Author's response for the manuscript "Assessment of numerical schemes for transient, finite-element ice flow models using ISSM v4.18"

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On behalf of my co-authors, I would like to thank the Editor, Steven Phipps, for handling the review process of our manuscript. We also thank the two reviewers, Stephen Cornford and Daniel Martin, for their positive and constructive comments. The comments helped improve the manuscript and clarify some important points.

Please, see below our responses (in **blue**) for each comment (in **black**).

Attached to this author's response are:

(a) the original manuscript highlighting the modifications (**diff file**).

(b) the updated plots (**new version**).

Response to Stephen Cornford

This paper reports on some worthwhile updates made to ISSM, a well known, widely used and world leading ice sheet model. It makes a systematic comparison of two sets of numerical methods. The first set is of standard methods used across the FEM world to deal with advection dominated problems, the second set is of more ice-sheet specific modifications to the numerical treatment of the driving stress. I found the paper straightforward to read and complete in its presentation, and it seems a good match for GMD.

We thank reviewer, Stephen Cornfornd, for his positive comments and support for publication.

1 General Comments

1. The paper gives the impression that modifications to the driving stress are not known in the community. In particular, line 75

   'While there exist comparison studies for basal friction and basal melt parameterizations (e.g., Seroussi et al., 2014a; Seroussi and Morlighem, 2018), little attention has been given to the sub-element parameterization of the driving stress.'

   Little perhaps, but not none. Modifications made for essentially the same reason as here are reported at least in Cornford et al 2013 (sorry to bring that up, but I do know the paper fairly well) and I think in Feldmann et al 2014

   Thank you for drawing our attention to these studies, we should have mentioned these in the first version of the draft. Please, see lines 80, 270, 502, 505 (diff file).

2. The word ‘convergence’ is used in a too-imprecise fashion throughout the paper. For example, ‘better convergence’ appears in a few places, but it is possible to be specific. If the error estimate is *ah^n* to leading order (*h* is the mesh spacing), then better convergence could mean that *a* is smaller, *n* is larger, or that the error estimate enters the asymptotic region at larger *h*. Elsewhere, there are phrases that suggest that something has converged, which is common in other papers but I would say, inaccurate. Finite portions of sequences may appear to be converging, but you can’t say more than that without analytic proofs.

   Thank you for pointing this out. We rephrased several sentences in the Results and Discussion Sections, as well as in the Final Remarks. For example, see lines 438, 494, 498, 588, 591 (diff file).

3. All of the convergence plots are log-linear. Figure 3 at least should be a log-log plot since it deals with positive definite quantities. With other figures, I would've liked to see error estimates rather than raw numbers (e.g. ∆VAF), again as log-log plots. Otherwise it is difficult to assess convergence beyond a general sense that ‘it looks like it is converging’. You can always look at |f(h)−f(2h)| (e.g for f= ∆VAF).

   This is a very good comment. We have kept the original plots, absolute values in linear scale, and we have added two panels to the plots showing the relative error (|f(h)−f(2h)|/|f(2h)|) in log-log plot. Please, see the additions in figures 3, 6, 8, 11, 13, and 15 (new version). See also the definition of the relative error in line 415 (diff file).

4. ‘Recommending’. The paper ends with some recommendations. Perhaps this is just a personal gripe, but to me, scientists report their findings, managers recommend actions.

   We rephrased some of the paragraphs, highlighting the findings rather than recommending specific schemes. See additions in lines 88, 518, 593, 596, 604, 611 (diff file).

Response to Daniel Martin

1 Overview

This work provides an overview of some numerical developments in the ISSM finite-element (FEM) framework designed to improve ISSM’s treatment of thickness advection. In general, naive implementations of FEM schemes can have difficulties with hyperbolic advection, suffering either from oscillations or instabilities as a result. Over the years, a number of approaches to improve FEM performance for hyperbolic transport have been developed, mostly for the context of high-speed compressible flows with shocks. In this work, the authors implement a range of such schemes and test them for a set of representative ice sheet modeling examples. The presentation is thorough and includes a good overview of the basic ideas in play in this work. The examples do a reasonable job of demonstrating the effectiveness of the various approaches. I should be noted that many of the schemes were developed in the context of compressible flow scenarios in which discontinuities and shocks are common. Such methods are potentially not well-suited for ice flows which don't display a tendency to steepen into shocks, so this work will occupy a useful place in the literature.

We thank reviewer, Daniel Martin, for his detailed comments, as well as for supporting the publication.

2 General Points

It would be helpful if you also included the current baseline ISSM results (i.e. using none of the treatments described in this work) as a comparison to demonstrate the usefulness of these approaches. If one of these is the current standard in ISSM, you should state that.

This is a good point. The default scheme was the "Artificial Diffusion" method. We have added a statement in line 127 (diff file).

