

Reply to Referee # 2

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We greatly thank Jeremy Bassis for his acceptance in reviewing our manuscript. We especially thanks him for the interest in our conclusions, especially regarding irregular calving event. These comments triggered an interesting scientific discussion. The comments which deserved a specific discussion were integrated in the revised version of the manuscript and highlighted in red.

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?

It is obvious that the tip of a crevasse which initiated upstream tends to become less sharp as it is transported downstream, with the ice flow. In such a case, a further fracture propagation would be harder to initiate.

This aspect is clearly a limitation in the model presented here, and we currently have not try to incorporate the consequences of such a mechanism in the model. One possibility to overcome our simplification could be to increase in a way the ice toughness to take account for the smoothing of the crack tip.

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 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.

We answered a similar comment from referee #1 (see above). The referee is correct in saying that our very simple damage criterion can be understood as a special case of Hayhurst. He is also right saying that we do not take into account the degree of triaxiality in our criterion. This is because we consider that (sub-critical) crevasse opening, which we describe through damage accumulation here, is driven only by the maximum tensile stress. Triaxiality should be taken into account in ductile failure to describe growth of cavities (this is why Hayhurst's criterion has been proposed in this context), but we think that such failure mechanism is irrelevant for ice.

However, we acknowledge the fact that our criterion is very simple and can only describe damage through crevasse opening in mode I. In this respect, it is unable to describe *e.g.* damage under shear. It would be possible in future work to elaborate on this initial formulation to take into account more complex situations.

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?

The idea of using damage is to keep a memory of the stress condition that an ice particle has undergone. Damaging of the ice at a given time and a given place changes the viscosity and alters the surface velocity, leading to further changes in geometry, stress field, and rate of damaging. This feedback is a reason why the damage does not accumulate everywhere in the same way, and so lead to different calving event sizes.

Considering the damage accumulation as instantaneous would probably lead to a more stable front position than using damage slow evolution. However, we did not realize this study for the moment, but this would be a very interesting point to focus on for further inter-comparisons between calving models.

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.

We are deeply aware of the misrepresentation of reality in the forcing we prescribed. The idea here was to have a compromise between reducing the level of complexity in order to understand more easily the model response, and improve the complexity enough to tune it in a reliable way.

However, what the referee highlights deserved a specific comment in the revised manuscript, in Sect. 3.1.

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 because a Gaussian normal distribution is defined between $[-\infty, \infty]$ 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.

Precisely, we defined a distribution following a normal law, which is only positive, and is defined such that the maximal extent of $\delta\sigma_{th}$ roughly accounts for $\pm 20\%$ of the value of $\overline{\sigma_{th}}$. Ultimately, as pointed out by referee #2, there is a probability that $\sigma_{th} < 0$. In this case, we imposed an arbitrary bound which sets σ_{th} to 0. On the other hand, even if σ_{th} reaches very high values, the damage criterion χ cannot become lower than 0, as written in Eq. (11). Consequently, the damage is bounded too. Additionally, we imposed a numerical bound of 0.7 on the value of D , in order to prevent computation degeneration.

As a consequence the damage can never become negative, nor higher than 1.

However, thanks to this remark, we noticed a typo in the writing of Eq. 11, which should read:

$$\chi(\sigma_I, \sigma_{th}, D) = \max\left\{0, \frac{\sigma_I}{(1-D)} - \sigma_{th}\right\}$$

with $\sigma_{th} = \overline{\sigma_{th}} \pm \delta\sigma_{th}$, instead of

$$\chi(\sigma_I, \sigma_{th}, D) = \max\left\{0, \frac{\sigma_I}{(1-D)} - \overline{\sigma_{th}}\right\}$$

This correction means that the threshold entering Eq. 11 is the noised threshold, not the averaged one.

Modifications have been applied accordingly in the manuscript.

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?

