Dear Referee,

Thank you for the time and effort you have devoted to providing us with constructive and highly valuable feedback. We hope you find our proposed suggestions to enhance the manuscript to a shape ready for publication.

Sincerely,

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

## General Comments

**COMMENT**: In this manuscript the authors describe a technique for approximating the Backward Euler method for time stepping in a coupled Stokes-free surface system for glacier evolution. The so-called free surface stabilization algorithm only involves the inclusion of an additional term in the Stokes equations, and so is cheap to implement and seems to increase the size of allowable time steps (though probably not to the same degree that a proper implicit solver would). The main difference between this paper and a previous one by the same author on this subject is its implementation in Elmer/Ice and its application to a realistic geometry. I think the paper is a nice contribution and the method described is potentially useful. Unfortunately, I think that the work suffers from a lack of specificity that hampers a careful reader from really understanding the performance characteristics of the method. Towards the purpose of improving clarity and providing a more sober view of what can be expected of this method, I have included some comments below.

**RESPONSE**: Thank you for this nice summary, and for your suggestions for improving the clarity of the manuscript. Below you will find our point-by-point response to each of your comments.

## Comments

**COMMENT 1**: L29: I don't think models 'suffer' from time-step restrictions, but they are subject to them. They are not necessarily 'parabolic' either - when flow is dominated by bedrock slopes, the equations have a more hyperbolic character.
**RESPONSE**: We'll change the wording, and specify that the parabolic time-step size constraint is valid under shear-dominated flow.

**COMMENT 2**: L32: Here and elsewhere, the word 'stability' is used without precision. How is this concept characterized here? Is it just the lack of visibly detectable wiggles? Is it when a simulation blows up?
**RESPONSE**: By instability we mean the usual magnification of truncation and round-off errors. We'll define what we mean by instability in the introduction and for each experiment add a sentence on how we detect them. However, finding a consistent criteria for detecting instability has proven to be difficult in this study. For example in the Perlin case, detecting instability by means of checking the norm of the vertical velocity was useful to determine the presence of instability. On the other hand, for the Midtre-Lovénbreen case, the solution didn't blow up despite being considerably off from the reference, as indicated by Fig. 1 below (Fig. 8 in the manuscript). We believe this is due to the considerably negative surface-mass balance in that case, which causes glacier thinning, and in a sense "stabilizes" the solver such that it never blows up, but is still pol-

luted by the initial instability. For this reason we in this case take the presence of sloshing (i.e., spurious shifts in the sign of the vertical velocity) to indicate instability, rather than the norm of the vertical velocity blowing up to infinity. We'll elaborate on this point in the manuscript.

