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Discussions

Interactive comment on "Marine ice-sheet experiments with the Community Ice Sheet Model" *by* Gunter R. Leguy et al.

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We thank both reviewers for taking the time to review our paper during these difficult times. We appreciate the many constructive comments, and we think the changes made in response to the comments will make the manuscript clearer and stronger.

Reviewer 2: Rupert Gladstone

Two sets of idealised experiments have been carried out using an ice sheet model CISM that can also form part of a community Earth System coupled model (CESM). The first set of experiments investigate impact of dependence on effective pressure in sliding relations and grounding line parameterisations for basal resistance on

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Discussion paper

model performance, especially self-consistency (especially convergence with resolution). These experiments have some novel aspects but generally confirm expected results, and provide a useful reference point for future use of CISM (and CESM). The second set of experiments investigates the impact of parameterising basal melt near grounding lines, again on self-consistent model behaviour. In these experiments an unexpected result is obtained: parameterisations that allow some melt in partially grounded cells give better convergence than parameterisations that do not allow this. This has relevance not only to future use of CISM (and CESM) but potentially also to other ice sheet models and to the development of methodologies to handle high basal melt rates near to grounding lines. The paper also compares different approximations to the Stokes equations and provides some evidence to support use of a vertically integrated model (i.e. a 2D problem is solved) that is only marginally more computationally expensive that the commonly used "Shelfy Stream Approximation".

The paper is for the most part clearly written and thorough. I have only a few minor suggestions.

This paper might have worked well as two separate papers or as a two-part paper. The sets of experiments in sections 3 and 4 are quite distinct from each other and the paper is rather long.

We thought about splitting the paper into two parts. After consideration, we decided that there would be too much repetition between the two papers, and that a paper focused only on the MISMIP3D experiments would be rather short and not very novel.

In a few places the authors refer to simulations that have been "spun up" or "spun up for XXX years" to steady state. But it is not clear to me how steady state is determined. Are specific criteria used to determine steady state? Please state how you determine steady state.

We added some specifics on how steady-state is determined.

In Sect. 3.1, line 223, we revised the text to read, "In the first step, labeled 'Stnd', the model is initialized with a uniform 500-m thick slab of ice and is spun up over 20,000 years with a uniform basal shear stress factor. At the end of the spin-up, the ice sheet has reached a steady state in which the change in grounding line location is less than 10-3 m a-1 and the change in ice thickness at the grounding line is less than 10-4 m a-1."

In Sect. 4.1, line 339, we added: "The steady state is determined based on grounding-line location and ice thickness, as described in Sect. 3.1."

The experiment naming in section 4 is counter-intuitive. Your "experiment 1" comes after you've already presented a bunch of experiments based on the MISMIP3D setup, in which you used the MISMIP3D naming convention. You also seem to use the MISMIP+ naming convention within "experiment 1", which suggests that "experiment 2" refers to a bunch of experiments. So "experiment 1" is not the first experiment, it isn't even the first set of experiments, but perhaps it refers to the second set of experiments. . . Please make this more reader-friendly somehow. My suggestion is to call this section something like "Moderate basal melt rate experiments", and within it refer to MISMIP+ experiment names. If you need to, you can add to these names to distinguish unique aspects of your simulations that are not defined by MISMIP+ naming conventions. This is just a suggestion, deal with this however seems best to you, but I hope you can see why I don't find the current "Experiment 1" and "Experiment 2" naming helpful to the reader.

Thank you for this suggestion. We agree that the current naming is not helpful. We modified the names in Section 4 as follows:

- "Experiment 1" is replaced by "moderate basal melt experiments".

- "Experiment 2" is replaced by "high basal melt experiments".

- "Experiment 3" is replaced by "slow-moving ice shelf experiments".

In response to Reviewer 1, we added a section labeled as “calving experiments.” We kept the MISMIP+ experiment names (Ice1rr, etc.), since these indicate when the melt is turned on or off, and will be familiar to many readers.

I find myself bringing up my own papers in my comments. Please don’t feel under any obligation to cite these – it should go without saying that my review outcome is not conditional on you citing my papers! I just mention them as they spring to mind as relevant to specific topics.

Your papers were relevant in many parts in our study. We tried to cite them appropriately.

Line by line comments follow.

