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Interactive comment on "Comparison of four calving laws to model Greenland outlet glaciers" *by* Youngmin Choi et al.

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This is an excellent paper. It makes an important contribution to the calving literature by taking the timely step of testing alternative calving laws against a large set of observations. It is well written and clearly structured, and proceeds logically towards solid conclusions. The discussion is balanced and thoughtful, and is rich in insight.

There is likely a range of dominant calving processes at the studied glaciers (e.g. melt-undercutting; super-buoyancy), and this is inherently problematical for simple calving laws. The authors' strategy of including a melt-rate parameterization alongside a calving law goes some way towards addressing this complexity, but it is clear that there is still some way to go in the search for a universal method for calculating frontal ablation. The authors of course acknowledge this, and raise many important issues in the Discussion.

The impact and usefulness of the paper could be improved further if it were expanded slightly to clarify some fundamental issues associated with implementing and testing the calving laws. Two issues in particular would benefit from more detailed treatment: 1) the rationale behind model tuning; and 2) the methods employed to identify the 'best fit' between observations and the tuned models.

All of the laws - as implemented here - rely on tuning. As the authors explain, this places limits on their practical usefulness when applied to uncalibrated glaciers, or when projected into the future. The authors should also note that this is particularly problematic where the parameters span a wide range (orders of magnitude for HAB and EC compared with a factor of 2-3 for CD and VM [with one outlier in the latter]). A more fundamental point that should be made is that the ability of a tuned model to replicate observations does not prove that it 'works' in a meaningful way. The success of a calving law tuned on a glacier-by-glacier basis may simply be a test of its flexibility, as opposed to its actual predictive/diagnostic power.

The authors make some very interesting points regarding the tuning of HAB. The rationale behind HAB (as originally developed for Columbia Glacier) is that the glacier will calve as it approaches buoyancy. As noted by the authors, this does not allow floating tongues; however, the opposite is also true: HAB predicts that a glacier will not calve if HAB>Ho. But of course, many well-grounded glaciers do calve, for various reasons, meaning that HAB is problematical in both directions. A wide range of grounding conditions in the study glaciers - and associated calving processes - probably accounts for the very wide range of q in this study. This has major implications for modelling future conditions, if buoyancy conditions and dominant calving processes change through time.

The authors rightly flag up the problems with using crevasse water depth as a tuning parameter in the CD models. I am now of the opinion that water depth is neither useful nor appropriate as a tuning parameter in most cases (see Benn et al., 2017, p. 701). (Ice shelf hydrofracture may be a significant exception.) I agree with the authors that results obtained by water-depth tuning of CD should be treated with caution (for example, I think that the studies of KNS by Lea et al. are deeply flawed for this reason).

However, it should be noted that water depth is not included in the CD model as implemented by Todd et al. (2018). In that study, the CD model was able to reproduce seasonal calving variability at Store Glacier without any tuning - in stark contrast to the performance of CD in the present paper. A major difference between Todd et al. (2018) and the present study is the model physics (3D full stress vs. 2D plan-view). Therefore, the authors could be more explicit that the CD model may not be the best choice for 2D plan-view models because they do not accurately capture the required stresses.

To aid comparison between the present study and Todd et al. (2018), I would like the authors to show results of CD with dw = 0 alongside the tuned results. A model that does not require any tuning has obvious advantages, so it would be particularly interesting to see how it performs in this case.

The performance of VM is impressive, and it is worthwhile delving deeper into possible reasons for this. The results show that, on a glacier-by-glacier basis, there tends to be a consistent relationship between calving rate and v(sigma_vm/sigma_max) (Eq. 3). Perhaps the strength of these relationships partially reflects including the velocity vector in the calving rate. At the least, the strong correlation between calving rate & velocity means that VM is inherently primed to produce more reasonable calving rates. The extent to which this questions the model's predictive capacity/skill is difficult to address, but this is clearly an issue that requires further investigation in future. This point should be added to the Discussion (p. 14, around line 5).

Optimization of the model parameters was done by manually finding the values that "qualitatively best capture the observed variations". The authors should provide more information about this procedure. Figure 7 very usefully compares the modelled 2017 front positions, but what of the other characteristics of the records (e.g. timing of still-stands, advances or retreat episodes)? What criteria were used to decide on the best-fit parameters? Were some criteria weighted more than others? Were the criteria used consistently? To address these questions, more information should be added to the text around p. 6, line 3.

I also suggest the authors present a set of time-distance diagrams comparing observations and model results for each flowline (perhaps as Supplementary Material). This would then allow readers to assess the performance of each model in greater detail than is currently possible.

Minor points:

p. 2, L5: "This law only relies on tensile stresses..." add: "and frontal velocity"

p. 4 L26: Clarify what is meant by 'M = non-zero'. Does the method somehow require some melt rate, or is this simply intended to state that the appropriate melt rate is applied?

p. 5, Equation 9: B is already used for the ice viscosity parameter, so a different symbol is needed for the melt rate parameter.

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