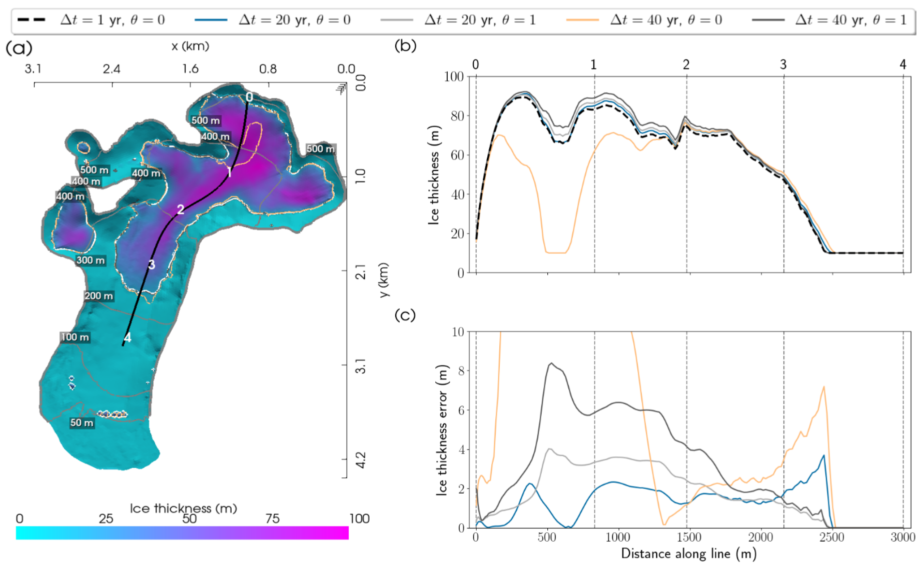


Figure 1: Midtre Lovénbreen at year 2195. The left panel (a) shows the glacier outlines of the reference solution (white) using a fine time-step size $\Delta t = 1$ yr, as well as outlines for a simulation using a larger $\Delta t = 40$ yr, with FSSA (dark gray) and without FSSA (orange). The thickness of the glacier as given by the reference solution is indicated with colors in panel (a). The upper right panel (b) shows the ice thickness along the black line in panel (a), for the reference solution (dashed black line in (b)) and simulations with and without FSSA for $\Delta t = 20, 40$ yr (solid lines in (b)). The lower right panel (c) shows ice thickness errors as compared to the reference solution.

**COMMENT 3**: L120: This comparison is a bit contrived because it relies on one particular paradigm for solving free-surface Stokes, namely that the nonlinear coupling is managed via Picard iteration between the velocity and thickness solves. There are alternatives: solving simultaneously with Picard, using Newton's method (although these are admittedly both easier to implement in a terrain-following coordinate system). These alternatives don't necessarily involve solving Stokes more than once in the way that is described here. It's important to be specific!
**RESPONSE**: We'll clarify that this specifically corresponds to a Picard linearization.

**COMMENT 4**: Eq. 3.1: This might be an appropriate place (although there are others) to mention the very important condition for all of your equations, namely that they are only valid when $z_s > z_b$! Also, how do you deal with this constraint (presuming that you do, because the 'ice-free' region in the Midtre Lovénbreen experiment expands).
**RESPONSE**: Thank you, we'll add a paragraph elaborating on this.

**COMMENT 5**: L132: These 'appropriate function spaces' are never stated explicitly. Presumably the Taylor-Hood element is used here?
**RESPONSE**: Appropriate function spaces refers to those satisfying the so-called *inf-sup* condition, e.g., Taylor-Hood elements. In case the function spaces do not satisfy this condition, as is the case for equal-order bilinear element (e.g., P1-P1 elements), stabilization has to be introduced into the weak formulation in order to circumvent this condition. In our study we are using the P2-P1 Taylor-Hood elements for 2D, while due to the 3D case being much more computationally expensive we opt to use a GLS stabilized formulation with P1-P1 elements. We'll insert a sentence in the methodology for each experiment where we mention this.

**COMMENT 6**: Eq. 11: How is the transport equation discretized in space? A finite element method? If so, which function space? If it's solved nodally, then how is the spatial derivative in surface elevation calculated? This is an advection equation, so often requires a stabilization scheme, e.g. upwinding. Is that done here? Does whatever representation of the surface elevation satisfy an inf-sup condition?
**RESPONSE**: Yes, it's discretized using FEM, and we do indeed use upwinding, specifically we're using residual-free bubbles for the 2D case and SUPG in 3D. We'll add a section (Sect. 3.4) on the spatial discretization and stabilization of the free-surface equation. Regarding the inf-sup condition, we're not sure what you are referring to; do you mean an inf-sup stability restriction between the surface $h$ and the velocity $\mathbf{u}$ as when they are treated as unknowns in a monolithic fashion? Interesting to think about, but we do not take any such restriction into account.

**COMMENT 7**: L150: Is this supposed to be referencing Eq. 12? If so should it be that the first term on the right side of Eq. 12 is zero?
**RESPONSE**: Thank you for catching this. You are indeed right.

**COMMENT 8**: Eq. 13: Maybe worthwhile to say that you're using a forward Euler discretization of the time-derivative in Eq. 12. Also, the superscript on $\Omega$ seems to be messed up.
**RESPONSE**: Thank you. We'll fix the superscript and add a sentence mentioning that we are using forward Euler.

**COMMENT 9**: Eq. 14: Does the $u$ that appears in the 'new' part of the

weak form need a superscript too?
**RESPONSE**: Good catch, it should indeed.

**COMMENT 10**: Eq. 15: I'm not sure that including the equation adds anything here. It might be clearer to just write that 'in the case of the SIA, the FSSA coincides precisely with evaluating the pressure at the end of the time integration. In the case of the Stokes' equations, this is an approximation, etc.
**RESPONSE**: Very elegantly put, we'll adopt this in the manuscript.

