Reply

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1 Chris Borstad Referee Comment

Before all, we would like to thanks Chris Borstad for taking time to read carefully our article and making significant suggestions to improve our manuscript. We took them into account, and our point-by-point answer is given below, in red. When a remark deserved a specific revision in the manuscript, we modified it accordingly. Changes have been highlighted in red.

Specific comments

My biggest potential concern surrounds the issue of damage and hydrostatic pressure in the ice. As such, I think that some explicit mention needs to be made of how the pressure term is being handled in the ice flow model. This is because the effective stress $\tilde{\sigma} = \sigma/(1-D)$ should be substituted for the Cauchy stress (not just the deviatoric stress) in the Stokes equations, with the result that the damage scalar should be mapped onto the pressure term as well. This can take different forms depending on whether an approximation to the Stokes equations is adopted, but damage should affect the pressure in addition to the viscosity, in the form of something like $\tilde{p} = p/(1-D)$, as pointed out by Pralong and Funk (2005). It's not clear whether you accounted for this, or whether you only modified the viscosity term (Section 2.2.2). For that matter, it's unclear whether Pralong and Funk (2005) accounted for this dependence either (or whether it drops out somehow), as their Equation 26 appears to contain the original (unmodified) pressure rather than the effective (damage-dependent) pressure. Maybe I'm missing something here, but I'm concerned that perhaps the pressure is being treated incorrectly.

It appears that there was a lack of accuracy in the description of how we considered the ice flow to be affected by the damage. Here, we consider the effect of damage on viscoplastic flow only (Glen's flow law). This latter involves the deviatoric part of the Cauchy stress tensor $\boldsymbol{S}$ only. The cryostatic pressure does not play a role on the viscoplastic flow, and thus should not be considered in the effective stress formulation. If we were considering elastic rheology as well, then, the damage should, in principle, affect the non-deviatoric part of the Cauchy stress tensor as well, which is not the case here.

As a consequence, the right expression for the effective Cauchy stress tensor is as following:

$$\tilde{\boldsymbol{S}} = \frac{\boldsymbol{S}}{(1-D)}$$

This formulation was the one implemented in our framework, but we made a typographical error in the manuscript. We modified the article accordingly, and added a supplementary explanation in order to justify our choice (Sect. 2.2, Eq. (8) and following explanation).

Another issue that relates more to the discussion and context of this work is the description of damage mechanics generally. It is incorrect (or at best misleading) to state that damage mechanics is only applicable up to the point where a macroscopic fracture first forms. Though the paper does not exactly say this, the reader might imply that damage mechanics only applies to the realm of slow, sub-critical crack growth. Damage mechanics is actually a much more general and versatile framework; it can be applied over viscous and elastic timescales, and is widely used to model both the initiation of a macroscopic fracture and the subsequent propagation of this fracture (for a review, see Bazant and Jirasek, 2002). Damage mechanics is especially appropriate for modeling fracture propagation in heterogeneous materials (such as glacier ice), for which the crack "tip" may be ill-defined due to an inherent zone of microcracking ahead of the traction-free crack. Damage as a notion of the "smeared" influence of fractures is just as appropriate for this type of fracture propagation as it is for the coalescence of a fracture to begin with. A great example of this in a glaciological context comes from the seismic data of Bassis et al. (2007), which show that a propagating rift tip is surrounded by a diffuse zone of fracturing. In a modeling context analogous to crevasse propagation, Borstad and McClung (2011) used damage mechanics to simulate elastic tensile fracture propagation in cohesive snow. My point here is to caution the authors against defining damage mechanics narrowly, especially since it is still a very new concept in the field of glaciology and might find applicability to a range of problems over a range of timescales.

The idea behind continuous damage mechanics is the use of a representative volume element, larger that the characteristic length of micro defects, in order to average the effects of these micro-defects at a larger scale. Damage mechanics has been used to describe the "fracture process zone" (FPZ) associated to the non-brittle (sub-critical) fracture propagation in ductile or highly heterogeneous materials (such as some composites). Indeed, in these cases, the propagation of the main fracture can take place through the nucleation of microcracks, microbreakings, or voids, ahead of the main crack tip, in the FPZ, and through the coalescence between the main crack and those defects. In this case, the damage process in the FPZ can be tentatively described within a damage mechanics framework. However, in case of ice, we have no evidence of such ductile crack growth at the lab scale, as ice remains brittle even close to the melting point. In the field, for temperate ice, the question remains open for sub-critical crevasse growth.

This is why in this study we consider damage to describe the effect of a field of crevasses on ice flow, and not to describe the propagation of a single crevasse. Thus, our treatment of brittle (critical) crevasse propagation fully relies on linear elastic fracture mechanics, which is different from the crack-growth process described here.