The sub-element driving-stress parameterization is similar to that used in BISICLES (Cornford, et al, 2013) and in PISM (Feldmann, et al, 2014), which use one-sided differences to compute surface gradients on each side of the grounding line. You should cite these (I realize I'm a co-author on one, but they are relevant here). The basic idea is that the discretization of the basal friction should match the discretization of the driving stress – the discontinuity in the basal friction gets matched by a discontinuity in the driving stress.

Thank you for drawing our attention to the driving stress parameterizations. Although we were familiar with these papers, we didn't pay attention to these specific implementations and they are very relevant to our discussion. Please, see changes on lines 80, 270, 502, 505 (diff file).

The experiment in which you apply melt in partially-grounded cells to produce a response is a bit problematic, since you're essentially basing the experiment on a numerical error which converges to zero as you refine the mesh. It reveals the sensitivity of the different schemes to numerical errors, so it's useful, but I think you should clarify that it's not a reasonable choice for realistic marine ice sheet simulations (which was the conclusion I took away from Seroussi and Morlighem, 2018).

Totally agreed. We have added this experiment to show how problematic applying melt on partly floating cells (elements, in our case) for different stabilization schemes can be. For artificial diffusion for example, the amount of VAF loss is much higher than for the other schemes. And it seems that there is no "convergence" at all, at least for the finer resolution we have used here. Please, see lines 336, 527 (diff file).

In general, plots of convergence work much better as log-log plots, since straight lines indicate consistent rates of asymptotic convergence. This is most relevant for plots like those in Figures 3, 6, 8, 11, 13, and 14. You're currently using semilog plots, which is helpful, but not as effective as log-log plots would be. This is natural for plots of errors (figures 3), For cases where you're demonstrating convergence to a particular value (Figures 6, 8, 11, 13, 14), it can possibly be more useful to plot either difference from an "exact" value, or even just the change between the the current value and the value from the next-coarser mesh (i.e. $\varphi_h - \varphi_{2h}$).

We agree. However, we think that it is important also to show the absolute values towards which the models are "converging to" (e.g., different stabilization schemes could be "converging" to different values). We have added new panels showing the relative error (|φ_h− φ_2h|/|φ_2h|) to these figures. We added a new plot to show the relative error on grounding line positions, preserving plot 14 as previously shown. Please, see figures 3, 6, 8, 11, 13, and 15 (new version). See also the definition of the relative error in line 415 (diff file).

Another general suggestion – I think it's clearer if you use "finer" and "coarser" when describing resolution (rather than "higher" and "lower").
Indeed. We changed to finer and coarser. Lines 398, 399, 406, 421, 447, 464, 516 (diff file).

Also, it would be useful if you showed examples of meshes and solutions for the example problems in the supplementary material.
This is a good idea. We have added a few examples of meshes (structured and unstructured meshes) in the Appendix B (new version).

3 Specific points

1. line 4: The ice thickness evolution equation isn't strictly hyperbolic. Because the ice thickness appears in the momentum equation used to solve for the ice velocity, it acts more like an advection-diffusion equation. There is advective transport (fast sliding), but also diffusion of thickness. This is partly why implicit methods are so useful.
   Agreed, we modified that term to "advection-diffusion equation" to avoid misunderstanding. Line 4 (diff file).

2. line 39: "Wiggles" is fine, but "oscillations" is a more-standard term…
   In fact "wiggles" was an old term used by the FEM community. We changed it to "oscillations". Line 40 (diff file).

3. line 55: "flux corrections"→"flux-correction"
   Done. Line 56 (diff file).

4. line 116 (or so): "integrating by part"→"integrating by parts"
   Done. Line 121 (diff file).

5. Section 2.5: A potentially simpler way to think of the Zalesak FCT scheme is as a hybridization of a higher-order (but potentially oscillatory and non-max/min preserving) scheme and a lower-order diffusive (but max/min-preserving) scheme. In smooth regions, you use the higher-order scheme. Where necessary (because the higher-order scheme produces new max/min values and therefore oscillations), you fall back to the lower-order scheme, but you add just enough of the flux from the higher-order (anti-diffusive) scheme to make things as accurate as possible without inducing oscillations (via the creation of new maxima or minima).

Thank you for this didactic explanation. We have added a description to help readers unfamiliar with the FCT scheme. Line 196 (diff file).

6. line 256: As I mentioned, you should cite the BISICLES and PISM references here for completeness.
Agreed. Line 269 (diff file).

7. line 279: The free-slip condition isn't just no normal flow, but also Neumann conditions on the tangential velocities: ∂vx∂y= 0.
Good point. We impose dvx/dy=0 in the weak formulation of SSA. It naturally appears in the integration of the stress over the domain boundary.