Page 2

Line 46 and line 52. The suggestion of 1km resolution is not a uniformly applicable result. The required resolution is dependent on several factors. Can you reword these instances to clarify this? This is also relevant: https://doi.org/10.3189/2012AoG60A148

Yes, this is a good point. We revised the text as follows:

“With a GLP, a resolution of 1–2 km may be sufficient to accurately represent grounding-line motion (Gladstone et al., 2010; Seroussi et al., 2014; Leguy et al., 2014; Cornford et al., 2016), with either continuous or discontinuous basal friction. The required resolution can depend on several factors, including basal drag, channel width, and bed topography (Gladstone et al., 2012).”

Line 61. Does the 1km here refer to with or without the GLP? This needs to be clarified.

The sentence now reads, “With a GLP, a resolution...”

Page 3

Lines 64-65. This is quite an important line because you are going to later argue the opposite. So please add a line or two to explain why previous studies suggest that no melting should be applied in the grid cell containing the GL. If I remember right Cornford found that the convergence is much worse, with a tendency to grossly overestimate GL retreat.

We added a sentence on line 65: “ A physical argument can be made that applying melt in the cell containing the grounding line will artificially drive retreat, by thinning grounded ice upstream of the grounding line.”

Page 6

Line 162-163. But isn’t this theory specifically for Schoof’s setup which involved SSA? I think that in a model in which the stress distribution is vertically resolved the precise vertical stress distribution at the grounding line would have some dependence on the shape of the shelf downstream. It might only be a small difference, but I think it worth noting that Schoof’s result here isn’t supposed to apply to real ice shelves in general, just the SSA setup for which he derived a solution.

Yes, Schoof’s setup assumes a 2D shelf that is rapidly sliding (hence with negligible vertical stresses). We clarified the text as follows: “According to theory (Schoof, 2007a), a rapidly sliding, two-dimensional ice shelf can be melted completely from below. . .”

Page 7

Figure 2. The GL is drawn differently in the left plot compared to the middle and right plots. I think this is probably accidental, but it is confusing to the reader. Please make them look more similar. I note that there seem to be some minor alignment issues with the boxes with each figure, but these are small enough not to be much of a distraction.

We modified the figure as suggested.

Page 8

Lines 183 – 184. Needs rephrasing. Either this has been demonstrated elsewhere, in which case reference it, or else you actually have considered it and decided it is not worth reporting on in this paper, in which case please change “do not consider” to something like “we carried out preliminary tests (not shown) . . . and found this to yield unrealistic. . .”.

Yes, we did consider this scheme in preliminary tests and decided it is not worth reporting in this paper. We rewrote this sentence as:

“We also considered a ‘Full Melt Parameterization’ in which the full basal melt rate is applied in partly grounded cells. We found in preliminary tests, however, that this scheme drives unrealistic grounding-line retreat in CISM, as in Seroussi and Morlighem (2018), and we will not consider it further.”

Line 185. Maybe “small changes” -> “sub-grid resolution changes” is more specific?

We changed “small changes” to “subgrid changes.”

Lines 198 – 202. Seems like a slightly odd choice to present conclusions at the start of a results section. I don’t have a big problem with this, leave it if you like, it just reads a bit odd.

We reworded this paragraph to leave things more open-ended. The revised text is of the form, “Leguy et al. (2014) demonstrated X for a 1D model. Here, we want to see whether X generalizes to more realistic 3D models.” Specifically, the text now reads as follows:

“Leguy et al. (2014) used a one-dimensional model to explore the effect of different basal friction laws on grounding-line migration. They found that a resolution of $\sim$1 km (and, under some circumstances, coarser) is sufficient to accurately represent grounding-line motion if the ice sheet is hydrologically well connected to the ocean, or if a GLP is used for basal shear stress. In contrast, ice sheet models without hydrological connectivity or a GLP (including models that participated in MISMIP3d, as shown in Fig. 5 of Pattyn et al. (2013) require very high resolution (∼500 m or finer) to accurately capture grounding-line dynamics. This 1D model was practical for running many experiments at low computational cost, but there was no guarantee that the results would generalize to three-dimensional models. We now use CISM for this purpose.

