Response to the reviews for the manuscript "Grounding-line dynamics in a Stokes ice-flow model (Elmer/Ice v9.0): Improved numerical stability allows larger time steps"

April 2026

We thank the reviewers and the editor for constructive feedback that has improved our manuscript. The responses to both reviewers are enclosed in this document, with reviewer comments in black and author responses in blue.

Kind regards,

Clara Henry, Thomas Zwinger and Josefin Ahlkrona

## Reviewer 1

The preprint under consideration presents the free-surface stabilization scheme (FSSA), a stabilization scheme for the free-surface Stokes equation. The free surface Stokes equation is a time-dependent equation that is generally solved with explicit time-stepping schemes, due to the difficulty in solving the free-surface evolution and the Stokes equations simultaneoulsy. A consequence of using explicit schemes is that time steps have to be small. The FSSA has been shown to enable higher time steps without reducing the numerical accuracy signficantly. In this preprint, the authors study the impact of using the FSSA in the context of marine ice sheets, where the free surface detaches from the bedrock at the grounding line.

The preprint is well written and contains two well-documented numerical tests that demonstrate the power of the FSSA in the context of marine ice sheets. My main criticism of this paper is that it is very similar to other existing preprints and publications, including

- Josefin Ahlkrona, A. Clara J. Henry, and André Löfgren (2025), A Fully Implicit Second Order Method for Viscous Free Surface Stokes Flow − Application to Glacier Simulations (under consideration for publication in GMD),
- Tilda Westling Dolling, A. Clara J. Henry, and Josefin Ahlkrona (2025), A numerical stabilization scheme for the shallow shelf approximation (arXiv:2510.02943).

Since the need for stabilization schemes like this one is urgent in the glaciological community, I recommend this article for publication once the following comments are addressed.

We thank the reviewer for their time in providing thoughtful comments on our manuscript, "Grounding-line dynamics in a Stokes ice-flow model (Elmer/Ice v9.0): Improved numerical stability allows larger time steps". We have altered the manuscript in response to these comments as far as possible, as outlined in our comment-by-comment responses in blue text. Regarding the concern that the manuscript is similar to Ahlkrona et al. (2026) and Westling Dolling et al. (2025), we clarify the key differences here. In Ahlkrona et al. (2026), we construct a fully implicit method based on Picard iterations, unlike the explicit discretisation of this manuscript. In Westling Dolling et al. (2025), we develop a stabilisation scheme for depth-integrated models. Although based on the same physical arguments as the FSSA stabilisation of this manuscript, the mathematical constructions are fundamentally different because the vertical stress distribution is hydrostatic and the horizontal velocities are depth independent.

## General comments

- As indicated above, this paper is closely related to a series of papers on the FSSA (and its depth-integrated variants) written by some of the authors. I think the introduction should contain a more exhaustive description of what exactly is novel about this publication (its application to grounding line dynamics) and in what way it is related to existing publications.

  Thank you for raising this. The present paper is the first to investigate the application of the Free-Surface Stabilisation Scheme (FSSA) to the ice-ocean interface, a problem that is fundamentally different to land-terminating glacier fronts given the complexity of handling floating ice and the grounding-line contact problem. We felt that a rigorous analysis of the performance of a Stokes–free-surface model in a grounding line problem in both 2D and 3D would increase adoption of the method in the community. The main reason for the non-exhaustive description of the development of these methods is that the two papers referred to (Westling Dolling et al., 2025; Ahlkrona et al., 2026) were published as pre-prints only shortly after this manuscript was submitted. The other two papers focus on (1) an analogous stabilisation scheme for the Shallow Shelf Approximation, and (2) an extension of the FSSA to enable fully implicit time stepping and higher-order time-stepping schemes. We have therefore adapted the Introduction to describe the various stabilisation schemes and to emphasise how the present study complements the existing work by introducing FSSA to floating ice Stokes simulations. The text included reads "FSSA was originally conceptualized by Kaus et al. (2010) for geodynamics applications. It was subsequently adapted to grounded ice sheet simulations by Löfgren et al. (2022); Löfgren et al. (2024). The FSSA has further been adapted so that the numerical stabilisation terms vanish on convergence in a framework that allows for large time steps in a fully implicit, higher-order time-stepping framework for grounded ice (Ahlkrona et al., 2026). In Westling Dolling et al. (2025) an FSSA-like stabilisation was constructed for depth-integrated Shallow Shelf Approximation models. In this paper, we present the first application of FSSA to full Stokes floating ice problems."

