Response to Reviewer #1

General comments:

This manuscript presents a novel model of shear-margins, designed to be applied to specific field sites in order to gain insights into the physical processes governing the shear-margin location at the specific site. A key strength of the manuscript is the clear step-by-step method, demonstrated by an example, for how to apply the model to specific field sites. This guidance makes that manuscript particularly valuable to the scientific community, with the potential for a strong uptake of Ortholine as a tool used in future studies. Moreover, the model achieves a good balance between being simple enough to be usable and complex enough to capture the physical processes of interest. Therefore, I commend the authors on their work and recommend the manuscript for publication. I have only a few minor comments.

We thank the reviewer for the careful and constructive reading of our manuscript, and for the encouraging assessment. We are glad the step-by-step guidance for applying Ortholine to specific field sites came through as a strength, since making the model genuinely usable by others was a central goal, and we appreciate the recognition of the balance the model strikes between simplicity and physical completeness. The specific comments have helped us improve the clarity of the manuscript, particularly around the terminology of the antiplane setup, the treatment of overburden and pore pressure, and the presentation of the figures. We respond to each comment in turn below; line numbers refer to the revised manuscript.

Specific comments:

Abstract: I suggest making it explicitly clear which of the physical processes listed can be assessed by Ortholine.

We have revised the abstract to state explicitly which processes Ortholine evaluates. It now makes clear that the model assesses the relative importance of subglacial lithology, subglacial hydrology (including channelized versus distributed drainage), basal topography and ice overburden, and englacial temperature through shear heating, while processes such as fabric development lie outside its current scope. The respective sentence in the abstract now reads:

Ortholine evaluates the relative importance of subglacial lithology, subglacial hydrology including channelized versus distributed drainage, basal topography and ice overburden, and englacial temperature through shear heating, while processes such as fabric development lie outside its current scope.

Model description:

The following aspects of the model should be clarified.

– Hardrock vs. sediment: I would like to see the distinction between the hardrock and sediment cases clearer in the model description (2.1 Governing Equations). I also suggest adding a brief description/comment in this section about how the two regions are practically included in the model, i.e. that the regions of hardrock and sediment are determined by observations.

We have made the distinction explicit in Section 2.1. The text now states that the bed is partitioned into hardrock and sediment regions, that this partition is prescribed from observations (here the rock–sediment transition inferred by Siegert et al., 2016), and that each region carries its own basal boundary condition: Coulomb-plastic behaviour for sediment (Eqs. 5–6) and a sliding law for hardrock (Eq. 7).

The location of the rock–sediment transition and the degree to which this transition implies a significant difference in basal properties needs to be assessed on case-by-case basis. For example, we rely on the geophysical imaging by Siegert et al., 2016, to assign boundary conditions for Institute Ice Stream, Antarctica, as detailed below.

– Please add some comment/justification of why advection of heat is neglected (in eq. 8), and the impacts of this. It would be natural to do this in lines 368–370, where the authors already mention that advection of cold ice is necessary for come physical explanations.

We have added a comment where Eq. 8 is introduced noting that the thermal model retains only diffusion of shear heating and neglects advection, together with the consequence that Ortholine cannot represent margin narrowing by lateral advection of cold ice. We have linked this explicitly to lines 368–370, where this same limitation is the reason we invoke an additional englacial process (fabric) rather than cold-ice advection to explain the narrow eastern margin.

We retain only the diffusion of this shear heating and neglect advection of heat. As a consequence, Ortholine cannot represent the narrowing of a margin by lateral advection of cold ice, a point we return to in Section 3 when discussing the eastern margin of Institute. We emphasize that analytical approximations of lateral advection could be added to the model, as done in Suckale et al., 2014, but our current solver is not designed to solve for lateral advection numerically.

– Line 114: I think some explanation of how "the location of these transition points depends on the integrated force-balance of the ice stream" would be beneficial.