However, as requested by referee #1, a short explanation of other uses of CDM was included in the revised manuscript.

Finally, how would the role of crack tip shielding influence the calving results? The stress intensity factor is calculated for the case of a lone fracture, or at least a fracture distant from any neighbors. Yet this stress intensity factor is calculated everywhere that the damage reaches the critical level, as if a field of crevasses existed (which is more likely to be the case). In a field of closely-spaced crevasses, the stress intensity factor at each crack tip is reduced due to the influence of stress shielding by neighboring cracks. This should make it more difficult for a single fracture in a field of crevasses to propagate down to sea level. There are a number of ways that this could be represented or accounted for in your model, and this would have implications for the appropriate values of the other model parameters necessary to pass the "sanity check" of your model. It might be worth at least discussing this issue in the text.

For a crevasse to propagate in a field of closely-spaced crevasses, tensile stress in the ice must be higher than for a lonely crevasse, due to the release of stress arising from the presence of multiple crevasses. Currently, this aspect is not taken into account in our simulations. However, the introduction of this feature in our framework would not be necessarily complicated: for example, van der Veen (1998) proposed a formulation which takes into account the distance between neighboring crevasse l and the crevasse depth d, reading;

$$K_I = D(L) R_{xx} \sqrt{(\pi d L)}, \qquad L = \frac{l}{l+d}$$

where R_{xx} is the tensile stress, d is the crevasse depth and $D(L)$ is a weight function which depends on L. This parameterization could then be implemented in Eq. (16). This add-on would probably have the effect of delaying the time and the position at which calving occurs, resulting in later and maybe smaller calving events.

However, the parameter l is unknown, and is not determined by our model, which only gives a contour for a crevasse field and an estimate of their depth. Thus, the implementation would be straightforward, as soon as the crevasse spacing can be estimated from observation or theoretical considerations.

At last, this suggestion is interesting, and a paragraph was added in the manuscript, summarizing potential improvements and respective requirements which could be implemented in the current model (as suggested by referees #1 and #2).

Line-by-line Comments

· p. 1632, line 21 (and elsewhere): Throughout the manuscript the word "important" is used in places where I think you are referring to a quantitative magnitude, or something being "large" rather than qualitatively important in the sense of being meaningful or significant. You might want to look at where you are using this word to make sure the reader is not confused or misled.

Done.

· p. 1633, line 28: the van der Veen references are formally about the propagation of single surface or basal crevasses, and are not strictly about calving events

Done.

· p. 1637, lines 18-20: See specific comment above about the broader applicability of damage mechanics that can also include macroscopic fracture propagation. In other words, damage does not have to be limited to long-timescale viscous deformation, nor does it cease to become applicable once a macroscopic crack forms. I have no problem with the way you use damage mechanics in this study, but I think that readers should know that there is a broader context in which damage mechanics can also be applied.

Following the answer given in the "Specific Comment" section above, we modified the manuscript to explain the context on which CDM should be applied when studying ice.

· p. 1638, lines 5-7: "Damage" is not mentioned anywhere in the work of Rist et al. (1999), moreover I don't see how your assertion here is supported by this reference.

This remark is absolutely right. We apologize for this mistake. The idea is that currently, we do not have reliable observation and/or data to decide if such anisotropy should be introduced in D. That is why we decided to keep it as simple as possible, and we rely on previous work from Pralong and Funk (2005), who remind that considering damage as isotropic is a common assumption when dealing with ice (See revised version of the manuscript, Sect. 2.2).

·p. 1638, line 17: does not the effective stress enter the Stokes equations in general, and not just the rheological law of Eq. 3?

It does, via the expression of the deviatoric part of the Cauchy stress tensor. The formulation has been clarified in the manuscript.

· p. 1638, line 26: I think that the use of the variable "B" for the damage enhancement factor is a bit unfortunate, since some ice flow models use "B" to represent the ice rigidity (related to the rate factor), especially since damage and ice rigidity are often written next to each other (e.g. $(1 - D)B$ in Borstad et al., 2013, also in this journal). Is there another variable that could be used here?

The choice for letter "B" comes from Pralong and Funk (2005). Morevover, in the current article, there is no possible confusion between the damage enhancement factor B, and the fluidity parameter A. However, we thank the referee for highlighting this point and if the editor advices for changes in the name of the variable, we will be happy to find another name for it.

· p. 1639, line 19: do you mean "splaying" crevasses?

Yes we do. The manuscript was modified.

· p. 1639, line 25: "envelope"

Done.

