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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.

Anonymous reviewer 1

Proper modelling of grounding line dynamics remains one of the main challenges for ice- sheet models. A suite of inter-comparison exercises has shown that models results are particularly sensitive to the mesh resolution in the vicinity of the grounding line. Sub-grid parameterisations have been shown to reduce the mesh size sensitivity

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allowing to give good results at resolutions that become achievable with typical grid sizes used in large scale simulations. Here the authors used two benchmark experiments (MISMIP3D and MISMIP+) to study the sensitivity of the results to sub-grid parametrisations of the basal friction and basal melting. The authors already studied the basal friction parametrisation in Leguy et al. (2014) with a flow line experiment and, here, extend this previous study to a 3D experiment. For the basal melting they describe sub-grid scheme implemented in CISM.

Intercomparison exercises usually focus on the inter-model differences and I found always useful to have detailed studies with individual models.

Most models report that sub-grid parametrisations decrease the model sensitivity to the grid size. In agreement with their previous 1D study, the authors found that the grid size sensitivity is decreased when there is a smooth transition of the basal friction in the GL vicinity. Results for basal melting are in contrast to previous studies that have reported that applying melt in partially grounded cells might lead to inaccurate results.

In conclusion, this is an interesting numerical study with a well established ice-sheet model. The manuscript is well written and clearly describes the experiments and results and I have only minor comments or questions detailed below.

Minor comments :

- Page 2, line 31, depth-integrated versions of the Blatter-Pattyn approximation: not sure if this is the good formulation, these cited models are indeed depth integrated but they approximate the 3D Blatter-Pattyn model.

We agree that the depth-integrated models are distinct from BP. We modified the text to read “along with depth-integrated higher-order approximations (Goldberg, 2011; Perego et al., 2012).”

- Page 2, line 44, The required resolution is coarser for sliding laws in which basal stresses are continuous across the grounding line. Maybe this is not as easy and depends on the transition. See for example results from Gagliardini et al. (2016) and discussion on this subject in Gladstone et al. (2017).

Thank you for pointing out these results and discussion. We modified the text on line 44 to read, “In higher-order models, the required resolution is coarser for sliding laws in which basal stresses are continuous across the grounding line (Leguy et al., 2014; Tsai et al., 2015; Gladstone et al., 2017). (This is not necessarily true for full-Stokes models, as discussed by Gagliardini et al., 2016.)”

- Page 2 Line 54, but less so for models configured to solve the full Stokes flow equations. Cheng et al. (2020) report that similar accuracy is obtained using sub-grid modeling with more than 20-times-coarser meshes in a Full-Stokes model. Please provide more references for sub-grid scheme in FS model to support this sentence.

We rewrote this sentence as: “. . . for Stokes approximation solvers, as well as models configured to solve the full Stokes flow equations (Cheng et al. 2020).”

- Page 3, Line 64 : to obtain more accurate results. Explain the meaning of accurate in this context. Seroussi and Morlighem (2018) and Conford et al. (2020) report that sub-grid schemes can result in numerical errors not inaccuracies.

You are correct. We replaced “to obtain more accurate results” with “to reduce numerical errors”.

- Section 2 model description; I think not all readers will be familiar with the staggered grid. As this is used for both parameterisations maybe it could be beneficial to have a small subsection before 2.1 to describe the CISM grid and introduce here that friction has to be computed at cell vertices and melt at cell corners.

Thank you for the suggestion. We added a short paragraph before the start of Section 2.1, referencing Fig. 1 (which erroneously was not referenced in the original manuscript):

“The CISM grid is shown schematically in Fig. 1. Scalars such as ice thickness H and bed topography b are located at grid cell centers, with ice velocity components (u,v) at cell vertices. Since basal melt rates modify H, they lie at cell centers, whereas basal friction is a forcing term for velocity and is defined at vertices. This staggering of variables is incorporated in the GLPs for friction and sub-shelf melting, as discussed below."

- Section 2.2 Grounding line parameterisation for basal friction, from lines 144 to 151; It would be useful to add a figure (or maybe in Fig.1) to illustrate this example.

We added a figure (the new Fig. 2) as suggested, with an explanatory caption.

- Page 6, line 153. Maybe start to say that you compute an effective basal friction coefficient using the friction law presented in 2.1 then that the sub-grid scheme is applied to this coefficient.

We added a sentence giving the functional form of beta for the Weertman and Coulomb laws presented in 2.1. With this addition, we think the text is clearer on how the subgrid scheme is used to modify the coefficient beta.

- Figure 2. Maybe add the cell centers in your Figure.

We added the cell centers in this figure (now Fig. 3). We also added a sentence in the caption stating that for the FCMP scheme, the flotation condition is evaluated at cell centers.

