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

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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 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 is 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.

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.

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.

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.

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

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

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.

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 her isn't supposed to apply to real ice shelves in general, just the SSA setup for which he derived a solution.

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.

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. . .”.

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

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.

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.

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 her would be useful to help the reader decide whether 4km is a sensible choice.

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?

Page 16

Line 339. "100-m" -> "100m thick "

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 closes the lines are, and the 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?

Page 26

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.

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

Page 27

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 mon 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.

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 2-4km 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?

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