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Interactive comment on "Representation of basal melting at the grounding line in ice flow models" *by* Hélène Seroussi and Mathieu Morlighem

R. Gladstone (Referee)

rupertgladstone1972@gmail.com

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The paper addresses the impact of schemes for handling sub-shelf melting in elements containing a section of grounding line in ice sheet models. The main points are that fairly strict resolution requirements might need to be imposed in order to provide a converged result in the presence of high melt rates near the grounding line, and that applying melt over partially grounded elements when resolution is not sufficiently fine is likely to give an overestimate of retreat rates and mass loss for a retreating grounding line. The paper is clearly written, the experiments simple and to the point, and the figures show the scientific content very clearly. This is a useful and timely contribution to the ice sheet modelling community. Anyone carrying out marine ice sheet modelling needs to be aware of the main points made by this paper.

No advance experiments were carried out. Of course one cannot conduct advance experiments by starting from a steady state without melting and then imposing melting, but starting from a steady state with high melting and then reducing the melt rate is perfectly viable. If we consider the grounding line convergence issue due to the basal resistance change across the grounding line, some models/parameterisations give better convergence in advance and some in retreat. There may be a similar issue with the melt problem. If you look at the bottom left panel of Fig 5 of Gladstone et al 2017 (also TC) you can see that we observed worse convergence in advance than in retreat in the presence of basal melting with no parameterisation (albeit with a different sliding relation to the ones used here). Of course in the current climate we're more interested in retreat than advance but temporary advance could occur as part of a larger retreat pattern (see also Jong et al TCD 2017 (now accepted for TC), and Torsten Albrecht's work on overshoot (work from last year, not sure if it is published yet, but you can contact him if you want to know more)). The addition of advance experiments would enhance this paper, but then again the paper is worth publishing as is and needs to be brought to the attention of other modellers. So I do not have a strong preference whether advance experiments should be added at his point. But at least add some brief discussion of the implications. Why does Weertman sliding with no melt parameterisation show such wonderful convergence? Perhaps it would be terrible in advance?

Why should experiments with the Tsai sliding law show less sensitivity to melt parameterisations than experiments with the Weertman sliding law? Is it something to do with the different geometries – Weertman having thicker ice and steeper slopes close to the grounding line?

Specific comments

Page 1

Line 4. add -> adds

Line 19. Yields -> leads?

Page 2

Line 16. “impact to” -> “impact on”

Line 21. “except if specified” -> “except where specified”

Line 32. Can you state what temperature this corresponds to?

Page 3

Line 10. The connectivity is between the subglacial hydrologic system and the ocean. Just saying “bed” allows the possibility of a dry bed, which cannot support sliding.

Page 4

Lines 8-11. This means the initial steady state is always approached through advance. This is a good design for retreat perturbations (I also have a paper coming out in TCD in the next few days that discusses multiple steady states and design of grounding line experiments). But you have not said how steady state is defined. You state that all spin up simulations are at least 50ka, which is good and should suffice for a robust starting point, but can you also add a statement about steady state, e.g. “dVAF/dt is less than xx kmˆ3/a in all cases” or similar? [edit: I see you discuss Exp0 further down to look closer at steady state. So ignore my comment about quantifying steady state here, but add a line like “Achievement of steady-state is analysed through Experiment 0 below”]

Page 5

Lines 12-17. Note that “-Xm below sea level” contains a double negative (because the minus sign and the word “below” imply direction) and technically would mean Xm above sea level. You should say “Xm below sea level” or “-Xm relative to sea level (where X is positive in the upward direction)”.

Lines 12-17. Please add the equation for this melt parameterisation, since you’ve shown one for the other parameterisation.

Page 6

Line 2. “resolution” -> “resolutions”

Line 2. Those numbers don’t look right to me, looking at the plot. Did you get the Tsai and Weertman laws mixed up in either the text of the plot?

Line 5. “small oscillations with minimal amplitude” -> “small oscillations”

Page 7

Lines 2-4. This is quite similar to our TC paper (Gladstone et al 2017): the first melt param used here (your exp 1) is similar to the water column scaling we used. Scaling the melt to zero as the GL is approached reduces the resolution dependency.

Line 6. “the type sub-element” -> “the sub-element” or “the type of sub-element”

Line 7. “a mass loss” -> “mass loss”

Page 8

Line 5. “why” -> “whereas”

Lines 6-7. Experiment names are repeated when they are clearly supposed to be different experiments.

Page 9

line 11. I think the point here is not that the melt rates are small generally, but that they approach zero as the grounding line is approached (due to the water column scaling).

Page 10 line 13 to page 11 line 5. Do you think this problem is specific to the finite element method? Steph Cornford essentially predicted these results based on theoretical reasoning a couple of years ago (this was in a conversation, don’t think he published anything like this). He said he would expect any parameterisation that allows melt on the last grounded grid point to overestimate retreat and to give worse convergence than a scheme that only applied melt to the first floating grid point. He mainly uses finite difference or finite volume methods.

Page 11

Line 13. “even other” -> “even over”

Page 12

Lines 11-17. Well, yes, but such processes could well mean that the melt parameterisations are actually closer to reality than NMP, though of course parameterising a tidal grounding zone should also not be resolution dependent.

Line 17. Isn't there a PISM paper that does exactly that – using the grounding zone concept as a justification for inaccuracy. . . not sure if it is constructive to point the finger by citing it though. . .

Lines 18-23. One of the main points is similar to Gladstone et al 2017 (also TC), which used a Stokes flow model: that the convergence is worse, and resolution requirements are stricter, for the case of high melt close to the grounding line. The importance of vertical shear probably depends on choice of sliding law – vertical shear should have a larger impact when using Weertman than with one of the sliding relations featuring a grounded transition zone.

Line 28. “large amount of” -> “large”

line 29. “for a Weertman and a Tsai sliding laws”

lines 29 and 31. Please indicate roughly what “large” and “small” mean here, for the benefit of people who just look at the pictures and read the conclusion!

Figures.

Fig 4. Right panel y-axis label. Minor formatting issue. Large gap in kmˆ2.

Fig 4. I presume the Tsai purple line is hidden behind the green one? i.e. perfect convergence at 250m? You should state this in the caption or readers might think the

purple line is missing. I find it confusing switching between Figures 5 and 7 because the colours have completely different meaning. Could you manage different line types in Fig 7 instead of different colours? Or if you want to stick with colours, can you make the colours different from those in Fig 5? I instinctively see the blue line in Fig 7 and think “ah that's the 2km resolution”...

What I am missing from all Figures is a way to compare the converged result across different melt parameterisations. Of course the converged result should be the same across all melt parameterisations for a given sliding law. But this is hard to compare in Fig 5 because they all have separate sub plots, and Fig 7 shows relative differences. I'm not sure the best way to show this, perhaps a new figure or just a table. . . and of course it may be complicated by the oscillations in the Weertman case. I don't view this as essential, just “would be nice”.

I found myself wanting to look at Fig 7 while looking at Fig 5. Maybe you should swap around Figs 6 and 7 and refer to the convergence plot a bit earlier in the text? Just a thought – I am not insisting on this.

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