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!

AC: We sincerely thank the reviewer for the positive assessment of our work and for recognizing the importance of incorporating calving processes into ice-flow models. We particularly appreciate the comments regarding the performance of the level-set implementation and its ability to preserve calving-front geometry during advance and retreat.
As noted by the reviewer, the original manuscript was limited by the absence of floatation. In the revised manuscript, we have substantially extended the model framework to include floating ice shelves and their interaction with calving-front evolution. We have also modified the synthetic marine glacier experiment using the updated framework of IGM V3.1.1 and present the corresponding methodology and results in the revised manuscript.

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.

AC: We sincerely thank the reviewer for the careful evaluation of the manuscript and for the constructive comments regarding the clarity and setup of the experiments. We appreciate the positive assessment of the overall manuscript structure and the progression from idealized to realistic applications.

We agree that several aspects of the manuscript required additional clarification, particularly regarding the configuration of the synthetic marine-terminating glacier experiment. In the revised manuscript, we have modified the descriptions of the experimental setup and domain configuration. Regarding the reviewer's concern about the synthetic experiment, we acknowledge that the original presentation of the flat -10 m ocean bed was insufficiently justified and may have caused confusion. In response, we have revised the synthetic setup to include a more physically consistent marine geometry extending into the ocean domain. The bed is now generated by a single analytic function across the whole domain, so the fjord channel continues seamlessly below the sea level, consistent with upstream topography.

Furthermore, we now incorporate the synthetic configuration that includes floating ice shelves, providing a more realistic representation of marine-terminating glacier dynamics within the level set framework. These revisions significantly improve both the physical realism and the clarity of the synthetic experiments.

We thank the reviewer again for these valuable suggestions, which have helped improve the clarity and robustness of the manuscript considerably.

Specific comments

RC: 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?

AC: We thank the reviewer for pointing out that the comparison as originally stated was confusing without further explanation. We have clarified this in the revised manuscript at lines 24–26, which now reads:

"Frontal ablation is a major component of mass loss from marine-terminating glaciers. Across the Northern Hemisphere, frontal ablation from marine-terminating glaciers totalled 559 ± 19.1 Gt a^{-1} during 2000–2020 period (Kochtitzky et al., 2023). For the Greenland Ice Sheet, the net mass loss for a largely overlapping period (2010–2018) is estimated as 286 ± 20 Gt a^{-1} (Mouginot et al., 2019), of which ice dynamics was a major contributor, underscoring the central role of frontal processes in the mass budget."

RC: 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.

AC: We thank the reviewer for raising this point. IGM provides two approaches for solving glacier flow: (i) a classical physics-based solver and (ii) a physics-informed deep-learning emulator (Jouvet and Cordonnier, 2023).

The classical solver computes ice flow by directly solving the governing ice-flow equations through a variational optimization framework using the Adam optimizer. In contrast, the emulator

approximates the solution using a neural network that has been trained on a large ensemble of glacier-flow simulations.
In this study, we employed the classical IGM solver rather than the neural-network emulator for two reasons. First, our experiments focus on marine-terminating glaciers with very high flow velocities near the calving front. The classical solver provided robust and physically consistent velocity fields in these regions, which are critical for accurately simulating calving-front evolution. Second, our synthetic experiments require glacier growth and advance from an initially ice-free bed. Such configurations may fall outside the range of conditions represented in the emulator training set, whereas the classical solver can directly compute physically consistent solutions from arbitrary initial states without requiring retraining.

To clarify this choice, we have added a short explanation of the two IGM flow-solution approaches and the rationale for using the classical solver in the revised manuscript.

RC: Line 92: Could you provide this function?

AC: Added

$$\mathrm{SMB}(z) = \begin{cases} \min\left(\beta_{\mathrm{acc}}(z - z_{\mathrm{ELA}}), m_{\mathrm{acc}}\right), & \text{if } z > z_{\mathrm{ELA}}, \\ \beta_{\mathrm{abl}}(z - z_{\mathrm{ELA}}), & \text{otherwise.} \end{cases}$$

RC: 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)?

