Dear reviewers and editor,

Thank you very much for your constructive feedback on our manuscript, to which we respond below. Corresponding edits in the manuscript are marked in magenta (in the tracked changes version).

Best regards,

Josefin Ahlkrona, Clara Henry, André Löfgren

Response to Reviewer 1

Summary and general comment, Reviewer 1

The article presents an ad-hoc-stabilized scheme for implicit time-evolution of the viscous free-surface Stokes equations. The idea relies on a relaxed explicit Euler discretization of the transport theorem that ties the discrete solution at two consecutive time steps. The main technical concern pertains to the introduction of a relaxation parameter controlling the size of the time-step appearing in the regularization. Its role is not discussed in full detail.

Since this parameter effectively reduces the time step size in the stabilization (but only in the stabilization and not in the general time-marching scheme) it introduces a mismatch in the timescales (and time grids) involved in the discretization. Thus, if the value of this parameter is not one, then it may introduce an inconsistency and lead to loss of convergence. The inconsistency, however, may only become dominant for much smaller values of the time step than the ones feasible in the article's simulations. The information provided in the text is not nearly enough to determine if this indeed would be the case, or if some additional measures are taken to avoid the issue. As a side comment, this "reduced time step" could potentially be interpreted as an intermediate stage (as in a Runge-Kutta method). This technique is relatively common for stiff problems, but the numerical solutions at these intermediate steps should not be given any physical meaning (just like the stages of an R-K method are not). In that sense the value of the parameter theta could be taken to be of the form 1/n where n is the number of intermediate stages to be discarded, interpreting only every "n-th" step as a physical solution. Unfortunately, the manuscript does not discuss this parameter with enough details to determine if this interpretation or idea is correct.

Nevertheless, the authors provide numerical evidence and sufficient comparisons that seem to indicate stability and reliability of the technique in some realistic situations of simplified (2D) glaciar flows.

The article is in general well written and, modulo the aforementioned discussion of the re-

laxation parameter, and the correction of several details scattered through the manuscript (see files attached), the referee can recommend the article for publication.

Reply: Thank you for raising the issue of the parameter θ. The parameter has indeed just simply carried over from earlier articles, without being discussed. We have, in all experiments of this paper, set $\theta = 0$ or $\theta = 1$, as this is as you say the most natural choice, leading to an approximation of the gravity force on the next time-step. A full understanding of the role of θ in terms of both accuracy and stability does not yet exist, even without implicit iterations. In previous works, we have tested different values of θ for FSSA-stabilized simulations (without the implicit iterations) and found that $\theta < 1$ decreases stability compared to $\theta = 1$ but slightly increases accuracy for some settings, see eg. Löfgren et al 2022 (`https://www.sciencedirect.com/science/article/pii/S2590055222000105`). Some of our theoretical work suggests that for a similar FSSA-stabilization, $\theta = 0.5$ is enough for stability (`https://arxiv.org/abs/2506.10447`) (without implicit iterations).

Reply: We have for the purpose of this revision performed some tests of varying θ as $\theta = 0.5, 0.9, 1.0, 2.0$, for BDF2 and BDF1 and $\Delta t = 10$ on the epximerental setup of section 6. We get the following results:

θ	BDF1	BDF2
0.5	*Diverged*	*Diverged*
0.9	*Converged, average nof iterations: 14*	*Converged, average nof iterations: 13*
1.0	*Converged, average nof of iterations: 13*	*Converged, average nof iterations: 16*
2.0	*Diverged*	*Diverged*

(Reply:)

Reply: The results supports the choice of $\theta = 1$ and calls for future theoretical studies of the convergence of this scheme. In the manuscript, we have removed θ in equation 17 together with the comment "Note that the original FSSA method of Kaus2010 includes a parameter θ in front of the stabilization term which will not be used here". We have also indicated that θ_1 and θ_2 are set to 0 or 1.

Specific comments, Reviewer 1

Numbers indicate annotations on the manuscript (see the attached file). There are two threes to match the annotations in the manuscript.

1. limit

   Reply: Corrected.

2. Illustrate explicitly in the figure the domain ω

   Reply: We have added a bar |——| that illustrates ω.

3. either "the boundary terms vanish" or "the boundary term vanishes"

   Reply: Corrected.

3. Illustrate the region ω explicitly in figure 1

   Reply: We have added a bar |——| that illustrates ω.

4. "At the [free or top] surface... or even better, "on Γ_s"

   Reply: Changed to "the top surface Γ_s"

5. Indicate explicitly "on Γ_s" in equation (5)

   Reply: Added.

6,7 "on"

   Reply: Corrected.

8. Indicate explicitly "on Γ_b" in equation (8)

   Reply: Added.

9. Are all the curly brackets really necessary?

   Reply: No, thanks for pointing this out, they are now removed.

10. This expression and several related ones later on seem to be missing the terms corresponding to the y coordinate. In the numerical experiments at the end the problem is considered in only 2D, but the rest of the discussion pertains to the full 3D problem, and so the "y-terms" must be included or an explicit comment should be made at this point regarding why only a 2D slice is considered. Compare for example with eq (14), where the "y-terms" do appear and then eq. (15) when they disappear back.

