

Interactive comment on “Calving relation for tidewater glaciers based on detailed stress field analysis” by Rémy Mercenier et al.

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Received and published: 1 October 2017

This manuscript investigates the influence of water level, calving front slope and basal sliding on the stress and flow regimes in the vicinity of the calving front and proposes a new parameterization for calving. This relation has the advantage of its simplicity and ease of use, since it is only function of ice thickness and the water depth at the calving front. However, the manuscript has some issues that I think should be addressed prior to publication.

In the Introduction section, when the authors review the previous literature on calving modeling some previous work is misrepresented and the overall perspective is confused. I think that some paragraphs, at least from line 34 to line 71, have to be

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rewritten, keeping in mind the following tips.

- Lines 34-35. The crevasse depth criterion (Benn et al, 2007) isn't a generalization of the flotation criterion, is a completely different approach to the calving problem based on the physics.

- In lines 42-50 it seems that the authors claim that the depth averaged longitudinal stress is the "main driving force" of crevasse-depth calving based models. But from Otero et al. (2010) such limitation was overcome and subsequent models (e.g. Cook et al., 2014; Todd and Christoffersen, 2014; Otero et al., 2017; Benn et al., 2017) computed crevasse penetration locally based on nodal stresses.

- I also note the lack of any reference of the recent work of Benn et al. (2017) in which crevasse-depth calving approach they use the maximum principal stress, which is the same metric used by the authors in the second part of the paper.

- The importance of melt undercutting in the process of calving is underrepresented in lines 63-71. In some modeling works cited in the manuscript (Cook et al., 2014; Krug et al., 2015) did indeed conclude that melt undercutting does not significantly affect calving rates. On the contrary, in recent studies has been shown that melt undercutting play an important role in calving of some Arctic glaciers (Luckman et al., 2015; Petlicki et al., 2015; Cowton et al., 2016).

In lines 317-318 I miss any reference to the crevasse-depth calving criterion, that is a physics-based approach. And the calving parameterization proposed by the authors is "semi-empirical" since they tune two parameters using observational data.

In section 5.3 the authors describe how they tune the model against data from some tidewater glaciers in the Arctic, so I recommend to rename this section. And the statement "In this study we used the values shown in Table 5 from diverse data sources for comparison" (line 338) is unclear. The authors are using the data showed in Table 5 to tune the parameters of the model, not for comparison.

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It seems that, in the last two paragraphs of section 5.3, the authors compare their model predictions with the same data used to tune the model. So the statements “the general agreement shows that the parameterization is suitable to predict calving rates for many tidewater glaciers in the Arctic” (lines 344-345), “A simple calving rate parameterization was found that predicts calving rates of tidewater glaciers in the Arctic reasonably well” (line 355) and “we propose a simple and new parameterization for calving rates for grounded tidewater glaciers that is in good agreement with observations” (line 10) are not fully justified.

Check on the errors in data plotting pointed out by Benn and Todd in their Short Comment.

340. “Calving rates” instead “Velocity data”

344. “rates” instead “velocities”

Change the units of calving rate in Table 5 to m d^{-1} in coherence with Figures 13 and 14.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-183>, 2017.

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