

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

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### **General Appreciation**

This paper analyzes the stress near the calving front of idealized glaciers. Then, by relating the state of stress, as measured by the Hayhurst criterion, to different ice thicknesses and water depths, the authors combine the stress estimates with an analytic model of damage mechanics to estimate calving rates. The model proposed only depends on three parameters, but these parameters must be determined by comparison with observations, field or laboratory. In this case, the model is calibrated to a suite of Arctic glaciers and the model is shown to reproduce observed calving rates for the glaciers for which the model is calibrated.

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The paper takes an impressively innovative approach to an old problem and the agreement between observations and model predictions is impressive, although it is hard to evaluate the model's performance given the fact that it was tuned to match this specific set of glaciers. I suspect that the approach pioneered in this paper will ultimately become more commonly used. I do, however, have a few significant questions about the physical interpretation of the mathematical formulation. This paper has already benefited from the insightful comments of two very well qualified reviewers. My comments both build and diverge from these reviewer comments so I will first wade into the discussion already initiated by the reviewers and try to cast it in a slightly different perspective. This will hopefully better motivate my comments about the physical interpretation of the calving law that I present as the last point in the major comments section.

### **Major comments**

1. *What is the right stress metric to use and why?* The authors of this study use the Hayhurst criterion, a linear combination of stress tensor invariants. In contrast, Doug Benn and Joe Todd argue that this is a purely empirical relationship and that the largest principle stress is the more physics based metric and results from a generalization of Nye's zero tensile stress model. Physics tells us that in the absence of anisotropy, the failure criterion should depend on invariants of the stress tensor. However, it is unclear to me how or why *physics* provides any guidance as to which invariants it depends on. To be more clear, in the uniaxial case, failure is clearly related to the single component of stress and because ice, like most materials, is much weaker in tension than compression observations show that (tensile) failure occurs when the applied strength exceeds some threshold. In the Nye zero stress model, that threshold is set to zero. We know from experiments and field observations that ice has finite strength. Fortunately, Weertman showed that for fields of closely spaced crevasses, the depth of crevasses will approximate the Nye zero stress depth so long as the initial starter crack length

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is sufficiently large and the strength of ice, measured as critical stress intensity factor, sufficiently small. The generalization to multi-axial failure is less obvious. Benn et al., (2017) apply the maximum principle stress. We did the same thing in Yue and Bassis, (2017), but also considered the possibility that shear failure could occur. Under multi-axial loading there are a larger number of invariants of the stress tensor that must be considered. The Hayhurst criterion attempts to combine multiple modes of failure in a single fracture growth model by taking a linear combination of invariants. Both the Hayhurst and maximum principle stress criterion reduce to the uniaxial case when other stress components vanish and one could view the Hayhurst criterion as a generalization of the Nye criterion to multi-axial loading. The distinction between the two hypotheses is that the maximum principle stress criterion predicts that multi-axial loading will have *no* effect on the depth or rate of fracture propagation. To interpret the maximum principle stress criterion rigidly, implies that failure of ice only occurs through tensile failure and no other mode of failure is possible, an assertion that is falsified by laboratory measurements. In contrast, the Hayhurst criterion tells us that multi axial loading can increase the depth, trajectory and rate of fracture propagation. Moreover, the Hayhurst criterion reduces to the maximum principle stress criterion and is thus more general. Crucially, I don't see a way to deduce which— if either— is correct in the absence of observations. It is, however, clear that the maximum principle stress criterion has provided useful results that allow us to predict how fields of crevasses respond to changes in stress. That this relationship is not deduced from a more fundamental principle doesn't detract from its usefulness. It is also entirely possible that assuming under glaciological regimes tensile failure dominates is a useful *approximation*. I'm uncomfortable litigating the physical appropriateness of one model versus another without observational data to confirm or refute hypotheses, which is something the authors might think about reviewing. *Where I think this manuscript could improve is to provide a better motivation for why the Hayhurst stress is first introduced and then, why it*

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*is abandoned in favor of the maximum principle stress. I think a valuable result would be to show that results are insensitive to the choice of failure metric (Hayhurst or maximum principle stress). We may not know which is the correct law, but it might not matter. Similarly, it would be helpful if the authors could comment on any observational evidence to support or refute the use of the Hayhurst stress in glaciological applications.*

