The Cryosphere
Discussions

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Interactive comment on "Sensitivity of Greenland ice sheet projections to spatial resolution in higher-order simulations: the AWI contribution to ISMIP6-Greenland using ISSM" *by* Martin Rückamp et al.

Anonymous Referee #2

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This paper uses the ISSM model and the ISMIP6 protocol in order to investigate the importance of resolving the ice dynamics and the bed topography in Greenland ice sheet simulations. It does so by forcing ISSM using ocean-only retreat, SMB-only perturbations, and the combination of both ocean retreat and SMB perturbations. The paper concludes on the importance of model resolution with respect to the different forcing and how the bed topography resolution can be the ultimate limiting factor on how to choose adequate resolution for model predictions. The findings of this papers are of high quality and highly relevant for improving future projections from ice sheet

models. I would highly support the publication of this paper after revisions suggested thereafter.

General comments:

1. The paper feels that it was written in a hurry, lacks clarity (especially before section 4.2), and could be better organized. There is too much of a focus on ISMIP6 and how the model used here participated in this effort. The paper could simply mention that it extends ISMIP6 contribution by deepening the analysis on the impact of forcing on the model and bed resolution and leave it at that. It is useful to provide a quick summary of the ISMIP6 experimental protocol since it is used as experimental design in the paper but there is no need to add all these references to ISMIP6 throughout the text. I found model descriptions in many different sections while they should be gathered in one place. Similarly, the initialization technics and results should be discussed in a single section with more figures comparing to observations. Terms are misused throughout the text. In the ISMIP6 protocol, what is called "Initial State" in section 4.1 is really the historical run. The control experiment is relative to the state of the initial condition prior to running the historical experiment. The projection control is the one that should be performed starting from the end of the historical run until year 2100 and used in the result analysis section (instead of the control run). It does not appear in the paper and should be added.

2. This paper is very good in discussing and analyzing the results on the model resolution dependence versus the use of grid resolution. It would be of even higher quality if it improved the analysis and the discussion of the dependence with respect to the bed topography versus model resolution (after all, it is major result here).

3. The introduction tries to make a point on the importance of using an adequate resolution for modeling GrIS. However, this is only apparent towards the end of the introduction. It is difficult to follow why it was necessary to read the different paragraphs about initMIP, Greve and Hertzfeld, Aschwanden 2016 and 2019, as no direct conclu-

sions from these finding would lead to the thesis of this study. In particular the paper mentions that the resolution in Greve and Hertzfeld was too coarse to expect any better quantitative results (on top of using SIA). The introduction continues with describing Aschwanden's work that actually did use very high resolution with no real benefit over coarse resolution. At that point, I would expect a discussion of why that is and what this study will do differently.

4. The initialization techniques lack clarity and details. The inversion parameters and their variations with model resolutions are not discussed. After the inversion, is the model run long enough to bring it to a steady state? If so for how long? What dataset (climatology, geothermal heat flux,...) is used for the initialization? Is there a specific procedure (if any) initiated once the parameters are set? Please add more details on that topic. How are ice shelves constrained during the initialization? A figure highlighting how well the initialized ice sheet matches target observations would be a good addition (the authors mention they want this paper to describe in greater depth how the initialization was performed which could not be done in the ISMIP6 paper by Goelzer et al. 2020).

5. The logic behind not using a sub-grid scheme to simulate fractional retreat in section 3.3.2 does not make sense to me. The argument that it might mimic a higher resolution is not sound as it is something already done for grounding line dynamics. Furthermore, my understanding with how the text is written, is that the grounding line will most likely not coincide with the calving front for grounded marine termini glaciers. The text should be clearer about the description on how the calving front is handled in the model. The grid resolution still plays a role even when a sub-grid scale physics is used in a model. This assumption might play a part in the conclusion and, ideally, the authors should run and present a simulation that suggests otherwise. Also, the model uses an unstructured grid and a straightforward convergence analysis similar to when using a uniform grid is more difficult. Please, revise the argumentation in the text (line 228-230).

6. All the simulations are run with the calving front remaining fixed in space and time besides those with a calving rate mask forcing. This was very confusing as it is not clearly mentioned in the model assumptions in the appropriate section (it only became clear in the result section). The text should make this really clear in the appropriate section of the paper. I do not understand these different treatments and the text does not discuss it. The paper would benefit from more details behind this reasoning, and, also, more discussion on why their conclusions would hold shall this restriction on Rnone experiments be removed. Right now, Rnone experiments do not benefit from the reduced buttressing and from a stronger signal from bed topography adjustments as the other experiments do. The paper mentions this problem but does not discuss it.

