

1 Reviewer #1 (R. Gladstone)

The authors present approaches to parameterising grounding line modelling specific to the finite element approach. Such parameterisations have been used in different models, but parameterisations presented here are designed to work in particular with the finite element method. The authors have carried out experiments based on the MISMIP3D experimental design, and demonstrate that a combination of high resolution and parameterisation of the grounding line are necessary for self-consistent model behaviour. This is not a new result in ice sheet modelling in general, as it is broadly consistent with several other studies using different ice sheet models, but it provides new insight specific to marine ice sheet modelling using the finite element method to solve the SSA equations. The paper is, on the whole, clearly written and well supported by the plots. The choice of material relegated to the appendices seems good to me. Of the three parameterisations presented it is disappointing that SEP3 has not been investigated in more detail, and I would strongly urge the authors to carry out some further simulations with SEP 3 at a lower resolution, say 5km, and see how effectively this approach can address the grounding line problem. I know of other researchers in the area who have expressed a strong interest forms of p-refinement, and who would I am sure be very keen to see SEP3 explored further in this paper.

We followed the suggestion of the reviewer and added SEP3 runs for the 5 km resolution meshes, which confirmed our conclusions.

Goldberg 2009 used a very similar model, but with h-refinement and r-refinement. I think the SEP3 approach is perhaps similar to p-refinement? Would it make sense to describe it in such terms or do I misunderstand p-refinement?

In finite elements, h-refinement refers to the size of the elements of the mesh, p-refinement to the order of the polynomial basis functions of the space of solution and r-refinement to the relocation or moving of a mesh. The SEP3 approach is different from p-refinement, as the order of the polynomial basis function remains here the same (P1 Linear Lagrange elements), only the number of integration points is increased.

Please be careful about using the term “lower” with regard to spatial resolution as it can be ambiguous. Please use either “coarser” or “finer” as these terms are not open to ambiguity. E.g. lines 23-24 page 3338

Done

Specifics

Abstract

L9-12. I looked for where you say how your simulations “explain why some

vertically...” but I couldn’t find it. At one point in the discussion you seem to suggest it is coincidence. Can you support this statement?

We agree with the reviewer that our manuscript explains why the results from the HSE model in *Pattyn et al.* [2012] are different from the other SSA models and not why they are similar to the Full-Stokes results. This was clarified in the manuscript.

L15-16. Suggest rewording for clarity: “...the reversibility test can be passed at much lower resolutions than are required for convergence of the steady-state grounding line position.”

Done

L16. Surely here you mean “fixed grid SSA models” rather than “fixed grid models”? Or are you claiming to have demonstrated that Stokes flow models using a contact condition to determine grounding line position are also inadequate even at very high resolution?

Done

L18-20. The resolution recommendations are specific to this experimental setup and should not be presented as though they are generally applicable to real marine ice sheet systems. The actual resolution will vary with bedrock slope, buttressing, bed slipperiness (see for example Gladstone 2012 *Annals Glac.* paper). I think you either need to qualify or remove this statement.

Done

Page 3337

Line 18. “model data”? You mean forcing data, or model inputs? It may be difficult, but it HAS been applied to real glaciers, e.g. Favier 2014 for FIG.

We clarified the sentence.

3338

L22-24. Please state where these numbers come from. Note that the resolution requirements will also be a function of bed slipperiness and amount of buttressing (demonstrated in Gladstone et al 2012 *Annals of Glaciology* paper). There may be important real world systems for which coarser resolutions than 500m are adequate.

Done

3340

L6-7. I think this sentence can lead to confusion when introducing the parameterisations. Perhaps it would be more helpful to the reader (such as myself)

less familiar with finite element methods to say that, since the finite element method is being used and C is nodal, C is allowed to vary linearly (in the case of first order elements) through the element, but in fact C is spatially constant for most of the domain (all of the domain except the elements containing the grounding line in fact, since it is constant for all grounded nodes). I would then re-iterate in the paragraph on page 3341, lines 20-26, that integration is of a linearly varying quantity C . The visual representation in Fig 1 is excellent, but I feel this slight enhancement to the explanation would benefit those unfamiliar with finite elements.

We added clarification and specificities for this set-up in the experiment section.

3342 L27 and 3343 L1 please name the relevant variables. Specifically, refer to C and m in equation 2.

Done

3344.

L5 “models” → “simulations”

Done

L7 “runs” → “simulations”

Done

L9-11 I think it would be better to define your steady state criteria as part of the experiment design section (section 3). At page 3343 line 11 I think would be good.

We think that this criteria is not really part of the experiment but more a tool to analyze the results so we kept it in the results section.

