Reply to Referee # 1

J. Krug, J. Weiss, O. Gagliardini, G. Durand

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Correspondence to : jean.krug@ujf-grenoble.fr

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;

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

$$L = \frac{l}{l+d}$$

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:

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

where

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

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

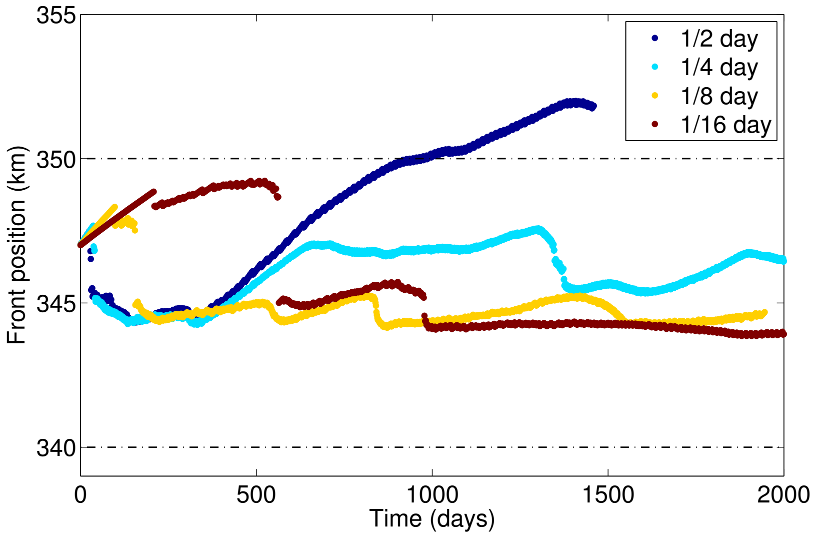

Figure 1: Time step size sensitivity for the simulation with corresponding damage parameters set ($\overline{\sigma_{th}} = 0.11$ MPa, $B = 1.30$ MPa^{-1}, and $D_c = 0.50$)

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

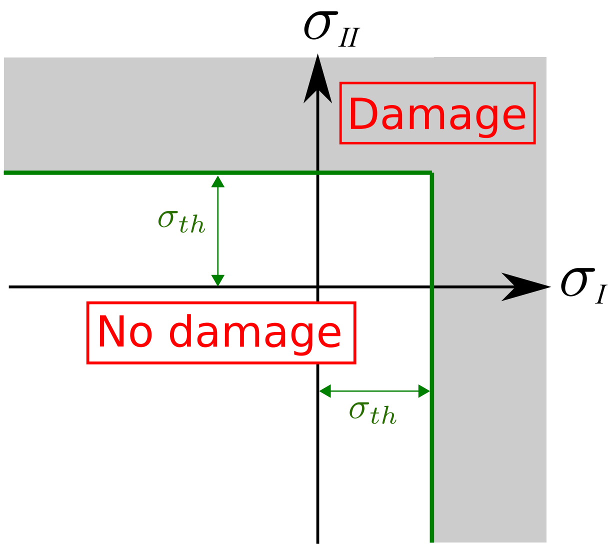

Figure 2: Damage envelope in the space of principal stresses. σ_I and σ_{II} respectively represent the first principal stress and the second principal stress, and σ_{th} is the stress threshold. The shaded area corresponds to the stress conditions under which damage occurs.

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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