We have expanded this explanation. The text now clarifies that whether the sediment at a given point fails depends on the local basal shear stress, which is set by the depth-integrated driving stress redistributed across the entire cross-section. Because that redistribution depends in turn on where the bed is slipping, the transition points and the velocity field must be solved together, which is precisely why we pose the problem as a free-boundary problem. The beginning of this paragraph now reads:

The primary difficulty in solving these governing equations arises from the unknown location and extent of the slip zone. When sliding occurs over perfectly plastic sediment, the slip zone ends at the point where the sediments transition from failing to locking, but the location of these transition points depends on the integrated force-balance of the ice stream. Whether the sediment fails at a given point is set by the local basal shear stress, which in turn reflects how the depth-integrated driving stress is redistributed across the whole cross-section. For example, if part of bed is very strong, the same change in driving stress could lead to a minimal change in width, because the weak portions of the bed play a relatively less important role in the force balance than in a scenario where the basal strength is relatively even.

Line 116–117: What happens when the melting point is reached in the model? Does melting occur at the bed/englacially? Is there then a water content? If so, what is the equation for this? Please add some explanation. Related to this, I have concerns about the Neumann condition (eq. 10) being applied in regions where the bed is temperate, in which case a Dirichlet condition $T = T_m$ would be appropriate, with the geothermal heat flux instead going into calculating a melt rate. Perhaps these issues are addressed by the numerical optimisation method – I am not sure. Please comment on these points.

We thank the reviewer for raising this important point, which prompted the most substantial addition in this revision. The reviewer's intuition is correct: the concern is resolved by the variational formulation, and we have added an explicit melt-rate calculation to make this more transparent. Where the constraint is inactive, the geothermal flux enters as the Neumann condition as written. Where the constraint is active, the temperature is pinned at the pressure-melting point, which is precisely the Dirichlet condition $T = T_m$ the reviewer describes. The optimization selects between these two regimes automatically, and the location of the temperate region is part of the solution rather than prescribed in advance.

The surplus energy in the temperate region now yields a melt rate directly. Where the constraint is active, the Lagrange multiplier λ associated with the constraint in the thermal minimization (Eqs. 14–15) is nonzero and equals the local excess heat that cannot raise the temperature further and instead drives melting. After solving the thermal problem, we recover λ at the basal nodes, normalize by the boundary integration weights d_γ to obtain a basal heat flux, and convert it to a melt rate $\dot{m}$ by dividing by ρL_f,

$$\dot{m} = \frac{\lambda}{\rho L_f \, d_\gamma}, \tag{1}$$

where L_f is the latent heat of fusion. We have added this expression and explanation to the Methods. The melt rate is therefore diagnosed directly from the optimality conditions of the free-boundary thermal problem rather than imposed as an additional constitutive relation, and it now appears as an additional outcome variable in Figures 8 and 9. We have added the following new paragraph to the manuscript to explain our approach:

The constraint $T \leq T_{\mathrm{melt}}$ makes the thermal problem a free-boundary problem in which the extent of the temperate region is part of the solution rather than prescribed in advance. Where the constraint is inactive, the temperature satisfies the diffusion balance (Eq. 8) with the geothermal flux entering through the Neumann condition (Eq. 11). Where the constraint is active, the temperature is pinned at the pressure-melting point, $T = T_{\mathrm{melt}}$, and the surplus energy that cannot raise the temperature further instead drives melting. This surplus is given by the Lagrange multiplier, λ, associated with the constraint: at the optimum, λ is nonzero only where $T = T_{\mathrm{melt}}$ and equals the local excess heat flux at those nodes. After solving the thermal minimization with CVX, we recover λ at the basal nodes, normalize by the boundary integration weights, d_γ, to obtain a basal heat flux, and convert it to a melt rate, $\dot{m}$, by dividing by the latent heat of fusion, L_f,

$$\dot{m} = \frac{\lambda}{\rho L_f \, d_\gamma}. \tag{2}$$

Line 272–273: Which parameter value in the model is adjusted to increase the till strength to represent frozen conditions? The sediment cohesion c? What are the parameter values used and how are these determined?