- Section 4. For the MISMIP experiment, you only solve the unstabilized system ($\theta = 0$) for one timestep. Does the scheme work for higher timesteps if $\theta = 0$. If so, it would be important to include these results so that the reader understands the effects of not including the FSSA. If the scheme breaks down for higher timesteps when $\theta = 0$, please indicate this.

  We performed additional simulations with $\theta = 0$ and time-step sizes of $\Delta t = 2$ and $\Delta t = 5$ years. We have included the sentence "Without the FSSA, a time step size of $\Delta t = 5$ did not converge, and a time step size of $\Delta t = 2$ converged well in the advancing phase but did not converge in the retreating phase even with the standard short periods of a smaller time step immediately after the viscosity jumps." in the last paragraph in Section 4.2. We have not added these simulations to Table 3 as the list only includes converged simulations.

- The numbering of figures throughout the text seems to be incorrect. See for example reference to "Figure 4.2" in Line 281 and "Fig. 5.1" in Line 300.

  This seems to have been a problem with the figure labelling in the LaTeX-template. We have corrected references to figures throughout.

## Minor comments

- Line 3. “The restrictive time step size of ice-sheet simulations ($\sim \Delta t = 0.01 - 0.5$ years) has led to the routine use of approximate models that compromise physical complexity compared to full Stokes models.” In my opinion, approximations of the Stokes equations are used in glaciology because the Stokes equations, as opposed to e.g. depth-integrated models, are very expensive to solve. Please rewrite along the lines of “The restrictive time step size of ice-sheet simulations ($\sim \Delta t = 0.01 - 0.5$ years) is one of the reasons why... ”

  Altered.

- Figure 1. This figure is not referred to in the text. Please refer to to it when introducing the notation for the domain $\Omega$ and its boundary.

  Fixed.

- Line 81. Clarify that, despite the Stokes equations appearing to be stationary in time, time-dependence enters the problem because the domain $\Omega$ changes in time.

  To make this clear, we have included the sentence “Although the equations show no explicit dependence on time, time dependence enters the problem because the domain, $\Omega$, changes in time.”

- Lines 105 and 110. Writing $v \in \mathbb{R}^3$ and $q \in \mathbb{R}$ is very confusing. Please rewrite as e.g. $v : \Omega \to \mathbb{R}^3$ and $q : \Omega \to$ RR.

  Fixed. We have assumed that the RR is a typo in your comment and have replaced this with $\mathbb{R}$.

- Figure 2. Please refer to this figure in the text.

  Fixed. We now refer to Figure 2 in Sections 3.1 and 3.2.

- Line 137. “However, due to the elliptical nature of the Stokes equations, the coupled system of the Stokes and the free-surface equations cannot be fully implicit in the velocity.” I do not understand this sentence. Are you not solving this coupled system implicity in the paper “A Fully Implicit Second Order Method for Viscous Free Surface Stokes Flow - Application to Glacier Simulations”? Please clarify and explain relationship to this paper.

  Yes, you are correct. This sentence may have been written at a time when the paper “A Fully Implicit Second Order Method for Viscous Free Surface Stokes Flow - Application to Glacier Simulations” had not fully been developed. We have simply removed this sentence.

- Line 185. What does the velocity with a tilde, $\tilde{u}$, refer to? It seems to be like this variable has not been defined.

To address this, we have adjusted Eqs. (25) and (26), so that $\boldsymbol{u}$ is replaced with $\tilde{\boldsymbol{u}}^{k+1}$ and added the sentence, "Here, $\tilde{\boldsymbol{u}}^{k+1}$ is an approximation of the velocity at the next time step, $k+1$."

- Line 221. Write "Table 1" as opposed to "Table (1)". Same for "Table (2)" in the next line.

  Fixed.

- Line 245. Why are Picard iterations used? Is a Newton solver not implemented in Elmer/Ice? The p-Stokes equations are the optimality conditions for differentiable convex optimization problem and therefore work very well, in general, when Newton's method is applied.