AC: We thank the reviewer for pointing out the missing reference and explanation. We have added a brief explanation, together with the Sethian (1996) reference, to the revised manuscript.

RC: 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.

AC: We thank the reviewer for this helpful suggestion. In the revised manuscript the sentence has been removed, and the description of Eq. (4) at line 109 now reads:
“where vf is the front velocity of the level set, defined as the difference between the ice velocity v which is perpendicular to the level set, and the calving rate c.”

RC: 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?

AC: We thank the reviewer for raising this point, which was indeed under-specified in the original text. In our implementation the narrow band is defined as a function of the grid

resolution rather than as an absolute distance: we use a band width of 50 grid cells on either side of the zero level set (i.e. a physical width of 50 × 100] m for a horizontal grid spacing 100). This ensures that the band scales consistently across resolutions and that the velocity extension and fast-marching reinitialization use a sufficient stencil on either side of the front. The revised manuscript at line 114 now specifies this explicitly:
"In our implementation the narrow band has a width of 50 grid cells on either side of the zero level set."

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

AC: We thank the reviewer for raising this point. In our model, the only physical quantities that explicitly enter the calving rate are the along- and across-flow strain rates $\varepsilon /\!/$ and $\varepsilon \perp$. All other processes that influence calving but are not resolved by this strain-rate product - in particular ice rheology and temperature, pre-existing damage and crevasse density, hydrofracture by surface meltwater, and submarine melt-driven undercutting are not represented in the model and therefore end up absorbed into Kec by calibration.
In this sense Kec is should be interpreted as an effective tuning parameter whose value reflects the net effect of these unresolved processes for a given glacier and period, rather than any one of them individually. This interpretation of the proportionality constant follows the original eigencalving formulation, in which Kec represents the material properties relevant to calving and other unresolved processes controlling front failure (Levermann et al., 2012).

RC: Line 142: What is the time step?

AC: The time step of IGM is estimated based on the CFL criteria and therefore varies according to the local velocity and model setup.

RC: 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?

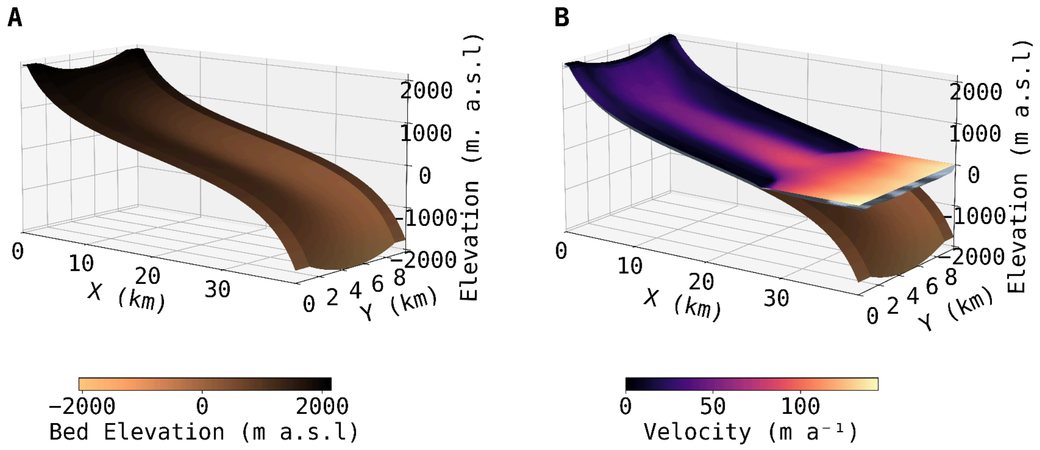
AC: The newly-iced cell carries zero velocity for one sub-step, then receives a stress-balance-consistent velocity at the next iceflow update on the same timestep. We do not separately extrapolate the ice velocity onto the new cell, because the IGM iceflow itself provides the physically correct value as soon as the new geometry is in place.

RC: 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.

