Review: Numerical stabilization methods for level-set-based ice front migration

Gong Cheng et al., Geoscientific Model Development, 10.5194/gmd-2023-194
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General comments

This study by Cheng et al. concerns itself with investigating the performance of various schemes of numerical stabilization and reinitialization for a level-set method in an ice flow model. Level-set methods are commonly used in ice flow modelling to track the migration of the ice front in response to the ice velocity, and rates of calving and frontal melt. The ice front is defined at the zero contour of the level set function, the motion of which is controlled by an advection equation. This study relates to stabilization and reinitialization procedures applied to the level-set method in two commonly used FEM ice flow models, ISSM and Úa. The authors assess the accuracy of the procedures by applying different combinations of stabilization method and reinitialization interval to an idealized test case with a known solution.

This study is important and novel, and will be a valuable addition to the literature. It has broad application to the field of ice sheet modelling, especially modelling of the outlet glaciers of the Greenland Ice Sheet where ice front migration is a crucial component of the ice flow dynamics. The results of this study demonstrate the importance of the choice of stabilization method and reinitialization interval in minimizing errors.

In general I find this study to be well written and concise. However, I did find some areas where the model description or justification for certain experimental choices wasn't entirely clear, and further detail is required for the sake of clarity. I also identified some questions and areas of interest that I believe could benefit from some further elaboration. Detailed comments are provided below. I am happy to accept this manuscript for publication subject to minor revisions.

Specific comments

Lines 23-26 – There are two sentences here dealing with calving laws and calving rates. The abstract mentioned that the discontinuous nature of calving poses challenges. However this isn't elaborated upon in the main body of the article. Could you include a brief comment here about the implementation of discrete calving laws vs continuous calving rates in models?

Lines 35-37 – I have a few comments about this sentence. Firstly, it would be better to refer each reference to the stabilization method directly. Secondly, it might be preferable to introduce the acronyms for the stabilization methods later, e.g. in the introductory sentence for Section 2.1, since these three methods plus one extra are the methods applied in this study and the acronyms become the experiment names. You might also consider whether this sentence is a redundant in the introduction and whether it should be replaced with a better introductory sentence for Section 2.1. This comment links to the following comment about the structure of Section 2.1.

Section 2.1 – The structure of this section needs a bit of reworking for the sake of clarity, to more explicitly state what the four methods applied are. Upon my first readthrough I was left with the impression that only three methods were going to be applied, and only realised my mistake when I got to line 105. In particular, the introductory sentence is very weak. I don't like to see "etc" in a formal paper. The first sentence should be restructured to explicitly state what the four methods are that will be described in this section, and introduce their acronyms. The descriptions of the methods in the section are generally fine, but care needs to be taken to make it clear that SUPG and SUPG+FAB are distinct methods. Finally, could you state more clearly which experiments are carried out using ISSM and which use Úa. This distinction isn't made except that the FAB method is only applied in Úa. When looking at the results later, it isn't clear which results were derived from Úa and which from ISSM. It would be helpful to include a brief note explaining why the comparison of results derived from two different models is still valid. It may be helpful to include a summary table for this section, but it isn't necessary.

Section 3 – This section could benefit from some more detail on the experimental design. In particular, could you define the bedrock and ice geometry? I understand that given the prescribed velocities these aren't as crucial as in e.g. a MISMIP-style design, but it's not clear from the description which part of the domain is initially ice-filled and which isn't.

Line 128 – What is the justification of applying three distinct velocity profiles? The uniform profile should preserve the front shape during advance and retreat while the other two will warp it. Is this the reasoning? If so, why not just two?

Lines 134-135 – Similarly, why apply two different velocity constants? Is there an *a priori* expectation that the errors will scale linearly with velocity?

Line 143 – Is there a benefit to fully reversing the velocity field to mimic advance and retreat, as opposed to having a constant flow direction and applying a time-varying calving rate to achieve the same end?