  As the type of iterative solver was not the main focus of the study, we did not develop the Newton solver capabilities in relation to this study.

- Line 308. "The isothermal ice fluidity, A, and the friction coefficient, C, were tuned..." Please indicate how they were tuned. Did you solve an inverse problem to find the corresponding scalar values?

  We found suitable values by testing combinations of ranges of basal friction coefficients, $C$ and ice fluidity parameters, $A$, both of which are spatially constant. Given that the aim of the study was to test the numerical stabilisation scheme and not to make predictions, we feel that this ad hoc approach is sufficient. We have altered the sentence to read "The spatially constant isothermal ice fluidity, $A$, and the friction coefficient, $C$, were tuned to values that caused minimal deviation from observed surface velocities (Rignot and Scheuchl., 2017) in an ad hoc manner given that the purpose of the study is to investigate the applicability of FSSA and not to make projections."

- Equation (28). Is this choice of boundary condition for the ice divide common? Please reference other works where this is used. What value is chosen for $C_n$? I can imagine that, as $C_n$ becomes very large, an impenetrability boundary condition is effectively enforced (this is equivalent to penalizing u.n at the boundary). Is this true? If so, please indicate this in the text to facilitate understanding for the reader.

  This choice provides great numerical stability compared to a Dirichlet boundary condition. We choose a value of $C_n = 10^{12}$ MPa m$^{-1}$ a, which results in an effective impenetrability boundary condition, enforced weakly. The text now reads "This boundary condition takes the form

  $$\hat{\boldsymbol{n}} \cdot (\boldsymbol{\sigma} \cdot \hat{\boldsymbol{n}}) = -C_{\mathrm{n}} \boldsymbol{u} \cdot \hat{\boldsymbol{n}}.$$

  This formulation alleviated spurious artifacts at the ice divide, which occurred when implementing a strong-form impenetrability condition (Seddik et al., 2017). We choose a value of $C_n = 10^{12}$ MPa m$^{-1}$ a in this weakly enforced boundary conditions."

- Line 343. "Figs 5.1 and 5.1". Please correct.

  Corrected.

- Line 363. "a time-step size of $\Delta = 50$ years". Please correct.

  Corrected.

- Line 382. "We hypothesis...". Please write verb correctly.

  Corrected.

## Reviewer 2

The manuscript "Grounding-line dynamics in a Stokes ice-flow model (Elmer/Ice v9.0): Improved numerical stability allows larger time steps" by Henry et al. extends a previous method developed by Löfgren et al. (2022) for stabilizing ice-sheet free surfaces (FSSA) to the case of a marine-terminating ice sheet. The FSSA method uses the Reynolds transport theorem to approximate the gravitational body force on the domain at the next time step, introducing new terms to the weak form of the Stokes equations. This stabilization complements the previous "sea-spring" method of Durand et al. (2009) that applies similar implicit approximations to the water pressure boundary condition. The method is clearly described and motivated, and will make a substantial contribution to computational glaciology by providing a roadmap for implementation in other ice sheet models beyond Elmer/Ice. However, the presentation requires improvement prior to publication, and I have several specific comments and suggestions that should be considered.

Many thanks for taking the time to provide feedback on our manuscript. We appreciate your positive evaluation and acknowledge that certain aspects of the manuscript could be strengthened. In response to your comments, we have made changes to the manuscript as outlined below.

1. Line 16: Specify that the model domain is the Ekström Ice Shelf.

   Done.

2. Line 17: In the abstract, I think you need some caveats about slow retreat/advance, because a time step of ten years will not be universally applicable to more rapidly evolving systems driven by ocean melting (e.g., Thwaites).

   In order to address this comment, we have included the sentence "However, these results are obtained for relatively slowly evolving grounding-line dynamics, and caution is required for more rapidly evolving systems driven by ocean melting (e.g., Thwaites Glacier)."

3. Line 28: Regarding that statement "grounding line can be subject to MISI", consider citing some of the recent papers by Sergienko et al.:

   a. Sergienko, O., & Haseloff, M. (2023). 'Stable'and 'unstable' are not useful descriptions of marine ice sheets in the Earth's climate system. Journal of Glaciology, 69(277), 1483-1499.

   b. Sergienko, O. V. (2022). No general stability conditions for marine ice-sheet grounding lines in the presence of feedbacks. Nature Communications, 13(1), 2265.