    Reply: We have now included the y-terms everywhere in the manuscript except for in the numerical experiment section.

11. See previous remark

    Reply: See previous reply.

12. See remark 10

    Reply: See previous reply.

13. $W^{1,2}$

    Reply: Corrected.

14. The line numbers in the algorithm are missing

    Reply: Corrected.

15. This statement is purely speculative. Either provide a proof/stringer argument or supress the sentence.

    Reply: We changed the statement to "for large time-steps".

16. do

    Reply: Corrected.

17. "... on weak form, recalling that $\partial_t(\rho\mathbf{g} \cdot v) = 0$, which leads to"

    Reply: We changed to "assuming that $\frac{\partial}{\partial t}(\rho g \cdot v) = 0$" (since there is an assumption that the test-functions are time-independent).

18. The first equality seems unnecessary

    Reply: Removed.

19. The stabilization seems to consist of connecting the terms at times k and k+1 through an explicit Euler discretization. In particular, the substitution of eq (17) into the right hand side of (1). The Euler discretization comes with the addition of a factor θ controlling the size of the time step. This is referred to as "user defined parameter". However, unless theta is identically equal to one, this would result on an inconsistent discretization and therefore would lead to failure of convergence. The role of the parameter needs to be discussed in more detail, taking into account the loss of consistency that it introduces.

    Reply: We have now removed θ from the manuscript, see our reply to the general comment. We are interested in exploring the role of θ in a theoretical setting in the future.

20. Just as in comment 15, this statement is purely speculative and it must be supported strongly or dismissed. In what specific sense do these solutions "mimic" the ones arising from an implicit discretization? Rather than this speculative statement it could be said that "numerical experiments seem to indicate that this results in more stable discretizations" simply pointing to the experimental evidence without speculating.

    Reply: We have removed this sentence.

21. This line belongs in the previous paragraph

    Reply: Corrected.

22. Line numbers missing

    Reply: Corrected.

23. This expression is missing the "y-terms" (see 10)

    Reply: Added.

4

24. indeed

   Reply: Corrected.

25. missing "y-terms" (see 10)

   Reply: Added.

26. This is not a distance, but it is a difference

   Reply: Corrected.

27. better factoring out Δt

   Reply: Done.

28. "...side of (15) and the expression above..."

   Reply: Added.

29. " , leading to: "

   Reply: Added.

30. this sentence should be right after the equation

   Reply: Corrected.

31. "is substituted into the Stokes equations"

   Reply: It is actually added, since the right hand side $(\rho g, \mathbf{v})_{\Omega^{k+1}} \approx (\rho g, \mathbf{v})_{\Omega^k} +$ FSSA-terms

32. "A key observation is that, if the iteration converges,"

   Reply: Added

33. this is not a distance, in this case "difference" can be used

   Reply: We choose displacement here. Note that there no intergration over the domain in these expression. The unit is $year \cdot meter/year = meter$. We have changed to "displacement" or "difference" or "net distance" in other places where we have used the term distance.

34. mark
   Reply: Corrected.

35. this is not a distance, but in this case "displacement" can be used

   Reply: Corrected.

36. two
   Reply: Corrected.

37. from

    Reply: Corrected.

38. "y-terms" missing, see (10)

    Reply: Corrected

39. Is this the domain in Figure 3? If so, it should be mentioned

    Reply: Yes, we have now added a reference to Figure 3.

40. This is not an error, unless you have an error estimator, it is simply the convergence tolerance.

    Reply: We have now changed "error tolerance" to "convergence tolerance" everywhere.

41. This figure is not referenced explicitly in the text. It should.

    Reply: It is now referenced in the beginning of the experimental setup section.

42. This is not measuring an error, so the term "accuracy" is incorrect. Do you mean that this is used to determine convergence of the iteration?

    Reply: Yes, we have changed to "the convergence is monitored..."

43. are

    Reply: Corrected.

44. this is not an error, it is the iteration tolerance

    Reply: Changed to "convergence tolerance"

45. Once again, this is not an error, unless there is an error estimator that is not mentioned anywhere

    Reply: We have now changed this to "terminate the iterative solver if $\|\Delta h\|$ grows"

46. "stabilization to be stable" is there such a thing as an unstable stabilization? Then it is no stabilization at all

    Reply: We changed the sentence to: "Just like the the first-order Euler method, the simulations are unstable without the stabilization"

47. Do you mean "convergence of the second order method"?

    Reply: We changed this sentence to "Just as with the full subtraction FSSA, achieving second-order convergence requires only two coupled iterations."

48. types

    Reply: Corrected.

49. remove comma

    Reply: Corrected.

50. Except for the first three points, all other errors are large to the point of making the numerical solution completely meaningless, so making a distinction between the intermediate and large step sizes seems a moot point

    Reply: We prefer to keep it in case it could be of use to some reader.

51. Forward Euler

    Reply: Sorry this is Swenglish, we changed to Forward Euler everywhere.

52. This is not an error, it is simply the relative change between iterations

    Reply: Changed to "convergence tolerance".

