TCD review

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.

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 mis-understand p-refinement?

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

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?

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.”

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?

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.

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 PIG.

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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.

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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.

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

3344.

L5 “models” -> “simulations”

L7 “runs” -> “simulations”

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.

3345

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

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.

L10 "models" -> "simulations".

L27 "models" -> "simulations".

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

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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.

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.

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.

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

L12 "verify" -> "satisfy" (2 counts)

L14 "exhibits" -> "exhibit"

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?

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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.

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?

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?

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).

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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.

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

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.

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.

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.

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

Figure 5. Caption. "green" -> "blue" or "teal". Can you please clarify the direction of te 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.

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?