We have altered the text to read "Furthermore, when the bed slopes inland, the grounding line can be subject to the marine ice-sheet instability (MISI, Schoof (2007)) and competing mechanisms such as accumulation-elevation feedbacks (Sergienko, 2022; Sergienko and Haseloff, 2023)."

4. Line 39: "Only a small number of models support full Stokes" Please specify which models, for example I know that ISSM supports full Stokes.

   In Larour et al. (2012), it is stated that grounding line migration doesn't currently work and we are not aware of any further developments in this direction. We have altered this sentence to read "Only a small number of ice-sheet codes support the use of a full nonlinear Stokes solver for transient marine ice-sheet modelling (e.g. Elmer/Ice; Gagliardini et al. (2013))..."

5. Line 49: I would rephrase this is as "extend FSSA to the ice-ocean interface" rather than "introduce FSSA at the ice-ocean interface".

   Done.

6. Line 56: Suggest motivating the choice for Ekström Ice Shelf here, as it is a very particular choice.

   The choice is made purely because we had already set up the domain for an adjacent project, which has not been published yet. Testing the stabilisation scheme relies on having a set up with a grounding line, so the exact choice of ice shelf is arbitrary.

7. Line 114: It is not clear whether the iteration j is referring to the time step or nonlinear solve step. In either case, how is the coefficient initialized?

   Here, we mean the nonlinear solve step. We have adjusted this to read "...previous nonlinear iteration, $j-1$,...". We have furthermore added the following sentence to clarify the coefficient initialisation: "To ensure that $C|\boldsymbol{u}_\mathrm{b}|^{m-1} \not\to \infty$, including during initialisation when $|\boldsymbol{u}_\mathrm{b}| = 0$, the coefficient is capped at $10^{20}$."

8. Line 130: A description of the algorithm for grounding line migration is needed, especially because the in-text description in Durand et al. (2009) is unclear. I am suggesting using standard algorithm notation to say exactly how the contact conditions are resolved during the nonlinear iterations. This could help illuminate how the stabilization scheme(s) influence grounding line migration.

   In order to address this comment, we have included a description of the grounding line migration as a new subsection.

9. Line 142 and elsewhere: I think it is worth emphasizing that the sea-spring and FSSA really complement each other by introducing implicit approximations of the gravity-driven forcings. That is, it makes sense to use both methods so that the forcings are consistently approximated.

We have added the following sentence in the relevant paragraph: "The *sea spring* numerical stabilisation scheme and FSSA complement each other by balancing forces at the ice-ocean interface." See also our reply to the comment below.

10. Line 168: Suggest providing some intuition behind why Reynolds transport is only applied to the gravitational terms, following some of the descriptions from Löfgren et al. (2022) , e.g., "This makes the FS equations 'aware' of the spatially evolving domain by estimating the impact of the force of gravity at the next time step."

    We have added the following sentence to clarify the purpose of Reynold's Transport Theorem in this study: "Reynolds Transport Theorem enables the Stokes equations to account for a spatially evolving domain by incorporating the effect of gravitational forces at the next time step, thereby treating the right hand side implicitly."

11. Line 176: Are the mass balance terms $a_s$ and $a_b$ evaluated at timestep k or k+1?

    The mass balance terms are time independent in this study. To avoid confusion, we have removed the time-dependence when $a_s$ and $a_b$ are introduced.

12. Line 179: remove "on" and reference previous weak form equation (13)

    Done.

13. Line 182: $\Gamma_{c,z<0}$ is confusing notation. Why not just modify the forcing term to be zero outside the submerged portion?

    We have adjusted the boundary notation to $\Gamma_c$. Eq. (9) has been adjusted accordingly.

14. Line 182: You say the calving front does not move, which is a simplification. This should be mentioned earlier like in section 2.2. Somewhere you need to describe if it is possible to extend the method to the general case where a calving law is implemented, because modelers will want that capability in the future.

    To address this, we have added the sentence, "Our simulations assume a fixed calving front, but we envisage that a dynamic calving front could be numerically stabilized using a similar approach to FSSA by treating relevant terms implicitly."

