Authors' Response

To The Editor:
Please find below our responses to the very helpful reviews of our manuscript, "Persistence and Variability of Ice Stream Grounding Lines on Retrograde Bed Slopes", with reviewer comments highlighted in blue. You can also find a version of our manuscript with changes tracked in blue. We hope that these responses and the revisions to the manuscript adequately satisfy all comments.
Thank you,
A. Robel on behalf of the authors

Reviewer 1
Review of a manuscript "Persistence and Variability of Ice Stream Grounding Lines on Retrograde Bed Slopes" by A. A. Robel, C. Schoof and E. Tziperman.

This study concerns with stability of the grounding line on beds with retrograde slopes under variable external (surface accumulation) and internal (basal) conditions. The authors use a one- dimensional flow-line model complemented with a two-dimensional (one horizontal and one vertical) ice thermodynamic model and a model of subglacial till evolution. The results show that under various parameter combinations, the grounding line can exhibit reach behaviours having a stable steady-state position, oscillating, being stable on a retrograde slope and then readvancing, etc. This study sheds light on previously disregarded aspects of a more than four-decades old theory of the marine ice-sheet instability. Overall, the manuscript is well-written and will be a substantial contribution to the existing body of literature on this subject.
We thank the reviewer for these generous comments and thoughtful suggestions on this manuscript. As we indicate below, we have added substantial descriptions of the model formulation to help frame the later discussion and save the reader from referring to Robel et al. 2014. Our detailed responses to your comments are inline below.

General comments
There are several questions/concerns with the model formulation and the performed experiments. The model description is very brief and lacks details about assumptions and approximations. Though the model is described in Robel et al. (2014), it wold be useful to provide a self-contained description of the model without asking a reader to refer to another publication. For instance, the authors mention basal meltwater (p. 4 line 8), however, the mass-balance equation (2) has only the accumulation rate ac. It is unclear whither the basal melt rate is disregarded in this equation because it is much smaller than the accumulation rate, or because ac is the net accumulation, i.e. the difference between the surface and basal ablation/accumulation rates, or because of some other assumption. It also appears (eqn (4) in Robel et al., 2014) that the basal

melting/freezing rate is disregarded in computations of the vertical velocity, w. It is unclear why.
These are all good points, and we can see how these would be natural questions, which one would have to hunt for in Robel et al. (2014) to answer. We have added language which indicates that basal melt rate is neglected because it is much smaller than accumulation rates and vertical velocities in this model configuration. We have also explicitly defined how vertical velocity is calculated in this model.

Even though the ice flow model accounts for the lateral shear (eqn (1)), the advection-diffusion equation for ice temperature (eqn (7) in Robel et al., 2014) does not include the internal heating due to ice 1 deformation. There is no explanation why the authors have chosen to disregard it.
We have added a discussion of this omission (in addition to a description of how temperature is calculated). As Suckale et al. (2014) argues, deformation-induced heating is only significant in the shear margins, and so we omit it in this central flowline ice stream model.

The authors mention that the model horizontal resolution in the grounding zone is 100 m, however, there is no indication whether the results (the mode of the grounding line behaviour and its specific location) are sensitive to this value. For instance, the authors find two instances of hysteresis for a very narrow range of the accumulation rate, ac, 6 mm/yr or $< 2\%$ (p. 7 line 13). It would be interesting to know whether the same result holds for higher spatial resolutions, and how numerical errors compare to such changes in the model parameters.
We show in Robel et al. (2014) that the range of grounding line migration is converged at these horizontal resolutions. We have added an additional sentence to point this out.

In the Conclusions section, in addition to mentioning indirect observations of subglacial water, the authors may consider mentioning inverse modeling results indicating highly variable basal conditions in the vicinity of the grounding lines (e.g. Sergienko and Hindmarsh, 2013) and numerical modeling studies exploring the effects of traveling patches of high/low basal traction on ice flow (e.g. Wolowick et al, 2014).
These others studies and evidence of strong traction have been added in the conclusions.

Specific Comments
Many parameters from table 1 are not used in this manuscript.
We have removed unreferenced parameters.

Figure 5 is difficult to relate to the model parameters. Adding a sketch illustrating what parameters are varied in what experiments may be helpful.
We have added table 2, which lists all parameter variations for all simulations in this study.