·p. 1639: for a flowline model, a maximum principal stress criterion seems like a good choice. However, a multiaxial criterion, such as von Mises, could still be used to represent the scalar level of stress for which fractures first appear (indeed the von Mises criterion reduces to the maximum principal stress criterion for a state of uniaxial tension). Vaughan (1993) found that a von Mises criterion corresponded well with the pattern of surface crevasse occurrence on many glaciers. As Rist et al. (1999) points out, crevasses indeed tend to open normal to the direction of maximum tensile stress even if the actual state of stress is multiaxial (you seem to be implying on line 18 that you wish to model crevasses opening under uniaxial tensile stress). Moreover, the fact that von Mises, or any other criterion, is often used for plasticity is irrelevant for whether it is physically applicable in another context.

We should have justify more deeply our choice for not considering the von Mises nor the Hayhurst criterion.

The von Mises criterion is a plasticity criterion, which is suited to describe the purely plastic yielding of ductile materials. This is why only deviatoric stresses enter the formulation. Von Mises can reasonably describe failure under tensile stress states if failure is purely ductile. Ice remains brittle even at high temperature, and so the von Mises criterion is not suited to describe brittle damage and fracture. Another inconsistency of Von Mises regarding failure is that it is symmetric in tension and compression.

The Hayhurst criterion is more complex, as it involves the maximum principal stress, the hydrostatic pressure, and the Von Mises stress invariant. Hence, the referee is right when he says that our damage criterion, which is based only on the maximum principal stress, is partly included in it. However, the Hayhurst criterion was designed to describe creep damage in ductile materials, and it allows damage under uniaxial compression. For reasons similar to those given above about von Mises, we think that such criterion is not suited to describe crevasse opening under tension.

However, we took these comment into account and adapted the manuscript to explain our criteria choice.

· p. 1641, line 10: similar comment, the ice does not need to undergo pure (uniaxial) tension for damage to evolve, it just needs the maximum principal stress (in a multiaxial stress state) to exceed the threshold stress.

Absolutely, that is what we meant, but the formulation was inaccurate. It has been modified following this comment.

·p. 1641, lines 17-18: a reference is needed here to support the claim about calving timescales approaching the speed of sound.

This assertion is supported by the fact that icequakes were recorded during crevasse formation and calving events (for example Walter et al. (2012)). But the idea behind is to say that the triggering of a calving event is much shorter compared to the timescales relevant for damage growth.

·p. 1643, lines 2-5: is it correct that the weight function method was calculated using vertical coordinates corresponding to the finite element mesh? If your mesh had a vertical resolution of 5 m near the surface, and if the typical depth of the critical damage contour was 5-15 m, then does this mean that only 1-3 vertices was used to calculate the weight function? This seems a bit coarse, and I would doubt that the results would be mesh-independent.

For a given set of parameters (σ_{th}, B, and D_c), a threefold increase of the number of nodes did not modify the calving bevahiour of the glacier front (*i.e.* the three areas in Fig. 7 in the original version of the manuscript) during the studying period ([0;1465] days).

However, since the paper has been reviewed, new developments have been implemented regarding the remeshing procedure, in order to improve the pressrvation of the mesh specificities at each calving event. This allowed us to carry out longer simulations without degenerating the mesh. However, it had consequences regarding damage advection, and then required a new sensitivity analysis.

As mentioned in the manuscript, the model is self working: the first calving event changes the glacier geometry and sets a new specific path for its evolution. Thus, comparing front position of different simulations at each time step is clearly inappropriate. Therefore, a comparison must be realized considering the global behaviour. This is why we used the sanity check described in the manuscript to compare pluri-annual glacier behaviour.

As referees #1 and #2 suggested, we carried out new experiments. on three different meshes.

Mesh1: 7371 nodes, left to right refinement from 150 m to 33 m, bottom to top from 100 m to 5 m

Mesh2: 10881 nodes, left to right refinement same as Mesh1, bottom to top from 68 m to 3.3 m

Mesh3: 16146 nodes, left to right refinement same as Mesh1, bottom to top from 46 m to 2.1 m

Additionnally, in order to limit mesh dependency in the along-flow direction, we implemented an horizontal interpolation: once the damage contour is computed, LEFM criteria are evaluated at the node validating the damage criteria, as well as at the one before. The initiation and arrest criteria are then horizontally interpolated, allowing calving to occur between these two nodes.

Parameters ranges had to be adjusted compared to those described in the original version of the manuscript. For these new experiments, σ_{th} ranges in [0.01 ; 0.2] MPa and B ranges in [0.5 ; 2.0] MPa^{-1}. 16 couples of parameters were randomly sampled within this new space. For each of them, three values of D_c were tested (0.4, 0.5, 0.6). So far, 48 simulation were carried out on the three meshes described above. The simulations lasted 10 years. The glacier behaviour, as defined from our sanity check, appears to converge with increasing refinement. From Mesh1 to Mesh2, 5 damage parameter sets switched from "insane" to "sane" (or the reverse), while only 2 of them changed from Mesh2 to Mesh3.