15. Equation 21: Why does the u have a tilde and k+1 here?

    To address this, we have adjusted Eqs. (25) and (26), so that $\boldsymbol{u}$ is replaced with $\tilde{\boldsymbol{u}}^{k+1}$ and added the sentence, "Here, $\tilde{\boldsymbol{u}}^{k+1}$ is an approximation of the velocity at the next time step, $k+1$."

16. Line 187: It seems like these details about the mass matrix are not used anywhere, so I suggest omitting them.

We have removed these details here. However, we have included information about the mass matrix in the subsection describing the numerical implementation of grounding-line migration (Section 3.3)

17. Line 195: In this paragraph, it seems like you are describing that the FSSA and sea-spring balance out in a way, in line with my previous comment about how they complement each other.

    To emphasise this, we have added the following sentence: "When both the *sea spring* numerical stabilisation scheme and FSSA are active, both the ocean pressure and the ice load are evaluated at the next time step, which results in a balance of forces."

18. Line 205: "size of the largest stable time step size" redundant size

    Fixed.

19. Line 235: The mesh is updated so that the refined area tracks the grounding line position. Specific details about how this is implemented would be helpful.

    We have expanded our explanation to include the following text: "This is done by finding the grounding line position, $x_{\mathrm{GL}}$, and defining the region within which the horizontal mesh should be refined, i.e. $[x_{\mathrm{GL}} - 50, x_{\mathrm{GL}} + 50]$ km. Outside this region, a linearly decreasing resolution is prescribed. Implementation details using the GMSH software (Geuzaine and Remacle, 2009) can be found in Henry (2025).

20. Line 250: This paragraph described a lot of numerical details about how you are solving the free surface equations. Consider writing down the discretization details to show exactly what you are solving, as you did with the Stokes problem.

    We have added the following text to Section 2.3:

    "In order to solve the free-surface equations, they are discretised in time as

$$z_s^{k+1} + \Delta t u_x \frac{\partial z_s^{k+1}}{\partial x} + \Delta t u_y \frac{\partial z_s^{k+1}}{\partial y} = z_s^k + \Delta t(u_z + a_s), \tag{1}$$

and

$$z_b^{k+1} + \Delta t u_x \frac{\partial z_b^{k+1}}{\partial x} + \Delta t u_y \frac{\partial z_b^{k+1}}{\partial y} = z_b^k + \Delta t(u_z + a_b). \tag{2}$$

In weak form, the equations are multiplied by a test function, $\phi$, and integrated over the upper and lower ice surfaces, so that

$$\int_{\Gamma_\mathrm{a}} \left[ z_s^{k+1} + \Delta t u_x \frac{\partial z_s}{\partial x} + \Delta t u_y \frac{\partial z_s}{\partial y} \right] \phi \, d\Gamma = \int_{\Gamma_\mathrm{a}} [z_s^k + \Delta t(u_z + a_s)] \phi \, d\Gamma \tag{3}$$

and

$$\int_{\Gamma_\mathrm{a}} \left[ z_b^{k+1} + \Delta t u_x \frac{\partial z_b}{\partial x} + \Delta t u_y \frac{\partial z_b}{\partial y} \right] \phi \, d\Gamma = \int_{\Gamma_\mathrm{a}} [z_b^k + \Delta t(u_z + a_b)] \phi \, d\Gamma. \tag{4}$$

A backwards difference formula (BDF) time-stepping scheme of first order (i.e., implicit Euler) is employed."

21. Figure 4: There are five colors and three linestyles in this figure. It is impossible to parse the results with all the closely spaced and overlapping colors and textures. Please split these line plots into multiple panels for the different timestep sizes, with the reference solution plotted in each panel.

    We have converted the two-panel figure to a four-panel figure, separating the panels by time step size. It remains difficult to decipher the differences between the $\Delta t = 50$ year simulations so we have add the following sentence to the caption: "Please note that differences in grounding line position between simulations with a time step of $\Delta t = 50$ are negligible."

22. It's never explained why the referenced grounding line solution in Figure 4 has a mesh resolution of 200m. I found this confusing because Figure 4b and the results section seems to use the reference solution to measure the "accuracy" (Line 264) of the stabilization methods, but it's not clear to me why this should be the case. Wouldn't a higher resolution simulation be better to measure accuracy?