Response to Reviewer Comments 2

Summary

Ice streams display a wide range of behaviors, including unforced variability as well as reversal of grounding-line migration across and/or sustained stabilization on retrograde slopes. Using a thermomechanical flowline model that excludes ice-shelf buttressing in order to focus solely on the potential impacts of dynamically-varying bed properties, the authors are able to generate a wide range of ice-streaming behaviors across regions of retrograde beds that are dependent on the length of the retrograde segment, its slope, and whether the grounding line advances or retreats towards that segment. This work logically builds on previous studies by the authors and others as it illustrates a wide array of ice dynamics that are often counter to those predicted by the MISI feedback on retrograde beds, but are predicted simply by the interaction of a dynamically-evolving plastic bed with regions of retrograde slope.

General Comments

This is a nice, largely well-written piece of work that independently supports previous findings by once again illustrating how critical basal rheology and prior ice-flow history (assumed initial conditions) are to our ability to predict the future evolution of streaming ice flow. My main concern, as discussed below, is that the explanations for changes in simulated streaming behavior rely heavily on omitted discussions of how the model treats important boundary conditions that ultimately lead to the reported behavior. The logical progression of thoughts is thereby lost within Section 4. With a few important revisions to the text, I believe this will become an important, publishable contribution to our field.

Thank you for the very helpful comments! As you will notice, we have added much more description of the model, including important boundary conditions.

Section 2: There is too much reliance on Robel et al., 2014. More needs to be included here for this to be a stand-alone publishable unit. For example: What is your ice-front condition for both your momentum and mass balance? These are too important to omit. I'm assuming (but shouldn't have to) that you are including a balance between water pressure and longitudinal stress at the ice front (including a sea-level line in your schematic will also visually highlight that you are simulating a marine ice-front condition). Your treatment of the flux condition at the grounding line should be stated so that discussion of advance and retreat is better framed and logical for the reader.

This is a good point. We have added the stress and flotation boundary conditions at the grounding line to set up the discussion of advance and retreat. We have also discussed how flux through the grounding line is treat and added a sea level line to Figure 1.

Subsections within 4: With the above omissions in Section 2, what should be clear and logical to the reader is often counter-intuitive in this part of the paper (see specific comments below). Because the findings and explanations within Section 4 serve as the foundation of this important contribution, Section 2 needs to be revisited by the authors.

Added discussion: With variable bed properties and oscillatory stagnation/activation of streaming flow, a discussion on the impact of omitting vertical shear on your results is warranted (unless vertical shear is indeed treated, as in Robel et al., 2014, and feeds back into the ice softness; again, not clear). With additional viscous dissipation (does not appear in the equations in Robel et al., 2014, so I assume is not included here) and softening of the ice, I would suspect that some of the transitions in behavior might be muted due to both thermal (reduction of the thermal gradient above frozen, or nearly frozen, regions) and dynamic (softening) feedbacks. I would consider an additional simulation or two where you vary Abar both temporally and spatially over and just upstream and downstream of regions where you have pronounced gradients in basal sliding to at least address the dynamic question and then use those findings to include a brief supporting statement.

It was certainly unclear, without referring to Robel et al. (2014), that vertical shear in velocity and spatiotemporally-variable $\bar{A}$ are indeed included in all the simulations in this manuscript. In our expanded description of the model formulation, these points are included. Internal deformational heating is not included, as we expect it to be small in the ice stream interior (as shown in Suckale et al. 2014) and at the location of activation waves, small in comparison to frictional dissipation at the bed. This is also discussed in the expanded section on model formulation.

Minor Specific Comments

p2, line2: I would suggest adding the impact of pinning points: “which buttress ice sheets through lateral contact with bedrock and/or localized basal contact with bathymetric highs”

Added

p4, Fig 1: see above comment regarding the inclusion of sea level.

Added.

p4, line5: Again, without the context of how you treat the ice flux once it reaches the grounding line, it is not clear why the grounding line must retreat (or advance). What drives a retreat? How is mass removed from the system? A short description of the ice-front condition applied when solving your continuity equation will clear all of this up.