AC: We thank the reviewer for the suggestion. We agree that the first synthetic case provides a clear and intuitive demonstration of the algorithm and is well suited to introducing the method before the more complex real-glacier applications. In the revised manuscript we have therefore moved this case from the appendix into the main text. The associated figures have been renumbered accordingly.

RC: 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?

AC: We thank the reviewer for this suggestion. Area below sea level is treated as water to make sure that synthetic domain will have a clear cut ice region and water interface. Further the topography was limited to a maximum depth of -10 m to ensure that the bed topography is small enough to make sure that the glacier tongue can stay grounded, as the glacier is advancing. However, we have removed this constraint from the updated revised version thus extending the topographic channel with the same distribution as that of the upstream as given below.



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

AC: We thank the reviewer for this suggestion. In the revised manuscript, we have added the parameterization used to estimate the surface mass balance (SMB) earlier in the section “Glacier dynamics and surface mass balance”. We have also revised the notation to ensure consistency with the introduced symbols.

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

AC: We thank the reviewer for raising this major concern. Flotation is now explicitly represented: a grid cell is treated as floating where the bed lies below the flotation level,

$$\mathrm{topg} < \mathrm{z\_sl} - (\mathrm{rho\_i} / \mathrm{rho\_w}) \cdot H,$$

where topg is the bed topography, z_sl is the sea level, rho_i (918 kg m-3) is the ice density, rho_w is water density (1028 kg m-3) and H is the ice thickness. Otherwise the cell is treated grounded. The results for synthetic setup are updated with the new results that included flotation.

RC: Line 183: Should this be vf = -200ma-1, if you are moving the front backwards?

AC: We thank the reviewer for pointing this out. The reviewer is correct and the value of Vf is corrected in the manuscript.

RC: 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?

AC: We thank the reviewer for this comment. In the original setup, the SMB was set to zero throughout the initial ice-free region (this was considered as ocean), that could potentially be occupied by ice as the glacier advanced using the level-set method; it was not restricted to the first grid cell upstream of the calving front or to the narrow-band region alone.

In the revised manuscript, we replaced this zero-SMB configuration with a spatially uniform SMB of 0.3 m w.e. a^{-1} over the entire domain. The value was selected through a series of tests to ensure that the glacier reaches a stable equilibrium state by the end of the 2500-year spin-up period. This modification allows a glacier to develop from an initially ice-free bed, advance naturally into the marine sector, attain flotation and remain in equilibrium throughout the subsequent simulations.

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

AC: We thank the reviewer for this suggestion. The figure in now referred in S2.1

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

AC: We thank the reviewer for this suggestion. The minimum and maximum depth observed at the terminus in 2000 is added to the manuscript along with maximum depth observed inland.

RC: 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?

AC: We thank the review for this important comment. For the results submitted, we decided for a manual calibration to reproduce the observed frontal velocities.

In the revised manuscript, we can now rely on the built-in inversion using the data-assimilation framework available in the current IGM release (v3.1.1). It produces more convincing velocity results. The updated framework now converges reliably for our setup and produces modelled frontal velocities that closely match the observed range. In this sense, we now follow the suggestion of the reviewer.
We thank the reviewer for prompting this improvement, which has strengthened the consistency between modelled and observed velocities.

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

AC: We thank the reviewer for this question. The enhanced basal sliding imposed below sea level in the original manuscript was an ad-hoc means of ensuring that the modelled ice flowed sufficiently fast near the terminus to match the observed near-frontal velocities, since the manual friction-calibration strategy used at the time did not otherwise reproduce the observed acceleration toward the front.

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

AC: We thank the reviewer for this comment. The calibration was performed as a parameter sweep over two different periods.

Line 225: Does this data need a reference?
AC: The reference (EROS, 2020) is added to the Landsat data set.

