```Review of « A numerical framework for modeling iceberg calving and ice-front migration of grounded glacier tongues » submitted to TC by V. Prasad et al.

This paper present the implementation of a level-set method into the Instructed Glacier Model to track the calving front position.
It is now well recognised that ice front dynamics has a major impact on the flow and mass budget of glaciers terminating in water. Including this process in ice flow model is challenging both in term of numeric and physics. The level-set method is a popular numerical method to track moving interfaces especially with unstructured grids and it has been first applied to track calving front position by Bondzio et al. (2016). The topic of the paper is then relevant and in the scope of TC.

After a description of the level-set equations and caving rate parameterisation, the authors test their implementation on 2 synthetic cases and one real world experiment.
In general I find that the paper lacks precisions in many places and shows some inconsistencies.
However, my main criticism is that there is no presentation of the coupling of the level-set with the force balance equation used to compute the velocities, here according to sec. 2.1.1 the Blatter-Pattyn approximation. At a calving front, the boundary condition in the normal direction should be stress free above sea level and should be the pression from the water below sea level. So that particular attention must be paid to correctly apply the boundary condition when the front is moving. Looking at the results of the synthetic glacier domain in Fig. 4, I'm very sceptical that the correct boundary condition is properly set. Indeed when the front advances the thickness is important (> 150m) with a water level of 10m. The stress difference at the ice front is so important that the velocity cannot be 0 as shown by the results.

AC: We sincerely thank the reviewer for this detailed and substantive assessment. We have added the methodological description on the coupling of level set with the velocities in the revised manuscript now. As these issues recur in the detailed comments, we summarise our response here and indicate where each is treated in full.
1. Coupling of the level set with the force-balance (velocity) solver: We agree this description was missing and have added a dedicated subsection (Sect. 2.1.1). At each timestep IGM first computes the velocity by minimising the energy functional; the level set is then advected by the front velocity v_f, newly exposed cells are initialised by the thickness-extension procedure and cells beyond the front are emptied; and IGM finally solves the mass-conservation equation over the updated domain. The marine stress condition is re-evaluated at the level-set-tracked front each timestep, so it follows the migrating terminus.
2. Velocity at the front and boundary conditions: We have modified the current experimental setup and rerun to check for the boundary conditions. The revised experiments use the higher-order MOLHO solver with a flotation-aware basal condition and the marine stress condition. The details are provided as the reply to specific comments on line 93. Also the modified results given here shows that the velocities are not zero at the front. We once again thank the reviewer for this valuable suggestion .
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Moreover, I don’t think this experience is very designed for something that’s supposed to be a realistic synthetic glacier. Even if the purpose of this experiment is not to test the caving rate, the velocity used to advect the caving front is totally unrealistic. First, it is said in Eq. 4 that vf should be perpendicular to the calving front (which is not correct, in general the caving rate is given perpendicular to the calving front, but the calving front velocity does not have to be. However in general the ice velocity at the front will be also close to the perpendicular), but here vf is directed along the x-direction. Second, for the advance, as the flow velocity falls to 0, this implies a calving rate of -400 m a-1, i.e. an accretion of 400 of ice at the calving front. It would be more realistic to let the front advance at the ice flow speed, which as said above should not be 0 at a caving front interface.

AC: We thank the reviewer for this constructive review. The primary objective of the experiment was to verify the numerical behaviour of the level-set implementation under controlled advance and retreat scenarios rather than to reproduce realistic calving dynamics. The vf is directed only in one direction as it served to be a substantial simplification that allowed us to investigate the reversibility of the level set module. We agree with the reviewer that the front velocity need not be normal to the front (Eq. 4). We have corrected Eq. 4 and consistently distinguished vector and scalar notation (per the Eqs. 3–4 comment).

In addition, the implied -400 m a^{-1} is removed from the revised experiment. In the revised experiment flotation gives a non-zero ice velocity at the front (≈ 200 m a^{-1}), and the prescribed front velocity is reduced to ± 10 m a^{-1}. We agree that the experiment is presented as a controlled numerical test of the level-set advance/retreat (reversibility, mass conservation), not as a realistic calving simulation.

Finally, as the synthetic experiments are presented as a way to assess mass conservation, symmetry preservation, and accuracy of initialization and re-initialization, I would expect to have some quantifications of the errors, and maybe some sensitivity tests to some numerical parameters, and not only 2D plan view plots.

