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Discussions

# *Interactive comment on* "A simple model of mélange buttressing for calving glaciers" *by* Tanja Schlemm and Anders Levermann

**Anonymous Referee #3**

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This paper attempts to set a physically motivated bound on calving rates (this is clear from the paper but perhaps not from the abstract). This is an important task and the authors take a reasonable approach towards this goal.

I have a number of comments which I think should be addressed before this paper is accepted:

Numerical experiment

Because the boundary condition on the sides of the channel are periodic, in this setup any potentially formed ice shelf would be unconstrained and therefore incapable of providing ice shelf buttressing. To some extent ice melange can be though of as a weak ice shelf with different rheology, and therefore melange buttressing will also be absent in a setup that does not allow ice shelf buttressing. I find the fact that a melange buttressing parameterization is tested in a setup that does not allow for ice shelf/melange buttressing to begin with inconsistent. Using no slip boundary conditions on the side walls would solve this inconsistency. For the no slip wall case then, the effective melange buttressing can be diagnosed from the model and compared with observed and modeled values of melange strength.

### Simplified calving relations

Section 3.4 doesn't make much sense. Calving relations are simplified by a fitting a function to a region generated by considering different water depths and freeboards. This simplified relation is then used in the numerical simulation. Because the water depth is known exactly in a given setup (the numerical experiment) an exact calving relation should be used directly, rather than a fit to the range of values generated from multiple water thicknesses. If this were not computationally feasible, linearization locally using Taylor expansion should be used, not an arbitrary global line fit.

### Melange properties

The authors ignore the granular character of the melange. Because melange is a sea ice/ice berg mixture, it is its concentration that has bigger impact on its strength than its thickness. Thickness becomes relevant only when the concentration is close to 1. Yet, in this paper it is thickness that is the key variable in deriving the bound on calving rate. It should be either stated that concentration is assumed to be 1, which is unrealistic, or the melange concentration should be taken into account, perhaps by elaborating on the relationship between melange thickness and melange effective thickness

The authors use the terms melange thickness and melange effective thickness interchangeably, however these are not the same. This has an effect on the mass conservation in equation 2. Because melange thickness is not melange effective thickness, the calving rate does not equal the rate of melange formation at the calving front. This needs to be addressed/corrected.

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Discussion paper

Melange flow and material properties are all lumped into one parameter b, it should be justified what the reasonable range of b is. There should also be a way to translate this parameter b to melange strength (under some assumptions) so that there is a clear way to evaluate the parameterization in the future when more observations become available. Also, as b is likely to be bounded because realistic melange has a finite maximum strength; this has implications for constraining the value of Cmax for a given embayment geometry.

Minor:

*Forcing in the numerical experiments is unclear - why is the ice shelf removed throughout the simulations, rather than just at the initial time of each experiment?

*Figures not well referenced through the text - there is a lot of statements floating around and it is unclear if they are based on a figure or equation or some previous work.

More comments and suggestions are included in the attached pdf.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2020-50/tc-2020-50-RC3-supplement.pdf

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