Response to Reviewer #2

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.

> We thank the reviewer for the careful and detailed reading and for the constructive comments, which have improved the clarity of the manuscript considerably, particularly the terminology around the antiplane setup, the treatment of overburden and pore pressure, and the presentation of the figures. We respond to each comment below; line numbers refer to the revised manuscript.

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.*

   > We appreciate the reviewer raising this, and we suspect they will not be the only reader to pause on the term, so we have addressed the ambiguity directly rather than removing the term.
   >
   > We have retained "antiplane" because it is the established term in the continuum-mechanics and elasticity literature for precisely our configuration, in which the displacement (here velocity) is directed perpendicular to the plane in which its gradients are resolved, and because it is the term used throughout the shear-margin modelling literature on which we build (e.g., Schoof, 2006; Suckale et al., 2014; Elsworth and Suckale, 2016). Removing it would weaken the connection of this work to that body of theory, and the model name "Ortholine" is tied to this framing. We note that in our setup the antiplane direction is the downstream (flow) direction x and the plane of resolved gradients is the cross-sectional (y, z) plane, so the antiplane velocity is horizontal, not vertical; the reviewer's concern about implied vertical motion stems from associating the plane with plan view, whereas here the relevant plane is the cross-section orthogonal to flow.
   >
   > We do agree on the second point: we model a strain *rate*, not a strain, so "antiplane strain" is imprecise. We have changed this to "antiplane shear" (or "antiplane flow" where the kinematics rather than the stress state is meant) throughout. We have also added a short sentence early in the Methods defining antiplane shear in our setting, so that the geometry is unambiguous for the reader.

2. *Line 1: What role do shear margins play in mass balance? Also, I think that the word 'our' could be deleted here.*

We have clarified the role and deleted "our". The revised sentence explains that shear margins set the width of the fast-flowing zone, and that because mass loss depends on both the speed and the width of fast-moving ice, the position of the shear margins directly affects the mass balance; migration of a margin changes the width and therefore the discharge. This sentence now reads:

Migration of a shear margin therefore changes the width of fast-moving ice and, with it, the discharge, so that shear-margin position directly affects the mass balance.

3. *End of abstract: It would be nice to know results came from this investigation, e.g., lines 422-425.*

   Agreed. We have added a closing statement to the abstract summarising the main finding from the Institute application: that spatial association alone is not a reliable indicator of the controlling process, since the western shear margin at Institute is co-located with a topographic trough yet our simulations indicate that a meltwater channel in that trough, rather than the topography itself, is the important control:

   Applying Ortholine at Institute, we find that spatial association alone is not a reliable indicator of the controlling process: the western shear margin is co-located with a topographic trough, yet our simulations indicate that a meltwater channel in that trough, rather than the topography itself, is the important control.

4. *Line 47: The 'plane strain' description is similarly confusing.*

   We have addressed this consistently with our response to comment 1. "Plane strain" here refers to the assumption underlying flow-line models, in which variation is confined to the along-flow plane; as above, the kinematic quantity is a strain rate. We have reworded to "plane shear" / "plane flow" as appropriate and rely on the defining sentence added in the Methods to make the contrast between the flow-line (plane) and Ortholine (antiplane) configurations explicit.

5. *Line 53: My understanding is that Haseloff et al. (2019) do not assume a flat basal topography.*

   The reviewer is correct that Haseloff et al. (2019) do not assume a flat basal topography, and we have checked our text to make sure we do not imply otherwise. The sentence at line 53 that lists models assuming a flat basal topography cites Haseloff et al. (2015), not Haseloff et al. (2019). Haseloff et al. (2019) appears earlier only in the broader list of idealized shear-margin models that have informed our approach, which makes no claim about basal topography. To remove any ambiguity, we have reworded the surrounding text so that it is clear which characterisation applies to which study, and that the flat-topography simplification is attributed only to the studies for which it holds.