Section 4 – As mentioned previously, it's not clear which results were produced using Úa or ISSM. However, I think this is best remedied with a change in Section 2.1.

Figure 2 – Consider a minor rewording to the caption to say "numerical solution".

Figure 3 – Same as for Figure 2.

Lines 149-150 – For n_R = 1 the error is visibly non-symmetric in y, which isn't the case for n_R = 100. Is there any significance to this?

Line 164 – There is also visibly less sensitivity to n_R for v_0 = 5000 m/a c.f. v_0 = 1000 m/a.

Section 4 – I would be really interested to see somewhere in this section timeseries plots of the evolution of evolution of the total absolute misfit area, either for all the experiments or a selection of them. Does the error increase linearly or exponentially throughout the runs? Does it increase smoothly or do we get abrupt increases associated with the reinitialization interval or the annual cycle?

Lines 183-185 – Does this explain why there is less sensitivity to n_R for the high-velocity scenario? (See my comment re: Line 164)

Lines 206-209 – In the previous paragraph, it's mentioned that the errors scale proportionally with mesh spacing for AD and SU. Could you add an equivalent statement to this paragraph about the mesh spacing dependency of the errors in SUPG, for a more direction comparison against AD and SU?

Section 5.3 – This section seems a bit vague in its conclusions. Is it the form of the velocity profile that matters, or is it just the mean frontal velocity? If the different velocity shapes defined in Table 1 were scaled such that the mean velocities were the same, would we expect differences in the errors to vanish? Given the similarity in results, I'm not convinced that this comparison really enhances our understanding in any meaningful way. If the authors don't wish to completely remove this comparison, it could be simplified by comparing just two velocity profiles rather than three. However, I'm happy to leave this choice to the discretion of the authors.

Additional comments

The following comments refer to some questions that occurred to me while reading the manuscript which relate to possible extensions of the study. While these could be answered by carrying out additional experiments, I don't expect the authors to carry out those experiments, and my response to revisions isn't contingent on any additional experiments being run. As such I leave it to the author's discretion how to respond to these questions.

In *Line 164* it is mentioned that all stabilization methods overestimated the ice front advance. If instead the velocity time-cycle were reversed such that the negative velocity is applied first, would we expect to see overestimated retreat rather than advance?

The dependency on mesh spacing is discussed in *Section 5.2*. Were experiments with varying mesh spacing carried out? It would be interesting to see how the errors in the different schemes scale in response to the mesh spacing.

The test case was constructed with simple flow in the one dimension only, and no along-flow gradients. Do the authors think that their conclusions would translate directly to the more complex flow fields in realistic scenarios? Should we expect to see similar relative errors between the different stabilization methods in more realistic scenarios?

Technical corrections

Line 50 – Correct "Method" to "Methods"

Line 64 – Please reference equations as "Eq. (3)" (mid-sentence) or "Equation (3)" (beginning of sentence). There are numerous other examples of this throughout the manuscript on lines 65, 68, 76, 77, 82, 85, 96, 147, 153, 181, 196, 204, 206 and 207. Please correct these and any other I may have missed.

Line 68 – Acronyms have previously been defined. See previous comments on Section 2.1.

Line 76-77 – This sentence is awkward with too many clauses. Please revise for readability.

Line 92 – "For even values of *p*" reads better at the start of this sentence.

Line 93 – The FAB acronym was already defined previously.

Line 104 – "we will" reads better than "we are going to".

Lines 119 & 120 – Capitalize "Section". Please do the same for any other examples of this that I may have missed.

Lines 149, 150 & 215 – Please refer to "Figures" when there are multiple. Please do the same for any other examples of this that I may have missed.

Line 216 – Insert "and" before "both".

Line 220 – It would be better to start this sentence with "However" instead of "Although".

Line 234 – Remove repetition of “with”.

Line 233-234 – This sentence needs a bit of revision for readability