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Interactive comment on “Combining damage and fracture mechanics to model calving” *by* J. Krug et al.

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This manuscript described a novel combination of damage mechanics and fracture mechanics to predict the timing and size of iceberg calving events. The model is applied to Helheim Glacier and the authors find that their model is able to predict irregular calving events with characteristic size comparable to the ice thickness. The calving model the authors adopt is similar to the calving criterion proposed by Benn et al., (2007) in that calving occurs when a surface crevasse penetrates to the water line. The improvements made by the authors of this manuscript consist of (1) using a full Stokes model to (hopefully) more accurately simulate the stress distribution within the glacier and hence the depth of fractures and (2) using damage mechanics to estimate the distribution of starter crack sizes which is then fed into a quasi-analytic LEFM solution.

Overall, this manuscript describes an interesting result and merits publication. The manuscript could use a thorough reading by a copy editor to avoid some awkward phrases and terminology. I point out a few of these in the miscellaneous comments, but I primarily focus my review on technical aspects of the manuscript. These are roughly grouped into comments about (1) the model formulation and (2) the dynamics of the model.

1. Model formulation

One typically combines damage mechanics and fracture mechanics using a method that is nearly inverse to what is proposed here. What I have seen done in the literature is that one uses fracture mechanics to represent the crack surface and damage mechanics to represent the diffuse fracturing that occurs near the process zone where decohesion occurs. In contrast, the authors are instead using damage mechanics to assess the distribution of pre-existing crevasse sizes and then using this as the input to a LEFM calculation. This is an interesting idea that I have not seen explored before. One question it raises, however, is that because LEFM requires (infinitely) sharp starter cracks to seed fracture, it is unclear how fractures that initiated upstream and advect toward the calving front can remain sharp for so long? I would have thought that sharp starter cracks need to be locally generated because the characteristic Maxwell relaxation time is typically much less than a day. (I know the relaxation time depends on stress, because the viscosity depends on stress, but one can do a back-of-the-envelope calculation based on characteristic values.) Do the authors have a physical mechanism in mind for how the damage remains sharp as it advects or is this purely phenomenological at this stage?

Equation(11): The authors argue that the von Mises and Hayhurst criteria are not appropriate for ice and instead adopt a criterion for fracture that assumes damage accumulates when the largest principal stress exceeds some threshold. However, my understanding of the Hayhurst criterion is that the Hayhurst criterion is merely a linear combination of the three invariants: pressure, second stress invariant (von Mises

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stress) and largest principal stress. Hence, it would appear as though the criterion posed here is merely a special case of the Hayhurst criterion in which two of the proportionality constants are set to zero. More interesting is that this assumption physically asserts that damage accumulation only depends on the uni-axial stress state defined by the largest principal stress. If the authors are correct, the presence or absence of a stress in a direction that is perpendicular to the largest tensile stress makes no difference to damage accumulation. This is very different from the way we think of elastic damage accumulation and how we think about the effective rheology of ice, which do depend on the tri-axial state of loading of the glacier.

In the past, when I have used complex formulation of damage mechanics, what I ended up determining is that damage accumulates to the point in the ice where the tensile stress ceases to exceed the prescribed threshold. Damage mechanics does tell you how long this takes and allows you to make some pretty pictures of how this happens, but the end result (for my calculations) has been that you get the same simple answer that that the old Nye model predicts. Is this what the authors get here as well? Suppose you assume that damage always accumulates to its maximum depth instantaneously. Do you still get irregular calving events or do you get a constant terminus position?

Assuming a temperature of -4.6 Degrees Celcius has interesting thermodynamic consequences for the model because it implies that any water within crevasses will freeze shut. If sufficient water refreezes this will raise the temperature of the ice to the pressure melting point, which contradicts the assumed temperature profile. This is an interesting point because it seems to put some thermodynamic limits on the criterion you use for calving in that there can only be a permanent connection between the calving front and ocean if the ice is nearly temperate. This probably doesn't make a difference for the model considered here, but it might be worth mentioning that the constant temperature case considered may be inconsistent with the calving criterion.