Increasing refinement makes calving easier. However, it does not significantly modify the calving event size distribution and frequency. Consequently, Mesh2 was used in the revised version of the manuscript. The manuscript was modified accordingly, including more information regarding mesh size and time step size sensitivity

·p. 1643, line 15: "notched"

Done.

· p. 1645, line 9: you might substitute "becomes filled with water" for "is filled by water" as the latter makes it sound like the crevasse is already filled with water before it propagates down to sea level.

Done.

· p. 1646, line 21: why not specify a vertical temperature profile? Wouldn't that be more physically realistic, since the temperature at the base should be warmer than the temperature at the surface?

This suggestion meets the one from the referee #2. 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 obtain a reliable behavior when compared to observations. However, what the referee highlights deserves a specific comment in the manuscript, and has been done in the revised version in Sect. 3.1.

· Equation 17: can you be a bit more specific about how and where this friction coefficient is applied in the model?

The lateral friction coefficient k depends on rheological parameters, as well as on the width of the fjord. The fjord width varies from 17 km upstream to around 6 km at the initial front position. This parametrization enters as an additional external force $\boldsymbol{f}$ in the momentum equation, reading:

$$div(\boldsymbol{\sigma}) + \rho_i \mathbf{g} + \boldsymbol{f} = 0, \quad (1)$$

where

$$\boldsymbol{f} = -k|\boldsymbol{u}|^{m_{lr}} - \boldsymbol{u}, \quad (2)$$

where $m_{lr} = 1/3$.
In our simulation, this lateral resistance is applied along the whole glacier length, and over the whole lateral surface. This simplification suffers from a lack of realism, as it does not incorporate some three dimensional aspects, especially the effect of the tributary glacier that merges the principal stream around the middle part of the geometry.

Some explanation was a added in the revised version of the manuscript

· What is the time step size in the model? Do the results (calving event size or frequency) have any dependence on the step size?

The time step used in the initial version of the manuscript was 1 day.

As stated above, major changes were applied regarding the remeshing procedure. New sensitivity tests have been carried out. The chosen timestep in the revised version of the manuscript is 0.125 day. This value satisfies the Courant-Friedrichs-Lewy number, which furnished a severe constrain for the damage advection. Below 0.125 day, no major deviation appears in the evolution of the front position, nor in the calving event size distribution or frequency. An example is given on Fig. 1

Reducing the time step decreases the number of large calving events, but it does not heavily affect their mean size distribution and frequency.

The revised manuscript was modified to make this distinction appears.

· p. 1648, lines 23-24: the stress threshold seems like it should be physically related to (if not equivalent to) the tensile strength of snow, firn, or ice, depending on the nature of the glacier surface. Pralong and Funk (2005) considered hanging alpine glaciers, for which the tensile strength of the snow or firn at the surface, which is likely to be in the range of 10-50 kPa (Borstad, 2011), would be appropriate for the stress threshold. Vaughan (1993) inferred tensile strength values in the range 100-400 kPa from analyzing strain rate data in the vicinity of crevasses. Since an approximate diagonal in Figure 7 defines a set of acceptable model parameters (threshold stress and damage enhancement factor), it might be possible to

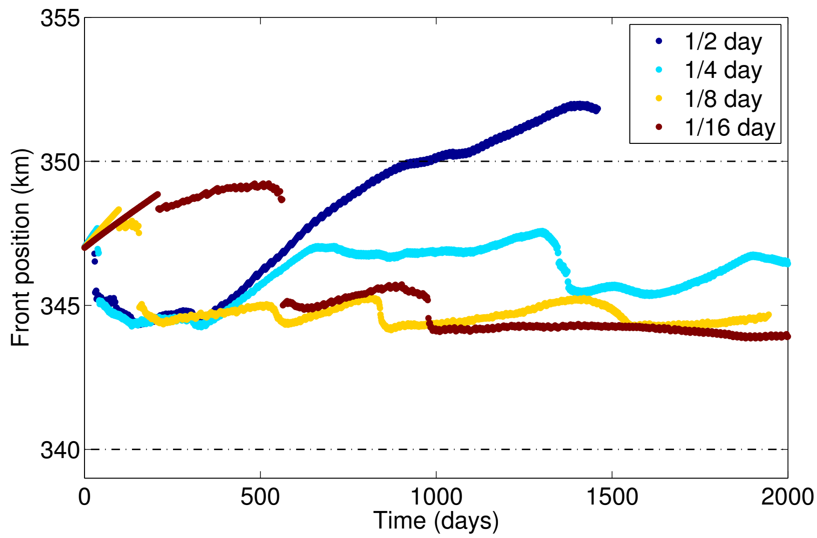

Figure 1: Time step size sensitivity for the simulation with corresponding damage parameters set ($\overline{\sigma_{th}} = 0.11$ MPa, $B = 1.30$ MPa^{-1}, and $D_c = 0.50$)

further constrain these parameters from a knowledge of the properties of the firn layer where the fractures first originate. Do you have any information about the depth and density of the firn layer for Helheim? Even seasonal snow can have tensile strength reaching 0.1 MPa, so it might be possible to further constrain your considered range of threshold stresses to something like 0.1-0.2 MPa.

