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The Cryosphere Discussions

Interactive comment on "A simple stress-based cliff-calving law" *by* Tanja Schlemm and Anders Levermann

Anonymous Referee #2

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The objective of the paper is to derive a parameterisation of calving rates for *"cliff-calving"*. *"Cliff-calving"* is defined as calving for ice fronts where the ice thickness exceed a stability limit. However, the paper lack a clear definition for this *"cliff-calving"* making the objective of the paper and applicability of the parameterisation relatively uncertain. The *"cliff-calving"* mechanism has been introduced by Pollard and al. (2015) and De Conto and Pollard (2016) to explain high mass loss rates from the Antarctic in the geological past. However, a recent study (Edwards et al., 2019) shows that this mechanism is not required to reproduce past sea-level changes. These controversial results require improved physically-based models, which is the aim of this paper. As this process is not currently observed, validating these models with observations is, by definition, impossible. It is thus crucial to detail the physical basis of proposed models

and parametrisations and analyse their sensitivity. As pointed by the first reviewer and comments by Vieli and co-authors, the manuscript in his present form is too short on these aspects.

To derive the calving relation, the authors compute the stresses in the vicinity of synthetic ice fronts with various thicknesses and water depth using a full-Stokes ice flow model. A stress criteria (based on the maximal shear stress) is used to define the region that will calve. This is further converted to a calving rate using a reference failure time. As pointed by Vieli and co-authors, this study is extremely similar to Mercenier et al. (2018), and do not really acknowledge it. As Schlemm and Levermann re-use the failure time calibrated by Mercenier et al. (2018), the only difference is the stress criteria and thus the failure region. The first reviewer and Vieli and co-authors, already provide guidance to improve the paper by clarifying the hypotheses, running more sensitivity experiments, comparing with previous similar studies and improving the discussion to define the applicability of the proposed calving law. I fully support their main comments and this implies major changes in paper.

Finally, at the end, Jackobsahvn is presented as one of the few glaciers that is *"in the calving cliff regime"*; However, this *"cliff regime"* is not really defined, from page 7 lines 29-30, I understand that the authors define the cliff regime from their critical shear stress; So a glacier would be in the cliff regime if their critical shear stress is reached somewhere in the domain; which from their numerical experiments appends only for freeboards larger than 100m? At the end it is a bit disappointing that the proposed parameterisation underestimate the calving rate of one the few glaciers in what the authors call the *"calving cliff regime"*, by more than one order of magnitude. Especially when the parametrisation from Mercenier et al. (2018) does a fairly good job for the same glacier. As shown by Vieli, the Schlemm and Levermann caving rates become higher than the Mercenier et al. calving rates for larger free boards. So the paper should really focus on giving better description and justification for their mechanism, and its domain of applicability. Should it replace existing parameterisations for large

freeboards? In this case how to define the transition to the cliff regime? Should we sum the processes or take the maximum calving rate? Without answering theses question properly I don't see how the proposed parametrisation could be used by the community.

I give few more detailed comments on the paper below :

- Abstract: the mechanism "cliff-calving" is not really defined in the abstract and there is a confusion with "normal" calving of tide water glaciers as currently observed, see comment above. This distinction and the definition of "cliff calving" is also not really clear in the introduction. It should be clear since the beginning that the paper propose an extension (extrapolation) of the calving mechanism to glacier freeboard heights that are not currently observed.
- Page 1, lines 18-22: the word "loss" introduces a confusion between the processes that remove ice from the ice sheets (what is implied with the reference to Antarctica), and the fact that the ice sheets are not in balance due to increase losses by calving and/or melt (the numbers for Greenland are the respective contribution to the unbalance). Please clarify.
- "Failure region" everywhere in the text and Figures 3-4-5. There is a confusion between the region where the stress is higher than the threshold and the "failure distance" L. It seems that L is the maximum distance from the front where the stress is in excess to the critical stress. Please clarify.
- Figure 4. What is the color scale? Indicate that the outline for H=1000m is also shown in Fig. 3 (top-left).
- Page 9, line 4: clarify the "bend" and the "two fits" at the critical freeboard.
- Page 9, Eqs. 14-17: explain the values for the fit; which ones have been optimised, which ones are prescribed and why?

- Page 11: comparison with Jackobsahvn; clarify the discussion about the grounding line and front.

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