AC: We sincerely thank the reviewer for this valuable suggestions. In response we have now added error metrics in the revised manuscript. RMS errors between modelled and analytical solutions are added to both synthetic setups. Further a reinitialization accuracy test is also provided showing the front movement at different reinitialization intervals. Reeversibility is further tested in the retreat advance cycle. The results confirm that the advance and retreat phases are reversible at the sub-grid level.Details of the experiments are now provided in the results and described in the discussion as well.

Detailed remarks:
RC: Line 25: 559.0 +/ 19.1 is the total frontal ablation for all glaciers in the northern hemisphere, not only Greenland. Moreover the frontal ablation is quantified as ice Discharge + mass change rate associated with changes in front position. The cited mass loss for Greenland is the cumulative anomaly of surface mass balance – ice Discharge, so that the 2 numbers can not be compared in the same sentence.

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: Line 31 “Many existing studies…”: Clarify the sentence. Many studies only quantify ice discharge and not frontal ablation, i.e. do not quantity mass change associated with front position change?

AC: We thank the reviewer for this helpful comment. Our intention was to distinguish between ice discharge and frontal ablation more clearly. We agree that the original wording did not sufficiently convey this distinction.

To address this, we have revised the text as follows:

“..insufficient knowledge of accurate glacier bed topography and ice thickness near the terminus exists for water-terminating glaciers (Fürst et al., 2024). As a result, many existing studies quantify only ice discharge, the ice flux through a fixed gate near the terminus, rather than full frontal ablation, which additionally accounts for the mass change associated with advance and retreat of the calving front. Neglecting this front-position term contributes substantially to the uncertainty in predictions of sea-level rise (Li et al., 2023).”

RC: Line 32 “Future calving …”: Preceding sentences refer to observations while this sentence and reference are for prediction. The link is not straightforward, and modelling is discussed in the next paragraph.

AC: We thank the reviewer for this comment. We have revised the sentence to maintain the focus on observational estimates and their limitations.
“As a result, many existing studies quantify only ice discharge, the ice flux through a fixed gate near the terminus, rather than full frontal ablation, which additionally accounts for the mass

change associated with advance and retreat of the calving front. Neglecting this front-position term contributes substantially to the uncertainty in predictions of sea-level rise (Li et al., 2023).”

RC: Line 58 “Recent studies also”: This sentence is used to introduce the numerical difficulties in tracking moving fronts. But the “also” is a bit misleading. Reformulate.

AC: We thank the reviewer for this observation. To improve clarity, we have reformulated the text as follows:

“Beyond estimating the calving rate, recent studies also evolve the calving front dynamically, advancing or retreating the terminus according to the physics-based calving law. A widely used approach is to track the calving front along the flowline..”

RC: Line 85 : I don’t think that the given references introduce or give details on the LSM in Kory. Reformulate.

AC: We thank the reviewer for this observation. The cited references describe the implementation of the Kori-ULB ice-flow model. We have therefore revised the text as follows

“In this section, we present the implementation of the calving algorithm and its coupling with the ice-flow model. The calving algorithm consists of two key components: a sub-grid front-tracking framework and a physics-based calving law. The front-tracking framework is implemented using a Level Set Method (LSM) and is adapted from its implementation in the Kori-ULB ice-flow model (Pattyn, 2017; Coulon et al., 2024)”.

RC: Line 93 “numerical optimisation”: what is numerical optimisation here? Reformulate the paragraph as it is said 2 sentences above (before the description of the SMB) that the ice dynamics rely on a classical solver of IGM. As mentioned in the main remarks, give a description of the boundary condition at the calving front for the ice dynamics.

AC: We thank the reviewer for this suggestion. We have reformulated the paragraph to specify that the MOLHO approximation of Blatter-Pattyn equations are used.

The revised paragraph now reads:
“For ice dynamics, we do not rely on the emulated velocities from IGM but rather rely on the solution of the built-in classical solver (Jouvet et al., 2022). This solver obtains the velocity field by numerically minimising the variational energy functional associated with the MOLHO approximation of the Blatter–Pattyn ice-flow equations through gradient-based optimization and automatic differentiation within TensorFlow.”

