

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

## **Anonymous Referee #2**

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### — Overview —

In this paper, the authors present a series of transient model simulations of the Eemian Greenland ice sheet (from 127 to 115 ka) where they compare the impact of a) discrepancies between external surface mass balance (SMB) forcings, b) perturbations to the assumed basal friction conditions, and c) two different ice flow model approximations. Most of these simulations are performed using both a recent version of a higher-order ice flow model and a simpler version of it that employs a faster shallow shelf approximation (SSA). The setup uses basal friction conditions obtained by an inversion from observed (modern) surface velocities, and leaves out some processes

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such as, e.g., glacial isostatic adjustment, ice-ocean interactions, and thermal spin-ups due to the computational expenses of the higher-order model. Based upon the results, the authors draw the conclusion that—for simulations of the Greenland ice sheet during warm periods—the representation of surface mass balance is more important than the representation of ice flow and that efforts should focus on improving the former rather than the latter.

Thus, the title of the paper is, in my opinion, well suited and highlights the interesting use of a higher-order ice sheet model on paleo-climatic setups, which so far have been dominated by models with simpler ice flow approximations. Therefore I think that this study is well within the scope of TC. The abstract is adequate. The presentation of the paper (structure, language, etc.) is very good, the text is easy to follow, experimental steps are for the most part clear (see my comments below), references are in general good, and the existing figures complement the text well (although I have some requests). I support this manuscript, providing that the authors address the minor issues described in the general and specific comments below.

#### — General Comments —

Although I think that the conclusions do indeed follow the results, my main concern is that those conclusions could be limited by the methodology used to arrive at those results (although not necessarily incorrect). In other words, I am left with the impression that the ice flow representation (including basal friction) could have a stronger impact if the above-mentioned compromises due to computational expenses were not present. As an example, the lack of a spin-up implies that the initial conditions do not include thermal or isostatic rebound signals representing past ice sheet states. Related to this, those past ice sheets configurations can be dependent on the degree of complexity of the ice flow approximation used, particularly if the ice sheet's advances and retreats involve interaction with the ocean. At the end, the initial conditions could be very different depending on the ice flow representation used during a spin-up, and these impacts can be amplified by other model components such as glacial isostatic rebound. Con-

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necting to this, I also wonder if the inclusion of rebound (and its interaction with the SMB altitude-feedback) would decrease the contribution to the ice thickness change from the differences in surface mass balance.

Furthermore, I think that the "parameter space" explored for the ice flow-related experiments is not enough to showcase the total range of impact, especially for the basal friction experiments. For example, the authors test a relative small change to these coefficients and an extreme change, and then discard the latter due to unreasonable (and preliminary, since these are not shown) results. However, there are no attempts at testing the impact of less extreme changes with the aim of finding a "maximum impact" that is still reasonable.

To sum up, from my experience I mostly agree that getting the surface mass balance right plays consistently a crucial role on the evolution of a modelled ice sheet (even outside of interglacials or land-based ice sheets), but from the paper it is not completely clear that the significantly smaller impact of ice flow-related processes is not (at least in part) a consequence of the limitations required in order to utilize the sophisticated higher-order ice flow model. If additional experiments using the higher-order model are not possible due to computational constraints, there is (almost) always the possibility to run those experiments in the simpler SSA model, especially since the authors do not find extreme differences between the models. This could be done at least as supplementary materials to shed some light on the mentioned issues. I will elaborate on these issues as they occur in the text below.

— Specific Comments —

Page (P) 2, Line (L) 20-23: These lines give the misleading impression that the SIA and SSA are used separately in hybrid models, with marked boundaries between the regions where each of them is applied. As far as I understand, there is a difference between using the SIA and SSA separately for, e.g., grounded and floating ice, respectively (i.e. with the grounding line as the "boundaries between these two approxima-

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tions", as in the main experiments of Pollard and De Conto, 2009), and using what is currently known as "hybrid model". In fact, one of the main motivations stated in Bueler and Brown (2009) was to overcome the flux and velocity problems where SIA and SSA meet, when applied to model grounded ice streams, and to provide a scheme that generates well-behaved, "continuous" intermediate states. Modern hybrid models (mostly following Bueler and Brown, 2009) usually combine both approximations in various ways to obtain a smooth transition between SIA dominated and SSA dominated regions. Please reformulate.