8. line 300: Note that the steady-state initial GL position for SSA in MISMIP3D is downstream of that produced by higher-fidelity schemes (full-Stokes, Blatter-Pattyn first-order, L1L2, etc). I'd suggest specifying that the projected GL location of 600km is only true for SSA models.
Good point. Thanks for reminding us of this. We have added a few words in line 312 (diff file).

9. line 338: You refer to the "Amundsen Sea Sector", but then use the (common) abbreviation "ASE". (I'm guessing you don't want to use the abbreviation for "Amundsen Sea Sector", however)
In fact, it was a "mix" between "Amundsen Sea Sector" and "Amundsen Sea Embayment" terms. To avoid confusion, we changed "Amundsen Sea Sector" to "Amundsen Sea Embayment" everywhere, and we keep the abbreviation "ASE".

10. line 385 (Figure 2): The convergence of ice speed in Figure 2 at the grounding line and ice front tells a consistent story in the under-resolved (5km and 2km) cases – too much friction near the grounding line (SEP 1) means that GL ice velocities are too slow. Reducing friction (SEP2) but not properly discretizing the driving stress (NSED) means an unbalanced driving stress and thus a too-high speed at the GL. If they're discretized consistently (SEP2+SED2), then they achieve something closer to the correct balance even in the under-resolved cases. Something similar is happening on the floating side of the GL, propagating out to the ice front speeds.
This is true. To clarify, we added something similar in the Discussion section. See line 500 (diff file).

11. Figure 3: As mentioned above, this figure would be much clearer as a log-log plot because it would make convergence regimes and their associated convergence rates apparent.
We agree. We have changed to log-log plot. We have also used another reference model with finer mesh (50 m) to improve the convergence computation. Please, see Fig. 3 (new version) and lines 384, 392 and Legend of Fig. 1 (page 17, diff file).

12. line 396: I think that “2km" is more compact and clearer than “2000 m"
We agree. Line 421 (diff file).

13. Figure 5: It would be helpful if you rescaled the y-axis so that the lines used more of the plot range. (something like [-200:200], perhaps?)
Done. We have also rescaled other figures to improve the visualization.

14. Figure 6: this would also be more effective as a log-log plot – perhaps using absolute values? The few coarse-resolution positive values are less important than the convergence tendencies as you refine your mesh…
We inserted two new plots on Fig. 6, which show the convergence of relative error of VAF changes. The original panels are kept as before on top of the new ones.

15. Figures 6, 8, 11, and 13 would be more accessible if you used the same color legend for the right and left plots – it would make comparing them much simpler.Alternatively, you could keep the red-blue colors and collapse them onto a single(larger) plot.
Agreed. We changed to the same color legend. We also added two new panels for each figure showing the convergence of relative error. Please, see Figures 6, 8, 11 and 13 (new version).

16. line 425: “1,3000"→“1,300"
Thanks, it was a typo. Line 458 (diff file).

17. line 147: What exactly were the convergence issues for the DG implementation? Is the momentum solve not converging? Something else? It’s perhaps not surprising that it’s potentially not as robust as implementations which have seen a lot more use.
DG generated spurious oscillations in ice thickness, which caused issues with the stress balance solver. We have changed to spurious oscillations in line 470 (diff file). We believe there is still improvement to be done to make DG more robust in ISSM.

18. line 397 (and elsewhere): You mention that artificial diffusion helps provide stability in the presence of strong discontinuities and shocks (and then mention that DG and SUPG may produce oscillations in the presence of shocks), but it’s not clear to me how relevant that really is for ice sheets. It’s likely that methods developed to provide stabilization in flow regimes which include shocks and discontinuities are overkill for the (presumably) less-demanding ice-sheet case.
Indeed. We believe that there is room for development of schemes that take into account the nature of ice sheet flow. It seems that the diffusions generated by Artificial Diffusion and Streamline Upwinding are excessive in ice sheet simulations. We added additional discussion of this aspect in lines 554, 585, 613 (diff file).

19. line 518: “when Backward Euler approach"→“when the Backward Euler approach"

Done. Line 576 (diff file).

20. line 519: " solution using DG scheme"→"solution using the DG scheme"
Done. Line 577 (diff file).

21. line 526: This would be a good place to point out that ice sheets don't see the shocks and discontinuities that most of the stabilization schemes were designed to handle.
Agreed. Please, see lines 554, 585, 613 (diff file).

22. line 535: "recommend to avoid"→"recommend avoiding"
We removed the word "recommend" and rephrased that part. Line 597 (diff file).

23. line 535: "as in all transient"→"as all", or perhaps "because all"
We rephrased this part. Please, see line 597 (diff file).

24. line 543: "strong recommend"→"strongly recommend"
We rephrased this part. Line 605 (diff file).

25. line 543: "although a carefully attention"
We changed to "although careful attention". Line 606 (diff file).