Review for "A numerical framework for modeling iceberg calving and ice-front migration of grounded glacier tongues" by Prasad et al.

This manuscript introduces an implementation of the level set method (LSM) in the glacier model IGM. It is written as a module which can be coupled to the main model code, and specifically uses the eigencalving law. Initial tests on synthetic and realistic geometries are presented to demonstrate the functionality and usability of the module. The module is shown to implement the LSM well, preserving calving front shapes in both advance and retreat without introducing numerical artifacts.

It is good to see more ice models incorporating calving in recent years, as it has been underrepresented or entirely missing for too long given the importance of the process. I applaud the ambition and the work which has gone into implementing this into IGM. The LSM itself appears to be working very well within this framework, although there are some limitations to the current setup (as acknowledged by the authors) especially since it does not consider floating ice shelves. I'm sure these limitations will be overcome with further work, and I look forward to seeing it!

The structure of the paper is effective, starting with explaining the level set methodology, then moving through a simple circular test case to a synthetic domain to a realistic geometry. However, I find the manuscript in its current form to be light on details and lacking clarity in places. As such I find it difficult to understand why certain choices have been made in setting up the domains and experiments presented within. I am particularly unsure about the synthetic experiment which claims to simulate a marine-terminating glacier on a bed which is prescribed to be entirely flat at a very shallow depth of 10m, rather than continuing the fjord-like bed shape into the "ocean" part of the domain. I think that more attention needs to be given to explaining or justifying this setup, or perhaps it should be done differently.

I would recommend publication only after revisions have been made to improve clarity and particularly to address the concerns I have about the synthetic experiment.

Specific comments

Lines 24-6: Could these examples be explained a little more? It seems odd to have frontal ablation alone contributing twice the overall mass loss. Is this due to large positive surface accumulation counteracting the loss through ablation?

Lines 90-1: How do the IGM and classical solvers differ, and why was the choice made to use the classical one? This needs some explanation.

Line 92: Could you provide this function?

Line 103: Some explanation of how this "fast marching method" works would be welcome, or at least a reference. From a quick search I assume this is the method of Sethian (1995)?

Line 109: To be clearer, you could just specify that v is the velocity perpendicular to the ice front, rather than adding an extra sentence.

Line 114: What width do you choose for this band in your implementation? Is it applied as a distance, or a function of the grid resolution?

Line 129: What specific physical properties is it supposed to represent?

Line 142: What is the time step?

Line 155: What happens to the ice velocity on newly formed ice cells? Is it also extrapolated, or does the ice enter with zero velocity?

Line 160: It isn't clear to me why the first synthetic case was relegated to an appendix. It's a very nice simple demonstration of the algorithm working and I think it would fit well as a section here in the manuscript.

Line 174: I'm not sure of the reasoning behind this limitation on the geometry. In what way is the area below sea level treated as water, if the setup of your domain means that the entire thickness of the ice (minus 10m) remains above sea level, creating unrealistically high surface elevations if the intention is to simulate a MALT glacier coming into contact with a body of water. Why not have the topographic channel extend below sea level, or at least not be entirely flat at -10m?

Line 175: Could you include the parameterisation? Also, it would be useful to include the previously introduced symbols when giving the parameter values (z_{ELA}=200 etc.)

Lines 179-80: What is the reason for not including flotation?

Line 183: Should this be v_f= -200ma^{-1}, if you are moving the front backwards?

Line 188: Where are the SMB values zero? Just on the first element upstream of the calving front, or across the whole of your narrow band?

Line 193: A reference to this figure earlier on might be useful (in S.2.1 maybe, since you refer back to it here)

Line 208: Could you state the depth here, as you did for Kronebreen?

Line 217: Why pick a distribution which exceeds the observed range, rather than finding one which gives frontal velocities in the middle of the range (~1000ma^{-1})? Using the data assimilation framework in IGM would presumably also provide a solution close to observed velocities. What reason was there not to use it?

Line 218-9: Why is enhanced basal sliding desirable for all areas below sea level? Is this an attempt to represent some physical process?

Lines 223-31: How exactly was this calibration done? Did you run the model with several different values of K_{ec} over the two periods you've defined?

Line 225: Does this data need a reference?

Fig. 3: I would personally find it useful to see Panel C here overlaid on some data (bed or surface elevation would be good), rather than the satellite imagery, to get a better picture of the physical setting.

Lines 254-6: While I agree overall that the level set is working well, I don't think the velocities in this experiment are a great demonstration of it. It looks as if there is almost no velocity beyond the initial front position. Friction coefficient is mentioned as an explanation for this, but I don't think it was stated what the basal friction was in the synthetic case. Are you using higher friction downstream of the initial front? Details of this should be added into S.2.2. Could it also be related to the ice front being advanced at a much faster rate than the ice is actually flowing? The initial velocity profile in Fig.4 is rather odd, due to the bump in surface elevation caused when transitioning from the sloping fjord to the unrealistic flat "seabed", and I think it is notable that the profile is not returned to on the retreat phase, and nor is the thinner glacier front. I think the bed geometry and initial state of this experiment need to be improved.

Line 265: What is meant here by "not controlled by the calving module"? Are you not applying the calving algorithm in this location?

Line 287: If not using reinitialization does affect the accuracy, there must be some relationship with the frequency of applying it. Presumably the frequency at which a difference would be noticed is just higher than your test cases. You should state the range of frequencies have you tested.

Line 302: "ice-land/ice-free interface" is not a clear term to me. Are you referring to the grounding line?

Fig. 4: It would be nice to put Panel A on the same *x*-axis as the other panels.

Line 410: This criteria sounds like it should have a reference.

<u>Typographical corrections (not a complete list)</u>

Line 101: Italicise the variables *x* and *t*.

Line 128: No need to repeat K_{ec} in brackets.

Line 160: I think there should be a paragraph break here, to make it clear you are starting to talk about a new domain.

Line 177: Quotation marks formatted incorrectly.

Line 186: "interface"

Line 234: "use*s*"

Line 244: "continue*s*"

Line 281-2: Would be clearer to write “The numerical inconsistencies of the level set advection mainly come from…”

Line 328: “Although *the* eigen calving law…”

Line 344: “…enable physically consistent simualtions…”

Line 350: “…assumed that glaciers remain fully grounded”?

Line 412: “In the first, *the* level set function…”

References

Sethian, J.A., 1996. A fast marching level set method for monotonically advancing fronts. *Proceedings of the National Academy of Sciences*, *93*(4), pp.1591-1595.