**COMMENT 11**: Eq. 18: It's worth noting that with this strictly non-negative mass balance and no way for mass to enter or leave the system, that this glacier will grow without bound. This is unfortunate because it would be interesting to see the result of applying this method to grow a glacier to steady state
**RESPONSE**: Thank you for the suggestion. We'll add such an experiment for two-dimensional Perlin case. The experiment we have in mind is to start from the surfaces obtained at the end of the current simulations, and then introduce ablation into the SMB and continue the simulation for a few hundred years (until a steady state is reached).

**COMMENT 12**: L210: I think it's unfortunate that the solution is only compared to other model results run with a smaller time step. A more robust and complementary approach would be to evaluate this method against a manufactured solution. This would also potentially provide insight into the ambiguous results later about whether the FSSA is more or less accurate than without.
**RESPONSE**: Thank you for the suggestion, comparing against a manufactured solution would indeed be interesting. However, it is our experience that such solutions are far from real-world applications; constructing a manufactured solution that represent such cases we think is beyond the scope of this paper. **Everyone, feel free to chime in here.**

**COMMENT 13**: L216: Again, how is stability defined? Is the LST computed by using bigger and bigger time steps until the solver produces NaNs?
**RESPONSE**: Please see our answer to comment 2.

**COMMENT 14**: L240: Could this be explained in a way that relates more closely to theory? In principle, so long as the CFL condition is satisfied, the forward Euler and backward Euler (which the FSSA approximates) have the same order of numerical accuracy. Why would the accuracy deviate between how the time derivative is discretized?
**RESPONSE**: Good point, which we'll elaborate on. We should indeed expect this, but since the method is just an approximation of backward Euler, we thought it would be a good sanity check to confirm that the order of accuracy for stable solution is what we expect, i.e., linear.

**COMMENT 15**: Fig. 3: I really struggle to distinguish between the lines.

Can these be made thicker, or the plot larger or something to make this more easily seen?
**RESPONSE**: We'll make the lines thicker and the plots bigger.

**COMMENT 16**: L302: What is the 'derivative of the viscosity' ? Do you mean the whole Jacobian? If so, then including a relaxation parameter is pretty standard.
**RESPONSE**: That is indeed the case. The "new" refers to new in Elmer/Ice, we'll clarify this.

**COMMENT 17**: L324: I don't understand the notion of higher or lower accuracy for advancing or retreating glaciers. This needs to be justified or removed.
**RESPONSE**: We'll remove this sentence.

**COMMENT 18**: L329: Did these instabilities only appear in the absence of the FSSA or withit too? Later text seems to indicate the former, but it's not clear here. What does it mean for an instability to be 'specific to the setup' ? That would seem like a very bad property to not know whether a simulation is going to be stable or not a priori. I can't see how 'other' instabilities are being suppressed here - this doesn't seem to be shown.
**RESPONSE**: For the cases presented, no such instabilities occurred for FSSA. But we have during the review process found cases where the spikes also occur for FSSA, so we'll remove this conclusion.

**COMMENT 19**: Table 2: How do we know its 20 and not, say 17.8 or something?
**RESPONSE**: We tested also for $\Delta t = 25$ yr, so you're right it is something between 20 yr and 25 yr. However, we report 20 yr as the LST since it was the largest time-step size we tested and had a stable solver. This does not affect our conclusion that the FSSA allows for at least twice as large time steps. We'll clarify this in the manuscript.

**COMMENT 20**: L355: I don't understand what a 'more viscous behavior' means.
**RESPONSE**: By more viscous behavior we mean a larger shear- to slip-velocity ratio, which we'll clarify in the manuscript.

**COMMENT 21**: Appendix A: I don't think it's all that relevant as to how bedrock was generated (there are many methods of doing this, e.g. Gaussian random fields, random fourier features, etc.), but nonetheless this section is quite opaque. It might be better just to reference something rather than include this sort of insufficient description.
**RESPONSE**: It is indeed not the focus of the study, but we felt that it was warranted to include as it gives the reader insight into how we constructed our glacier. We'll, however, shorten it down to make it more concise.