The Cryosphere Discuss.,
doi:10.5194/tc-2016-18-RC2, 2016

The Cryosphere
Discussions
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Interactive comment on "Persistence and Variability of Ice Stream Grounding Lines on Retrograde Bed Slopes" *by* A. A. Robel et al.

Anonymous Referee #2

Received and published: 9 May 2016

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.

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Discussion paper

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.

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.

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.

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"

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

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.

p4, lines5-6: Although you are assuming a plastic bed (implicitly the bed strength>=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."

p5, lines22-24: Another sentence or two on how the critical accumulation rate values remove the solution branches would be beneficial.

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.

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.”

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. . .”

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.

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).

Sections 4.3 and 4.5: The explanations discussed here rely on clearing up the description of the enhanced overshoot in the previous section.

Technical Corrections

p7, line22: bifurcations remain (rather than remains)

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"?

p11, line13: delete the extra "to the"

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

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

p14, line16: parameter regimes

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