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Interactive comment on "The impact of model resolution on the simulated Holocene retreat of the Southwestern Greenland Ice Sheet using the Ice Sheet System Model (ISSM)" *by* Joshua K. Cuzzone et al.

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As more computing time becomes available for ice sheet modelling, applications to long-term ice dynamics can afford higher and higher horizontal resolutions, so that an important question will need to be solved: how high is high enough? J. K. Cuzzone et al. present an application of the higher-order ice sheet model ISSM to the late deglaciation dynamics of the southwestern Greenland Ice Sheet margin. The authors present the results of a sensitivity test to model horizontal resolution. The model shows a different response to increased horizontal resolution in areas of different topographic

complexity, therefore bringing elements of answer to the aforementioned question.

The author's choice of model and study area make sense. While still an approximation of "full-Stokes" ice flow physics, the higer-order physics embedded in ISSM allows to escape some fundamental limitations associated with high-resolution applications of the Shallow ice approximation. Besides, applying ISSM to a paleo-glaciological context provides not only long-term (geological) validation data but, even more importantly, topographic data of much higher quality than is available under current ice sheets. The manuscript is logically organized and very well written.

I strongly support publication of these results, but provide below a list of comments (the first perhaps a bit far-fetched, the others mostly very specific) which the authors may use to complete the presentation of their model set-up and the discussion of their results.

1 General comments

Which resolution is high enough?

Although the high-resolution runs yield increased performance in regions of complex fjord topography, discrepancies with geological data remain. A potential explanation for these discrepancies is the lack of marine ice sheet processes such as calving and submarine melt in the model. However, they could also imply that even the high resolution of (up to) 2 km is not yet high enough. Do the model results allow to decipher between these two potential sources of error? Could you comment on this in your conclusions?

Visualization of geological data

The southwestern Greenland Ice Sheet margin is introduced as an area where the deglaciation history is well documented by the geological record. While differences between the model results and this record are discussed, no visualization of the geological data is presented in the manuscript. If geological data is available, I think the manuscript would gain much by adding some of it in the figures.

Repetitions in the discussion of model results

I feel that parts of the results and discussion sections are repetitive, and that the manuscript could gain in clarity by minimizing these repetitions. Perhaps additional subsections in the discussion would help.

2 Specific comments

p. 1, l. 22: and one non-uniform

I suggest to replace with "and one using a non-uniform".

p. 1, l. 28: simulate unrealistic retreat

You could better highlight the main result in the abstract. Is the retreat unrealistically fast or too slow?

p. 1, l. 32: the SMB drives retreat

I suggest "the SMB predominantly drives retreat".

p. 3, l. 11–12: not well tested, however, is the sensitivity of simulated ice retreat to the ice flow dynamics model [...] and to model resolution

Such a sensitivity was conducted by Zekollari et al. (2017, Fig. 7). Comparative studies on multi-millenial time scales have also been published by Bernales et al. (2017); van Dongen et al. (2018).

p. 4, l. 2–3: model resolution is a constraint that is typically not explored when studying the past

I don't think this is correct. Although the results do not always appear in publication, I assume most modellers explore sensitivity to horizontal resolution. Some results are displayed for instance in Golledge et al. (2012, Fig. 6), Seguinot et al. (2016, Fig. 5), and Zekollari et al. (2017, Fig. 7).

p. 4, l. 8: We use the Ice Sheet System Model (ISSM)

For reproducibility, could you add a version number here?

p. 4, l. 10: this ice flow approximation is typically not used

Again you could refer to Zekollari et al. (2017) here.

p. 4, l. 28–30: coarse-resolution models do a poor job of capturing the complexities of the underlying topography (Aschwanden et al., 2016).

Although not exactly a paleo-ice sheet modelling study, I think it would be fair to mention here the grid sequencing approach used in spin-up by Aschwanden et al. (2016).

p. 5, l. 2: areas of smooth bed topography (primarily over the interior of the domain)

What do you mean with interior? Is this the area presently covered by ice and could the smooth bed topography be an artifact of the lack of data?

p. 5, l. 5: the GMIP DEM

I think this is "the GIMP DEM" (Greenland Ice Mapping Project).

p. 5, l. 6: In Figure 2

The order of figures 1 and 2 is inconsistent with the text.

p. 5, l. 10: Nuuk and Jakobshavn

These place names appear several times in the text. Could you please add them to at least one of the maps?

p. 5, l. 15–16: We use the positive degree day method outlined in Tarasov and Peltier (1999) to construct the necessary accumulation and ablation history

Could you please detail more precisely how your approach relates to theirs? The positive degree day method is used to compute ablation. The PDD method described in Tarasov and Peltier (1999) has been established in older references (see the first paragraph of Seguinot, 2013 for a concise review).