We currently do not have information about the firn density at Helheim Glacier. Actually, the underlying original idea was to use a broad range for σ_{th} to see whether abnormally small values lead to a realistic behaviour or not. However, when resampling our space of parameters, we noted that the range for σ_{th} for which the sanity check is the most satisfied is [0.01 ; 0.15] MPa, which is partly included in the range the referee suggested. We added the information regarding measurements and potential constraint of the σ_{th} damage parameter in the revised version of the manuscript.

· p. 1649, lines 5-6: I'm confused by this statement, can you clarify? Are you trying to keep $B\chi$ within some range?

We do not. The only range is applied on B.

When the damage criterion χ is positive, the source term of the advection equation is set to $B\chi$. The corresponding damage releases the level of stress in the ice, as ice can flow more

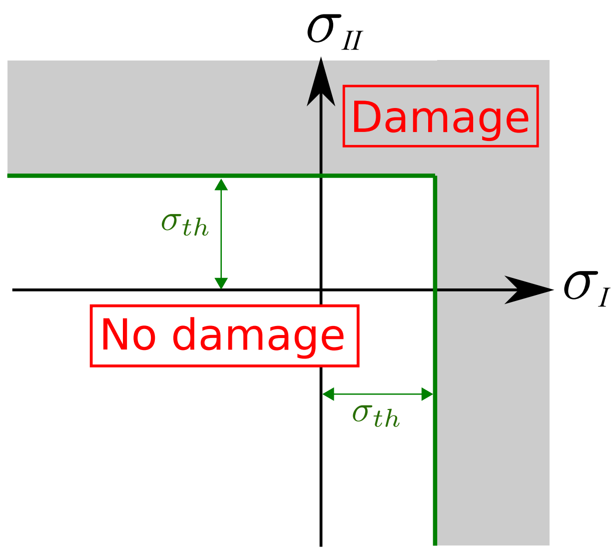

Figure 2: Damage envelope in the space of principal stresses. σ_I and σ_{II} respectively represent the first principal stress and the second principal stress, and σ_{th} is the stress threshold. The shaded area corresponds to the stress conditions under which damage occurs.

easily. In reality, this process is happening continuously, in a way such that the level of stress cannot exceed the edge of the envelope defined in green line in Fig. 2.

However, as our model deals with a finite time step size, after each time step, the stress is located in the "damaging" area (grey-shaded area in Fig. 2). The role of damage is then to "push" the level of stress back to the edge of the envelope: the rate of this stress displacement is set by the value of B. Theoretically, we should prescribe B such as the stress do not exceed the failure envelope.

Ultimately, this consideration may be used to constrain B with a better accuracy, but we did not prescribed any constrain on the product $B\chi$. This explanation was added in the revised manuscript, in Sect 3.3.1.

·p. 1650, line 20: The "steady" advance...

Done

·p. 1651, line 20: is it really appropriate to report all of these parameters to 3 significant digits?

There is no specific reason for that. 2 digits are sufficient. The revised version of the manuscript takes account for these modifications.

· Figure 9: it's clear that the calving event sizes do not fit a gaussian pdf, so why plot them as such? It would seem better to first determine what kind of distribution (e.g. log-normal, Poisson, etc.) best fits the results, and then plot the appropriate cdf. This could facilitate comparison with observational data in the future. For example, if observations indicate that calving event sizes follow a log-normal distribution, then you would want a model that also produced calving event sizes that follow such a distribution.

The idea was not to find a specific shape for the calving event size distribution, but to compare the distribution of calving events. This anamorphosis showed how the results differs from a common normal distribution associated with a mean and standard deviation, and it highlight the presence of "outliers" (higher than 300 m).

However, the new simulations highlight that some calving event mean sizes and frequency are related to specifics damage parameters sets. It results in a multimodal distribution that should not be globally average. The revised version of the manuscript takes into account for these corrections and the figure is reduced to a single histogram, as well as a short discussion.

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2 Doug Benn Short Comment

We would like to thank Doug Benn for comments and advice, who greatly improved the description and the hierarchy of calving processes presented in the literature, and for highlighting the necessity for further inter-comparison benchmark for calving models. We tried to incorporate most of these comments within the revised version of the manuscript.

