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

D. Benn

dib2@st-andrews.ac.uk

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The paper by Mercenier, Lüthi and Veili presents a new calving relation based on analyses of stresses and a damage evolution function. New approaches on the calving problem are welcome, and the paper contains some useful perspectives and comments. However, the paper suffers from some flaws. First, the calving relation is not entirely physically-based as claimed, but is actually semi-empirical and relies on some questionable assumptions. Second, the authors' claim that their model is "in good agreement with observations" is unjustified, because the model has simply been tuned to fit a set of observations, not validated against independent data. Third, there are significant inconsistencies between Eq. 22 and Figures 11,13 and 14, each of which produces a different calving rate for any chosen glacier in Table 5. Fourth, the authors have misrepresented the existing literature on calving modelling, providing misleading context for their work.

Model formulation

The Hayhurst stress used in the first part of the analysis is not a physical quantity - it is an ad hoc combination of different stress metrics that - in the absence of any physical understanding of mechanisms - can be tuned to match observations. The Hayhurst stress is, in essence, a semi-empirical approach to divining which factors may control calving behaviour. This notwithstanding, the authors then abandon the Hayhurst stress approach and adopt the maximum principal stress as the foundation of their calving relation. The maximum principal stress is exactly the same stress metric used by Benn et al. (2017), and it is in fact a generalisation of the Nye crevasse depth formula used by Benn et al. (2007a). (Martin Lüthi provided a detailed review of Benn et al (2017), so at least one member of the author team has been aware of these results for some time.) The maximum principal stress is also equivalent, in 2D, to the 'effective stress' metric used by Todd & Christoffersen (2014) to model calving at Store Glacier.

The authors then formulate a calving rate law using a damage evolution function. Calving rate laws are attractive from a modelling perspective, but their physical justification is unclear. Rate laws are likely valid where calving is driven by melt undercutting, although this process is excluded from the analysis of Mercenier et al. In our work, we have focused on calving position laws, which predict the location of the calving front from the state of stress at any given time. Model experiments with the discrete element model HiDEM provide justification for this approach, because calving events occur rapidly in response to specific states of stress (Benn et al., 2017). Given the existence of these contrasting approaches to formulating calving laws, we feel that some discussion of this issue would benefit the present paper, and ideally the authors should provide more detailed justification for choosing a rate law.

In lines 317-8, the authors claim that their proposed calving parameterization is “physics based, as opposed to the purely or semi-empirical nature of other approaches”. This is wrong on two counts. First, crevasse depth calving laws (Benn et al., 2007; Nick et al., 2010; Todd and Christoffersen 2014; Benn et al. 2017) are physics based. Second, the calving parameterization proposed by Mercenier et al. is itself ‘semi-empirical’, and is reliant on tuning to data.

Model predictions and observations

In Section 5.3, the authors obtain values for two empirical parameters B (damage evolution rate) and sigma_th (damage threshold) using data from calving glaciers in the Arctic. The text in this section is rather obscure, but it seems that the data plotted in Figs. 13 and 14 are the same as those used for model tuning. Thus, the calving data in Fig 13 (wrongly described as ‘velocity data’ on line 340) are not shown ‘for comparison’, but are in fact the data points used to tune the position of the isolines of calving rate. Furthermore, Figure 14 does not compare calving data with model predictions, but compares calving data with predictions from a calving law tuned using the same data. It is therefore a representation of model fit rather than model performance. The authors are not justified in claiming that the model “predicts calving rates. . . reasonably well” (line 355), or that it is “in good agreement with observations” (line 10). The results simply mean that it is possible to tune the model to fit the data, not that the model has actual predictive power.

Errors in data plotting

Equation 22 and Figures 11, 13 and 14 do not seem to show the same calving parameterization. For example, plugging the “Columbia 2000” data into Eq. 22 (H = 382m, Hw = 260m, w = 0.68, measured Uc = 24.7 m d-1), with B = 37 MPa-r a-1, σth = 0.17 MPa, r = 0.43, â■ťig = 0.009, gives a calving rate of 20.72 m d-1. However, in Figure 11, taking w $\sim$ 0.7, and ice thickness = 382m, calving rate is > 23 m d-1. In Figure 13, the intersection between H = 382m and w = 0.67 gives a value greater than 45 m d-1. Finally, in Figure 14, ‘Col 2000’ is shown with a predicted calving rate of $\sim$36 m d-1.

