Dear Referee,

We are grateful for the highly insightful and constructive feedback you have provided us with. Below you will find our point-by-point response to each of your comments.

Sincerely,

André Löfgren, Josefin Ahlkrona, Thomas Zwinger, Peter Råback, and Christian Helanow

General Comments

COMMENT: The manuscript "Increasing numerical stability of mountain valley glacier simulations: implementation and testing of free-surface stabilization in Elmer/Ice" applies a method developed in the context of mantle-convection modeling to stabilize the evolution of the free surface. The method has been already studied in the context of ice sheet modeling but with simplified problems. I think the paper can be useful to the ice sheet community. However, the presentation should be improved and critical details about the discretization of the equations should be added. In general, I wish the authors were more ambitious with this work, trying to understand better the method from a theoretical point of view, considering higher-order time-discretizations and exploring the effect of different spatial discretizations of the free-surface equation with the coupling method proposed.

RESPONSE: Thank you for sharing your overall impression, it pleases us that you believe our paper might be useful to the ice-sheet community. To raise our ambition to your expectations we've decided to include two more studies. In the first study we consider a retreating glacier starting from the final surface obtained in the Perlin glacier case; within this experiment we'll explore how upwinding (residual-free bubbles) affects the LST (largest stable time-step size). In the second experiment we'll include multiple mesh resolutions to study the effect of mesh resolution on the LST. However, while we also believe that considering higher order time-stepping is of great interest we have, given what we believe is feasible within this review, decided to limit the current study to only look at first order time stepping. We'll mention, however, that this is something to consider for future studies. It's also something we're already planning to do within a future project.

Comments

COMMENT 1: Eq. (7): This is a pet peeve of mine, but I think that calling the friction coefficient β^2 is really bad notation despite being commonly used in ice sheet modeling. It makes you think that its square root is a meaningful physical quantity. Why not just call it β, or C or μ?
RESPONSE: The β^2, I believe, is to indicate that it is a positive quantity. We'll follow your suggestion instead and use β to denote the friction coefficient and explicitly state that $\beta \geq 0$.

COMMENT 2: Section 3.1: This section contrasts an explicit solution of the Stokes free-surface equation with an implicit solution of such equations. However, there are hidden assumptions in both approaches. As for the explicit solution, it seems that the authors have in mind a low-order, explicit time-integration scheme like Forward Euler. However, other high-order scheme could

be used. As an example, one could use the Runge-Kutta scheme, where the Stokes equation would need to be solved multiple time per time step (at each stage of the scheme). Regarding the implicit solution, in addition of limiting the analysis to a low-order scheme like Backward Euler, they also assume that the coupled Stokes free-surface problem would be solved by iterating the solution of Stokes and the free-surface problem until convergence. This implies an outer nonlinear iteration loop (at each iteration the Stokes and free-surface equations are solved) and an inner nonlinear iteration loop for solving the Stokes problem. While this might be the simplest method to implement, it is likely not the most efficient. In fact, one could consider the Stokes and free-surface coupled problem in a monolithic fashion and solve it with a single iterative scheme, linearizing at the same time Stokes and the coupled Stokes free-surface problem. I recommend that the authors better explain their choices and possible alternatives.
RESPONSE: You're right that also higher-order explicit methods such as RK4 would involve solving the Stokes equations multiple times in each time step. We'll clarify in the manuscript that we're considering first-order time stepping. Regarding the implicit solver, other linearizations than the Picard linearization we have in mind might be more efficient; the point, however, is that implicit time stepping in general is considerably more expensive than explicit. Furthermore we believe it's a relevant example as it is the only implicit scheme we are aware of implemented in a large ice-sheet solver (e.g., Elmer/Ice). We'll also mention this in the manuscript.

COMMENT 3: Eq. (11): I would write here eq (10) here as well, with u evaluated at times t^k and all the domains evaluated at time t^k except for the forcing term that is evaluated at time t^{k+1}. This is the scheme you are after, that is, account for the evaluation of the forcing term at the time t^{k+1} to anticipate the effect of the domain change at least on the forcing term.
RESPONSE: Thank you, we'll rephrase this.

COMMENT 4: Figure 2: Please mention at least in the caption that these are not the only two time-stepping / coupling options.
RESPONSE: We'll clearly specify that this particular example, as used in Elmer, corresponds to a Picard linearization.

COMMENT 5: Line 131: Definition of inner product. Inner product has two arguments. What you wrote and what you used in eq. (10) is just an integral over the domain Ω. If you want to call it inner product then use the notation $(\cdot, \cdot)_\Omega$.
RESPONSE: Thank you, we'll change the notation.

COMMENT 6: Lines 150-153: This part is confusing. I think there are a couple of typos. Eq (13) is referenced instead of equation (12), and "first term on the left-had side" should be "first term on the right-hand side". Further, it is not true that in eq. (12) the domain is assumed to move only due to the velocity of the deformation. Eq. (12) is Reynolds theorem and is valid in general.

However, u_b is the velocity of the ice boundary, which is not the velocity of the ice at the boundary. At the surface, $u_b = u + a_s \hat{z}$, that is, the velocity of the surface is the sum of the velocity of the ice u and of the accumulation/ablation rate. Please rephrase this paragraph.
RESPONSE: We'll fix the typos and rephrase this paragraph.

COMMENT 7: Eq. (13): typo, check the subscripts.
RESPONSE: Thank you for catching this.

COMMENT 8: Section 3: Please add details about the discretization of the Stokes equations and the free surface equation. In particular, are you using any stabilization for (11), e.g., upwind, flux limiters, SUPG? How the spatial discretization of (11) and (14) affect the effectiveness of the proposed approach?
RESPONSE: Thank you, we'll add a paragraph on the spatial discretization of Stokes in Sect. 3.2. In particular we'll state the elements and stabilization used. For the free-surface equation we'll add another subsection on the weak formulation and state the stabilization scheme we're using (residual-free bubbles for Perlin and SUPG for Midtre Lovénbreen). To address your concern regarding how the spatial discretization affect the FSSA, we'll include a 2D experiment evaluating the impact of adding upwinding into Eq. (11). For this purpose we'll evaluate the largest stable time-step size (LST) for the three cases: FSSA and upwinding, FSSA and no upwinding, no FSSA and no upwinding.

COMMENT 9: Section 4.2.1: In addition to the results presented, it would be informative to have results where the same (relatively fine) mesh is used for all the simulation (including the reference one). This would help separating the effect of the spatial discretization from the time discretization, which is the main focus of the paper
RESPONSE: Good suggestion, we'll add such a case in the experiment considering different mesh resolutions.

COMMENT 10: Appendix A: Can you detail how the "random generated gradients" are generated?
RESPONSE: The gradients are random generated from a uniform distribution over [-1, 1], i.e., gradients with slope angles between $-45°$ and $45°$. We'll mention this in the manuscript.