First, in the introductory section referencing of earlier work is very careless, and suggests a very casual approach to the literature. Proper acknowledgement needs to be made of how the present model builds on previous work, especially the crevasse depth model developed by Benn (2007a, b) and Nick (2010). Indeed, the present model is not so much a "new framework", as a refinement of the existing crevasse-depth model framework, using more sophisticated treatments of damage and fracture.

Regarding this first comment, we do not meant to suggest that our work success better than the classical approaches in simulating iceberg calving. Benn's criterion performs well, and several experiments prove the multiple interests of this criterion, which have been largely explained in various papers, and this is the reason for which this criterion is the most physically-based used criterion.

Following the work of Benn et al. (2007a,b), we actually stated that the calving event occurs as soon as the tip of the crevasse penetrates below the water line (even if the condition is not only on crevasse depth, but also on the criterion $K_I > K_{Ia}$). But this argument is the only point in common between Benn's criterion and ours. The use of damage mechanics to define the initial crevasse depth and the linear elastic fracture mechanics describing crevasse propagation does not appear neither in the formulation of Nye (1957) nor Benn et al. (2007a,b), and as such represent a new approach in calving modelling. Thus, we believe that our framework is not just a higher refinement of the crevasse-depth framework.

However, the manuscript has been modified accordingly following the general suggestion regarding the literature.

Second, it is arguable whether the new model has been "validated" by the Helheim model exercise, as against "tuned". As the authors are no doubt aware, tuning of model parameters to fit output to observations does not mean that the model correctly represents reality. I believe that the current paper represents a significant conceptual advance in how calving processes might be represented in continuum models, but it has not been demonstrated that the model will necessarily perform better than simpler formulations. Formal model inter-comparisons will be required to test this.

The used of word "validated" is indeed a misleading from our part. Obviously, "calibrated" describes better the level of development of the presented calving law. We prefer this word to "tuned", which holds a negative connotation.

Additionally, we totally agree with Doug Benn's remark, regarding the fact that a model inter-comparison should be undertaken, to evaluate the different behaviours of the currently-used criterion. Creating such a benchmark would probably lead to more clarity in discretizing calving parameterizations, but it is out of the scope of the present study. The manuscript has been modified to include these comments.

Third, as regards modelling calving at Helheim glacier, the omission of basal crevasses in the new model is a major shortcoming. Observations by Murray et al. (2013) demonstrate that surface crevassing does not contribute significantly to large calving events at Helheim, but basal crevasses do. The model used by Nick (2013) to model calving losses at Helheim includes basal crevassing, and is more likely to capture the actual processes of mass loss than a model based on surface crevasse propagation alone. This means that in its present form the damage/LEFM almost certainly misrepresents calving at Helheim Glacier.

What Doug Benn is pointing out here clearly highlight one of the current limitation of our model. As mentioned in our paper, we did not include the basal crevasses, and we are perfectly aware that this omission could biased the amplitude of the resulting calving events.

However, we are currently working on the modelling of the effect of basal crevasses on iceberg calving, and this suggestion also pointed out by referee #2 has been included in a section regarding potential improvements of the model.

Some detailed comments:

Before answering the specific comments, we just want to remark that the pages and lines numbers refer to the old version of the manuscript, which was closed on February, 25^{th}, before review, following authors request. However, most of the reader's comments are still valid for the new version. Otherwise is specified in the answer.

p. 1111: modify wording in the abstract on line 2 (”new calving modeling framework” to ”new development of crevasse-depth models”) and line 8 (”validated” to ”tuned”).

As stated above, we do not believe that our model represent a simple development of crevasse-depth model. However, we changed the manuscript according to pertinent remarks regarding the literature.

p. 1113, line 6. The referencing here is very inaccurate. The wording appears to imply that Nye proposed a calving criterion, not just a simple formula for the penetration of crevasses. Instead, it should be stated that the crevasse depth calving criterion was proposed by Benn (2007a, b), and implemented in a higher-order flow model by Nick et al. (2010). Mottram and Benn (2009) did not ?use? the calving criterion, but compared predictions of the Nye and LEFM models with field data. Their study found that the Nye model performed almost as well as the LEFM approach, and had the advantage that crevasse spacing did not need to be known. The choice of the Nye formula in the model of Nick (2010) was therefore based on a rational argument backed up with field data. At present, there is no means of telling if the new approach is better or not.