> The quantity that matters for the force balance is the total basal strength τ_c, and we represent frozen conditions as a prescribed increase in that total strength outside the eastern shear margin. Formally this increase could also be attributed specifically to cohesion c and one could use experimentally derived values for cohesion. However, that approach would require modelers to set multiple parameters, when simply adjusting τ_c suffices. We have clarified in the text that it is the total basal strength that is raised to represent the frozen bed, but that the other parameters could be adjusted as well:

Rather than raising the total strength τ_c directly, one could equivalently adjust individual parameters such as the cohesion c where independent information exists for how they change under frozen conditions.

I find the section talking about the overburden pressure, pore pressure, basal strength and their relations (i.e. how the effect of overburden on the hydrological system is captured – line 287 onwards) somewhat confusing. I think the following could be clarified:

– Should line 288 say "function of effective pressure", rather than "pore pressure"? τ_c in eq. 32 is a function of $\rho g H - p$, i.e. the effective pressure, and in the end the pore pressure is removed from eq. 32, so I think the term "pore pressure" in the quote above could cause confusion. I think it should also be made clearer how exactly the choice made in eq. 32 captures the effect of overburden pressure, i.e. is it through making τ_c a function of effective pressure, or through taking the pore pressure to be a linear function of overburden, etc.?

We have corrected the wording throughout. The text now describes τ_c as a function of the effective pressure $N = \rho g H - p$, consistent with Eq. 32, and we have removed the misleading reference to "pore pressure" at line 288 and line 303.

– Related to this, in eq. 32, the substitution $p(y) = k_p \rho g H(y)$ is made, but this is done without explanation. I think a statement is needed to say that this substitution is made, as well as an explanation of why the pore pressure is taken to be a linear function of H. Line 303, which quotes eq. 32 as where the pore pressure is taken to increase with overburden, could also be modified.

We now state this substitution explicitly and explain its physical basis: we take the pore pressure to be a fixed fraction of the overburden, corresponding to a spatially near-constant flotation fraction across the trunk, so that the effective pressure scales with ice thickness. This makes clear that the overburden control enters through the pore pressure tracking H, which is what produces the bed strength varying linearly with H. The revised text reads:

To link the pore pressure to the overburden, we take the pore pressure to be a fixed fraction of the overburden, $p(y) = k_p \rho g H(y)$, which corresponds to a spatially near-constant flotation fraction across the trunk. ... Because the effective pressure then scales with ice thickness, the bed strength varies linearly with H.

– Line 304: "linearly varying bed strength" – perhaps saying "linearly varying bed strength in H", or similar, would be clearer.

We have changed "linearly varying bed strength" to "bed strength varying linearly with ice thickness H".

We have adopted this suggestion as evident from the quote above.

– Line 313–314: please provide details of how k_p is fitted, and perhaps mention when k_p is first introduced (in eq. 32) that is a parameter that will be fitted.

We do not treat k_p as an independent free parameter in the convex solve. Instead, we prescribe a basal-strength profile that varies linearly with ice thickness and adjust the corresponding strength coefficients so that the modeled surface speed and transverse strain rate best match the MEaSUREs observations along the ortho-flow line. The reported flotation fraction $k_p \approx 0.996$ is then diagnosed from this best-fit strength profile, together with representative values of the friction coefficient C_f and sediment cohesion c. We have added this explanation to where k_p is first introduced:

> We do not treat k_p as an independent free parameter in the convex solve for lack of constraints on how it might vary. Instead, we prescribe a basal-strength profile that varies linearly with ice thickness H following and adjust the corresponding strength coefficients so that the modeled surface speed and transverse strain rate best match the MEaSUREs observations along the ortho-flow line.

Notation:

I find the use of u and v in the minimisation slightly confusing, with reason for using one of the two symbols over the other not always being clear. I suggest that the authors clarify the meanings of u and v and use these consistently throughout the manuscript. For example, perhaps it would be clearer to use u in eq. 13, as is done in eq. 12, but making clear that the solution of eq. 12 and 13 is $u = v$. Similarly, why is v use in eq. 27 but u used in eq. 28?

> We agree and have made the notation consistent. We now use a single symbol for the velocity field throughout and state explicitly that the minimiser of Eqs. 12–13 is the velocity, i.e. the solution v equals u. We have made Eqs. 13, 27, and 28 consistent so that the symbol no longer changes between equations without explanation.

