Dear Editor,

We addressed all the points highlighted by the referee.

You will find below the answers to the issues that you and the referee pointed out.
As requested, the manuscript was proof-read by a qualified service, which pointed out several typos and awkward terminologies. We corrected them in the final version of the manuscript, but for clarity, only the remarks addressed by the reviewer (see below) were highlighted in red.

Regards,

Jean Krug

Reply to the Editor comment:

The paper has now gone through a review and a shorter re-review. The reviews were very positive and your work can now be published in TC.

The reviewers found number of typos and were worried that some more typos may have gone undetected. Would you be in a position of having someone going very carefully, line by line, through the final manuscript? Alternatively, we can have the Copernicus team doing some final proof-reading. However that will involve some cost.

We asked to a qualified translator to carefully read the manuscript, which now, we hope, free from awkward terminologies and typos.

Another small issue is your discussion of the Hayhurst criterion. It is not fully clear to me why you consider that to be inadequate for glaciers. Maybe you could expand on this and justify this a bit better, or conclude that here some further work is needed.

As stressed on page 9 of the manuscript, the main reason for not considering the Hayhurst criterion is because it allows damaging under compressive stresses and we want to represent fracturing processes occuring under tensile stresses only. In the context of our study, we believe that this criterion is not appropriate.

Reply to the Referee comment:
Overview:

This manuscript is a revised version of a previous manuscript that describes the implementation of a combined fracture mechanics/damage mechanics model for iceberg calving. Overall, my assessment of the manuscript remains largely similar to my previous assessment: the paper represents an interesting approach with results that a broad section of the ice sheet modeling community should be interested in. In general, I don't think it is appropriate to torture authors by forcing them into multiple revisions. Moreover, most of my comments represent relatively minor suggestions or nit-picking on technical details and I leave it at the discretion of the authors and editors if they wish to incorporate these minor comments into a revised version of the manuscript. However, I found a large number of grammatically dubious sentence structures within the manuscript and these need attention by the authors, editors or a diligent copy editor before a print (or electronic as the case may be) version of this article appears in press. I point out some of these in the miscellaneous comments section, but I have not thoroughly gone through the manuscript to ferret all of them out. I'm terrible at spotting my own typos as well so I'm sympathetic to the authors on this point. These typos and grammatical mistakes are, nonetheless, distracting to readers.

We would like to thank Jeremy Basiss for taking time to review the manuscript again. His reading highlighted a few mistakes and misleadings, which have been considered in the new version of the manuscript. Answers to reviewer's request are highlighted using red characters, and subsequent changes were applied in the final version of the manuscript.

Major comments:

Despite my reluctance to suggest major changes in a manuscript after it has already progressed once through the review process I have a couple of suggestions for the authors that I think will make the manuscript more easily digestible by readers and hence (hopefully) more widely cited, although I leave it at the discretion of the authors whether they feel these points are important enough to merit the changes requested.

1. Organization of model description:

I strongly recommend condensing and moving all of section 2.4 to a discussion section. I fully understand that this section was introduced to address the vagaries of the previous round of reviewer comments. Here the authors provide a very detailed description of how they would (but do not) incorporate a variety of effects such as shielding, basal crevasses, water in crevasses, etc. The problem is that readers are going to read this section and wonder why the authors didn't incorporate these effects, given the fact that they explained exactly how they could be easily incorporated into the model. Given the fact that none of these effects are currently incorporated, reading about these things in the model description section distracts the reader from what is actually done in the model. My suggestion is to condense this down (I don't think the authors even need equations for something they have not done) into 1-2 paragraphs or even a list of things that could be done to improve the model. I suspect that many readers will read through this section and wonder why the authors didn't account for all of these things given that they described exactly how to do it.

Indeed, this suggestion would enhance the clarity of the model description. We moved this section at the end of the manuscript, renamed as « 3.4 : Outlook and further improvements ». However, we kept the detail of the physical approach (including the equations), in order to insist on the fact that the incorporation of such parameters do not require heavy further developments, but are currently limited by the lack of knowledge / observation on these aspects.