It is true that our referencing was inaccurate. Changes have been proposed in the new version of the manuscript. Additionally, we did not mean to suggest that our approach was better or not than Nick's one, and we apologize if the understanding was biased.

p. 1113, lines 12-15. It is true that the Nye crevasse depth formula does not incorporate stress concentration effects, but as pointed out by Benn (2007b), stress concentration effects are relatively small in fields of closely-spaced crevasses. The statement that the Benn-Nick model does not account for "the crevasse depth" is wrong, as calculation of crevasse depths is at the core of the model. The statement about the "ice discharge" is also incorrect. The approximations used in the Benn model may result in some error in the position of the margin, but the predicted discharge mostly reflects basal sliding functions, which are not at issue here. In addition, as pointed out in Borstad's review, it is by no means clear that the concept of a lone crevasse running ahead of all others is an accurate representation of reality. So it remains to be demonstrated that the new approach is superior in practice to models incorporating the Nye function.

The sentence about the "crevasse depth" was an unfortunate shortcut. We meant that the Benn-Nick model supposes that the crevasse depth is given by the stress field alone. In the LEFM theory, the stress intensity factor, which describes the ability of a crevasse to propagates and thus determine the final crevasse depth depends on both the stress field and the initial crevasse depth. The paragraph was clarified by highlighting the instantaneous approach on which Benn-Nick criterion relies, compared to the combined CDM-LEFM approach.

Wording modification regarding the "ice discharge" now focuses about calving time and amplitude.

Intercomparison would be an essential tool to highlight the potential differences between calving criteria, and to understand them. In such an absence of comparison, it is clear that no one can claim that a model performs better than another one.

P. 1113, line 19: the Åström model can simulate both viscous and brittle behaviour, so it is wrong to say that its "non-continuous approach" is a limitation. Its main limitation at present is that it is very computationally demanding, but in concept it is actually better for all types of glacier modeling than continuum models.

The idea was to say that theoretically, discrete element models are especially designed to represent heterogeneous media, which is not the case when dealing with ice. However, it is right that Jan Åström's model performs well in representing the ice fracturing and the calving size distribution.

Consequently, we modified the introduction section to precise that the main limitation is the computational cost, and to mention that coupling is engaged between discrete element models and finite element models.

At present, the paper is rather dismissive of previous approaches, but in fact it borrows heavily from earlier work and it should be clearly acknowledged that the authors' model is closely similar in concept and structure to the existing crevasse-depth calving model. The new paper develops the concept to incorporate more detailed formulations of damage and fracture propagation. This is a significant development of the concept, although it remains to be seen whether the modifications result in improved model performance.

Once again, we did not mean to be dismissive of previous approaches, and we apologize if the reader has encounter this feeling. We have modified the referencing, and we hope that the improved version of the manuscript is now free from misleadings and inaccuracies.

p. 1113, line 20: "apparition" in English usually means the appearance of a ghost. "development" would be a better word.

Done.

p. 1113, line 26: Van der Veen's papers did not apply LEFM to calving, but to analyze the penetration of surface and bottom crevasses. Also change "employed to described" to "employed to describe"

Done.

p. 1114, line 3: The IGS is a "large glaciological body". Better to say "large ice masses"

Done.

p. 1121, line 18: "pretty well" is a very vague! And of course, the Nye model performed almost as well.

Done.

p. 1123, line 27. "we do consider" should be "we do not consider". As noted above, this is a major shortcoming when it comes to modeling Helheim glacier.

This sentence was changed since the second version of the manuscript was submitted.

p. 1124, lines 4-5: this is inaccurate. For a fixed small (several m) water depth, there will be a corresponding finite crevasse depth. Full depth crevasse penetration requires continuous input of water to keep the crevasse nearly full.

Yes, this is what we meant, but the formulation was inaccurate. However, maintaining water level at the same height requires either a free connection with the pro-glacial water body, or a continuous input of water (melt water for example) into the crevasse. This latter case requires the knowledge of the rate at which water flows into the crevasse, which should be also evaluated before been incorporated in the model.

A paragraph regarding this question and possible improvements was added to the revised version of the manuscript.

P. 1125, line 7: It is now possible to acknowledge the source of the re-meshing scheme - which was vital to the simulations - by referencing Todd and Christoffersen (2014). This should be done as the brief statement in the acknowledgements does not communicate to the reader the importance of this input.

The reference was added into the revised version of the manuscript.

p. 1125, line 22: "validate" should be "test" or "evaluate".

We changed it to "calibrate".

p. 1131, line 6: the glacier studied by Mottram and Benn is in fact in Iceland, not Svalbard, and calves into a lagoon, not the sea. Did the authors actually consult this paper? But in any case, the crevasse depths observed at their site, where strain rates are quite low, do not provide useful validation of the modeled thickness of damaged ice on Helheim.

We apologize for the mistake in the manuscript, which has been modified according to Doug Benn's comments.