53. Iteration tolerance

    Reply: Changed to "convergence tolerance".

54. convergence or iteration tolerance

    Reply: Changed to "convergence tolerance".

55. stabilization

    Reply: Corrected.

56. This point is quite important and is somewhat glossed over very lightly. It should be discussed in more detail, or at least references to a deeper discussion should be provided

    Reply: We have tried to clarify the description, and also added a reference to Section 3.5 in Durand et al 2009 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JF001170) and Section 6.5 in Gagliardini et al 2013 (https://gmd.copernicus.org/articles/6/1299/2013/gmd-6-1299-2013.html). We previously cited the wrong Durand et al 2009 article and now corrected this. Note that we did not implement the handling of the constraint ourselves in Elmer/Ice.

57. The notation is not clear. Is this a gaussian random variable and the first argument of octave(,) is the mean and the second the standard deviation? Is it a different type of random variable? If so, what distribution does it follow and what do the arguments control?

    Reply: The octaves are cubical splines where the gradients at nodes are randomly generated angles. The approach follows from Perlin 1985 https://dl.acm.org/doi/pdf/10.1145/325165.325247 and is described in more detail in Appendix A of Löfgren et al 2024 https://tc.copernicus.org/articles/18/3453/

2024/#bib1.bibx43. We have clarified the notation in the manuscript and referred to Appendix A in Löfgren et al 2024.

58. There is a double comma
    Reply: Corrected.

59. ms^{-2}
    Reply: Corrected.

60. convergence tolerance

    Reply: Changed to "convergence tolerance".

61. This is not an error, is simply relative change. An iteration can converge to the wrong solution, thus even id the relative change is 0 the error might be very large. Since there is no error estimator invovled, the term error should be avoided.

    Reply: Changed to "but no divergence (growing $\|\Delta h\|$) check"

62. scheme
    Reply: Corrected.

63. keep
    Reply: Corrected.

64. If I recall correctly there was an issue with the use of github. Should this be updated?

    Reply: We have created zenodo repositories and added all relevant scripts. The code and data availability section has been updated correspondingly.

Response to Reviewer 2

Summary and general comment, Reviewer 2

This is a very interesting and well-written manuscript that improves my earlier free surface stabilisation (FSSA) algorithm to allow for significantly larger timesteps in glacier and geodynamics codes that have a free surface.
The authors demonstrate that a purely implicit timestepping algorithm doesn't give much advantage compared to an explicit scheme with FSSA if a first-order timestepping scheme is employed. This surprised me somewhat (I had thought otherwise), but is rigorously shown here. In addition, they show that using a second-order timestepping scheme can be made second-order by using a modified FSSA algorithm.
This is initially described for a simple linear viscous setup, which is expanded to a more realistic ice flow simulation in section 7.

Overall, this is a very nice contribution that will be helpful for both the glaciology and geodynamics communities. I think it can be accepted with minor revisions, and I just have a few minor remarks.

Minor remarks:

1. You seem to use Picard fixed-point iterations. If you have a good initial guess, a faster way to converge nonlinear iterations is Newton iterations, which should work particularly well for moderately nonlinear problems such as powerlaw viscous materials with an exponent of n=3. This would potentially also help the convergence if the implicit timesteps, even though it requires deriving the appropriate Jacobian. Is this not commonly done in the glaciology community, or are there other reasons you stick with Picard?

   Reply: For handling the non-Newtonian rheology, Newton solvers are used in ice sheet modelling, typically with some initial Picard iterations. However, the convergence is often poor, and the reason why is to our knowledge not fully understood. In our case we did not use a Newton solver since it is not sufficiently robust in the new vectorised Stokes solver in Elmer/Ice IncompressibleNSVec.f90. For the implicit/coupled iterations we believe Picard iterations are reasonable since only two iterations are needed to reach second order convergence, however it is a very interesting idea to switch to Newton in that context, that we did not think of!

2. eq. 29: Can you give a function or algorithm on how "*octave?*" is computed? The way this is written is a bit puzzling to me, but this might be because I am not from glaciology...

   Reply: The octaves are cubical splines where the gradients at nodes are randomly generated angles. The approach follows from Perlin 1985 `https://dl.acm.org/doi/pdf/10.1145/325165.325247` *and is described more in detail in Appendix A of Löfgren et al 2024* `https://tc.copernicus.org/articles/18/3453/2024/#bib1.bibx43`*. We have clarified the notation in the manuscript and referred to Appendix A in Löfgren et al 2023.*

Minor typos:

1. l. 299: kg `m^{-3}`

   Reply: Corrected.

2. Fig. 8 caption: "stabilisation"

   Reply: Corrected (with American spelling).

Unsolicited changes

Line numbers here refer to the revised manuscript.

1. Caption, Figure 1: Changed "stabilisation" to "stabilization" to be consistent with the spelling of the rest of the paper.

2. On line 233 the Figure label was corrected, it was changed from Fig 6d to Fig 7.

3. Line 372: added "are"

4. In the introduction, we added a reference to Kramer et al 2012 `https://www.sciencedirect.com/science/article/pii/S0031920112000027`