On the other hand, the accumulation scheme described by Tarasov and Peltier (1999) is more elaborate than most, but it seems not exactly consistent with the description of your accumulation model further in this paragraph. Am I correct?

p. 5, l. 25: a purely thermodynamic relationship as precipitation rate changes 7.3% for every 1°C

This formulation is unprecise, because it implies a different exponential factor depending if you consider a negative or positive change (92.7% is not the inverse of 107.3%). I would suggest an equation here or at least a reference. What do you mean with "a purely thermodynamic relationship"?

p. 5, l. 27: with allocation for superimposed ice

Do you allow for refreezing of melted snow/ice? If so, how is this parametrized, and is the surface mass balance model ran on yearly or sub-yearly time intervals?

p. 5, l. 29–30: elevation-dependent desertification is included

Presumably the temperature lapse-rate combined with temperature-dependent precipitation reductions implies elevation-dependent precipitation reductions. Is this new mechanism applied on top or independently?

p. 6, l. 15: the spatially varying basal drag coefficient (k) in equation 2 is derived using inverse methods

Is this inversion performed independently for each horizontal resolution? From which resolution and how is the basal drag map otherwise interpolated?

p. 6, l. 25–26: a spatially varying temperature dependent scaling parameter (λ) as a function of time.

Could you please comment on the physical basis for this relation? Basal sliding is typically related to subglacial water pressure, while cold-based ice is theoretically assumed non-sliding irrelevant of its temperature. Does warming of cold-based ice below pressure melting induce unrealistic basal weakening in the model? Could this be the reason why a cap value is needed for λ, and is the scaling really an improvement over a constant friction map?

In the discussion section, could you add a few sentences to describe how the temperature evolves through the deglaciation simulations and how this affects the results?

p. 7, l. 26: For the regional model, we initialize the model with present day geometry

Could you please explain why the regional model is initialized in steady-state while transient boundary conditions from the global model are available? I wonder if this imply an inconsistence between the regional and global model initial states, where the global model's ice "remembers" the Last Glacial Maximum (temperature) whereas the regional model's ice do not.

p. 7, l. 29: the ice margin over southwestern Greenland was near or at the present-day coastline

Could you please detail what makes this condition an interesting starting point?

p. 8, l. 1–2: While grounding line migration is simulated in these experiments, calving and submarine melting of floating ice is not included.

How does floating ice then leaves the model? Is surface melting enough to constrain the ice shelves to somewhat reasonable size?

p. 8, l. 26: increasing ice volume with decreasing model resolution

Shouldn't this be "decreasing ice volume"? Low resolution yields low ice volume.

p. 9, l. 21: details in the ice margin similar in scale and sinuosity to the mapped ice moraines

A visualization of the mapped moraines would be very useful here.

p. 12, l. 12–14: small-scale marginal fluctuations are likely responsible for the moraine record and seem to be related to high-frequency variability in temperature

Are the fluctuations of the ice margin / volume in the model in phase with input temperature? Perhaps this could be visualized by plotting the input temperature on Fig. 6?

p. 12, l. 24: None of our experiments match the geologic observations.

Is it possible to say from your results, if this mismatch is due to physical processes (e.g. calving, submarine melt) missing from the model, or (still) too coarse resolution?

p. 12, l. 29–31: the ability of the high-resolution models to capture [...] is well captured.

There is one "capture" too many in this sentence.

p. 14, l. 8: Lecavalier et al. 2014)

The opening bracket is missing.

Fig. 1:

The caption is not visible in the manuscript.

Fig. 2:

For those non familiar with unstructured meshes (like me), would it be possible to display the mesh used in each experiment (esp. experiment A), or is it too dense to show? For visualization puposes I would also recommend to use the same inline arrangement of figure panels as in Fig. 3 and 6.

Fig. 3:

To save space I think you could remove the observed velocities (already shown on Fig. 1, and too similar to those modelled using the non-uniform mesh for a direct comparison).

Fig. 6:

I think there is a problem with the labelling of the x-axis. The unit “yrs BP” corresponds to an age which should be a positive number. Either remove the minus signs and replace “time” by “age”, or remove the BP (similary on Fig. 8 and 9).

Is it possible to display the input temperature forcing alongside the model output? I think this would help one understand the short-term fluctuations in ice volume as well as the longer-term deglaciation dynamics.

Besides, this might be a personal preference but I find the min-max normalization not so informative. I understand that the different runs start with different ice volumes because of their resolutions, but I find it difficult to know what I am looking at after normalization. I would personally omit panel B and instead combine panel A with Fig. 4. Then, one quickly understands the different initial ice volumes.

Fig. 7:

I do not understand the choice of separate panels A and B with (only a small) area missing in between. It would be more visual to combine them. If space is to be saved, more ocean and modern ice area can be cropped.

Is geological data on the mapped moraine available to be plotted alongside the model results?

Congratulations again on your work and I hope you will find my comments useful in bringing your manuscript to final form.

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