

Review: “An antiplane strain model for evaluating shear-margin stability (Ortholine v1.0)”

by Suckale and Elsworth

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1 General

Here the authors present a new computational model for glacier shear zones that is focused on ice streams in Antarctica. The primary advance is that this model determines the location of the shear margin based on where the bed has yielded rather than a specified location. Building on prior work by the authors and others (e.g., Schoof, 2006; Suckale et al., 2014; Elsworth and Suckale, 2016), this paper describes model advancements and an application to Institute Ice Stream. Based on these results, I could support publication after some significant revisions.

2 Comments

1. Title and throughout: I am not sure about the use of ‘antiplane strain.’ It is confusing for a number of reasons. First, the antiplane description suggests that the flow is into or out of the plane. Yet the dominant plane of a glacier is plan view (lat, lon) and antiplane would suggest vertical motion. Second, the flow of the ice is modeled here as a strain rate, not as strain. I think this lexical ambiguity would be ameliorated by removing the term antiplane and referring to u as the downstream velocity.
2. Line 1: What role do shear margins play in mass balance? Also, I think that the word ‘our’ could be deleted here.
3. End of abstract: It would be nice to know results came from this investigation, e.g., lines 422-425.
4. Line 47: The ‘plane strain’ description is similarly confusing.
5. Line 53: My understanding is that Haseloff et al. (2019) do not assume a flat basal topography.
6. Equation 8: Are τ_E and $\dot{\epsilon}_E$ defined here? And is advection important in shear margins?
7. For the boundary conditions equations, e.g., eqn. (10), I would rather see something like $-k\nabla T \cdot n_{\text{lat}} = 0$ on Γ_{lat} rather than the floating vector equation.
8. Figures and Acknowledgements: I find it strange that Cooper W. Elsworth is given credit for each figure and included in the Acknowledgements. He is an author on the paper!

9. Verification section: My understanding from the text is that the Schoof (2006) solution is an exact analytical solution, so any differences that arise in the solution come from the numerics? Would it not make sense to compute the error against the analytical solution?
10. Paragraph around line 220: The discussion about the orthogonality of the velocity seems to come out of nowhere. Is this an important point? Could be preempted and expanded?
11. Figures 6 and 7: I suggest combining these figures since they are so similar. It is jarring to see them back to back.
12. Lines 264 and 267: I am confused about whether H is 1700 m or 1275 m. What did I misunderstand?
13. Equation (31): Why do we expect an exponential?
14. Paragraphs around line 285: I find the discussion of the topography, pore pressure, and ice thickness to be confusing. It seems important because it is a key distinguishing feature between Figures 8, 9, and 10. Also, why is the sliding interface called p , isn't that the pressure?
15. Figures 8, 9, 10: I think describing the speed as 'downstream velocity' would make more sense. Combining these three figures into 1 or 2 figures would be ideal. Then the cases could be clarified on the figure. Right now, the differences between the figures are slight and the results are subtle. I tried to follow the text and the figures but it was still confusing.
16. Section 3.3: My understanding is that this could be set up as an inverse problem where the pore pressure could be inferred based on the velocity observations, correct? Then, the goal of this section is to test hypotheses for what could set that pore pressure? The data should constrain the answer.
17. Line 318: should 'comparison' be 'contrast'?
18. Line 366-368: having already cited a bunch of these papers earlier, I am left wondering which paper is relevant to supporting the claims and, re: self citations, if there are other papers (i.e., beyond those that the authors have contributed to) that might be relevant to supporting their results?
19. Line 394: Sanderson et al. (2023) should be a parenthetical citation (Sanderson et al., 2023). Also, the citation is the Cryosphere Discussion paper rather than the published version.
20. Discussion section: the beginning of this section describes a lot of the authors' opinions about the value of their simulations, verging on proselytizing. While some defense of approach is valid, I find these statements to go beyond that line.
21. Lines 435: the citations here should be parenthetical, i.e., (MacGregor et al., 2013; Summers et al., 2023)

References

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