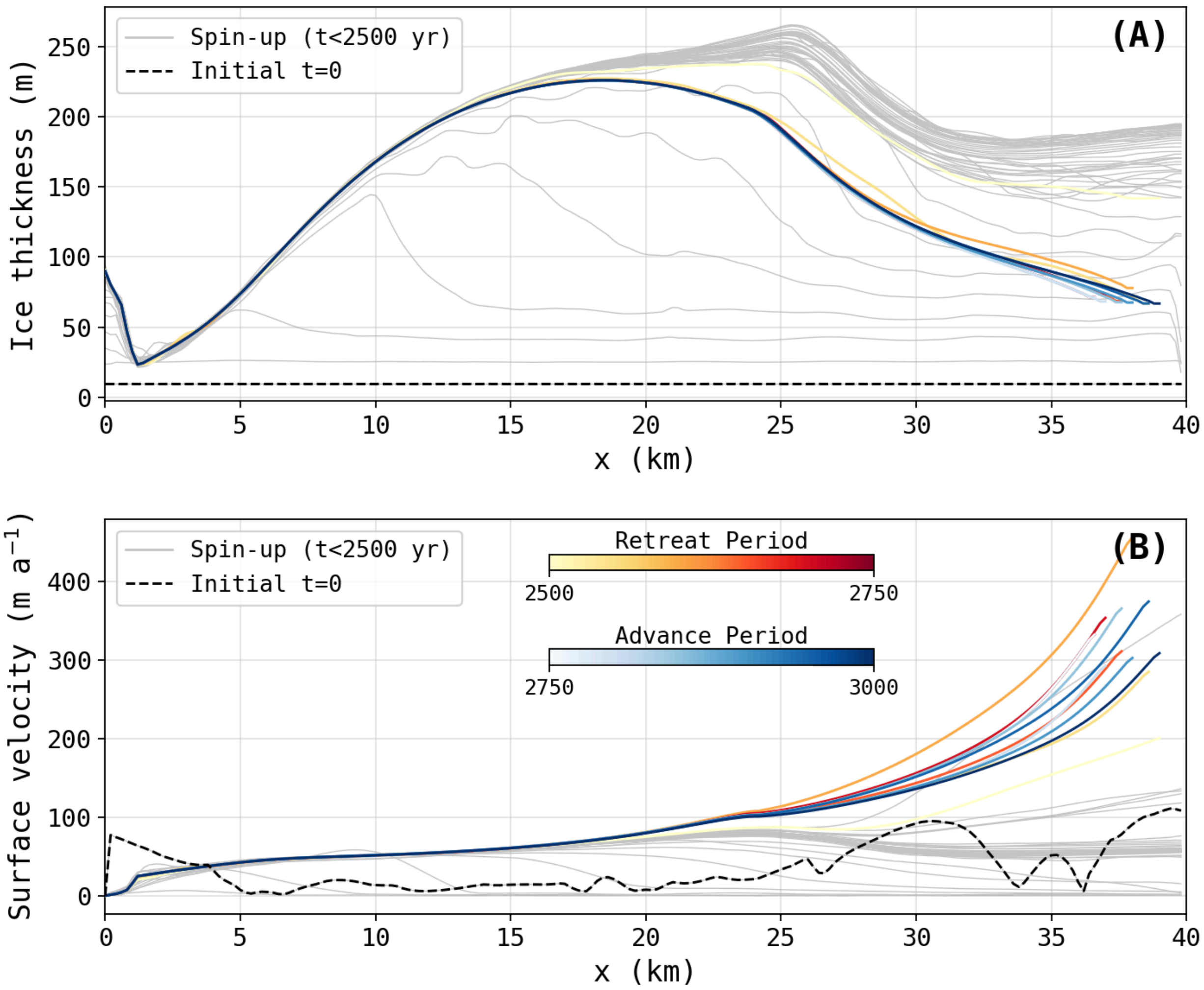
RC: Fig. 3: I would personally find it useful to see Panel C here overlaid on some data (bed or surface elevation would be good), rather than the satellite imagery, to get a better picture of the physical setting.

AC: We thank the reviewer for this valuable suggestion. The figure 3, panel c is now modified with the glacier surface topography.

RC: 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.