- Page 7, Line 165-166 : For buttressed ice shelves, however, the dynamics are more complex (Gudmundsson, 2013), and it is not obvious which melt treatment is best. Please explain what do you mean by more complex and not obvious.

Buttressed ice shelves impact grounding line migration via the backstress they impose on upstream grounded ice, as a result of which the GL can be stable on reverse-sloping beds. This is dynamically more complex compared to unbuttressed ice shelves, which are unstable on reverse-sloping beds. We expanded the text as follows:

“On a retrograde bed (i.e., a bed that slopes upward in the direction of ice flow), grounding lines are unconditionally unstable, assuming no flow variation in the transverse direction. Buttressing, however, can stabilize the grounding line on retrograde beds (Gudmundsson, 2013). Thus, it is not obvious which melt treatment is best for a buttressed ice shelf.”

- Page 8, Lines 188-192 : I don’t really understand this part and why this is here. It seems strange to say here that CISM usually uses the quadrant method but that another method has been presented before. See previous comment ; maybe it would be beneficial to have a specific section in the beginning of section 2 to explain how grounded fractions are computed at cell vertices and corners.

We agree that this presentation is confusing. We tried to clarify the presentation by moving this paragraph, with some modifications, to Section 2.2, just after the equations for computing phi_gˆv. The revised paragraph states that we can use the same equations on a quadrant-by-quadrant basis instead of for an entire staggered grid cell, and the reason to do this is to obtain consistent areas when summing over the staggered grid (where the area fraction is phi_gˆv) and the unstaggered grid (where the area fraction is phi_gˆc).

- Page 8, bottom line. uniform basal shear stress factor. C was introduced as a coefficient (page 4 top line) and is referred to as shear stress factor in the tables. Maybe better to use basal shear stress factor, C, everywhere.

Yes, thanks for spotting this inconsistency. We changed the text on p. 4 to read “C is the basal shear stress factor.”

- Page 9, line 221 : We will consider an experiment to be reversible if the difference in grounding-line location is 4 km or less. Maybe give a better justification for this 4km.

Indeed, this value is somewhat arbitrary. We thought that this was a reasonable threshold for an experiment during which the grounding line moves between 20-60 km depending on p, resolution and Stokes approximation. To be sure that the results are not sensitive to this specific value, we repeated the analysis with smaller thresholds. This adds two more irreversible cases at 2 km, and a third case at 1 km.

We added the following text in the discussion of the old Fig. 4 (now Fig. 5): “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.”

At l. 221 we deleted the sentence, “We will consider an experiment to be reversible if the difference in grounding-line location is 4∼km or less,” since we have not yet mentioned grid resolution at this point in the discussion.

- Section 3.2 ; might be beneficial to have distinct sub-sections for the steady state solution and the transient.

We divided Section 3.2 into two sub-sections, as suggested.

- Page 9 bottom line , When the grounding-line position no longer changes significantly as resolution is increased, we consider the solution to have converged. Please quantify “significantly”.

We originally had in mind agreement within ∼20 km of the solution from the highest available resolution (0.5 km). However, this value is arbitrary. We decided not to give a threshold, but instead to reword the discussion in terms of relative rates of convergence. The text now reads:

“The threshold for ‘significantly’ depends on the application, but without stating a specific threshold, we can see that for p <= 0.5, the runs with a GLP converge much faster than those without a GLP. Using DIVA with a GLP, for example, the 1-km solution differs from the 0.5-km solution by 6 km with p = 0 and by 9 km with p = 0.5 when we use a GLP. Without a GLP, the respective differences are 37 km and 28 km. For p = 1, however, a GLP does not clearly improve convergence."

- Page 10, top line ; Maybe would be more clear to break this sentence in two; and following previous comment it would be more precise with a given threshold to define the convergence. Or avoid to use converged if there is no given criteria.

Please see the text changes in the previous comment.

- Page 11, Line 249 : and far cheaper than BP. Could you give numbers ?

For these simulations, DIVA is computationally 10–40 times faster than BP depending on resolution, as mentioned on line 298. We moved the statement from line 298 to this earlier point in the text.

- Fig. 5. For p=0 there is no difference between Stnd and P75R ?

Yes, the difference is too small to be seen in the figure. We added the following statement in the caption: "The Stnd and P75R positions are visually indistinguishable for p = 0 and p = 1."

- Table 5 ; would be useful to directly add the values from Seroussi and Morlighem (2018) here.

We added the values of Seroussi and Morlighem (2018) to the table in parentheses, and modified the caption accordingly.

- Page 21, lines 414-415 : "For a given melt parameterization, increasing the lubrication at the bed should lead to faster flow towards open water and greater IMAF loss." Not sure if this is as simple as the rate factor is tuned so that the grounding line is at the same position, so in steady state the fluxes through the grounding line should be the same; and as the rate factor has been adjusted this might also change the buttressing ?