Page 1640: The authors use a normal random distribution to describe material heterogeneity. I don't think I understand what the authors mean by normal distribution

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because a Gaussian normal distribution is defined between [-infinity, infinity] and would include the possibility that damage could be negative along with the more remote possibility that damage is greater than one. It seems like the authors need to impose a distribution that is only defined in the interval [0,1), but that is not what I normally (pardon the pun) think of as a normal distribution.

Local damage evolution often exhibits a mesh dependence. The cure for this usually to regularize the damage law so that it is slightly non-local. This allows for a non-zero energy dissipation in creating new fracture area and removes the mesh dependency. Was this done here? Are there sensitivities to mesh size in the results?

2. Model dynamics

Other reviewers have commented on this so I will not belabor the point, but tuning a model to reproduce observations does not prove that the model is correct. The fact that the model can be tuned to match observations, however, at least makes the model plausible. What interests me more is whether the set of tuning parameters used here is approximately correct for *other* glaciers. If we need a new set of tuning parameters for each glacier modeled then the model is of limited use for predictions, but if the authors can show that similar model parameters are roughly appropriate for different glaciers than this is a much stronger result. I suspect this is a bit much to ask for this paper, but the authors might want to consider some idealized geometry experiments or back-of-the-envelope calculations to see if much thinner glaciers exhibit plausible behavior or extend off to infinity.

What interests me most about the model is that the authors can reproduce irregular calving events, similar to what is observed. My understanding is that this originates from the interplay between the time scale of damage accumulation and the time scale of ice advection. That is an interesting result, which indicates that the rate at which damage accumulates (or crevasses deepen) is an important control on the calving cycle. I think that this interplay merits an extra paragraph in the discussion section explaining why this arises in the model because it may actually end up being a strong constraint on the dynamics required of this and other models.

A related question is if you perform the simulations long enough do you get a quasi-steady-state with regular calving events? From the plots, it looks like the glacier is in a transient state, but I wonder if you run the model long enough if it settles down to something that is more steady-state like. Also, does the glacier ever form a floating tongue? Is this permitted by the model?

The final provocative question I would pose to the authors is whether the model can be used to define a set of field or remote measurements or even laboratory measurements that can be used to constrain the model?

3. Miscellaneous comments

Figure 7 is hard to digest. It would be helpful to readers if the authors could shade and label regions to indicate the stable and unstable parameter space.

Page 1640, line 5: Pure shear can be decomposed into principal stresses and this, presumably, can become damaged using the tensile stress criterion proposed. I think what the authors are saying is that they only allow for tensile failure and do not parameterize shear or compressive failure mechanisms?

I suspect that basal crevasses might also be important to the calving cycle. Have the authors considered if these can be added to the model? This point might be worth returning to in a discussion section which reminds readers of some of the model limitations and points towards places that improvements can be made.

Page 1641, line 15ish: The authors assert that calving events are "triggered by rapid propagation of preexisting fractures" and the speed of fracture propagation approaches the speed of sound. This seems like a reasonable statement and to a certain extent has to be true because calving does produce seismicity and we measure that seismicity. However, I do wonder if observations fully support this view point. We have

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measured rift propagation speeds on ice shelves and the rate of propagation is typically much less than 10 m/day, which is **much** smaller than the speed of sound. Furthermore, observations of iceberg calving events from Greenland Glaciers indicate that berg separation can take much longer than tens of minutes, which is a less direct measure of fracture propagation but might imply a small rupture velocity. All of this together makes me question if we really know that fracture propagation during calving events really occurs at the speed of sound?

Page 1642, line 25ish: “This formula *lays” “. Replace *lays* with relies.

Section 3.2 “Ox” and “Oz”d? Should this be “x-direction” and “z-direction”?

Interactive comment on The Cryosphere Discuss., 8, 1631, 2014.

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