A second point on notation is that I suggest using a different symbol for the elements and element area, instead of τ, since τ (although with a different subscript) is also used to represent the shear stress, with some equations featuring both uses of τ.

> We agree. We have changed the symbol used for the elements and element area so that it no longer collides with the shear stress τ, including in the equations that previously contained both uses.

Figures:

I think many of the figures would benefit from being a page or so later in the manuscript, to better match up with the text, but this is something to be addressed in the final version of the manuscript.

> Agreed. We will position the figures to track the accompanying text in the typeset version.

Figure 2: I think the figure could be made clearer by adding an arrow to indicate the flow direction, and adding a title to each of the diagrams: "Previous models" and "Ortholine".

> We have revised Figure 2 to include a flow-direction arrow and titles for the two diagrams. With the titles, "Previous models" and "Ortholine", the comparison in the figure should be clearer.

Figure 4: I suggest adding a key to the plot so that the colours are not necessary to identify the lines (e.g. when looked at in greyscale).

> We have revised Figure 4 to add a key that distinguishes the four benchmark curves by line style and marker, so that they remain identifiable in greyscale.

Figure 5: I suggest joining together the two arrows that form the corner arrow to make a single cornered arrow.

> We have revised Figure 5 to join the two arrows into a single cornered arrow.

Figure 6: Perhaps consider including an arrow to indicate the flow direction.

Following a comment from the other reviewer, we have combined Figures 6 and 7. We have included an error indicating the flow direction into one of the panels.

Figure 8–10: Some details are missing from these figures: the symbol τ_b for the basal strength, a label for the colour bar, and, in fig. 10, the axis labels in b3. I also recommend using different shades or red and blue for the sliding coeff. and basal strength, because the two colours currently look the same in greyscale.

We have revised Figures 8–10 to add the τ_b label for the basal strength, to label the temperature colour bar, and to add the missing axis labels in Figure 10 b3. We have also tried to adjust the sliding-coefficient and basal-strength colours to shades that are easier to distinguish in greyscale.

Figure 8: Consider changing the scales in a3, b3 so that it looks visibly like the sliding coefficient is 'high' in both segments at the western shear margin in b3, rather than it appearing that the two segments are more similar in a3.

We have played around with the axis limits trying to find the right balance.

Other:

Line 197–198: I would consider rewording the final sentence, because it makes it sound like Schoof (2006b) used discretisation parameters, but the start of the paragraph says that Schoof (2006b) provided an analytical solution.

Reworded. The sentence now attributes the small deviations to our own discretisation relative to the analytical solution of Schoof (2006b) rather than implying that discretisation parameters were used.

Technical corrections:

Line 56: typo: "heterogeneous (and) basal condition"

Corrected.

Line 120: typo: "equivalent minimising" → "equivalent to minimising"

Corrected.

Lines 131–132: replacing the commas with hyphens would make the sentence more readable.

Done.

Line 134: typo: "minimisations" → "minimisation"

Corrected.

Fig. 3 caption: separately → sequentially (otherwise it makes it sound like there is absolutely no dependence of one on the other).

Corrected. The caption now states that the mechanical and thermal problems are solved sequentially within each coupling iteration, making clear that the two solutions are coupled rather than independent.

Line 168: "$J_c(v_i)$" → "Since $J_c(v_i)$" and "and" → "it" (to avoid starting a sentence with mathematical notation).

Corrected.

Line 238: typo: "orto-flow"

Corrected to "ortho-flow".

Line 363: "with a meltwater channel within can" → "containing a meltwater channel can"

Corrected.

Line 423: lower case "d" in "Despite"

Corrected.

Line 435: brackets in the wrong places in the citations

Corrected. The citations are now parenthetical, i.e. (MacGregor et al., 2013; Summers et al., 2023).

Line 450: missing space after the second citation

Corrected.

Line 452: "five key steps" → "four key steps"

Corrected. We have made this consistent throughout, in agreement with the abstract and Fig. 5.