Yes, thanks for catching this. At steady state, the fluxes through the grounding line should be very similar (not exactly the same due to the small differences in grounding-line positions). Modifying the rate factor changes the buttressing, with a smaller rate factor leading to more viscous ice and larger buttressing. The increased viscosity required to advance the grounding line to the desired position can offset the effects of increased lubrication.

To acknowledge this complication, we added some text at l. 363: “On the other hand, the smaller values of A (i.e., greater viscosity) for p = 1 can influence the transient response, and therefore the different responses with p = 1 relative to p <= 0.5 must be attributed to differences in both bed lubrication and viscosity.”

At l. 416, we added the following caveat: “Since the flow factor A is tuned in the Stnd experiment to adjust the initial grounding-line location, differences in both viscosity and bed lubrication can influence the transient response.”

- General comment on the sub-melt scheme; In Cornford et al. (2020), the effect of the basal melt parametrisation is discussed and shown in their high melt scenario. The difference is especially visible in the evolution of the grounded area and position of the grounding line on the edges of the domain where the ice is relatively thin. I would find useful here to show the same plots and maybe to repeat their experiment 2; to see if the results of PMP are consistent with the results of the subgroups using sub-grid schemes in Cornford et al. (2020).

We repeated experiment 2 from Cornford et al. (2020) and added a new subsection. In the revised manuscript, this section will go between the current sections 4.3 and 4.4, as it starts from the same steady states as in the moderate and high basal melt rate experiments. This section includes two new figures. One figure (attached to the reply and named “Fig13_IMAF_calving_exp”) shows the change in ice mass above flotation, similar to figures in the other subsections. The second figure (attached to the reply and named “Fig14_GLpos_calving”) shows the grounding line location at the edge and center of the domain at 2-km and 8-km resolution for p = 1, with PMP and NMP, for

1000 years. This figure can be compared to Figs. 13 and 14 in Cornford et al. (2020).

We are not copying the entire section here, but we can highlight the main points:

1. As is the case for other experiments, results with FCMP are similar to those with PMP, and results with p = 0.5 are similar to those with p = 0.

2. For resolutions of 2 km or finer, the change in IMAF is relatively insensitive to resolution for all p values and melt schemes. That is, the 2-km results are close to the 0.5-km results.

3. For high resolution (2 km or finer): At the edge of the domain, the grounding line maintains a nearly constant position under Ice2rr. Under Ice2ra, the groundling line readvances slightly (by ∼2 km) after the melt is turned off. This is the case for both melt schemes and p values.

4. For coarse resolution (4 km or 8 km): Results are less clean. In some cases, there is grounding line retreat at the domain edge.

5. Our results differ from what might be expected based on Cornford et al. (2020). In the Cornford paper, models using a subgrid melt parameterization (group B) show a grounding line retreat of ∼10 km during the melt experiments, with little to no advance when the melt is removed. Our PMP results, however, are similar to the models that do not apply a melt parameterization (group A). At high resolution (2 km or finer), our PMP results are similar to NMP.

These results suggest that compared to other models with subgrid interpolation of basal melt rates, CISM with PMP is less prone to GL retreat under large melt rates.

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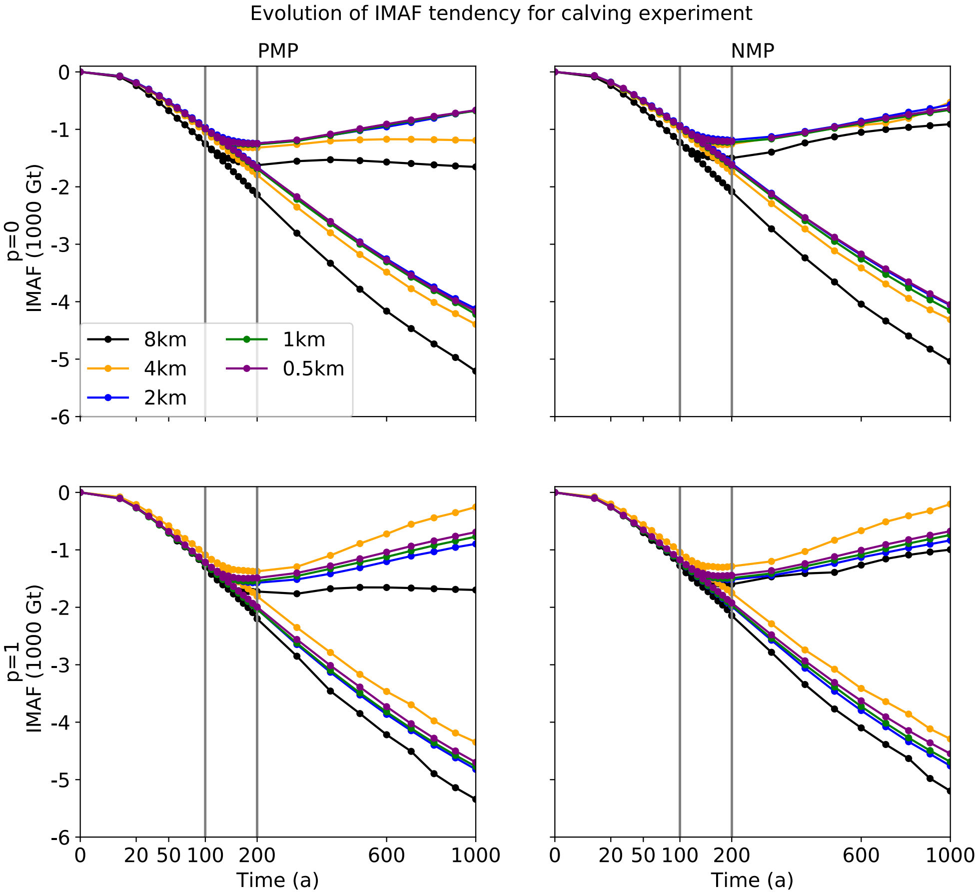

