The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-244-RC2, 2018

The Cryosphere Discussions

Interactive comment on "The role of subtemperate slip in thermally-driven ice stream margin migration" *by* Marianne Haseloff et al.

Anonymous Referee #2

Received and published: 4 May 2018

This paper derives a parameterization of the rate at which thermally-controlled shear margins migrate outwards based on several physical parameters that can be constrained observationally or theoretically. The model is based on a boundary-layer approximation published previously by the authors and the resulting governing equations are solved through the standard software package Elmer/Ice. The paper is well structured and clearly written.

To the best of my knowledge and understanding, the models and results presented here are novel, relevant, and mathematically and physically sound. The conclusions are thoroughly backed up by the results presented in the manuscript. There are no results that appear to be unsupported. I recommend this manuscript for publication after minor modifications have been made, as it would constitute a valuable contribution

to the community and readers of The Cryosphere.

That being said, I think there are a couple of opportunities for improving this manuscript that the authors should consider.

(1) A key goal of this paper is to propose a parameterization of the rate of margin migration that can be incorporated into large-scale ice-sheet models to capture this process. I like the ambition, but I think it would be valuable for the authors to state more explicitly that this parametrization is probably not generally applicable. There is the obvious issue of topographic controls, which are certainly relevant in some cases. Then, there is the assumption that the shear margin is thermally controlled and I think it would be useful for readers to learn whether the authors argue that they believe all margins are thermally controlled or whether they simply focus on this subset for the purposes of this study. Finally, the boundary-layer approach might not be equally applicable everywhere? For example, it is not clear to me that a margin that is wide as compared to the ice thickness scale (e.g., the Whillans ice plain with margins of 30km+) would be well described by this particular approximation.

(2) The boundary-layer model is very similar to the approach previously published by the same authors in JFM. One difference that stands out is the inclusion of subtemperate sliding leading to a different stress boundary condition. Are there other differences that I've missed? Given that the model approach as such has been published already, I'm not sure that it is necessary to introduce the model quite in this level of detail. Instead, I would suggest emphasizing the differences between the models more clearly. Given that the audience of TC is less theoretically inclined than the readers of JFM, I suggest a concise summary of the governing equations and boundary conditions together with a summary of the key assumptions behind the boundary-layer approach and the limitations entailed for applying this idea to field data. For example, the surface correction s' does not actually couple back to the mechanical model, so would this approach be suited to think about the Thwaites shear margin given the rapid thinning rates and differences in ice thickness in ridge/stream? In the discussion section, the

authors argue that they make two simplifying assumption, (1) assuming subtemperate slip and (2) ignoring temperature-dependence of stress and viscosity. I would argue that there are a lot more assumptions entailed in the model setup. I think it would be helpful to spell these out explicitly in the discussion.

(3) I was very surprised to read that the authors are unable to incorporate the temperature-dependence of the viscosity and the basal yield stress because of the large computational times apparently required to capture this. After all, this is "just" a 2D model? Ignoring the temperature-dependence of these two parameters strikes me as a very significant drawback of this analysis, both because both parameters are sensitively dependent on temperature (e.g., likely multiple orders of magnitude in the viscosity) and because both of these parameters are very consequential for margin migration. I am convinced that a more complete solution to this problem within reasonable computational time is well within the reach of modern numerics. That being said, I realize that switching the numerical approach would require an unreasonable amount of time by the authors, so I think the current route is acceptable. That being said, I suggest that the authors qualify the generality of their solutions very clearly and point out that ignoring temperature-dependence is likely not a minor detail.

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