P 3, L 9: In "best guess Eemian SMB simulations", "guess" sounds a bit out of place / redundant. Also, based on what are these simulations the best? A few words here giving the criteria used and the types of SMB models tested in Plach et al. (2018) would be appreciated.

P 3, L 13: Since the SMB is computed using modern topography and the altitude-SMB feedback turns out to be quite important (as shown by your results), I wonder if the gap between the control and corresponding MAR-BESSI experiments would be smaller under a different topography. For example, (looking at both 125 ka panels of Figure 3) if you started the simulations from a lowered and/or retreated ice sheet in the north-east this could potentially trigger a positive feedback that turns the MAR-SEB SMB negative, causing a similar retreat to that in the MAR-BESSI experiment. You do mention something similar in page 11, line 10, but I would like to see an additional experiment testing this, or (if this is definitely not feasible) at least a discussion on this possibility in the manuscript, since I think it is within the scope of this study.

P 3, L 17: "... lower threshold of 100 points ..." points of what? Although I appreciate a summary of previously published methods, it seems to me that this description utilizes terms assuming that the reader is already familiar with those same methods. Please reformulate so the description can be read independently from the cited paper.

P 4, L 5: What is the (real) time required to run each of these 12 kyr simulations on your

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higher-order configuration? Since using a higher-order model is part of the novelty of this study, this could be an interesting detail to some readers.

P 4, L 8: "with a modern GrIS" What is the internal thermal structure of the ice sheet at the beginning of the experiments? How is it computed? Please specify.

P 4, L 9-12: This is not clear to me. How are these limits for the friction coefficients computed? Do you simply prescribe different (homogeneous/spatially varying?) coefficients until the resulting elevations change "too much" compared to ice core data? Is this done before any inversion? If so, and considering that the inversion will produce spatially varying coefficients, why would these limits be valid for the entire domain? Please clarify.

P 4, L 12-14: Do you run two independent inversions to derive the basal friction coefficients for the higher-order and SSA setups? Under what internal and boundary conditions is this inversion performed? Please clarify and elaborate a bit more on this procedure. Also, I would like to see a figure with the inverted distribution(s) of these coefficients, since the perturbation of these coefficients is an integral part of the study.

P 4, L 25: "the ice sheet domain remains fixed throughout all simulations" This line is confusing at first, since it gives the impression that the ice sheet area is fixed (i.e. cannot retreat or advance). Then it is clarified that it cannot advance "beyond the (modern) ice domain", although it is not clear if it can retreat. Only in page 8, lines 12-14 it is clear that it can retreat and re-advance. Please reformulate so this is clear from the beginning.

P 4, L 27-29: Why is the surface temperature prescribed and kept fixed at pre-industrial values? Is this simplification necessary? I would like to see a confirmation that its influence on the thermal structure is negligible, particularly in the regions where the ice sheet elevation decreases significantly.

P 5, L 16: Did you consider other velocity thresholds during initial testing? What is the

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reason for this particular value of 500 m/yr? Please elaborate.

P 6, L 3-6: Did you test other values for the multiplying factors (i.e. between 0.5-0.9 and 1.1-2.0) over the entire ice sheet? While halving/doubling the value of the friction coefficients might indeed give unrealistic results, it would be interesting to see the rate of change of the ice volume curves (as in Fig. 1) for more modest increments (e.g. 0.7 and 1.5). Also, I think it might be useful to show anyway the curves for 0.5 and 2.0 (at least as a supplement), just to see how they relate to the rest of the results.