3345

L9. “estimate” → “quantification”. This isn’t an estimate for the spread, it IS the spread!

Done

L13-16. This should be merged with the figure caption. The caption should be a concise summary for the reader to understand what is shown in the figure. It doesn’t need to be repeated in the text. The figure should just be referred to here rather than caption information given.

Done

L10 “models” → “simulations”.

Done

L20 “models” → “simulations”.

Done

3346

L11-12. This is not true for NSEP 250m, which does show retreat after the advance.

Done

3347

L5-6 what does the phrase “buttressing from basal friction” mean? Do you just mean the resistance to flow due to basal friction? If so this is not buttressing. Or perhaps you mean that where the perturbed basal friction is reduced the relatively higher basal friction in the other part of the domain has a retarding effect on the more slippery region through long stresses, in which case I think a little more explanation than “buttressing from basal friction” would be helpful.

We changed the sentence to “resistance from the basal friction”.

L4-6. I am not convinced by this explanation, possibly because I don’t fully understand it. I think the key here is the basal friction rather than flux where the model thinks the grounding line is. It is clear from your experiments that if you apply zero basal friction to the first floating element you underestimate grounding line position, whereas if you apply full basal drag over the whole of the first floating element you overestimate grounding line position (ok, you haven’t done this exactly, but you can see that SEP 1 and SEP2 are intermediates to these extremes: SEP 1 has higher basal friction than SEP2 and slightly over estimates grounding line position whereas SEP 2 slightly underestimates grounding line position, referring to Figure 2). Basically, the more drag you apply to the element that should contain the grounding line, the more resistance to motion you impose, the thicker your ice gets, and the more your grounding line will tend to advance.

We agree that the critical aspect here is where the basal friction is applied and rephrased the sentence.

3348

L9-10 I think the other Gladstone 2010 paper (in The Cryosphere) has a better analysis of convergence errors, though I don’t consider it essential to go further into convergence issues in the current study.

Done

L11 please avoid the term “higher” with regard resolution as it can be ambiguous. Please say “coarser” or “finer”.

Done

L12 “verify” → ”satisfy“ (2 counts)

Done

L14 “exhibits” → “exhibit”

Done

L20-23. Do you think this indicates a weakness in the MISMIP3D experimental design, or a fundamental difference between steady state and transient grounding line behaviour in models?

This is a very good question and I am not sure that we have what we need to address it here; it would require additional experiments with different configurations.

3349

L2 please indicate that you recognise that the suggested 2km resolution requirement is specific to this experimental setup and not generally applicable. See also Gladstone 2012 Annals Glac.

Done

L4-5 is it not true that SEP2 always leads to lower basal friction than SEP1? Because they both use the same area fraction for the grounded portion of the element while calculating basal friction, but SEP 1 uses C over that area whereas SEP2 integrates between C and zero. Is that right? It is my interpretation of SEP1 and SEP2 but doesn't seem to be explicitly discussed in the paper, so maybe I misunderstood?

The friction law used in this experiment depends on the basal friction, which varies within an element. So if the integral of the friction coefficient C is the same for SEP1 and SEP2 in this case (as C is constant over the whole grounded area), the integral of basal friction varies as the velocity varies within an element. This is why we did not mention this point in the paper.

L7-9 Why? For fully grounded elements SEP1 and SEP2 are the same. For elements containing the grounding line SEP2 will always give lower friction

than SEP1. So why should there be greater difference between the two with spatially varying basal friction coefficient?

We agree that for fully grounded elements, both SEP1 and SEP2 are the same. For the elements that are crossed by the grounding line, SEP2 have either lower or higher friction than SEP1 depending on the distribution of the friction coefficient within the element, so if the distribution of friction has large variations, we expect the difference to be larger.

3350 final paragraph. This is a very interesting discussion. I don't know of anyone who has yet worked on a SEP for the contact condition in a Stokes Flow model. Could be important for the future. It might also be worth considering that different basal drag parameterisations could lead to easier grounding line migration (e.g. Gunter Leguy 2014 TCD).

We are not aware of any SEP for full-Stokes flow.

3351

General comment on conclusions. Remember that these results were achieved with an SSA finite element model using the MISMIP3D setup, and may be specific to those conditions. I think it reasonable to generalise up to a point, as these results are similar to other studies with different model types. But the conclusions read as though you are presenting new general conclusions. But really you are presenting new conclusions specific to SSA and your FE SEPs, which are consistent with existing results in supporting more general conclusions across model types. I would suggest subtle rewording along these lines.

We rephrase the conclusions following the suggestions of the reviewer.

L17 again the 2km is specific to the setup up. Please qualify this statement or remove it.

