plach et al. 2018, only use one climate model. However, as previous Eemian climate model intercomparison papers have shown (Bakker et al., 2013 and Lunt et al., 2013) the simulated Eemian climate is quite different between climate models.

We will clarify in the introduction that the aim of our study is to test SMB basal sliding specifically (also see the response to remark 1). We will also add a discussion on how different initial conditions may impact the sensitivity of different boundary conditions.

Bakker, P., Stone, E. J., Charbit, S., Gröger, M., Krebs-Kanzow, U., Ritz, S. P., Varma, V., Khon, V., Lunt, D. J., Mikolajewicz, U., Prange, M., Renssen, H., Schneider, B., and Schulz, M.: Last interglacial temperature evolution – a model inter-comparison, Clim. Past, 9, 605–619, https://doi.org/10.5194/cp-9-605-2013, 2013.

Lunt, D. J., Abe-Ouchi, A., Bakker, P., Berger, A., Braconnot, P., Charbit, S., Fischer, N., Herold, N., Jungclaus, J. H., Khon, V. C., Krebs-Kanzow, U., Langebroek, P. M., Lohmann, G., Nisancioglu, K. H., Otto-Bliesner, B. L., Park, W., Pfeiffer, M., Phipps, S. J., Prange, M., Rachmayani, R., Renssen, H., Rosenbloom, N., Schneider, B., Stone, E. J., Takahashi, K., Wei, W., Yin, Q., and Zhang, Z. S.: A multi-model assessment of last interglacial temperatures, Clim. Past, 9, 699–717, https://doi.org/10.5194/cp-9-699-2013, 2013.

Plach, A., Nisancioglu, K. H., Le clec'h, S., Born, A., Langebroek, P. M., Guo, C., Imhof, M., and Stocker, T. F.: Eemian Greenland Surface Mass Balance strongly sensitive to SMB model choice, Clim. Past Discussions, pp. 1–37, https://doi.org/10.5194/cp-2018-81, 2018.

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.

Thanks, we will add more explanation and clarify the description of the SMB methods and the basal friction in the revised manuscript. The basal friction coefficient is spatially varying but constant over time in all experiments (in both the HO and SSA experiments). The following sentence in the manuscript (p. 4, l. 11-12) is a bit misleading and will be revised: "...i.e., we exclude basal friction coefficients which lead to unrealistic elevation changes at the deep ice core locations." We performed all HO experiments also with SSA. In addition, with SSA we performed additional experiments with more extreme basal friction coefficients. The most extreme basal friction coefficient values were excluded for the HO experiments as they lead to unrealistic elevation changes (in the SSA experiments).

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.

We agree that the surface temperature is important for spin-ups and similarly long simulations. However, we chose to keep the surface temperature constant for simplicity as we think it has only minor impacts on our simulations. The evolving surface temperature has two

impacts: 1) Influencing the SMB if a temperature dependent SMB scheme like PDD is used. Which is not the case in our setup; we use a SMB which is evolving over time (plus the SMB gradient method is applied). 2) The surface temperature affects the thermal structure of the ice sheet. However, our simulations run for 12 kyr (which takes around 3-4 weeks), and ice that is newly formed during this period will not reach deeper than a few hundred meters, i.e., this newly formed ice will not reach regions near the base where the largest deformation happens. We therefore think it is unlikely that the newly formed ice will have a large influence on the ice dynamical response.
We will add a clarifying sentence or paragraph in the method section.

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).

We agree that it would be good to include bedrock adjustment. However, ISSM does not include a GIA model (in its transient simulation mode) at the moment, because not many people have used ISSM for paleoclimate simulations until now. We will add a discussion on the missing bedrock uplift which will likely reduce the total ice loss (especially in the MAR-BESSI experiments).

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.

We performed all HO experiments beforehand with SSA. We also performed additional simulations with SSA to find the appropriate parameter values for our HO sensitivity experiments. However, we chose not to discuss the SSA experiments in detail because SSA does not appear to us to be an appropriate approximation for the entire ice sheet and SSA alone was also never used in paleo ice sheet simulations (only SIA alone or a combination of SIA and SSA). Furthermore, when using SSA alone, ISSM is probably not the best tool because it is more computational expensive than other SSA ice sheet models (among other things because it uses the finite element method rather than the finite difference method as many other SIA/SSA ice sheet models). The idea behind using ISSM was to test the more advanced higher-order setup for paleo applications in a semi-realistic setup.
We will clarify that we are using a semi-realistic setup (missing GIA, non-evolving surface temperature, no ocean forcing, ice sheet domain,…) and we will clarify how many and which additional SSA experiments we performed in the revised manuscript.

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.

Thanks, as mentioned further above, we will add additional information about the two SMB models in the methods section and also discuss their differences shortly in the discussion section. However, it will be challenging to provide a “most realistic” model. Although we chose SEB as a reference in our previous paper, shortly said because it uses the most comprehensive physics, in a paleo application with large uncertainties of the climate forcing, it is challenging to clearly pick one model over the other.

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.

We will clarify that we understand our manuscript as a sensitivity study rather than one aiming to provide a precise estimate of the Eemian GrIS. The study focuses on impacts of SMB vs. higher-order/basal stress and we will clarify this in the discussion.

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).

Thanks, we will add panel numbers to these figures.

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.

Thanks, we will edit figures 1 and 2 following your suggestions.

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).

This is a SMB feature specific to BESSI. The BESSI SMB is positive in some regions in the southwest in all SMB simulations, also the 130 and 125 ka simulations. At this point it is unclear to the authors what exactly causes this spatial feature. We will further investigate and discuss this issue in the revised version.

Figure 5
I suggest to put the y-axis in meters. The grey lines are not so well visible, would use a colour (blue?).

We will try a different color for the grid in the background. However, concerning the y-axis, we chose to use km to be able to make the panels a large as possible.

General comments

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

We thank Bas de Boer again for the overall positive evaluation of our manuscript and his thorough comments which will improve our manuscript significantly! The comments directly in the pdf are very useful, and will be implemented into the revised manuscript.