Fig. 1. Evolution of IMAF relative to steady state for the full set of calving experiments. The layout is the same as in Fig.11, but for Ice2 instead of Ice1 protocols.

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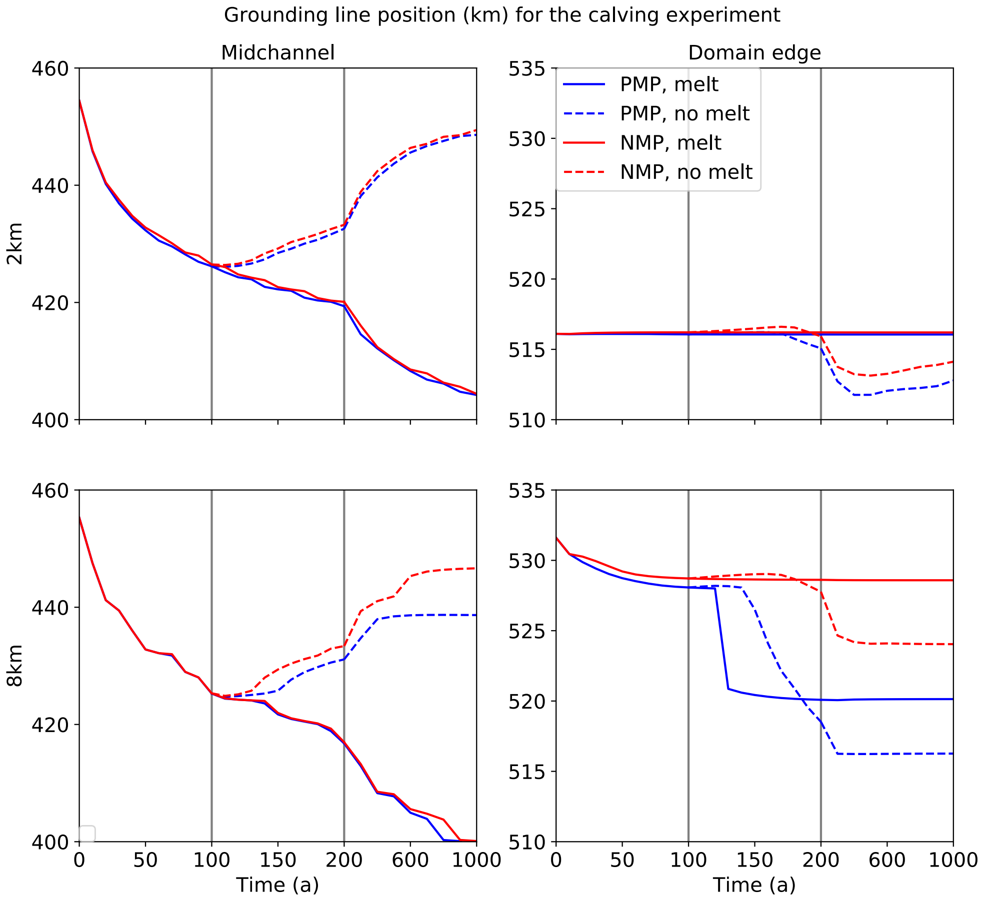

Fig. 2. Evolution of the grounding-line position (km) for calving experiments. We show two resolutions, 2∼km (top) and 8∼km (bottom).

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