2. *Calving rate versus calving positions laws:* As a minor point, the relationship between calving rate and calving position laws is in fact direct (see Bassis, 2010). Calving rate laws can be deduced from statistical averages over many calving events and are valid over time scales that are much longer than the typical recurrence interval between calving events provided the spatial scale of calving events is small compared to the glacier system. The relationship between the two, in a statistical sense, is really just a switch in time scale. Calving position laws have the advantage that they better encompass fluctuations in calving front position, but are less practical over longer timescales (e.g., millennial) when the only data available is average position at discrete intervals of time. Moreover, many of the 'position laws' can be equivalently formulated as rate laws. A simple example of this is the height-above-buoyancy law and others in its family. It is straightforward to cast this as a continuous rate law for calving front position analogous to those used for grounding line migration. *What is a more fundamental issue for me is whether the terminus position of the glaciers used to calibrate the law are relatively constant or changing. If the glaciers are in or near steady-state than many variables can be correlated without indicating causality. A more convincing argument is if the calving law can predict the rate of retreat/advance for one or more glaciers that is changing.*
3. *Empirical, semi-empirical versus physical calving laws:* Both reviewers brought up the point that the calving law is empirical rather than physical. I have similar concerns, although my physical concerns are slightly different and will be raised

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in the point that follows. To preface, I do increasingly worry that we are using the concept of 'physical' versus empirical as a blunt cudgel to beat each other. The rheology of ice that we use is an empirical flow law. The formulation presented by Cuffey and Paterson is excellent and results from a set of calibrated model experiments and laboratory measurements. Despite this empirical basis, most of us don't usually describe our ice dynamics models as 'empirical' or 'semi-empirical' due to the fact that the parameters in Glen's flow law are not calculated from first principles. We glaciologists scorn well calibrated empirical data at our peril. *Instead of wading into the empirical debate, I would encourage the authors to ask a couple of questions. (1) What predictions can the calibrated model make that are independent of the data set used to calibrate the model that can be used to falsify the model? The model is calibrated for a suite of Arctic glaciers, but can it be used to predict the calving rates of glaciers that are not included in the data set? For example, there are only a couple of datapoint for Columbia Glacier, but the retreat has been well documented for several decades. Alternatively, one could use different subsets of data to calibrate the model and then validate against an independent set. More physically, the authors are inferring a threshold stress at which damage begins to grow. This threshold could be compared to observations that indicate the stress at which surface crevasses first appear. (2) How sensitive are the results to the model calibrated parameters? If the model results only weakly depend on the calibrated parameters, then the fact that they are determined empirically is not much of a concern because we only need to get ballpark estimates. However, if the results depend sensitively on one or more parameters than we need to think careful about how to measure these parameters independently and may be concerned that the model predictions may be less reliable when applied elsewhere.*

4. *Physical interpretation of the calving law:* The point that I struggle with the most in this paper is physically interpreting the mathematical model. My interpretation

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of Equation 22 is that authors are stuffing the maximum principle stress as measured at the surface into their damage evolution law and then evaluating how long it takes for a surface crevasse at the location of the maximum stress to develop. The calving rate is then the distance to the maximum stress at the surface divided by the time scale of the calving event. This calculation, however, seems to give the time scale for a crevasse at the surface to develop and **not** the time scale for a crevasse to penetrate the entire ice thickness or some fraction thereof. In simulations that we have done using a similar formulation of damage mechanics as presented here, but simulating the propagation of individual crevasses, dry surface crevasses never penetrate the entire ice thickness. I can accept the arguments that lead to a shallow surface crevasse at the surface, but the magic that then asserts that the surface crevasse will penetrate deep enough to cause a calving event is not clear. In fact, looking at the stress field, it looks like the maximum principle (or Hayhurst stress) decreases with depth. Why then does the surface crevasse propagate the entire distance and why doesn't it take longer to propagate down as the stress decreases? It is this step more than the details of the calibration that makes the calving law seem empirical or divorced from 'physics' to me. Getting fractures to propagate the entire ice thickness has always been a problem for calving models and this study seems to sidestep these issues. I guess I'm OK with postulating a calving form base on the position and magnitude of the surface tensile stress, but the departure from physics should be more clearly emphasized. Furthermore, given this departure, I don't quite understand why the complex damage evolution law is used. What if a linear relationship between damage and principle stress was postulated instead? The rate of damage growth would then have two parameters (a rate factor and stress threshold). Or what if the stress threshold was set to zero, giving a single parameter? Do these choices significantly degrade the fit? Does the calving law depend sensitively on the form of the assumed damage law or is this calibrated out? Can we say anything about the form that the damage evolution law must take if the data is to

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be matched?

### Detailed comments

Page 1 abstract “stress state” or “state of stress”?

Use of crack: considering using crevasse or defining how the term crack includes more than just crevasses for your glaciological audience.

Page 2, near line 35: “Benn et al. (2007a, b) generalized the flotation criterion by setting the terminus position at the location where crevasses penetrate below the water level” I think the authors are getting at the fact that in the height-above-buoyancy criterion, the position of the calving front is specified rather than a calving rate (as in the Brown et al., water depth model). The Benn et al., approach thus provides a different model to compute the terminus position based on when surface crevasses penetrate to the water line. This is mechanically different than the height-above-buoyancy criterion, but falls into a similar type of law whereby the position of the calving front is determined.