Review of previous literature

Previous literature is cited in a very partial way, and some previous work is misrepresented. For example (line 34) the authors state that Benn et al. (2007a, b) “generalized the flotation criterion”, and later (line 318) they imply that these papers take an empirical or semi-empirical approach to modelling calving. These statements are untrue. The papers by Benn et al. proposed a new, physically based approach to modelling calving, setting the position of the calving front where crevasses penetrate to the waterline (Benn et al., 2007a). This was later modified by Nick et al. (2010) to include crevasse penetration through the full thickness of the glacier. These initial formulations computed crevasse depths from only longitudinal stresses, but subsequent work has generalised the crevasse criterion to include extensional stress in 2D (Todd and Christoffersen, 2014) and 3D (Benn et al., 2017). Benn et al. (2017) also discussed at length the issue of stress balance “snapshots”, and proposed strategies for overcoming these limitations. The criticism that crevasse depth models lack “validation with field observations” (line 39) is also unwarranted. Some authors have tuned the model to match observations (e.g. Nick et al., 2014; Lea et al., 2014), which is exactly the same approach as taken by Mercenier et al. Comparison of the predictions of an untuned crevasse depth model against independent observations has been done by Todd et al. (in press), although of course Mercenier et al. cannot be expected to cite this work. A copy can be supplied on request.

In lines 42-50, the authors present an approximation for depth averaged longitudinal stress and state that it is the “main driving force” of crevasse depth models. However, the crevasse depth criterion is not fundamentally a depth-averaged law, although it has previously been implemented in 1D dynamic models (e.g. Nick et al. 2010). Todd and Christoffersen (2014) implemented the crevasse depth criterion in a 2D full-Stokes model, similar to that presented by Mercenier et al., and computed crevasse penetration locally based on nodal stresses. The version of the crevasse-depth calving law adopted by Benn et al. (2017) uses the maximum principal stress, which is exactly the same metric used by Mercenier et al. in the second part of their analysis.

The review of melt undercutting (lines 63-67) also misrepresents the literature. Hanson and Hooke (2000) did not look at undercutting at all, and O'Leary and Christoffersen did not "suggest that an increase of water depth leads to a higher rate of oversteepening development". In fact, O'Leary and Christoffersen argued that increasing water depth increased the stress response to a given amount of undercutting, and hence the magnitude of the 'calving multiplier' effect. The opposite trend was found in the more detailed model experiments by Benn et al. (2017), who showed that the effect of undercutting is diminished by increasing water depth. Cook et al (2014) and Krug et al (2015) did indeed conclude that melt undercutting does not significantly affect calving rates. However, this is due to the insensitivity of their models to undercutting, as discussed in detail by Benn et al. (2017). In fact, recent studies have shown that melt undercutting is the primary driver of calving in Svalbard and some Greenland fjords (Luckman et al., 2015; Cowton et al., 2016).

Additional Point

Regarding the role of basal sliding, the authors provide a useful perspective on their results in lines 303-4, in which they point out that spatial variations in basal slipperiness (as would result from a pressure-dependent sliding law) would likely introduce velocity gradients that could affect calving. This caveat is not reflected in the statements in the abstract ("the effect from VARIATIONS in basal sliding is much smaller" (emphasis added) and the conclusions ("basal sliding likely has a weaker effect ...on stability": lines 353-4). Because they only impose uniform basal slipperiness, the experiments presented in this paper cannot evaluate the influence of basal sliding on calving - and this includes their important relationship with water depth. The summary statements in the abstract and conclusions should reflect this.

Concluding remarks

The proposed calving relation has the benefit of simplicity, and makes some interesting

and testable predictions. However, we are sceptical this it represents an improvement on existing approaches because it is based on only one control on calving - the stresses introduced by the force imbalance at the ice front - and neglects other important processes. Calving also occurs in response to longitudinal stresses caused by along-flow variations in basal and lateral drag; melt undercutting in response to heat flux from the ocean; and super-buoyancy where glaciers flow rapidly into deep water. Indeed, these processes are known to be the main drivers of calving on several of the glaciers listed in Table 5. The predictions of the model listed in lines 321-326 correspond to some observed behaviour (e.g. the increasing instability of high, unsupported ice cliffs) but not others (e.g. calving triggered by ice flow into deepening water).

But of course the search for a general calving law goes on, and alternative approaches may help us reach this elusive goal. With greater awareness of its limitations and better perspective on its context within the wider literature, the paper by Mercenier et al could provide a useful contribution.

Doug Benn and Joe Todd

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