We would very much like to thank the anonymous referee #2 for their constructive comments. Please find below the referee's comments in black font and the author's responses in blue font.

Responses to Anonymous Referee #2

— 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 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.

We thank you for your overall positive evaluation of our study and hope that we address your comments in the following paragraphs to your satisfaction.

— 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. Connecting 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.

Thanks, we agree that the missing isostatic rebound will most likely lower the total ice loss, particularly in the MAR BESSI experiments. We will emphasize this fact in the discussion section. Furthermore, we will clarify that we are using a semi-realistic setup (missing GIA, non-evolving surface temperature, no ocean forcing, ice sheet domain,…).

Concerning the "parameter space" issue you mention. We performed additional SSA experiments which we only mentioned shortly in the discussion paper. We will clarify how many and which additional SSA experiments (including also values in between the presented range) we performed in the revised manuscript and discuss their results.

— 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 approximations", 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.

Thanks, we will revise this with appropriate references.

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.

Thanks, we will add more details about the differences between the two SMB models and why they were chosen in the method section. See also reply to reviewer #1 who requested more details from the SMB simulations in Plach et al. (2018). We will also rephrase to avoid the formulation "best guess".

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.

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.

Thanks, we will address this important issue in the discussion section of the revised version. However, we are afraid that additional MAR-SEB simulations with a lowered ice sheet are indeed unfeasible due to the high computational demands of the regional climate model MAR. Furthermore, such a lowered ice sheet would be chosen rather arbitrarily, because Greenland ice cores do not constrain the GrIS geometry before 130 ka. As a result of the ill-constrained geometry, we would need to perform several sensitivity experiments with various geometries and we think this is beyond the scope of this paper. However, we will discuss the differences in our simplified *relaxed* experiment further, which was performed with a different initial ice sheet.

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.

Thanks, we will reformulate the description. The "100 points" is referring to 100 grid points. The methods looks at grid points in a radius of 150 km around the grid point for which the SMB gradient is calculated. For each grid point the method calculates an ablation and an accumulation gradient. During the calculation of the ablation gradient, if there are less than 100 grid points with a negative SMB, then the radius is extended. Similar for the calculation of the accumulation gradient.

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

One simulation takes about 3-4 weeks on a single node with 16 cores. We will add this information to the text.

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.

We prescribe pre-industrial temperatures at the surface (from the MAR climate simulations) and at the base we use the enthalpy formulation after Aschwanden et al., 2012 to determine the basal boundary conditions (cold or temperate ice) using a prescribed geothermal heat flux (from Shapiro and Ritzwoller, 2004). A thermal steady state calculation is performed, employing a heat transport equation to get the internal thermal structure. We will update the manuscript with additional details.

Aschwanden, A., Bueler, E., Khroulev, C., and Blatter, H.: An enthalpy formulation for glaciers and ice sheets. J. Glaciol., 58(209):441--457, 2012.

Shapiro, N. and Ritzwoller, M.: Inferring surface heat flux distributions guided by a global seismic model: particular application to Antarctica, Earth and Planetary Science Letters, 223, 213–224, https://doi.org/10.1016/j.epsl.2004.04.011, 2004.

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.

We perform an initial inversion for the basal friction coefficient and then we multiply this inverted basal friction (which is spatially varying) by factors of 0.5, 0.9, 1.1, and 2.0 (for the >500m/a sections) and 0.9, and 1.1 (for the entire ice sheet). The limits are referring to these multiplication factors and the basal friction coefficient is always spatially varying. We will clarify this point in the revised text. Furthermore, we will also discuss SSA experiments we performed with factors of 0.8 and 1.2, respectively.

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.