    With our choice of mesh, a fine spatial resolution is infeasible with such a small time step size. To address this, we have added the sentence "A finer spatial resolution is computationally infeasible because a time step size of $\Delta t = 0.5$ is too small to allow for reasonable simulation times."

23. Figure 5: Same comment about too many colors and linestyles. I cannot easily interpret this figure. Also, in the caption, specify that theta=0 is the black line.

    We have expanded the figure into two subfigures and specified in the caption that the black line represents the theta=0 simulation.

24. Line 286: Maybe this is mentioned in the Discussion (Line 383), but how does FSSA consistently having a further oceanward grounding line position relate to the previous discussion of changes in the predicted ice load or water pressure (or thickness) in advancing or retreating cases?

    We noticed some repetition in this paragraph, so we have removed one sentence. To address this comment, we have added a sentence mentioning that the reasons behind the mismatch are discussed later: "The reasons for this mismatch are discussed in Section 6.1."

25. Figure 7 is missing units in the axis labels.

    Fixed.

26. Line 296: This sentence is confusing because it's unclear if "respectively" is referring to the timestep sizes or spatial resolution.

    We have split this sentence in two to ensure clarity. The text now reads "In simulations with a horizontal grounding-zone resolution of $\Delta x = 25\,\mathrm{m}$, the speedup between time step sizes of $\Delta t = 10$ and $\Delta t = 50$ was 4.4. Simulations with a horizontal grounding-zone resolutions of $\Delta x = 200\,\mathrm{m}$ showed a similar speed up of 3.8 when comparing time step sizes of $\Delta t = 10$ and $\Delta t = 50$."

27. Line 308: How exactly were the friction coefficient and ice fluidity tuned?

    The friction coefficient and ice fluidity were tuned by testing combinations of increments in both parameters. The combination that fit with observations the closest was taken. This was not done with formal analysis given that the aim of the study was not to predict future scenarios. We have added the clause "...in an ad hoc manner given that the purpose of the study is to investigate the applicability of FSSA and not to make projections".

28. Line 323: Is this Neumann condition like a weak penalty condition? Do these numerical details belong in Section 2 or Section 3?

    Yes, this is a weak penalty condition that mimics a Dirichlet boundary condition, $\boldsymbol{u} \cdot \hat{\boldsymbol{n}} = 0$. To address this comment, we have placed this text in Section 2.2. We have also added the text, "This boundary condition takes the form

    $$\hat{\boldsymbol{n}} \cdot (\boldsymbol{\sigma} \cdot \hat{\boldsymbol{n}}) = -C_{\mathrm{n}} \boldsymbol{u} \cdot \hat{\boldsymbol{n}}.$$

    This formulation alleviated spurious artifacts at the ice divide, which occurred when implementing a strong-form impenetrability condition (Seddik et al., 2017). We choose a value of $C_n = 10^{12}$ MPa m$^{-1}$ a in this weakly enforced boundary conditions." We have corrected the text and changed "Neumann" to "Robin".

29. Line 340: Convergence tolerances have already been stated previously in Section 4, so I suggest removing unless they are really necessary here.

    Done.

30. Section 5: The evolution of the grounding line position is not shown for the 3D results. I know this might be more tedious for the 3D simulation, but it seems necessary as the paper is about grounding line dynamics.

    We will refrain from including a figure of the grounding line migration, the reason being that due to the small slopes, the grounding line is not as well defined as in the simulations with a higher slope. However, we have added a few sentences to the manuscript to the manuscript that include a quantification of the grounding line position advance/retreat in the transect $\boldsymbol{A} - \boldsymbol{A}'$.

31. Line 347: How does buttressing relate to the thickness statistics in the previous sentences? I don't see the connection

    We have removed the sentence that mentions buttressing.

32. Line 349: clarify what "good convergence" means here. Converged in a small number of iterations?

    Yes, we have now replaced this with "...converge in a small number of iterations..."

33. Figure 9: Include horizontal ticks on the upper panels

    Fixed.

34. Line 363: Delta is missing a t

    Fixed.

35. General suggestion to discuss applicability to shorter time scales, e.g., fast evolving or vulnerable systems like Thwaites, and 100-yr projections. The discussion on Lines 393-396 needs to appear or be mentioned much earlier like in the introduction.

    We had moved these sentences to the Introduction.