Page near line 40: “However, the crevasse depth estimation lacks validation with field observations and is based on a snapshot of the stress balance, neglecting the pre-existence of cracks and their effect on the stress state of the glacier (Krug et al., 2014).” This is an excellent point. Most crevasse penetration models assume that crevasses have a negligible effect on the state of the stress and are purely passive. These crevasse penetration models also ignore the advection of previous existing crevasses into the near terminus region. It is unclear how these effects are incorporated into the proposed model.

Equation (1) is a depth averaged equation. We don’t have to rely on it and can instead compute approximations of the state-of-stress using finite element models. However, Equation (1) has the advantage that it is non-parametric (i.e., independent of ice rheology). More sophisticated methods of calculating the stress field require additional, often unknown parameters like the temperature of the ice and an appropriate sliding

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law.

Page 2 near line 50: “The meaning of such a ‘depth averaged’ longitudinal stress for local fracture, for example for assessing surface crevasse formation, and the calving processes is not clear.” I’m not sure I understand the complaint here. The depth integrated approach yields an estimate of the tensile stress based solely on the ice thickness and water depth. This estimate of stress allows us to estimate the depth of crevasses. Of course, near the terminus the depth integrated formulation may not accurately estimate the stress due to the absence of bending effects and neglected terms in the force balance. This seems like it points to a lack of accuracy rather than a difficulty with interpretation of the meaning of depth averaged stress.

Page near line 55: “However, observations of the appearance of surface crevasses on glaciers in relation to the strain rate field suggest a much lower cohesive strength of glacier ice between 0.09 and 0.32 MPa (Vaughan, 1993) for cold Antarctic ice streams, or as low as 0.05 MPa for a temperate Alpine glacier (Lliboutry, 2002).” This is an interesting point about the uncertainty in the yield strength, but unless I am misunderstanding, might be related to some confusion about different modes of failure. The fact that surface crevasses are detected at low stresses doesn’t imply that the strength of ice in shear must be much lower. Tensile and shear failure can be distinct modes of failure and each can have their own yield strength.

Page 3 near line 70 “This implies that thick glaciers approach flotation at their front but for shallow water depth the constraints on geometry seem less clear.” It is true that the bounds are very wide for small ice thicknesses. I would say that bounds are clear, but not particularly useful for small ice thicknesses given the spread of permissible values.

Equation 3: Should have a dot between the del and u to enforce the divergence of velocity is zero not the gradient of velocity is zero.

Numerical implementation: How is the incompressibility condition enforced numerically? Is this a mixed element formulation?

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Section 2.2. This might be more clear if one uses the scales introduced to non-dimensionalize the governing equations. Then, I assume, we could write the equations in terms of a set of non-dimensional numbers, like the aspect ratio ( $H/L$ ) that describe the dynamic and geometric similarity between solutions.

Section 2.3. I think the boundary condition is traction free not stress free. One is not usually able to prescribe the entire stress tensor.

Page 5, line 130: It is also possible to specify zero slip in the vertical direction along the inflow boundary condition. This prevents edge effects near the zero velocity boundary condition.

Results: Section 3.1: It looks like when the authors say “stress” they mean Hayhurst stress, but what parameters were used to calculate the Hayhurst stress? I would have thought that the patterns of, say Von Mises stress would be very different from the largest principle stress? I can’t find this information in the text or figure caption. This difficulty in understanding how the Hayhurst stress was calculated continues throughout the rest of the results section. Would it be more more helpful to show the largest principle stress, Von Mises stress in separate panels?

Page 7, line 185: The extrusion flow is an example of the Poison Effect.

Page 8, section 3.1.3. I’m not sure I understand the basal slipperiness results. When we did experiments using a full Stokes model we considered no-slip boundary conditions and free-slip boundary conditions and these two experiments resulted in significant differences in velocity and stress.

Page 9 line 235: “Figure 9 clearly illustrates that water pressure at the calving front exerts a stabilizing effect on the calving front by both lowering the stresses and decreasing the distance from the calving front at which the stress maximum is located.” This is exactly what we argued in Bassis and Walker (2012), although our analysis was less numerically sophisticated.

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Page 9, line 240: Now I think I have lost the thread. Why consider the Hayhurst criterion at all if the maximum principle stress is all that is going to be used? Is the reason the Hayhurst stress is going to be abandoned because all of the invariant combinations give similar answer? This could use a bit more motivation.

References:

Bassis, J.N.. (2010). The statistical physics of iceberg calving and the emergence of universal calving laws. *Journal of Glaciology*. 57. 10.3189/002214311795306745.

Ma, Y., C. S. Tripathy, and J. N. Bassis (2017), Bounds on the calving cliff height of marine terminating glaciers, *Geophys. Res. Lett.*, 44, 1369–1375, doi:10.1002/2016GL071560.

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

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