We answered a similar question from referee #1, regarding the mesh dependency within the context of LEFM calculation, but most of the difficulties arise from damage growth and advection. As we changed the remeshing procedure, we performed the calculation, as well as a wide series of sensitivity tests regarding the mesh size and the time step. The details are given in the response to referee #1. To summarize, we observed that below a given mesh size, the variation in front behaviour becomes negligible. We compute again our simulation, using a 0.125 days time step over a 10 years period, and the result remains similar, despite a slight change in the damage parameters range validity.

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.

The question of the ability of the set of parameters to apply to other glaciers is the key-point of the robustness of our model. The sensitivity study which is carried out here relies on calibrating the damage parameters (B , σ_{th} , D_c) in order to reproduce consistent behaviour of Helheim glacier, and is not necessarily appropriate to carry out such experiments.

However, we are running simulations on synthetical geometries, and the observed behaviour remains consistent *i.e.* the calving does also happen for thinner and thicker glaciers, with similar damage parameters (Krug et al., 2014). It should be emphasized, nevertheless, that the surface topography arising from bedrock topography is essential to generate damage at surface. Thus, for some idealized geometries flowing on very smooth bedrocks, the damage parameters should be slightly adjusted to facilitate damaging.

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.

Damage increases in the ice where the stress state is larger than σ_{th} (*i.e.* where the surface topography exhibits tensile stress, usually over the bumps). It reduces the viscosity of the ice, allowing faster flow, lowers the stress field accordingly and the geometry adjusts to reduce the stress level. Additionally to this process, as the damage is advected, it reaches conditions where the crevasses can trigger calving events (probably because of bending stresses at the front or processes referred to as second-order processes in Benn et al. (2007)). The consequence of calving event is an immediate increase in the stress field in the vicinity of the front, and a rapid change of geometry in the area. This goes along with subsequent damage increase where the geometry readjusts, and may trigger a cascade of calving events leading to an important (cumulative) retreat of the glacier front over a limited time scale (a few days), until the front reaches a position where the damage is too low to initiate calving.

To sustain our claim, we tested the sensitivity of the front position to the internal variability, and we computed the Fourier spectrum of the terminus position (Fast Fourier Transform, Fig. 1), for 8 simulations (grey lines) and the corresponding mean (black line). We observed a peak for a period (500 ~ 600 days) approximately equal to the time necessary for the ice to be advected between the two main bumps visible in Fig. 8 a in the manuscript. This period is likely related to the “cascade” mechanism described above. This feature remains visible for longer runs (up to ~20 years) and different sets of parameters (B , σ_{th} , D_c).

According to our simulations, it looks that the changes in the surface geometry constrained by bedrock topography can explain a part of the irregular calving events. However, further simulations on different geometries would be required to confirm this hypothesis. But at least, this is a part of the reason why we suggest that the chronology of calving and front position may be related to the internal glacier dynamics.

A discussion regarding this feature was added in the revised version of the manuscript.

A related question is if you perform the simulations long enough do you get a quasisteady-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 new simulations were conducted for 10 years, which is sufficient to reach a quasisteady-state (See Fig. 6 in the revised version of the manuscript). Some parameters sets lead to a steady state front position, characterized by steady calving events of similar amplitude. Others lead to a steady advance or retreat of the glacier terminus. In both cases, the model allows