36. Line 370: What does "correction of datasets" mean? Are you referring to bed topography?

    Yes, we are referring to the bed topography. In our experience, such datasets tend to have some erroneous anomalies in the vicinity of the grounding line. To address this comment, we have clarified in the text that we are referring to the "correction of bed elevation datasets".

37. Line 382: Change "hypothesis" to "hypothesize"

    We have changed this to "hypothesise" to align with British English spelling standards.

38. The attached code repository needs a readme that describes the different files, dependencies, how to run the Elmer code, etc.....

    We have uploaded a new code repository that includes a README.

# References


J. Ahlkrona, A. C. J. Henry, and A. Löfgren. A fully implicit second order method for viscous free surface Stokes flow – application to glacier simulations. *Geoscientific Model Development*, 19(6):2333–2348, 2026. doi: 10.5194/gmd-19-2333-2026.

O. Gagliardini, T. Zwinger, F. Gillet-Chaulet, G. Durand, L. Favier, B. de Fleurian, R. Greve, M. Malinen, C. Martín, P. Råback, J. Ruokolainen, M. Sacchettini, M. Schäfer, H. Seddik, and J. Thies. Capabilities and performance of Elmer/Ice, a new-generation ice sheet model. *Geoscientific Model Development*, 6(4):1299–1318, 2013. doi: 10.5194/gmd-6-1299-2013.

C. Geuzaine and J.-F. Remacle. Gmsh: A 3-D finite element mesh generator with built-in pre-and post-processing facilities. *International journal for numerical methods in engineering*, 79(11):1309–1331, 2009. doi: 10.1002/nme.2579.

A. C. J. Henry. Code for the publication "Grounding-line dynamics in a Stokes ice-flow model: Improved numerical stability allows larger time steps", 2025. URL `https://doi.org/10.5281/zenodo.19596308`.

B. J. Kaus, H. Mühlhaus, and D. A. May. A stabilization algorithm for geodynamic numerical simulations with a free surface. *Physics of the Earth and Planetary Interiors*, 181 (1-2):12–20, 2010. doi: 10.1016/j.pepi.2010.04.007.

E. Larour, H. Seroussi, M. Morlighem, and E. Rignot. Continental scale, high order, high spatial resolution, ice sheet modeling using the ice sheet system model (issm). *Journal of Geophysical Research: Earth Surface*, 117(F1), 2012.

A. Löfgren, T. Zwinger, P. Råback, C. Helanow, and J. Ahlkrona. Increasing numerical stability of mountain valley glacier simulations: implementation and testing of free-surface stabilization in Elmer/Ice. *The Cryosphere*, 18(8):3453–3470, 2024. doi: 10.5194/tc-18-3453-2024.

A. Löfgren, J. Ahlkrona, and C. Helanow. Increasing stable time-step sizes of the free-surface problem arising in ice-sheet simulations. *Journal of Computational Physics: X*, 16:100114, 2022. ISSN 25900552. doi: 10.1016/j.jcpx.2022.100114.

J. M. Rignot, E. and B. Scheuchl. Measures insar-based antarctica ice velocity map, version 2, 2017. URL `https://nsidc.org/data/NSIDC-0484/versions/2`.

C. Schoof. Ice sheet grounding line dynamics: Steady states, stability, and hysteresis. *Journal of Geophysical Research*, 112(F3):F03S28, 2007. doi: 10.1029/2006JF000664.

H. Seddik, R. Greve, T. Zwinger, and S. Sugiyama. Regional modeling of the shirase drainage basin, east antarctica: full stokes vs. shallow ice dynamics. *The Cryosphere*, 11 (5):2213–2229, 2017. doi: 10.5194/tc-11-2213-2017.

O. Sergienko and M. Haseloff. 'stable'and 'unstable'are not useful descriptions of marine ice sheets in the earth's climate system. *Journal of Glaciology*, 69(277):1483–1499, 2023.

O. V. Sergienko. No general stability conditions for marine ice-sheet grounding lines in the presence of feedbacks. *Nature Communications*, 13(1):2265, 2022. doi: 10.1038/s41467-022-29892-3.

T. Westling Dolling, A. C. J. Henry, and J. Ahlkrona. A numerical stabilization scheme for the shallow shelf approximation. *arXiv*, 2025. doi: 10.48550/arXiv.2510.02943.