Yes, we perform independent inversions for the higher-order and SSA setup. The inversion chooses different basal friction coefficients because the stress balance is represented differently in the higher-order and the SSA setup. For the inversion to infer the basal friction

coefficient, the ice viscosity is prescribed. After this thermal steady-state simulation the ice viscosity is updated according as a function of the newly calculated thermal profile (Cuffey and Paterson, 2010). The basal friction coefficients are then iterated to minimize three cost functions (Table 1 in the manuscript) --- absolute misfit between the modeled and observed velocity fields, logarithmic misfit between the modeled and observed velocity fields, and absolute gradient of the basal drag. We will add more detail on this in the revised manuscript and add a citation for the inversion details.
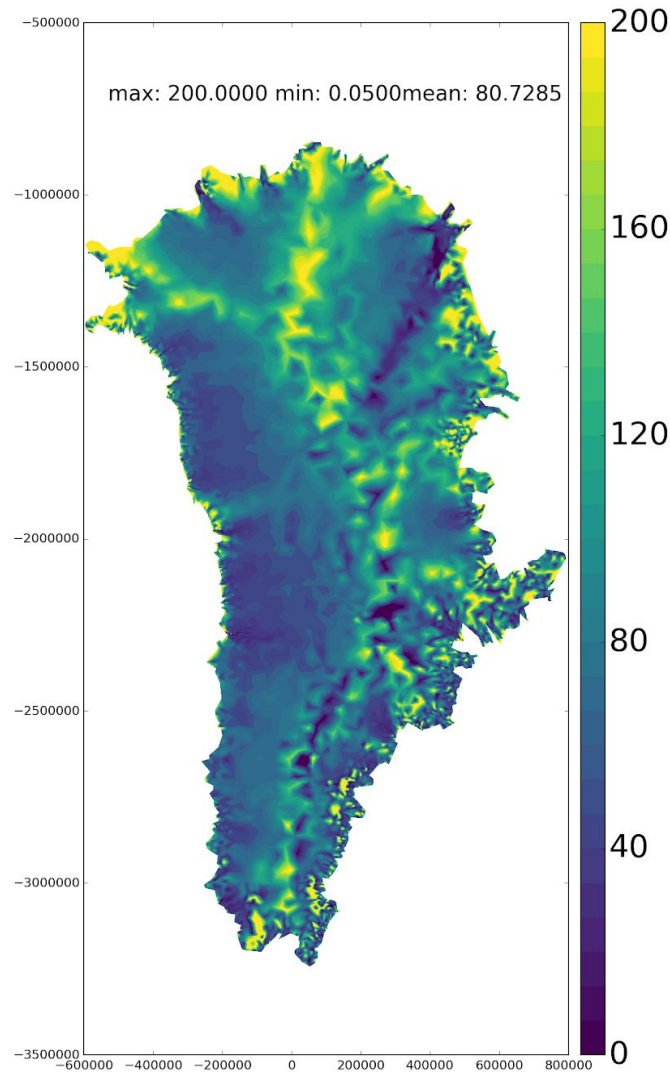
Figure 1: Distribution of basal friction coefficient for the 3D higher-order experiments after inversion from observed present-day surface velocities (no factors applied). The higher the value, the higher the friction.

Cuffey, K. M. and Paterson, W.: The Physics of Glaciers, Elsevier Science, Burlington, 4th edn., 2010.

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.

Thanks, we will rephrase this.

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.

Although we agree that the surface temperature is very important for the thermal structure during a spin-up over a glacial cycle. We chose to do keep the surface temperature constant for simplicity as we think it has only minor impacts on our simulations. 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. Please also see a similar remark of reviewer #1 (remark 3, third point). We will further discuss this shortcoming in the discussion and emphasize that we are using semi-realistic experiments.

P 5, L 16: Did you consider other velocity thresholds during initial testing? What is the reason for this particular value of 500 m/yr? Please elaborate.

The threshold of 500 m/yr was chosen to include all major outlet glaciers, large parts of ice streams or other fast flowing ice (See contours in Fig. 8c). We did not test other thresholds.

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.

With the SSA setup we also tested factors of 0.8 and 1.2 for the outlet glaciers as well as for the entire ice sheet. We will add additional details about these SSA experiments in the revised manuscript. Please, also note our respond to reviewer #1 (remark 4).