2. Mapping of effective stress:

The authors perplexingly assert that the effective stress mapping depends only on the deviatoric stress and not the Cauchy stress. Few issues associated with damage mechanics in the glaciological literature appear to be as controversial and confused as this subtle mapping question. Previous damage mechanics models, such as those by Pralong and Funk (2005) and Duddu and Waisman (2013) apply the mapping directly to the Cauchy stress. However, others mysteriously apply the mapping solely to the deviatoric stress (e.g., Borstad et al.). As I understand it, the author's argument is that because the viscous flow of ice only depends on deviatoric stress, the mapping from physical space to effective space should also only depend on deviatoric stress. However, consider the following thought experiment in which we consider damage evolution under compressive failure. In this case, the author's theory would require that damage evolution still depend solely on deviatoric stress (the same arguments apply). However, both the micromechanics of elastic crack formation in compression and observations indicate that the rate of material failure under compression depends on more than just the largest principal stress and certainly not only on the deviatoric stress, as the authors argument would lead us to assume. Of course, compressive failure occurs through fundamentally different mechanisms than tensile failure so one might be tempted to dismiss this argument. However, if we merely dismiss this argument we are then forced to assume a radically different mapping from physical space to effective space for tensile versus compressive failure and this discontinuity in formulations is troubling to me. To be clear, I'm uncertain of the solution to this dilemma and don't strenuously object to only applying the mapping to the deviatoric stress. Nonetheless, I urge the authors to be clear about the assumptions of their model and to point out that this mapping issue remains very much uncertain and controversial.

Regarding the question of the mapping of effective stress, we stated that :
1 - As reminded by the referee, the damage affects the viscous flow of ice, *i.e.* the Glen's law, *i.e.* through the deviatoric part of the Cauchy stress tensor **S** only.
2 - The incrementation of damage, however, depends on the Cauchy stress tensor $\boldsymbol{\sigma}$.
These two processes are clearly different from each other. Regarding the first one, if we would have considered *e.g.* the impact of damage on the elastic behaviour of the ice, then, we should have used the full Cauchy stress tensor $\boldsymbol{\sigma}$ instead of **S**. However, this is not the case here.

3. Appropriateness of Hayhurst and other damage mechanics criterion.
The authors provide a passionate critique of the use of the Von Mises, Hayhurst and other criterion used for damage evolution in metals. However, the fact that a criterion or model formulation is appropriate for a (plastic) metal is not sufficient to prove that it is an invalidate model for a material that is not a metal (plastic). This is especially true when one is trying to formulate a model that applies over scales that are large compared to those of the laboratory setting. For example, rocks at typical pressures and stresses at the surface of the Earth undergo brittle failure and laboratory experiments suggest no ductile deformation preceding failure. However, because rocks on the surface of the Earth are typically highly fractured and these fractures occur at (most) orientations, failure of rocks can be approximated as failure along distinct slip-lines and the slip lines can be predicted by plasticity theory. Thus the macroscopic theory of rock failure can be approximated using plasticity theory even though we know very well that the micromechanics of failure are distinct. Similarly, we know from theory that shear failure of brittle specimens occurs through the echelon growth and eventual coalescence of tensile of parallel tensile cracks. In this case even though failure is purely tensile, the growth of micro-cracks depends on the bi-axial state of principal stresses. Finally, I think it is worth recalling that the socalled Hayhurst and Von Mises criteria have their origin in theoretical and not experimental arguments. The Von Mises criterion was proposed based on the theoretical argument that the failure envelope maximizes energy dissipation. That this happens to be true for metals was not discovered until after Von Mises first proposed the criterion based primarily on theoretical arguments. Similarly, Hayhurst clearly recognized that failure can only depend on the invariants of the stress tensor if one is to avoid introducing anisotropy. This fact remains true for ice and any other material and is solely a consequence of the need for an objective criterion. The linear combination of criterions proposed by Hayhurst may be the simplest such criterion that goes beyond the trivial assumption that failure is only a function of the largest principal stress invariant. The authors are free to argue that experiments or field studies show that fracture initiation and propagation depend solely on the largest principal stress and are independent of the triaxial stress state (if that is indeed what the experiments support). Alternatively, the authors can argue that given all of the uncertainties it is reasonable to start with a simpler model in which damage accumulation only depends on the largest principal stress. However, the quasi-theoretical argument that parameterizations used for metals cannot be correct for ice because ice is not a metal needs to be strengthened or discarded.