Further the boundary conditions are as : ‘At the calving front, the front is imposed as a stress boundary. The front is stress free above the sea level and below sea level the ice stress is balanced by hydrostatic pressure, yielding frontal condition,

$$P = \frac{1}{2}\rho_i g H^2 - \frac{1}{2}\rho_w g D^2$$

where ρi and ρw are the densities of ice and seawater, respectively, H is the ice thickness, and D = max(zsl − b, 0) is the water depth at the calving front (Jouvet and Cordonnier, 2023). This boundary condition is re-evaluated at the level-set-tracked front position at every time step, ensuring that the stress boundary evolves consistently with calving-front migration.”

RC: Sec. 2.1.2: As I understand the fast marching method is used only for the initialisation, so use t=0 or some notation for initialisation time to avoid confusion with the procedure to compute the evolution of the LS. Give a reference for the fast marching method, and I think it computes the signed distance function from the front not only “outward”.

AC: We thank the reviewer for these comments. The fast marching method is indeed used only to initialise the level-set field. To remove the ambiguity, we now explicitly denote the initialisation time and refer to the initial level-set field as φ(x, t = 0). We have added the reference for the fast marching method (Sethian, 1996).

The wording “which extends the signed distance function outward from the front” in the manuscript is also corrected to specify that the signed distance function is computed on both sides of the front, not only outward. The revised sentence now reads: “The remaining nodes of the domain are then populated with the fast marching method (Sethian, 1996), which propagates the distance function away from the front into both the ice-covered (Ω_i) and ocean-covered (Ω_c) regions, so that the magnitude of its gradient is one (|∇φ| = 1).”

References:

- Sethian, J. A. (1996). A fast marching level set method for monotonically advancing fronts. PNAS, 93(4), 1591–1595.

RC: Line 107 “differentiating …”: Eq. 3 is the material derivative of the LS?

AC: Yes the reviewer is correct. Equation 3 expresses that the material derivative of the level-set function following the front is zero, dφ/dt = 0. We have revised the text to state this explicitly and to label the result as the material derivative.

RC: Eqs. 3 and 4: Use different notations for vectors and scalars. vf in eq. 3 is a vector, but a scalar in eq. 4. As mentioned above vf does not have to be normal to the front.

AC: Corrected.

RC: Line 114: the narrow band. There is no mention here or in the experiments of the width of the narrow band.

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: Eq. 6: Use the dot only for the dot product between vectors (or tensors) and not for scalars to avoid confusion. The velocity (and thus strain rate fields) is 3D. Are the strain-rates used to compute the calving rate vertical averages?

AC: We thank the reviewer for this suggestion. The equation is modified accordingly. The strain rates we have used in the calving law are from surface velocity fields. The estimated calving rate is representative of the surface velocities for now.

RC: Eq. 7: The equation is solved to steady-state?

AC: We thank the reviewer for this suggestion. We like to clarify that the reinitialization equation is not integrated to a steady state. It is advanced in pseudo-time for a fixed, small number of iterations (with pseudo-timestep, until the signed-distance property $|\nabla \phi| \approx 1$) within a narrow band around the zero level set. These clarifications are added to the manuscript along with the control parameters.

RC: Line 141 “The control parameters are finalized”: What are the control parameters? Time step and criteria to define steady state. I don’t find real discussion of this in the experiment results.

AC: We thank the reviewer and have made the control parameters explicit. We have now added them explicitly in the manuscript, mainly the narrow band size, iterations used and pseudo time step for reinitialization.

RC: Line 151 “… the thickness distribution remains consistent”: Define “consistent” more precisely. The extension is not mass conservative, and you could assume that the thickness (and not the surface) is exported.

AC: We thank the reviewer for this suggestion. By "consistent" we meant that the surface elevation of newly incorporated cells is extrapolated from the upstream ice so the surface remains continuous across the advancing front, with thickness obtained from the bed (grounded) or the flotation condition (floating). We have made this explicit and acknowledge that the extension is not mass-conserving; it is a geometric initialisation, after which thickness evolves by Eq. 1. The text at Line 151 now reads: "When the front advances, newly incorporated cells inherit the ice thickness of the adjacent upstream ice, so that the thickness distribution is continuous with the existing glacier across the front.”

RC: Eqs. 9 and 10 : Is the eq. 9 correct? What is nx? I understand x_c=10km? topg is equal to 3250 for x=x_c and y=0, which is not the case in Fig. 1. Give the value for alpha.