P 7, L 4-7: According to the text, the outlet glaciers are defined as the areas with ice (surface?) velocities  $> 500$  m/yr. The resulting "outlet regions" are shown in Fig. 8 and 9. How much would the resulting outlet regions change if a lower threshold was used? Would the bigger area-of-effect impact the results/conclusions? Additional tests here are welcome. Also, it seems that these outlet regions are defined at the beginning and not updated over time as the ice sheet retreats and new areas reach the required threshold. Would a continuous identification of outlet regions change the results as well? I would like to see these points addressed in the discussion.

P 9, L 4-5: How do these 600 m and 1500 m of elevation change compare to the changes due to halving/doubling the friction coefficients? Is the impact of the latter even stronger than, e.g., 1500 m? Connected to a previous specific comment (P6,L3-6), I think it would be useful to see the results of those particular discarded experiments.

P 11, L 11-16: There is a build-up of ice in the northeast margin (and most outlets) in the "basal\*0.9" experiment, whereas there is a local thinning of the outlets in the outlets\*0.5 experiment. Do you think that the latter would be able to compensate for the additional influx from the interior in a hypothetical experiment that uses \*0.5 at the outlets and \*0.9 elsewhere? This could cause a bigger impact than the cited experiments, while still keeping the assumptions in the manuscript. If possible, please test this. I would like to see this possibility addressed in the discussion.

P 12, L 2-3: "Note that the thinning affects ice thickness upstream from the outlet

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region". Yes, and this connects to my previous points regarding basal friction: I think it is very possible to maximize the impact of basal friction uncertainty if other choices are made, e.g., lower threshold for outlet identification (thus increasing the area-of-effect), lower multipliers for the coefficients, different multipliers for interior and outlets, etc. I think it would be useful to see the outcome of such choices at the "extremes" of reasonable assumptions in additional experiments.

P 12, L 5-8: Fig. 9 (right) does not seem to clearly support the text, as the changes in the velocity field seem far from local, even reaching close to the divides. In other basal friction configurations (see my previous point P12,L2-3), these changes could have significant effects. Please discuss.

P 13, L 3: While the sentence is technically correct, I would add "among our tests" (or similar) after "gives the biggest difference in the simulated Eemian ice sheet evolution", to acknowledge the possibility of different results under conditions or configurations not tested here.

P 13, L 9-14: Following my previous points (e.g. P12,L2-3), I would like to see here a discussion on other basal friction configurations that could potentially have a larger impact on the Eemian ice sheet volume, addressing those points.

P 14, L 6: "develop a new equilibrium ice sheet" The ice sheet configurations at the end of these transient experiments are not in equilibrium, at least not in the usual sense (e.g. steady-state simulations under non-varying climate conditions). Please replace using "a new ice sheet state" or similar.

P 14, L 11-12: Following my previous point (P3,L13), I would like to see here a discussion on the potential impacts of a smaller initial ice sheet and its interaction with the altitude-SMB feedback.

P 16, L 4-5: This sentence highlights my main concern described above. On the one hand, the higher-order model is too expensive to perform additional experiments. On

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the other hand, the SSA model is described here as unable to run those simulations due to its limitations. Connecting to my previous comment (P2,L20-23), it seems that there are other options you could use to assess the impact of these important processes. Since one of your conclusions mentions a "limited influence of the ice flow approximation on the simulated minimum ice volume", would not a, e.g., hybrid model be a good candidate to (at least) clarify these issues?

P 16, L 10-12: These lines suggest that the inversion of friction coefficients is done separately for the higher-order and SSA models (see my previous point P4,L12-14). Please clarify this in Section 2.2.

P 16, L 16-17: Would the impact of including basal hydrology on the computational expenses be large enough to make your experiments unfeasible? Has this been tested?

P 16, L 19-21: In connection with my previous point (P7,L4-7), if the goal is to compensate to some degree for the lack of basal hydrology, I think that a continuous identification of outlet regions would be a better choice; otherwise, you are accounting for it only where the initial conditions do not change. Please address the possibility of other basal friction / outlet regions configurations that could have a stronger impact on the results.