AC: We thank the reviewer for raising this serious concern regarding the velocity. We agree with the reviewer and have revised the experiment accordingly. As we described earlier, disabling floatation and capping bed topography at -10m contributed to near zero velocities at front during initialization itself.
We have revised the manuscript with these changes to produce realistic velocity fields.
Flotation is now enabled with basal friction removed from the floating tongue. The revised experiment uses the unified IGM v3.1.1 ice-flow solver, together with flotation-aware basal boundary conditions. Hydrostatic water back-pressure is imposed at the calving front through the floating-energy formulation using ice and water densities of 918 and 1028 kg m^{-3}, respectively. Consequently, the terminus stress boundary condition evolves consistently with the glacier geometry and yields physically reasonable velocity fields near the front. Furthermore, for the synthetic setup, the friction coefficient in the revised experiment for grounded cells uses a single uniform value (0.02), and floating cells uses zero, i.e. basal friction is removed entirely from the floating glacier tongue, which therefore deforms as a free-slipping shelf. We now state these values explicitly in the manuscript. The revised results can be found in the figure.
As the prescribed calving front velocity far exceeded the ice velocities, we now replaced it with a smaller frontal velocity. This enabled the terminus to develop a physically consistent velocity field, as the front no longer advances faster than the ice can transport mass into it, restoring mass continuity at the moving boundary.



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

AC: We thank the reviewer for this comment. The phrase “not controlled by the calving module” was intended to indicate that, at this stage of the simulation, the glacier terminus was not in contact with the ocean and therefore did not constitute a marine-terminating calving front. Consequently, the calving module was inactive and did not influence terminus evolution. Changes in glacier extent during this period were governed solely by ice-flow dynamics and mass conservation, without any contribution from calving-related mass loss.

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.

AC: We thank the reviewer for this suggestion. The level set was tested with a re-initialization frequencies of 0.1 yr, 0.5yr, 1 year and 2 year for the synthetic setups. The details of these experiments are now included in the revised manuscript in the result section along with new figures showing the front retreat under varying re-initialization frequencies.

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

AC: The ice-land/ ice-free interface was used to describe the lateral boundaries separating glacier area from adjacent glacier free (ice-free land). In the revised manuscript no such lateral margins are defined and the entire domain is now ice filled.

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

AC: We thank the reviewer for this suggestion. The Figure 4 in the revised manuscript only shows the thickness and velocity distribution. The calving front positions along with the error quantification is given as a different figure.

Line 410: This criteria sounds like it should have a reference.
AC: The reference Courant et al, 1967 is added to the manuscript.

Typographical corrections (not a complete list)
AC: the manuscript has been corrected for the following typographical corrections.

Line 101: Italicise the variables x and t.
AC: Corrected
Line 128: No need to repeat Kec in brackets.
AC: Corrected
Line 160: I think there should be a paragraph break here, to make it clear you are starting to talk about a new domain.
AC: Corrected
Line 177: Quotation marks formatted incorrectly.
AC: Corrected
Line 186: “interface”
AC: Corrected
Line 234: “uses”
AC: Corrected
Line 244: “continues”Line 281-2: Would be clearer to write “The numerical inconsistencies of the level set advection mainly come from…”
AC: Corrected
Line 328: “Although the eigen calving law…”
AC: Corrected
Line 344: “…enable physically consistent simulations…”
AC: Corrected
Line 350: “…assumed that glaciers remain fully grounded”?

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

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.

Courant, R., Friedrichs, K., and Lewy, H. (1967). On the partial difference equations of mathematical physics. IBM Journal of Research and Development, 11(2), 215–234.

Earth Resources Observation and Science (EROS) Center. (2020). Landsat 8-9 Operational Land Imager / Thermal Infrared Sensor Level-1, Collection 2 [dataset]. U.S. Geological Survey. https://doi.org/10.5066/P975CC9B

Levermann, A., Albrecht, T., Winkelmann, R., Martin, M. A., Haseloff, M., and Joughin, I.: Kinematic first-order calving law implies potential for abrupt ice-shelf retreat, The Cryosphere, 6, 273–286, https://doi.org/10.5194/tc-6-273-2012, 2012.