AC: We thank the reviewer. Equation 9 as originally written was inconsistent with Fig. 1, and we have corrected it. The values of alpha is 2 and b0 is 0.

$$\mathrm{topg}(x,y) = -\tan\left(0.4\pi\,\frac{x-x_c}{x_c}\right)\,\tan(\alpha)\;x_c \;+\; \frac{(y-5000)^2}{50000} \;+\; b_0,$$

RC: Line 179 : define “sufficiently stable”.

AC: We thank the reviewer for this comment. In the revised experiment the domain is run during the spin-up period for a 2000 year, thus bringing it to steady state. We have removed the incorrect wording and explained the glacier steady state in the revised manuscript

RC: Line 182 “more realistic ice cliff”: define what should be a realistic ice cliff.

AC: We thank the reviewer for this comment. In the previous setup the thickness at the front stayed quite low, which was the reason to introduce the requirement for a higher thickness and thus the term. We have now removed this from the manuscript

RC: Line 183 “vf=200”: I understand that you impose a retreat so should be -200. Is vf normal to the front or along x as in following experiment?

AC: We agree with the reviewer that the vf is -200. In the revised manuscript however these values are now replaced. We no longer use the criteria “To ensure that the initial geometry shows a more realistic ice cliff at the glacier front, we first impose a constant frontal velocity vf = -200 m a−1 (Eqn. 3), for 2 years to cut back the margin position”. This has been replaced by +- 10 m/yr velocity for retreat and advance right after the steady state.

RC: Lines 189-190: This is confusing. I understand from sec. 2.1.5 that this is used to initialise the ice thickness in grid points where the front advances, but after it remains part of the solution of the ice thickness equation. As mentioned in the main remarks, describe of the level set is coupled to the IGM equations (i.e. how the domain change is taken into account in IGM).

AC: We thank the reviewer for the helpful comment; our understanding is indeed correct. In the revised manuscript, we have removed the statement that the terminus thickness is "exclusively updated based on the level-set evolution." We also no longer impose a zero surface mass balance (SMB) at the terminus. Instead, a spatially varying SMB, defined as a function of elevation through the equilibrium-line altitude and the accumulation/ablation gradients (Sect. 2.3.1), is applied across the entire glacier domain.

The role of the level-set method is now limited to updating the glacier geometry. Specifically, the level-set extension described in Sect. 2.1.5 is used only to initialise ice thickness in grid cells newly incorporated into the glacier domain following front advance. Thereafter, the thickness evolves according to IGM's mass-conservation equation (Eq. 1), driven by both ice flux divergence and SMB, in the same manner as all other grid cells.

To clarify the coupling between the level-set method and IGM, we have added the following description to the Methods section: at each timestep, IGM first computes the velocity field; the level-set equation then updates the glacier front position using the front velocity (v_f = v − c); newly added cells are initialised using the thickness-extension procedure, while cells removed from the glacier domain are emptied of ice. Finally, IGM solves the thickness evolution equation over the updated domain using the spatially varying SMB. Thus, the level-set method controls the evolving glacier geometry, whereas IGM governs the evolution of ice thickness and velocity fields.

RC: Sec. 2.2: there is no description of the parameters for the ice dynamics. Friction parameter? The densities used for ice and water should also be given as they have an impact on the boundary condition at the front.

AC: We thank the reviewer for this suggestion. The ice dynamics parameters are now described in the manuscript specifically in Data and methods. These include description of friction parameters, water and ice density, and Arrhenius factors used on both synthetic and real glacier setups.

RC: Fig.2 : Clarify the workflow. I understood that IGM-SHOP will provide observations of ice velocity and topography, so that the calving law should not depend on the initialisation but on the velocity from IGM. Why is there an arrow from the Re-initialisation to the initialisation? It is a bit confusion that the arrow from IGM to the caving module comes from the SMB.

AC: We thank the reviewer for these helpful observations.The eigen calving uses IGm derived ice velocity, not the velocity from OGMM-SHOP.
Further re-initialization restores the signed-distance property of the level-set field, which is then used by the level-set advection at the next timestep. We have redirected this arrow to point to the level-set advection step within the migration loop, rather than back to the Initialization stage, since re-initialization is a recurring operation and does not re-trigger the one-time initialization. The initial signed-distance field is supplied to the loop only once, from the Initialization stage at t

= 0. Lastly, the link from the IGM module to the calving module originates from the ice velocity, not the SMB. We have redrawn Fig. 2 accordingly to include these changes.