We have added a complete description of the stress and flotation boundary conditions at the grounding line and sentences stating that ice flux at the grounding line removes mass from the system and changes in grounding line ice thickness drive migration of the grounding line.

p4, lines5-6: Although you are assuming a plastic bed (implicitly the bed strength$\geq$basal shear stress), this sentence should be reworded in terms of till strength to be clear that you are not implying a force imbalance in the upstream direction (i.e., rather than basal shear stress + lateral shear stress > driving stress, I believe you meant to state bed strength + lateral shear stress > driving stress): "Eventually, till becomes sufficiently strong that the combined basal shear strength and lateral shear stress exceeds the driving stress and the ice stream stagnates."
Very perceptive point, this has been changed in the way you suggested.

p5, lines22-24: Another sentence or two on how the critical accumulation rate values remove the solution branches would be beneficial.
Sentence added on the physical basis for the loss of stability on retrograde slopes.

p6, last complete sentence: Rather than just stating that there is a lack of aperiodic oscillations, it would be informative to also offer insights into why you think there is this notable difference in behavior.
We added an explanation in this sentence to indicate that the regularity of oscillations is likely due to the lack of interactions with other ice streams, which are cited as mechanisms for aperiodicity in Brinkerhoff and Johnson (2015).

p8, line2: Here, I believe "over a wide range of parameter values" continues to refer to a wide range of a_c values, not variations in empirical parameters in your sliding law (again, not actually included in this manuscript) related to bed properties. Given how this paragraph begins, consider rewording to explicitly state: "over a wide range of a_c values."
Done

p10, line6: Consider adding a reference to Fig. 3a here: "In contrast, when bed properties are allowed to freely vary (Figure 3a), such a retreat. . ."
Added

p10, lines8-9: Again, without explaining how grounding-line migration is treated, this discussion is not intuitive. Why should grounding line retreat necessarily be a response to the accumulation of ice thickness and the deformation of ice, which could drive more ice across the grounding zone and lead to an advance? Sorry to belabor the point, but without a discussion of how you are treating key processes within the current manuscript, the understanding of temporally and spatially varying dominant processes is unnecessarily muddled. This is a solid contribution and these minor issues are easily remedied.
This is a good comment for a critical part of the paper. As mentioned above, we have added a more detailed description of the processes that drive grounding line migration in section 2. We

have also added further description throughout this entire paragraph to describe how imbalances in ice flux at the grounding line during different stages of the thermal oscillation cycle drive this behavior.

p11, lines7-8: Same problem. . . not clear why extra ice advected to the ice front, leading to thickening there, doesn't promote ice-front advance.
p11, lines8-12: And because the above is not clear, this discussion isn't intuitive (although, I am sure it would be with additional discussion of the ice-front and grounding- line treatment).
In addition to the added description of grounding line migration in section 2, there was definitely a key confusion in how these sentence were written. In the early part of the active phase, the grounding line undergoes thickening as ice is delivered from upstream, but thinning occurs upstream as the reservoir of ice is depleted by high velocities. Then at the end of the active phase, deactivation begins at the ice divide, causing reduces ice flux from upstream (and thinning), but then reaches the grounding line, causing reduced grounding line flux (and thickening). These confusions have been cleared up by adding additional sentences here and taking more time to discuss these processes step-by-step.

Sections 4.3 and 4.5: The explanations discussed here rely on clearing up the description of the enhanced overshoot in the previous section.
We have cleared up the description of enhanced "overshoot", and added additional explicit references to this "overshoot" starting in section 2 to clarify that this same process is relevant to explaining each of these different cases.

Technical Corrections
p7, line22: bifurcations remain (rather than remains)
Fixed

p10, line7: This situation is contrary to what happens with MISI (it stops short of a full retreat off the retrograde bed), so shouldn't this be worded: ". . .such a retreat may not occur when yield stress. . ." rather than "may occur"?
Good point, we have changed it to this

p11, line13: delete the extra "to the"
Fixed

p11, line19: "of" should be "or"
Fixed

p11, line31: Consider rearranging to better prime the reader for the negative feedback to come: ". . .the positive feedback initially dominates, causing an overshoot. . ."
Fixed

p14, line16: parameter regimes
Fixed

References

Brinkerhoff, D. and Johnson, J. (2015). Dynamics of thermally induced ice streams simulated with a higher-order flow model. *Journal of Geophysical Research: Earth Surface*, 120(9):1743–1770.

Robel, A., Schoof, C., and Tziperman, E. (2014). Rapid grounding line migration induced by internal ice stream variability. *J. Geophys. Res.*, 119:2430–2447.

Suckale, J., Platt, J. D., Perol, T., and Rice, J. R. (2014). Deformation-induced melting in the margins of the west antarctic ice streams. *Journal of Geophysical Research: Earth Surface*, 119(5):1004–1025.