7. The discussion about N, tau_b, tau_d, and the sliding velocity in section 4.4 could be extended more. N decreases with the SMB evolution in these experiments. The SMB perturbations lead to a decrease in ice thickness to which N directly depends on, hence a reduction in tau_b. Also, at higher resolution, the marine portion of the glacier shows deeper bed (figure 10) which will result in a lower N, a lower basal friction and an increase in sliding velocity in order to balance the driving stress. Gagliardini et al. 2007, and Leguy et al. 2014 study these relationships and they can be used as references for the discussions. Also, this discussion item should be tied in with the discussion of the importance of the bed resolution especially when using effective pressure dependent basal friction laws. Oddly enough, these points are being mentioned in the conclusion but not before, why?

Specific comments:

Page 1: Line 16: remove the character "N". Line 14-16: "A major response ..." By invoking the sliding mechanism using effective pressure you are inherently talking about the dependency with respect to the basal sliding law used in the model. I would simply live it as that in the abstract as there is no further modeling details given at that point.

Page 2: line 34: add the citation of Nowicki et al. 2020. line 44: Please rephrase. line

46: replace "affect" by "affects". line 54: replace "well" with "will". last paragraph (line 50-54): be careful with the first sentence here as Fig. 1 clearly shows (with ISSM) that a resolution of 0.5 km was necessary to see a drop in SL contribution and no other models (in this figure) submitted results at that resolution. Also, please clarify that the importance of resolving the ice margins in the initMIP simulations is because they are subjected to the strongest SMB anomalies and SMB anomaly transitions compared to the interior of the ice sheet.

Page 3: Line 59: please spell out SIA as it is the first time it is employed in the text. Line 70: "however, the SMB..." please clarify what it means and why it matters here. Last sentence: Why is this information of importance? Are you trying to make a point that their choice of Stokes approximation is a limiting factor? As stated, I would simply remove it.

Page 4: Line 74: "which is ... (Church et al., 2013)" this comment feels out of place in what you are trying to say here. I would remove it. Line 77: "The adequate resolution..." please add citation(s) to support this claim. Also, as stated it is quite confusing because increasing the resolution is a good thing (up to a certain point) regardless of the Stokes approximation. The resolution dependency is typically greater with sub-grid scale physical mechanism such as grounding line tracking, ... or when needed to better resolve bed topography. Line 78: "higher-order approximation is providing..." please add citation(s) supporting your claim (for similar reason as previous remark). Line 82: "to this task" please clarify what you mean here. Line 82: "Therefore, the main..." I would suggest beginning a new paragraph with this sentence adding directly what will be the major difference compared to what Aschwanden did (which is very similar). Line 85: "Blatter-Pattyn-type" Is it different than BP? If so, how does it differ? Otherwise remove "type". Please, add a reference to BP here as it is the first time you mention it and you can remove the one on line 99. Line 87: "For comparison..." What is the relevance of this information here. Line 90: "A secondary aim ..." This sentence is confusing here as it sounds like the aim of this paper is to

redo the ISMIP6 exercise. Line 91: "which could be valuable..." not necessary there. Line 91: the footnote on the word "audience" This footnote is confusing. Are there any differences between ISSM and AWI-ISSM? If so the text should highlight these differences to improve clarity. For instance, which release of ISSM did AWI branch from? Was there major development(s) made since then and if so add a reference. Line 99: Blatter-Pattyn is a very expensive model to run. Please clarify what you mean by "balancing computational cost", are you referring in comparison to full Stokes? Line 102: please add citations for the characteristics of the model (Glen's flow law, temperature dependent rate factor ...).

Page 5: Line 104: add "," after "base". Line 104: please add a citation for this form of sliding law. Also, please clarify your choice of sliding law. This formulation is typically avoided as it can grow unboundedly (schoof 2005). Also, it would be good to provide a map of the k^2 friction coefficient. Line 109: "At lateral..." The sentence is confusing, please reword. Line 111: Please provide a citation or link for EPSG:3413 grid. Line 122: Please indicate if the grid is fixed throughout the simulation or evolving. Line 124-125: typically, modelers think of high resolution being the smallest mesh size used in a model and the coarse (low) resolution being the biggest one. It is less confusing for RESmin to be the coarsest resolution and RESmax to be the highest. Line 127: "Additionally, we ..." This information is out of place here and should be omitted.