RC: Sec. 2.3.2: Improve the description of the friction field. Is the coefficient above sea level constant or increasing (linearly?) with elevation?

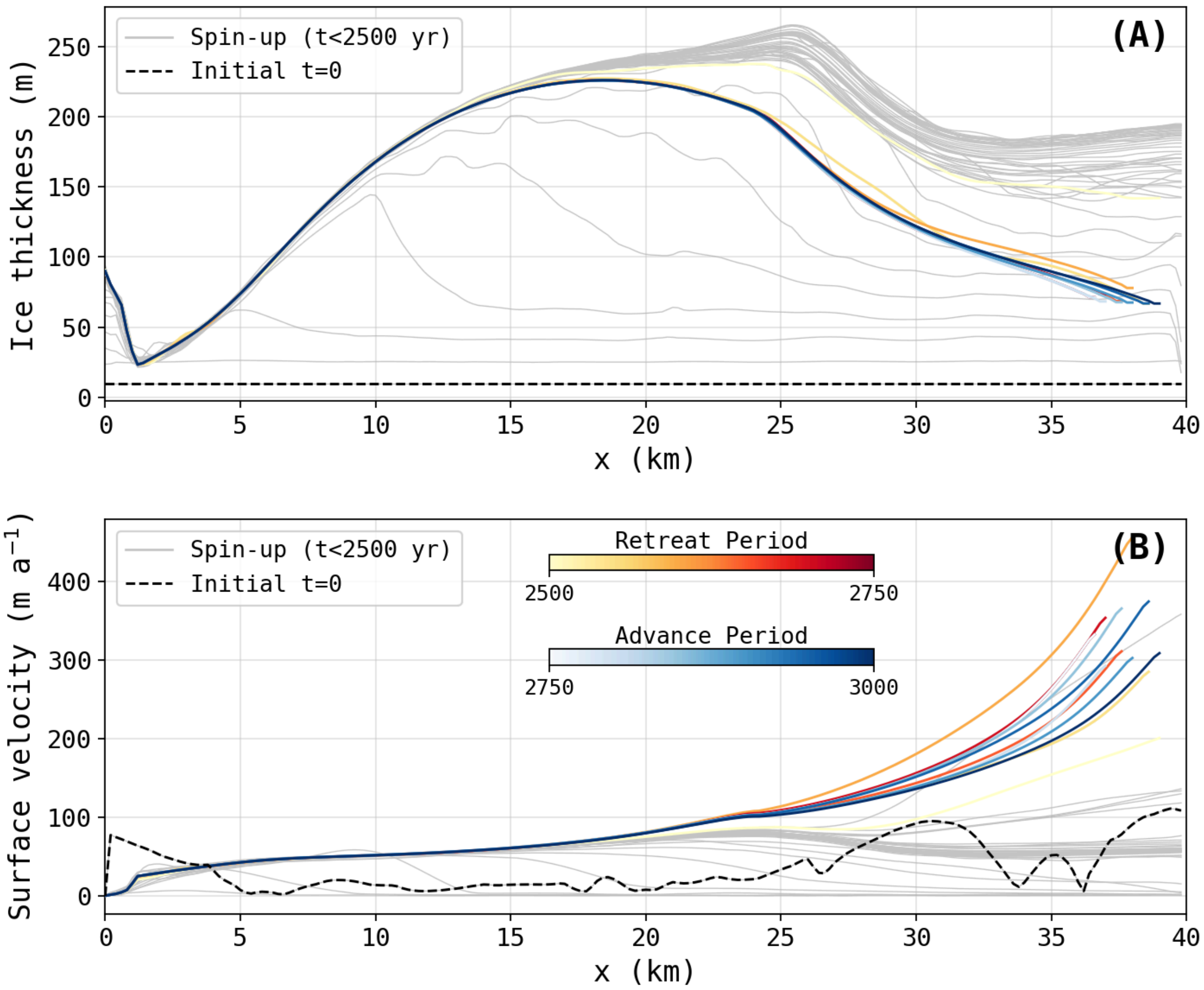
AC: We thank the reviewer for this comment. In the revised manuscript, we have now included a detailed description of the friction coefficient. In the revised framework we have now included floatation. For grid cells that satisfy the flotation criterion, the basal friction coefficient is set to zero, reflecting the absence of basal traction beneath floating ice. For all remaining grounded regions, the basal friction coefficients are obtained from IGM inversion and are retained throughout the simulation period.