P 16, L 26-27: Similar to how the outlet experiments attempt to account for basal hydrology, additional experiments with a reduced initial ice sheet could attempt to account for the influence of pre-Eemian ocean forcing (see also my previous point P3,L13)

P 16, L 31-32: "Furthermore, a spin-up would require ..." It is still unclear to me what are the internal ice sheet temperatures at the beginning of the experiments, but what if the spin-up is performed with a fixed topography, i.e., letting ice velocities, temperatures, etc. evolve under a transient climate signal? Would not that be more realistic than a modern temperature profile or no profile at all? Please clarify.

P 16, L 33-34: This is not clear to me. If the mesh cannot easily adapt to changes

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in topography, what happens with the mesh in, e.g., the MAR-BESSI experiment at 125 ka where the ice margins have retreated? Do you simply use the initial (low?) resolution mesh there? If so, does this low-res mesh affect the results (e.g. enhancing the retreat)? Please clarify this.

P 17, L 1-3: I am not 100% convinced that the uncertainties in the initial conditions are completely outside the scope of your study (see P3,L13; P14,L11-12; P16,L26-27). In any case, I would replace "will be attempted" with something like "will become feasible", so it sounds like a possibility rather than a promise.

P 17, L 5-6: I would like to read an interpretation after this observation. Although this result does not necessarily mean that MAR-BESSI is better than the control SMB, it could point to systematic biases in the modelling setup that cause the former (and the latter) to underestimate its contribution to sea level rise.

P 18, L 5-10: The conclusions do not mention the impact of basal friction conditions, which is a significant part of the study. Please summarize these results as well, keeping in mind the potentially stronger impact mentioned in many of my previous points.

P 18, L 10-12: I think that the strength of this conclusion contrast (at least in magnitude) with the general conclusions the authors draw elsewhere (e.g. in lines 2 and 3 of this same page). Again, I agree that getting the SMB right is important, but do not think that reducing the focus on the representation of the ice flow (and here I include basal and internal processes too) is an appropriate call here. After all, a better (and more efficient) representation of ice flow could eventually allow the inclusion of the very same processes neglected in this study. Please reformulate.

— Technical corrections —

P 3, L 6: Here I am missing a core reference for MAR and the setup used.

P 3, L 25: Is there a reference that documents ISSM version 4.13? I suggest to merge this detail with the sentence in P 3, L 31: "... higher-order configuration of ISSM ver

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4.13 is used (Cuzzone et al, 2018)".

P 7, L 3: change -> changed

P 9, L 6: in simulated ice surface between -> in the simulated ice surface elevations between

P 10, L 2: within the reconstructed surface elevation change -> within the uncertainty of the surface elevation change reconstructed from total gas content

P 10, L 5: with ~2.5 m difference -> with a difference in sea level rise of ~2.5 m

P 12, L 7: 0.5 \* -> outlets\*0.5

P 16, L 30: is allows -> it allows

— Papers mentioned —

Bueler and Brown: Shallow shelf approximation as a "sliding law" in a thermomechanically coupled ice sheet model, *Journal of Geophysical Research: Earth Surface*, 114, F03 008, <https://doi.org/10.1029/2008JF001179>, 2009.

Pollard and DeConto: Modelling West Antarctic ice sheet growth and collapse through the past five million years, *Nature*, 458, 329–332, <https://doi.org/10.1038/nature07809>, 2009.

Plach, Nisancioglu, Le clec'h, Born, Langebroek, Guo, Imhof, and Stocker: 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.

Cuzzone, Morlighem, Larour, Schlegel, and Seroussi: Implementation of higher-order vertical finite elements in ISSM v4.13 for improved ice sheet flow modeling over paleoclimate timescales, *Geosci. Model Dev.*, 11, 1683-1694, <https://doi.org/10.5194/gmd-11-1683-2018>, 2018.

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