Line 212. Sorry for not referring back to the MISMIP3D paper (lazy reviewer), but one of the most important aspects of the design here is how this steady state is approached. The GL “stickiness” problem can be characterised by the existence of a region of multiple steady states (which is what leads to irreversibility). So depending on which of these states one starts with, reversibility may or may not be shown. There is more about it here: https://tc.copernicus.org/articles/12/3605/2018/. Please add a line or two about the initial conditions and how steady state is approached.

We expanded the text to read: “In the first step, labeled ‘Stnd’, the model is initialized with a uniform 500-m thick slab of ice and spun-up over 20,000 years with a uniform basal shear stress factor. At the end of the spin-up, the ice sheet has reached a steady state in which the change in grounding line location is less than 10ˆ-3 m aˆ-1 and the change in ice thickness at the grounding line is less than 10ˆ-4 m aˆ-1.”

Page 9

Line 222. This 4km seems arbitrary in the absence of any knowledge about the size of the GL response to the perturbation. I presume this is much greater than 4km, so please give some indication of this! Also, knowing the grid cell size would give some context to this. I don’t think the reader has yet been given this information at this point? I’m commenting as I read through so I might find this later, but some indication here would be useful to help the reader decide whether 4km is a sensible choice.

We agree that this discussion is premature and that the threshold of 4km is somewhat arbitrary. We repeated the analysis for thresholds of 1 km and 2 km and found that the finding of reversibility is not very sensitive to the chosen threshold, but this needs to be made clear in the text.

We deleted the sentence, “We will consider an experiment to be reversible if the difference in grounding-line location is 4 km or less.” The 4-km criterion is now introduced later, in Sect. 3.3.

At line 265, we added the following text: “Since the 4-km threshold for reversibility is somewhat arbitrary, we note that the results are not very sensitive to this threshold. With a 2-km threshold, the SSA and DIVA tests with p = 0 are labeled as irreversible at 4-km resolution. When the threshold is reduced to 1∼km, the BP test with p = 0 at 2-km resolution becomes irreversible. Otherwise, Fig. 5 is unchanged.”

Page 10

Lines 235-236. If they’re directly comparable, why not add Schoof’s GL position to the plot? Just as a horizontal line?

We added Schoof’s solution to the plot.

Page 16

Line 339. “100-m” -> “100m thick ” Page 19

Changed to “is initialized as a uniform slab with H = 100 m”

Page 19

Figures 9 – 12. The different lines are hard to distinguish. I think this is partly because the circles are a bit too large given how close the lines are, and they overlie each other quite often. It is also partly because the connecting line segments are in black and the circles are outlined in black. In Figure 10 especially it is hard to distinguish between 8km and p5km (perhaps because the circles are smaller but still outlined in black, which dominates?). These figures need to be clearer. It might be that changing the black to the appropriate colour will fix it, but it is hard to say without actually seeing a modified version. I also find “p5” instead of 0.5 a bit counter-intuitive. It looks like something a programmer might write but I don’t see why you wouldn’t just use 0.5 in a paper. Minor detail: you refer to “vertical line” in Fig 9 caption but “vertical lines” in fig 10 caption. Did you mean to mention also the 100m line again in Fig 10?

Thank you for these suggestions. We changed the figures as follows: - removed the black contours around the circles; - connected the circles with lines of matching color; - replacing “p5” by “0.5” throughout; - modified the Fig. 10 caption to explain both vertical lines.

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Line 496. Here you say that results are sensitive to choice of p, but you don’t actually state that you get better convergence with p=1. Better convergence with p=1 is expected based on past studies, and I would say your results do indeed support this. So I think you can make a stronger statement here than just saying that the results are sensitive to choice of p.

Our results are not consistent on whether there is better convergence with $p = 1$. In the MISMIP3d experiments, convergence is better with $p = 1$ when running without a GLP, but not with a GLP, as shown in what is now Fig. 4. In some melt experiments, convergence with $p = 1$ is significantly slower than with $p <= 0.5$, as shown for the FCMP and PMP moderate-melt experiments in what is now Fig. 10.

In the statement on line 496, we agree that “less benefit” is a bit misleading, since in some cases there is no discernible benefit. We changed the wording to “although MISMIP3d runs show little or no benefit from a GLP when p = 1.”

Line 507. Brondex 2018 (J. Glac) is probably the best reference here.

