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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 #1

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This paper presents a new calving law designed to calculate calving rates from unstable cliff-like caving fronts. The authors use a two-dimensional flowline geometry to calculate maximum shear stresses in the ice. A critical shear stress is then used to define a region of failure - the ice which will be lost in a calving event. An analytical fit to the results is used to develop a generalised formula relating calving length to freeboard and relative water depth, and the authors then use a constant failure time of the ice to convert this to a calving rate.

This paper will be of interest to the community, as it provides a new method of calculating calving rates for unstable ice cliffs. This is a topic of considerable interest, as calving models which include cliff failure produce substantially different sea level rise predictions than other ice sheet models (e.g. DeConto and Pollard, 2016). A calving

law of this nature will always require simplifications, and I appreciate the authors have taken time to be clear about the assumptions made. However, these assumptions are significant and are likely to be highly limiting in when the calving law could or should be applied. I would like to see the authors comment further on the conditions under which this calving law would be valid and how widely they occur. I strongly recommend expanding the discussion of the assumptions made, so readers fully appreciate when and where the model can reasonably be used. There are also a number of errors and omissions in the paper which should be corrected. A list of specific recommendations is provided below.

Major comments

The model used to derive the proposed analytical calving law has a highly simplified geometry – with zero surface or bed slope, no lateral drag, and no sliding at the bed. Previous studies have shown that the extent of the "failure region" discussed in this paper is strongly affected by basal sliding rates (Ma et al., 2017). Likewise, other studies have shown that the stress regime around the calving front is strongly affected by surface slope (Mercenier et al, 2018).

As the authors point out, "there are no glaciers currently available where cliff calving is the primary failure mechanism", but modelling studies such as DeConto and Pollard (2016) suggest cliff failure could occur in future in deep Antarctic basins, after rapid retreat of their buttressing ice shelves. These environments are highly likely to experience basal sliding, as well as lateral drag. It is hard to say what proportion of ice cliffs might meet the authors' conditions, but the proposed model for predicting calving rates seems a lot less generally applicable than simply using the maximum shear stress to define a new calving front location. At the very least the paper should include more discussion of precisely what circumstances the model is valid for, and under what conditions (e.g. basal sliding) it is likely to fail.

Minor comments

Page 2 line 6: Columbia glacier is in Alaska, not Canada

Page 2 lines 28-29: The description of Mercenier et al. (2018) is extremely brief and doesn't contrast the model with other studies, which would be much more informative. This also seems a suitable point to reference Morlighem et al. (2016).

Introduction: The introduction misses damage mechanics methods which have been used to implement calving in a tidewater glacier (Krug et al., 2014).

Page 3 line 5: "it is not clear what a cliff calving law would look like". Are the authors aware of Bassis et al. (2017) which already implemented a calving law based on cliff instability?

Page 4 lines 5-10: No boundary condition is provided for the upstream boundary of the model (r.h.s. in figure 1)

Page 5, eq. 11: Should y in this equation be z?

Page 7 lines 2-3: "However, it does not take into account whether deviatoric stresses are tensile or compressive or shear stresses and this is likely to be important for ice failure." Surely this is the advantage of using the von Mises stress as a criterion – it is able to allow for failure under both tension and shear, and is therefore more widely applicable than a criterion that considers only one mechanism of failure?

Page 7 lines 5-10: these uncertainties should be explored further in the discussion, which doesn't currently make their magnitude clear.

Page 7 eq. 13: I'm not sure where the term sqrt(μˆ2+1) on the l.h.s. comes from here.

Page 9 line 1: "Above a critical freeboard of about 1000m the failure region encompasses the whole ice thickness." Is this based on results from figure 4?

Page 9 lines 3-4: "The freeobard [sic]- failure region relation has a bend at the critical freeboard and hence the two parts require separate analytical fits" Figure 5 shows no freeboards above 800 m, so readers cannot see how this conclusion was reached.

Page 10 eq. 18: What are k, r and B?

Page 11 eq. 19 & 20: I think σ and σ0 here are not the same as in previous equations?

Page 11 eq. 21: Is part of this equation missing? What values have you used for k, D0 and Dc?

Page 13 bullet point 3: This sentence does not make sense, please rephrase.

Page 13, figure7: I don't think this figure is referenced in the text?

Page 14 line 4: "Where the failure region does not encompass the whole ice thickness, an analytical fit was made." This sentence is quite unclear. To my understanding, your results use an analytical fit which is only valid for freeboards less than 1000 m? Is that what was meant here?

Page 14, line 6: The authors conclude that the application to Jakobshavn glacier demonstrates that the modelled calving rate can be "realistic". I'm not sure that the results support a strong conclusion here. The modelled calving rate is lower than the observed calving rate, which is appropriate. But the modelled calving rate could increase by a factor of ten and still meet this condition. I think the discussion needs to be a lot more clear about the very large uncertainties in calving rates produced by this model.

There are also quite a number of spelling and grammar mistakes in the document, and I suggest additional proof reading before resubmission.

Additional references

Bassis, Jeremy N., Sierra V. Petersen, and L. Mac Cathles. "Heinrich events triggered by ocean forcing and modulated by isostatic adjustment." Nature 542.7641 (2017): 332.

Krug, J. W. O. G., et al. "Combining damage and fracture mechanics to model calving." The Cryosphere 8.6 (2014): 2101-2117.

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