6. *Equation 8: Are τ_E and $\dot{\epsilon}_E$ defined here? And is advection important in shear margins?*

   We have added definitions of the effective stress τ_E and effective strain rate $\dot{\epsilon}_E$ at Eq. 8, where they first appear. On advection: we agree this deserves comment, and Reviewer 1 raised the related question of why heat advection is neglected. We are coordinating a single consistent response on the thermal treatment and will provide it in the revised version; in brief, the thermal model retains only diffusion of shear heating, and we will state the consequence (that margin narrowing by lateral advection of cold ice cannot be represented), connecting this to the discussion at lines 368–370.

7. *For the boundary conditions equations, e.g., eqn. (10), I would rather see something like* $-k\nabla T \cdot n_{\mathrm{lat}} = 0$ *on* Γ_{lat} *rather than the floating vector equation.*

   Done. We have rewritten the thermal boundary conditions (Eqs. 10 and 11) in the form $-k\nabla T \cdot \mathbf{n} = 0$ and $-k\nabla T \cdot \mathbf{n} = G_{\mathrm{geo}}$, making the projection onto the boundary normal explicit rather than leaving a floating vector equation. We have applied the same convention to the mechanical boundary conditions (Eqs. 2–4) for consistency.

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!*

   The reviewer is right. We have removed the "Figure created by Cooper W. Elsworth" credit from all figure captions and removed the corresponding line from the Acknowledgements, since Cooper is a co-author. His contributions are reflected in the author-contributions statement instead.

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?*

   The reviewer is correct that the Schoof (2006b) solution is analytical. However, that paper reports the depth velocity profile rather than a surface velocity field, so a direct pointwise error against the analytical surface velocities is not available without substantial additional derivation. We therefore verify against the depth velocity profile, which is the quantity Schoof (2006b) provides, and separately perform a self-convergence study of the surface velocities against our most refined solution to confirm the expected second-order convergence of the linear finite elements. We have clarified in the revised text which comparison is made against the analytical solution and which is the self-convergence study, so the distinction is no longer conflated. We have reworded the section so that it no longer conflates the qualitative reproduction of the Schoof (2006b) field with the quantitative self-convergence study, and we explain why each is used.

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?*

    It is an important point, since the orthogonality of the ortho-flow lines to the local ice-flow direction is what makes the antiplane approximation appropriate away from the idealised symmetric case, and it is the basis for step 1 of the workflow. We have preempted it: we now introduce the idea earlier, where we first motivate the antiplane setup, and we have expanded the paragraph around line 220 to explain why ortho-flow lines correspond to isocontours of the velocity potential and what is gained by orienting the model domain along them rather than along a straight transect.

11. *Figures 6 and 7: I suggest combining these figures since they are so similar. It is jarring to see them back to back.*

    Agreed. We have combined Figures 6 and 7 into a single figure, retaining the ice-speed map with the ortho-flow lines and the observational constraints on subglacial conditions as panels of one figure, and have updated the in-text references accordingly.

12. *Lines 264 and 267: I am confused about whether H is 1700 m or 1275 m. What did I misunderstand?*

We apologise for the confusion, which arose from using one symbol for two quantities. The value of 1700 m at line 264 refers to the maximum ice thickness of the trunk near the western margin, whereas the $H = 1275$ m at line 267 is the representative (trunk-average) thickness used to evaluate the characteristic driving stress $f = \rho g H \sin\alpha = 27.5$ kPa. We have revised the text to distinguish these explicitly, giving the thickness range across the trunk (approximately 1000 m at the eastern to 1700 m at the western margin, consistent with lines 285–287) and stating clearly which value is used for the characteristic driving stress and why.

13. *Equation (31): Why do we expect an exponential?*

    We have added a justification at Eq. 31. The exponential form follows the treatment in Perol et al., 2015 and Elsworth and Suckale, 2016: the reduction in pore pressure near a channel reflects the competition between drainage into the channel and resupply from the surrounding distributed system, which to leading order produces a perturbation that decays exponentially away from the channel, with a decay length set by the film thickness of the distributed system. We now state this explicitly and cite the underlying derivation rather than presenting the exponential as an ansatz.