Done

Tables and Figures Table 1 and 2: "15" or "fifteen"? I don't mind but be consistent. I think better to say "simulations" than "models". ISSM is the model.

Done

Figure 2. I think you could add SEP 3 to fig 2. Of course you would need an extra x-axis (perhaps place it at the top?) and the axes would not be directly comparable, so maybe you would prefer to place it in a separate subplot. But one way or another I would love to see the convergence of SEP3 plotted.

Results from SEP3 are presented on figure 7. This figure shows the convergence of SEP3 with the order of the integration.

Figure 3. Add to the caption that where the black line is not visible this means it is overlain by the blue line (if that is indeed the case?). Clarify in the caption that the blue line is the new steady state position after the forcing perturbation has been reset.

Done

Figures 3 and 4 should really be one big figure if such a large figure is allowed.

We agree with the reviewer that one big figure would be much better. The format of TCD (landscape paper) did not allow us to do it but it will hopefully be fixed with the TC format (portrait).

Figure 5. Caption. “green” -> “blue” or “teal”. Can you please clarify the direction of the time axis: is it reversed for the blue/green lines? In other words, after the perturbation evolution is in the direction from $y=100$ to $y=0$? If this isn't the case then I can't understand why the blue curves don't start from the same x values as the final position of the red curves. If it is the case then please make this clear in the caption.

Done

Figures 5 and 6 should really be one big figure if such a large figure is allowed. Figure 7. Blue stars is SEP3? Why not label it as such?

We agree that this would be better as for fig. 3 and 4.

2 Reviewer #2 (F. Pattyn)

General appreciation

This is a timely paper that investigates how numerical models can be improved to capture grounding line migration based on the intercomparison work of MISMIP and MISMIP3d. The participating model HSE1 in the latter intercomparison (Pattyn et al. 2013) did show a behaviour that was not possible to explain at that time, and the work presented by Seroussi et al. gives (i) a clarification on this and - more importantly - some solutions as how to improve its accuracy and performance. Since the MISMIP3d intercomparison other people have investigated the possibility to improve both accuracy and performance. One of these is due to Feldmann et al (2014) - Journal of Glaciology, a paper that not only should be referred to, but also needs closer attention to put the results of Seroussi et al in a wider context, since it is not the first paper that explores grounding line interpolations in planview (vertically integrated) models. At the time of MISMIP3d very few (if not only one) model(s) used grounding line interpolations, which limited conclusions beyond recommendations of grid

resolution. The advent of interpolation studies show that alternative techniques may aid at obtaining solutions for coarser resolutions. However, in this sense, and interpolation can be regarded as locally increasing spatial resolution by subdividing a mesh into sub-elements.

We thank the reviewer for suggesting to compare our results with [*Feldmann et al.*, 2014]. This is now included in the new manuscript.

I was quite intrigued by this paper, and more so by its promises. The abstract clearly mentions that “Our simulations explain why some vertically depth-averaged model simulations exhibited behaviors similar to full-Stokes models in the MISMIP3D benchmark”. However I did not find any clarification beyond the fact that we are dealing with a numerical artifact. Moreover, it fortifies my belief that this is pure coincidence.

We agree that the agreement of the grounding line position with the full-Stokes models is probably a coincidence even though our results suggest that it might be linked to the absence of sub-element parameterizations in full-Stokes models, but this should be further investigated before any conclusions can be drawn. We clarified the text to remove this ambiguity.

This brings me to another point that is stated in the abstract: “The results reveal that differences between simulations performed with and without sub-element parameterization are as large as those performed with different approximations of the stress balance equations and that the reversibility test can be passed at much lower resolutions than the steady-state grounding line position.” This statement argues that numerical noise is of the order of magnitude as the effect of stress approximations and that the difference can only be reduced by making use of sub-element parameterization. Here we need to make a clear difference between approximating a physical process and the way it is numerically approximated. Both are two different types of approximations: the goal of a numerical model is to be free of numerical bias and to demonstrate that the physical model (represented by full Stokes or any of the approximations to this system) is accurately represented/captured. This was the focus of the Pattyn and Durand paper (2013): by only selecting models that were free of numerical biases, i.e., that not only showed reversibility but also took the finest spatial resolution computationally possible resulting in smooth grounding lines (void of numerical noise - see the selection criteria in the Supplementary Material of that paper), it was clear that a distinction between physical approximations could easily be made. Therefore it was possible to investigate what the impact of physics is on grounding line migration. So, it is not only a question of spatial resolution, it is also question of having a stable numerical solution.