Page 6: Line 136: The sentence here contradicts the title of section 3. Maybe rename section 3 as "Forcing experiments" or something similar, and simply state that you are following the ISMIP6 experimental design. Line 138: I suggest writing "Slater et al. (2019a, b)" similarly to what you did on page 9 line 222. Line 139-142: Why is it necessary to mention initMIP here?

Page 7: Line 152: there is also a projection control experiment that starts at the end of the historical run. Have you run it? Line 155: "The ensemble..." I believe it refers to the ensemble from ISMIP6? If so this sentence does not add any value to the paragraph. Line 159: Please briefly recall how low, median, and high oceanic forcing

were defined. Paragraph 3: "Conducted projection ..." This paragraph is out of place and should be combined somehow with section 3.3.2. The definitions of the runs (which are highlighted in Table2) could be given at the beginning of the result section.

Page 8: Line 191: "That means..." This sentence is confusing. Do you mean that grounded and floating ice cells are not allowed to retreat? If so it restricts the purpose of the historical run. Please clarify.

Page 9: Line 224: "The imposed ..." Is this sentence supposed to explain how the prescribed calving front retreat was obtained? If so, say so.

Page 10: Line 228: "This enables ..." See the general comments. Additionally, this statement is ambiguous because you are using an unstructured grid. While you can compare the results from the simulation using different grids, you cannot claim your comparison to be consistent to grid resolution. Please rephrase. Line 237: The title of section 4.1 reads "Initial state". This title Is confusing. Typically, the initial state is the one obtained at the end of the inversion procedure and the one used as initial condition for the historical and control runs. Please rephrase. Section 4.1: this section contains information that should be stated in section 3.1 such as the restriction of the calving front during the inversion procedure ...

Page 11: Line 261: "Similar as ..." There is no need to repeat this sentence here since the MSD metric is used again. Line 268: "As the ice ..." Please discuss further the reason of keeping the calving front fixed throughout the historical run. Line 274: "with the control" The projection runs should be corrected with a projection control run instead which is not discussed in this paper. Line 275: "in the absence of additional forcing" This defines the control run. It is an unnecessary repetition. Line 276: "... as a prediction of actual behavior ..." This is out of place because the text is talking about the control and have not induced any forcing yet. Please rephrase.

Page 12: Line 279: replace "simulation" with "simulations". Line 282: replace "with" with "to". Line 282: "(see above)" Please refer to a section for clarity (unless you are

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referring to the mass gain numbers?).

Page 14: Line 304: replace "compared the total" with "compared to the total". Line 307: remove the repetition of "the". Line 311: replace "RCP8.5-Rnone" with "RCP8.5-Rnone and RCP8.5-Rlow"? Line 316: replace "lesser than" with "less than".

Page 15: Line 347: reword "early in the century an increase" with "an increase early in the century".

Page 16: Line 357: replace "worth to mention" with "worth mentioning". Line 359: "remains fixed in time ..." See my general comment. Line 366: replace "reduce" with "reduces,". Line 367: replace "not obvious" with "non obvious". Line 368: remove "come into play". Line 369: "The general picture ..." Please rephrase.

Page 17: Line 378: "To study ... grid size" Please rephrase. Line 392: replace "together an increase" with "together causing an increase"? Line 392: replace "thinning an acceleration" with "thinning and acceleration"? Line 392: "The transient ..." Please rephrase the end of this sentence.

Page 18: Line 400: replace "nasal" with "basal". Line 405: add "we" before "find". Line 416: please rephrase end of sentence. Line 423: replace "it is worth to investigate this influence isolated" with "it will be worth investigating this influence only"?

Page 19: Line 428: replace "in numerous cases" with ", in numerous cases,"? Line 431: replace "assessing the importance of it" with "assessing its importance"? Line 434: remove "thus"?

Tables: Table 1: Is the computational time listed here for all the experiments or simply for the 86-year run after the historical run?

Figures: In the relevant figures, please add a black contour for the grounding line.

Figure 2: replace "G8000" with "G4000". The small ice cap above 79N should not be present for consistency with the text and the other figures in the paper. Figure 5: it

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should really be Figure 6 since its reference appear after figure 6 in the text. Figure 6: it should be relabeled Figure 5 (see figure 5 comment above). Figure 9: the subfigure labels b and c are misplaced. What are the units for Year? (I have never seen CE before as a unit). Figure 10: the x-axis is labeled "distance". What is it relative to? Please add this reference to the figure. Also, please try to increase the font size of the labels as they are difficult to read on printed paper.

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