The point raised here seems to be based on a misunderstanding of our discussion. The referee states here that we argue that given the fact that von Misès and Hayhurst criteria were proposed for metals damaging, they were not suited for ice damaging.
We did not mean what seemed to be understood:

1 - The main reason for refusing the von Misès criterion is because it is symmetric in tension and compression, which is not the case for ice fracturing processes (see Schulson and Duval, 2009).
2 - The Hayhurst criterion, if not symmetric, allows damaging under compressive stresses. However, we want to represent the crevasse opening in mode I only, *i.e.* under tensile stresses. In this case, it is not suitable, and we chose a simpler criterion, depending on the maximum component of principal stress.

These points were addressed in Sect. 2.2.1, page 9, l. 5-16. We rewrote some of these sentences, and we hope that the message is now clearer.

Miscellaneous comments:

Page 2, Abstract: framework vs. model. What is a framework and how is it different from a model? I recommend calling it a model, because that is what it is.
Done.

Page 2, Abstract: “producing a dynamic equilibrium in agreement with observed stable positions” as previous reviewers noted, although it is reassuring that you can tune the model to agree with observations, this doesn’t imply that the model is correct. Until the model is able to make a prediction it was not tuned to make, it still has zero predictive skill.
We totally agree with this point, and we do not claim the opposite.

Page 2, assorted comments
“an intensified” --> “intensified”
“evaluated to” --> estimated?
Done.

Page 3:
“In the sake” --> for the sake
“a deep understanding” --> “a deeper (or improved) understanding”
“basal crevasse opening” -->”basal crevasse propagation”
“combined to an empirical criterion for calving” --> “combined with an empirical . . .”
line 15 “accumulation of damage” This is the first time damage is mentioned and it has not yet been defined. The authors need to define damage here or use a different word, say fractures or crevasses, which is more familiar to readers.
Done.

“computational cost is important” --> important to what? I suggest “their high computation cost limits their effectiveness”
Done.

“For a few years, some authors have focused on continuum damage mechanics in order to represent both the development of micro-defects in the ice to the development of macroscale crevasses, and their effects on the viscous behaviour of the ice while keeping a continuu approach” --> give references.
Done.

Page 4:
“the critical fracture propagation in the vicinity of the calving front” --> why is critical fracture propagation only permitted near the calving front?
Changed to : "the critical fracture propagation characterizing calving event"

“The slow development of damage represents the long timescales evolution of purely viscous ice” --> Isn’t damage incompatible with the flow of purely viscous ice? To get damage you need fractures or defects of some kind and this is not possible with a purely viscous continuous material. Also, check if you really want timescale to be plural.
The damage allows for the representation of the ice flow using a continuous approach. As long as the ice isn't completely damaged, we still consider that it exhibits a viscous behaviour. However, the reviewer is right when saying that it contains micro-cracks. Thus, we replaced « purely viscous » by « viscous ».

Page 7, line 5ish: The authors argue that there is no evidence of a fracture process zone for ice. However, it is well known that in LEFM sharp starter cracks have a one-oversquareroot r singularity in the stress field associated with sharp crack tips. This singularity is, however, unphysical, and represents a breakdown in the theory. One accommodates this singularity by acknowledging that the linear elastic hypothesis fails in a cohesive zone surrounding the crack tip and this region of irreversible flow is called the fracture process zone. Fortunately, so long as the size of the process zone is small compared to the size of the area dominated by the stress singularity, the far field stress acts as a boundary condition and uniquely determines the stress field within the process zone. This is the reason why it is permissible to represent the fracture toughness of ice using a single experimentally derived fracture toughness. These theoretical arguments are strong enough that I’m unmoved by any reported absence of laboratory evidence for a fracture process zone for ice. The fracture process zone needs to be there to avoid the unphysical singularity.
Absolutely, the fracture process zone is present in the ice. We did not say the opposite : when considering *ductile failure*, the nucleation of defects occurs in the FPZ. We are not considering ductile failure, **but** dislocation-mediated plasticity is likely to take place near crack tips in ice to screen stress singularities.

Page 7, line 20ish:
“which effect” --> “whose effect”
“a mesoscale” --> Need to define the mesoscale. How are microscale, mesoscale and macroscale defined? Are there specific ranges over which each is appropriate? Is mesoscale comparable to the laboratory scale?
By mesoscale, we mean the grid size, *i.e.* a characteristic size of a few meters. This distinction wasn't mentionned in the previous version of the manuscript. It was added now.