However, we do not understand why this measurement of crevasse depth would not support our claim. Crevasse depth measurement in the vicinity of glaciers front are difficult to obtain, and the paper we rely on presents measurements of crevasse depth near to the front.

As our model furnish a potential accumulated depth of crevasse field, which is slowly advected with the ice flow and deepens under the effect of the stress field, it does not contradict the statement of low strain rates in this area.

p. 1131, line 27: "At last" should be "Finally"

Done.

p. 1132, last paragraph: the arguments here are rather weak. Given that: a) the simulations on which this statement are based are an arbitrarily chosen subset of an ensemble that exhibits a huge range in behaviour, b) the simulations do not replicate the observed oscillations of the front, and c) a major process (basal crevassing) is missing, there are really no grounds for making any claim about the causes of the observed glacier behaviour.

We do not question the fact that several processes are not represented in the model, and that our simulation does not replicate the observed behaviour of Helheim Glacier between 2001 and 2005 (which was not the aim of the study anyway).

However, the simulation used to sustain this statement all belong to those which successfully overcome our sanity check (*i.e.* for which the front remains between the observed extent of ten kilometers), and thus are not arbitrarily chosen. Moreover, we stated that the cycles of advance and retreats are not related to any variability in external forcing, which is true, as we are applying the same constant forcing for all these simulations.

This is why we stated that, according to our modelling, a specific dynamics of the front can emerge from glacier internal dynamics and geometry, and does not necessary requires external forcing. Further information were provided in the answer to Referee #2.

p. 1133, lines 19 and 22: “reliable” should be “reasonable” in both cases.

Done.

Additional references:

Benn, D.I., Hulton, N.R.J. and Mottram, R.H. 2007a. ?Calving laws?, ?sliding laws? and the stability of tidewater glaciers. Annals of Glaciology 46, 123-130.

Murray, T., Rutt, I.C., O?Farrell, T., Edwards, S., Selmes, N., Martin, I., James, T., Aspey, R., Bevan, S., Loskot, P. and Bauge, T. 2013. High-resolution monitoring of glacier dynamics during calving events at Helheim Glacier, southeast Greenland. AGU Fall Meeting abstract, C21A-0614.

Todd, J. and Christoffersen, P. 2014 Are seasonal calving dynamics forced by buttressing from ice mélange or undercutting by melting? Outcomes from Full Stokes simulations of Store Gletscher, West Greenland. The Cryosphere Discuss.

3 Jeremy Bassis Referee Comment

We greatly thank Jeremy Bassis for his acceptation 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 one which deserved a specific discussion were integrated in the revised version of the manuscript.

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 accumulates 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 [-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.

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. Additionnaly, 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 writting of Eq. 11, which should read:

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

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

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

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. (2007b)). 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 to 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. 3), for 8 simulations (grey lines) and the corresponding mean (black line). We observed a peak for a period ($500 \sim 600$ 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 $\sim$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

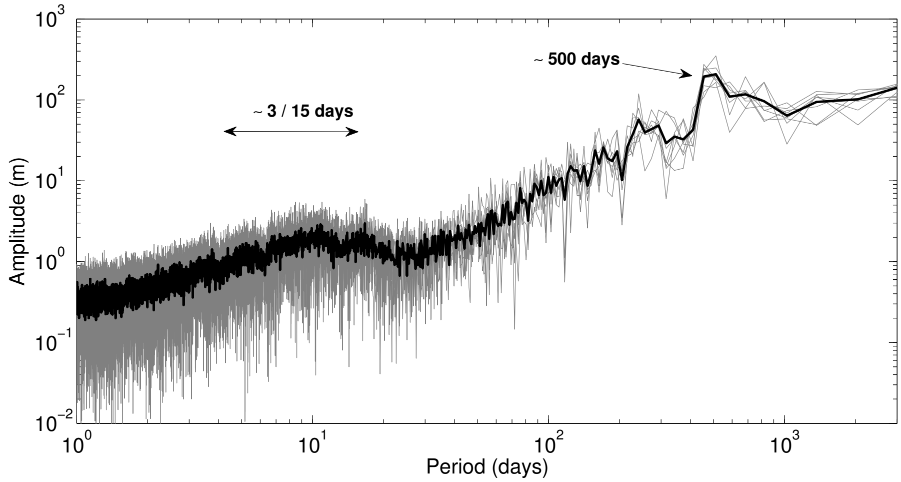

Figure 3: Frequential response of the calving front for the set of parameters ($\overline{\sigma_{th}} = 0.11$ MPa, $B = 1.30$ MPa^{-1}, and $D_c = 0.50$), for a simulated time of 10 years, with 8 different realizations of local disorder. Grey lines represent the frequency spectrum for each of the 8 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.

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