This is a very good point. However, we show here that all grounding line parameterizations do converge as we refine the mesh, but the final positions of

the grounding lines are different. It is therefore not a problem of numerical noise or stable implementation. All the parameterizations presented in this paper are stable and the steady-state grounding line converges towards an infinitely refined grounding position at fine mesh resolution. However this converged grounding line position varies depending on the sub-grid parameterization chosen, leading to very large differences, comparable to the spread of results obtained with different approximations of the stress balance solutions. The question of sub-grid parameterization is still open for full-Stokes models in particular, as no sub-grid parameterization exists today and we do not know the impact it has on model simulations.

In this respect, the authors should reformulate their abstract/discussion/conclusions

and make numerical noise reduction their ultimate goal. The reversibility test is only a parameter amongst so many that helps at improving our understanding of grounding line migration. It should be clear that “passing the reversibility test” does not make your model correct. Furthermore, the conclusions should be put in the light of “this particular experimental setup”. Since other setups are not tested, we don’t know whether the slightly convex grounding lines are representative of capturing grounding lines in Antarctica (nevertheless, Favier et al (2014) have shown that model numerics did have a minor effect compared to model physics in their simulation of Pine Island Glacier with three different approximations to the Stokes equations). One should remain very careful.

We rephrased the discussion and conclusions to clarify that reversibility is only one of the possible tests and that our conclusions are based only on one particular set-up. However, as mentioned above, using sub-grid parameterization is not just a problem of noise reduction, and is actually very relevant to full-Stokes models, as no sub-grid parameterization exists today for this case.

Detailed remarks

Page 3341: “... in the reset of the manuscript”: remainder of the manuscript.

Done

Page 3343, last line: I wouldn’t state “usually associated with”. Only the MISMIP papers put spatial resolution forward as a solution due to the lack of other measures. You could be precise and specifically mention that those papers demonstrate that increase in spatial resolution improved the accuracy of the solution.

Done

Page 3344: Results section, first sentence: add “, respectively” at the end of this sentence

Done

Page 3350: line 17-20: this is only valid for this particular setup of the experiment and cannot be generalized.

Done

Figures 3 and 4: make coloured lines thicker

Done

3 Short comment #1 (A. Levermann)

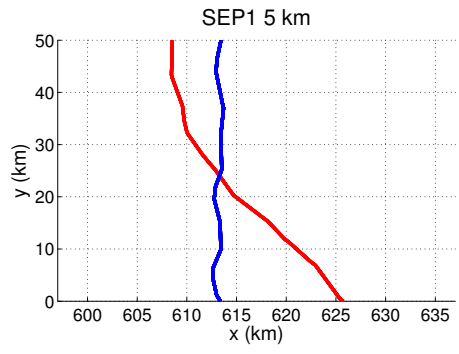
Perhaps our paper Feldmann et al. Resolution-dependent performance of grounding line motion in a shallow model compared to a full-Stokes model according to the MISMIP3d intercomparison Journal of Glaciology 60 (220) (2014), 353-360, doi:10.3189/2014JoG13J093. would be of interest for this study. Cheers, Anders

Done

4 Short comment #2 (G. Durand)

“We expect our results to be similar for higher-order (HO) models”. This would be, to my opinion, very valuable to demonstrate this assertion. This all the more important, that the authors usually do not use an SSA model but the Batter-Pattyn for simulation of actual glaciers (e.g. <http://www.the-cryosphere-discuss.net/8/1873/2014/tcd-8-1873-2014-discussion.html>).

We tried to run a couple experiments with a higher-order (HO) model to strengthen our conclusions and demonstrate the accuracy of the HO model. However these runs are computationally intensive if we want to start with a 1 m thick ice and not bias the model with the initial conditions. We were able to run a few simulations at 5 km resolution. Results are similar to SSA with a steady-state grounding line located at 213.8 km, 613.4 km and 503.9 km respectively for NSEP, SEP1 and SEP2. This is respectively 26 km downstream, 18 km upstream and 45 km upstream of the corresponding SSA models. Reversibility was achieved for the both SEP1 and SEP2 (see figure below in the case of SEP1). It is however impossible to extrapolate these results and difficult to draw some strong conclusions using simulations performed at 5 km mesh resolution only.



References

- Feldmann, J., T. Albrecht, C. Khroulev, P. F., and A. Levermann, Resolution-dependent performance of grounding line motion in a shallow model compared with a full-Stokes model according to the MISIMIP3d intercomparison, *J. Glaciol.*, *60*(220), 353–359, doi:10.3189/2014JoG13J093, 2014.
- Pattyn, F., et al., Results of the Marine Ice Sheet Model Intercomparison Project, MISIMIP, *The Cryosphere*, *6*(3), 573–588, doi:10.5194/tc-6-573-2012, 2012.