RC: Line 220 “after this inversion, a 10 year relaxation” : Consistently use calibration instead of inversion. Constant thickness field? Usually a relaxation is used to dissipate ice flux divergence anomalies by relaxing the ice thickness field. What is evolving during the relaxation if not the thickness?

AC: We thank the review for this suggestion. The relaxation period was initially used to let the IGM reach the observed velocity field using the prescribed ice-dynamic parameters, mainly the friction coefficient. However, we recognize that this procedure was not essential and could lead to confusion regarding which model variables were allowed to evolve during the relaxation. In the revised manuscript we have fully removed the relaxation period along with the constant thickness field criteria. Instead, the inversion-derived ice-flow parameters, including the basal friction field, were used to drive the glacier simulations over the period 2000–2024. The revised manuscript now clarifies the inversion procedure and the application of the resulting parameter fields in the transient simulations.

RC: Sec 3.1: As mentioned in the main remarks, I found very suspicious that the velocity falls to 0 with a very thick calving front mainly above sea-level.

AC: We thank the reviewer for raising this important concern.
As discussed in our response to the main remarks, the zero velocity at front was primarily related to the friction coefficients at the front and the flat bed topography at the terminus. In the revised model setup, we now implement a deeper, sloping bed topography. These changes bring a more realistic stress behaviour and ice-regime. The reconstructed velocities are updated in the manuscript, which are more realistic in nature.



RC: Sec 3.2 the description of the results is really short and somewhat unprecise. It would be interesting to see the velocity field and maybe the evolution of the ice mass and area?

AC: We thank the reviewer for this valuable suggestion. In response, we have substantially expanded the results section. The revised manuscript now includes:

1. A detailed description of the calibration results of calving parameters.
2. Description of IGM inversion results and resultant ice flow dynamics parameters.
3. The temporal evolution of glacier area, and ice volume, over the period 2000–2025.

A new table presenting the model parameters for both glacier configurations (Kronebreen Complex and Kongsvegen), including the calibrated eigen-calving parameters and inversion-derived friction coefficients is also included in the result section.

RC: Line 286: “as is evident…” would be good to give a quantification and not only a plan view.

AC: We thank the reviewer for this suggestion. We have now explicitly added the interface/front position error against the analytical solutions for both synthetic setups. In addition an error quantification based on different re-initialization intervals is also provided.

RC: Line 291 “physically consistent frontal ice thickness”: As mentioned above, during the advance, because for the ice equations (thickness and velocity) the domain changes discretely, the thickness initialisation where the front advances is not mass conserving. So not sure what “physically” means here.

AC: We thank the reviewer for this comment. We agree that the term “physically consistent” is confusing and does not convey the idea. We did not intend to imply mass conservation; we meant that the frontal thickness update is geometrically consistent and is continuous with the upstream glacier and free of artificial steps at the front.

The sentence is rephrased as “A key requirement for level-set-based calving models is the ability to simulate reversible calving-front migration, together with a frontal ice-thickness update that is geometrically consistent with the upstream glacier, continuous, without artificial steps at the front”.

RC: Line 302 “are controlled by the classical mass conservation”: Is this meaning that a minimal ice thickness is imposed and that the front position on land is where H reaches this minimum? Again, give details of the coupling of the LS with the ice equations in IGM.

AC: We thank the reviewer for this suggestion. At land-terminating margins the ice extent is not tracked by the level set; it comes directly from the mass-conservation (thickness) equation. A cell is treated as ice-free if the thickness falls below a minimum threshold (H_min = 0.1 m), reached through the combined effect of ice-flux divergence and surface mass balance."

We have also added the coupling of level set with igm more detailed in the section 2.1.1 Glacier dynamics and surface mass balance.

References (newly cited only):

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.

Jouvet, G., & Cordonnier, G. (2023). Ice-flow model emulator based on physics-informed deep learning. Journal of Glaciology, 69(278), 1941–1955. https://doi.org/10.1017/jog.2023.73