The study by Brondex et al. (2017, J. Glac.) compares Weertman, Schoof/Tsai, and Budd friction laws, but does not explore which value of p is most appropriate for realistic settings such as the Amundsen sector. At lines 506–507, we cited the study of Joughin et al. (2019) because it links directly to PIG observations.

We agree that Brondex et al. (2017) is relevant to the choice of basal friction law. We added this study, along with several other references, after the first sentence of this paragraph: “The choice of basal friction law remains a source of great uncertainty in ice sheet models.”

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Line 527. There is a great mystery behind “numerical details of the ice sheet model” which I hope the authors (and the wider ice sheet modelling community) will investigate in the near future. One thought that springs to mind and is not explicitly mentioned here is that the grid or mesh alignment may have some impact. I think it is fairly typical in MISMIP+ simulations for the central section of the grounding line to be aligned approximately across the flow. So if a non-adaptive structured grid is used (which is the case in the current study) then grounding line retreat may naturally occur a row at a time. If previous studies that found NMP to be the better scheme have used an unstructured mesh of triangular elements (ISSM) or an adaptive mesh (BISICLES), it might be worth considering that single element ungrounding may occur more easily in such setups than in the current CISM setup, and perhaps this could explain (part of) the difference? Please don’t feel under any obligation to consider this half-baked speculation in the current paper, though perhaps it could be one factor to consider when delving further into this problem.

Yes, the difference in model behavior between ISSM and CISM came as a surprise to us, and we do not yet understand the reason for these differences. We considered diving into the numerics of ISSM and other models to look for explanations, but decided it was better left for a future study. We added a short parenthetical suggestion, changing the text to “...numerical details of the ice sheet model (including, perhaps, the grid structure and the staggering of variables).” We think your “half-baked speculation” is intriguing and worthy of investigation.

Final paragraph. I am not convinced that this quantification of adequate resolution is fully supported by your simulations. Bear in mind that readers will mostly not read the full paper in detail but will look at key figures and conclusions. This suggestion of 2 - 4km being in general sufficient is dependent on many factors, not all of which are fully explored here, and in particular, this paragraph refers to the MISMIP3D experiments and not to the melt experiments. When I look at Figures 9-12 I do not have confidence that a converged result has been achieved at 2-4 km resolution. If you see, in the next year or two, a published future projection of the Antarctic Ice Sheet carried out at 4km resolution, and read that their justification for the choice of resolution is simply citing your paper, will you feel comfortable with that?

This is a fair point. We do not want to imply blanket approval for runs carried out at 4-km resolution. The final paragraph does mention the melt experiments, and we think that Figs. 9–12 support a resolution requirement of 2–4 km with $p <= 0.5$. We agree that with $p = 1$, the support for a 4-km grid is weaker. We also acknowledge that this study is far from exhaustive.

In the revised text, we made the last paragraph more cautious with the following changes:

- Changed “4 km” to “2–4 km” in the first sentence

- Changed “careful initialization” to “careful initialization and verification” in what was the last sentence. Verification would include the kind of experiments analyzed in this paper.

- Added the following sentence at the end: “Finer resolution of 1–2 km may be needed when using basal sliding laws with Coulomb behavior or strong connectivity to the ocean.” This is consistent with the last sentence of the abstract, and would discourage models with Coulomb sliding laws (including CISM) from using this study as justification for 4-km resolution.

In other recent work (Lipscomb et al. 2021), we ran ISMIP6-style Antarctic simulations at resolutions of 2 km, for comparison to the baseline resolution of 4 km. We found moderately increased sensitivity to ocean forcing with finer resolution, compared with much higher sensitivity to the basal melt parameterization and basal sliding scheme. We therefore added this text to the next-to-last paragraph: "For example, Lipscomb et al. (2021) found moderate sensitivity to grid resolution in multi-century, ocean-forced Antarctic Ice Sheet experiments with CISM when comparing results at 2 km and 4 km. This sensitivity was less, however, than the sensitivity to sub-shelf melting and basal friction parameterizations."

We are conscious of the fact that most experiments in the Lipscomb et al. (2021) study were run at 4-km resolution, which is probably too coarse for Coulomb laws. We are working at making the model efficient enough for routine use at higher resolution.

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