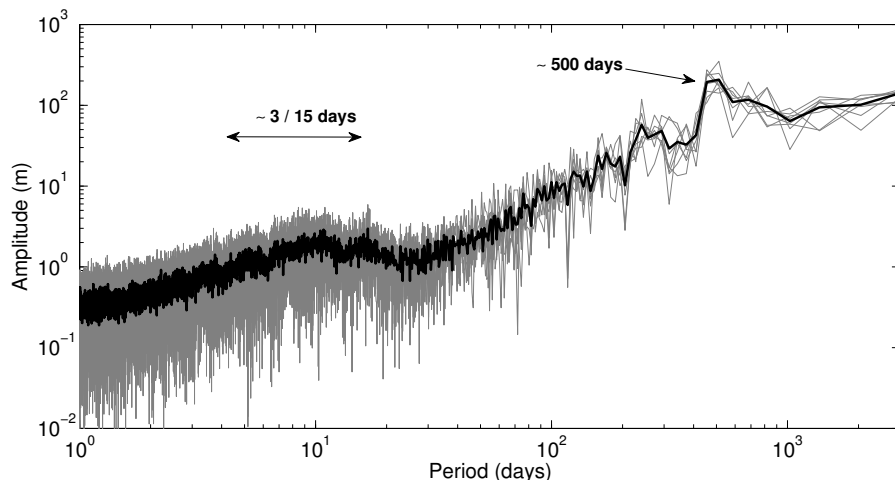


Figure 1: Frequency response of the calving front for the set of parameters ($\overline{\sigma_{th}} = 0.11$ MPa, $B = 1.30$ MPa $^{-1}$, and $D_c = 0.50$), for a simulated time of 10 years, with 8 different realizations of local disorder. Grey lines represent the frequency spectrum for each of the 8 simulations, and the black thick line represent the mean.

for the formation of a floating tongue where the bedrock get deep enough.

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?

At its current stage of development, the model is limited by the level of physics which is incorporated in: as mentioned before, several mechanisms, such as the basal crevasses or the water-filled crevasses, are not implemented yet. Additionally, the next outcome is to study the response to different external forcings, such as ice mélange, the undercutting, or variation in the basal sliding. Thus, many tasks remain to be accomplished before we would ask for specific observational data for damage model constrain

However, the best dataset that would be needed to better constrain the model would probably be robust statistics concerning the size and occurrence of calving events, as this information is what the calving law should primarily produce. As mentioned by referee #1, this distribution of calving event sizes is poorly observed and could become a benchmark for future models developments and/or intercomparison.

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.

It came from the fact that the 3D hypercube was represented on a plan view. However, when computing the simulations again, we reduced the total amount of simulation to 48, and we drawn it on a 3D figure, which is much clear now.

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?

Yes, that is what we meant. Damaging is allowed under shearing, accounting that shearing can be represented through tension into the principal directions. We added a modification to precise our meaning in Sect. 2.2.1.

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.

This question were also raised by Doug Benn and the referee #1. Following their recommendation, we added a supplementary section including further possible improvements, such as the crevasse shielding, the water-filled crevasses and the basal crevasses.

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 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?

It is clear that currently, we do not understand all the processes that happen in the iceberg formation. Additionnaly, our model does not deal with some specific processes such as mechanical fatigue, for example, or an incomplete fracture propagation, and the way we treat crevasse propagation remains simplified.

However, regarding rift propagation, we definitely observe opening rate of less than 10 m day⁻¹, but this is a daily averaged rate, and we don't know if is the rift opens continuously, or through

successive critical fracture propagation within a few meters (whatever recorded thanks to seismic measurements). In the latter case, the observation does not contradict our statement of considering LEFM to represent fracture propagation.

Considering the iceberg separation, observed time of several minutes cannot be ignored, of course. However, we may also suspect that some additional processes can slow down the berg separation from the glacier front. For example, Amundson et al. (2010) stated that the friction of the glacier base on the bedrock, or the presence of an ice mélange layer can prevent iceberg from rotating far from the glacier front, even if a full thickness fracture has occurred. This highlights the idea that the fracture propagation and the berg separation from the terminus may rely on different processes. The latter case is not considered in our model.

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

Done.

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

Yes.

References

- Amundson, J. M., Fahnestock, M., Truffer, M., Brown, J., Lüthi, M. P., and Motyka, R. J.: Ice mélange dynamics and implications for terminus stability, Jakobshavn Isbræ, Greenland, *Journal of Geophysical Research*, 115, F01005, 2010.
- Benn, D. I., Warren, C. R., and Mottram, R. H.: Calving processes and the dynamics of calving glaciers, *Earth-Science Reviews*, 82, 143–179, 2007.
- Krug, J., Weiss, J., Gagliardini, O., and Durand, G.: Investigating the impact of ice mélange on glacier dynamics, in: IGS Summer Meeting - 70A1060, 2014.