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?*

    We agree this passage was confusing, and Reviewer 1 raised the same concern, so we have rewritten it substantially. The revised text now describes τ_c as a function of the effective pressure $N = \rho g H - p$ (rather than "pore pressure"), states and justifies the substitution $p = k_p \rho g H$ that takes pore pressure to be a fixed fraction of overburden, and clarifies that the resulting bed strength varies linearly with ice thickness H. This is the feature that distinguishes the simulations in Figs. 8–10, and we have made the link to those figures explicit.

    On the second point, the reviewer is right that the notation collided: p was used both for the pore pressure and, loosely, for the sliding interface. We have corrected this so that p denotes only the pore pressure, and we refer to the sliding interface by name (the bed, Γ_{bed}) rather than by symbol.

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.*

    We agree that the speed should be described as downstream velocity and have implemented this change. However, since we have added two panels to both figures 8 and 9, we have decided to keep the figures separate. Both figure 8 and 9 already have 10 panels, making a merged figure too large and genuinely hard to follow.

    Also, each of the three figures tests a distinct hypothesis (the role of ice overburden, the role of the meltwater channel, and the counterfactual without a channel). Combining the three would therefore require packing even more information into a single panel array and, in our judgement, would make the comparison harder to read rather than easier. We would rather keep one hypothesis per figure and instead make each figure and its accompanying text clearer.

To that end we have: (i) revised the text of Section 3.3 so that it states, up front for each figure, which hypothesis is being tested, what the counterfactual is, and what the reader should look for in the panels, so the subtle differences between cases are signposted rather than left to be inferred; (ii) clarified on the figures themselves which case each panel represents; (iii) addressed the labelling and greyscale-legibility issues Reviewer 1 also raised (basal-strength symbol τ_b, colour-bar labels, missing axis labels, distinguishable colours). On the terminology, we note that we have retained "antiplane" per our response to comment 1, but we have added the defining sentence in the Methods so that the quantity plotted is unambiguous, and we label it consistently across Figs. 8–10.

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.*

    We appreciate this idea, and the reviewer is right that one could pose the problem as an inversion in which the basal strength (and hence effective pressure) is inferred from the surface velocities. We have chosen the forward, hypothesis-testing framing deliberately. Our reasoning is that there are always choices in how the basal parameters are set, and we prefer to make those choices explicit and physically motivated rather than folding them into an inversion, where the same choices enter implicitly through regularisation and priors. The forward approach keeps the small number of free parameters, and the assumptions behind them, transparent to the reader, which is the property we most want for a tool intended to be applied at many field sites. This is also the basis for our recommendation to keep the number of auxiliary free parameters small (lines 385–388).

    That said, the two approaches are complementary rather than mutually exclusive: Ortholine lends itself naturally to a combined forward–inverse workflow, in which an inversion proposes a basal-strength profile and the forward model tests which physical processes can produce it. We have added a sentence noting this, and noting that the balance between the two is ultimately for the user to decide. For the exposition in this paper we keep the current forward framing, which we find clearest for introducing the method.

17. *Line 318: should 'comparison' be 'contrast'?*

    Yes. We have changed "comparison" to "contrast" at line 318.

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?*

    This is a fair point, and we have addressed it in two ways. First, we have tightened the citations at lines 366–368 so that each claim points to the specific paper that supports it, rather than repeating the full block of references cited earlier. Second, we have added relevant literature from beyond the author group to broaden the support for these claims, specifically Jacobson and Raymond (1998), so that the passage no longer rests predominantly on our own prior work.

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.*

Corrected on both counts. The citation at line 394 is now parenthetical, and we have updated the reference to the published version in *The Cryosphere* (Sanderson et al., 2023, doi:10.5194/tc-17-4853-2023) rather than the Discussions version. We have checked the reference list for the same Discussions-versus-published issue elsewhere.

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.*

We agree and have toned this down. We have trimmed the opening of the Discussion to remove the more promotional statements about the value of the approach, keeping a brief and factual positioning of Ortholine relative to ice-sheet and flow-line models and letting the results speak for the method's usefulness. We have kept only the defense of approach that is necessary to situate the model.

21. *Lines 435: the citations here should be parenthetical, i.e., (MacGregor et al., 2013; Summers et al., 2023)*

Corrected. The citations at line 435 are now parenthetical, (MacGregor et al., 2013; Summers et al., 2023).

References

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