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.

Thanks, we will add a discussion of this issues in the discussion section.

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.

At the EGRIP location more extreme changes of the basal friction coefficient do not show a stronger effect since the lowering at this location is already substantial and only a thin layer of ice remains. At the more central locations at NEEM, NGRIP, and GRIP halving the friction shows a surface lowering of approx. 700, 900, and 600 m, respectively.
We will add a discussion of the additional SSA experiments we performed (and excluded so far) and their results in the discussion section.

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.

Thanks, this is an interesting idea. We will add a discussion about such a constellation in the revised manuscript.

P 12, L 2-3: "Note that the thinning affects ice thickness upstream from the outlet 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 an outlets, etc. I think it would be useful to see the outcome of such choices at the "extremes" of reasonable assumptions in additional experiments.

Thanks, we will discuss further SSA experiments we did in the revised version including the more extreme examples we excluded in the discussion so far.

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 the the divides. In other basal friction configurations (see my previous point P12,L2-3), these changes could have significant effects. Please discuss.

Thanks, you are right, there are also changes close to the ice divide. However, they are very small, in the range of 1 m/yr. We will rephrase the revised version to reflect your concerns.

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.

Thanks, we agree and will reformulate accordingly.

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.

We will make sure to discuss this in the revised version.

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.

You are right. We will change this accordingly.

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.

We will address this issue in the revised discussion (also see our response to P 3, L 13).

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 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?

We agree that a future study with more sensitivity experiments could indeed profit from a more cost-efficient hybrid model. However, here we wanted to focus on the impact of SMB vs. ice dynamics represented in the most realistic way feasible on millennial-scale simulations. Furthermore, we will emphasize the fact that we are using a semi-realistic setup and that our goal is not to provide the most accurate estimate of the Eemian GrIS in the revised manuscript.

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.

This is correct. The inversion is performed separately for higher-order and SSA. We will clarify 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?

At the moment basal hydrology is not implemented in ISSM. However, the computational demands of colleagues in our institute working on the development of advanced basal hydrology schemes, are large, so that we came to the conclusion that an implementation within our experimental setup is unfeasible at the moment.

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.

We agree that a continuous identification of the outlet regions would be a better choice. However, this would be technically very challenging with our current ISSM setup. The primary objective of our study was to provide a relative perspective on uncertainty in the SMB by showing it side by side with idealized variations in basal friction that cover a realistic range. It is not meant to be an exhaustive analysis of the latter.

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.

For the initial thermal structure please note our response to P 4, L 8. Please also note our response to a similar issue of reviewer #1 (remark 3). A spin-up experiment would very much dependent on the used pre-130ka climate (SMB, temperature, ocean forcing,...) which is not well constrained (e.g. by Greenland ice cores). Furthermore, our climate simulations only cover the period 130 to 115 ka. We therefore think that initial sensitivity experiments would add many additional simulation choices (i.e., more unknowns) which would require many additional experiments which is unfeasible with our model setup.

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

This is a challenging issue, which is also related to your comment on the identification of the outlet regions. We use the same mesh at all times and it would be desirable to adapt the mesh over time, and the outlet regions as well. However, this is not implemented in ISSM at the moment. We acknowledge this important issue and will address it in the discussion section of the revised manuscript.

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.

We will rephrase with "will become feasible". We think evaluating the uncertainties in the initial conditions would require many more experiments, which are unfeasible with our current setup. We rather want to focus on the difference impacts of SMB vs. higher-order ice dynamics.

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.

Thanks, this is an interesting point which we will discuss further in the revised manuscript.

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.

We will add the basal friction conditions to the conclusions as well.

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.

Thanks, we will rephrase this section and in general emphasize more that we are using a semi-realistic setup in the revised manuscript.

— 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 ver4.13 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

Thank you for these technical corrections.

— 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.

We thank the Anonymous Referee #2 again for the overall positive evaluation of our manuscript and his comments which will help to improve our manuscript.