Page 8, comments:
“viscoplastic flow of ice” --> Normally I wouldn’t object to the use of the term viscoplastic. However, the authors strongly stress that ice is not plastic and hence referring to it as viscoplastic would seem to be misleading here and may risk confusing readers.
We agree, this may add some supplementary source of misleading. We employed « creep flow ».

Page 8, line 15ish: “Damage is a property of the material at the mesoscale” --> Once again, you need to define what you mean by mesoscale so that readers can understand what the authors are talking about.
Yes, done.

Page 8, Equation 9: Note that Pralong and Funk use the objective derivative with the spin terms included in their advection equation. Here, the authors only include the material derivative. I have to confess that I don’t understand why Pralong and Funk included the objective time derivative as damage seems like any other scalar that is advected through the system, but this might be worth

commenting on.
Pralong and Funk used the spin tensor because their formulation may involve the case of anisotropic ice. In this case, the damage variable is defined as a tensor (and not just a scalar). When damage is a scalar (like in the current work), the equation reduces to the one presented in Eq. (9)

Page 9, line 15 ish: “we want to describe crevasse opening under pure traction” --> Unless the authors mean tension instead of traction this statement makes no sense because traction is merely the dot product of the stress tensor with a unit normal and there is no such thing as pure traction!
« Tension » is the appropriate word.

Page 9, line 20ish: Remove “anyway”, it is unnecessary verbiage.
Done.

Page 10, Equation 13: What happened to the tilde? Shouldn’t this S also be effective stress? I might have gotten you notation mixed up here, but shouldn’t the effective stress be proportional to 1/(1-D) instead of (1-D)?
We substituted the expression of the effective deviatoric stress tensor given in Eq. (12) with the deviatoric stress given in Eq. (8). This is why the tilde disappears. We made the explanation more straightforward.

Page 11: “is a reliable tool to deal” --> maybe nit picking, but whether it is a reliable tool or not remains to be demonstrated. If we knew damage mechanics was a reliable tool everyone would use it.
Changed in « can be used to deal ».

Page 15: “we implemented an horizontal interpolation” --> ?? do the authors mean “a horizontal interpolation scheme”? Grammar is off in this sentence.
Done.

Page 15: “Adittionnally” --> additionally
Done.

Page 18, line 5ish: “The abundance of observations there allows to confront and constrain our model” --> Grammar problems. Do the authors mean “The abundance of observations there allows us to confront and constrain . . .”
Yes, done.

Page 18, line 15ish: Covering how many kilometers? Need to be specific.
Around 10 km, in order to capture the basal features which may locally influence the front behaviour. The final version of the manuscript was modified accordingly.

Page 19, line 15ish: “Thus, our choice allows for a proper fracture initiation and damage advection” --> Do the authors mean numerically accurate fracture initiation and damage advection? What would improper fracture initiation and damage advection look like?
Yes, done.

Page 20, line 20ish: We let the geometry adjust to the prescribed boundary conditions and inversed basal friction for 8 years. --> What is “inversed basal friction”? Are these the basal friction values that were inverted for (at which point the sentence is grammatically incorrect) or something else?
Yes, this is what we meant. We rewrote this sentence to make it clearer.

Page 21, line 10: Is damage enhancement really unit-less? I can’t fathom how the equations are

dimensionally consistent without a unit of time in the damage rate production equation.
Definitely. The damage enhancement factor is in MPa^{-1} a^{-1}. We changed the manuscript and the parameters list accordingly.

Page 22, “frontal melting and calving procedure are activated.” --> “frontal melting and the calving procedure are activated.”
Done.

Page 25, line 4: “fastens the ice flow” --> in English fasten means to join or tie as in fasten a knot making this a peculiar choice of words.
« Fastens » was changed in « accelerates ».

Page 27, line 11: “physically-based calving model” --> I suppose the term physically based is now rigidly solidified in standard glaciological nomenclature, but I’ve always disliked the term because “physically” is vague and doesn’t imply the physical mechanism is in any way correct. Moreover, if at the end of the day the model is tuned to reproduce observations it is essentially an empirical despite any physical motivation. I personally prefer the term “process based” because it implies that calving is related to a set of processes that can be measured and understood and understanding of calving can be improved by improved understanding of the processes that control it.
We changed the « physically-based » term in « process based ».

Note that the distribution that you use is not formally a “Gaussian” distribution because you throw out values above and below your threshold. In future work, the authors might wish to consider probability distributions supported on a bounded interval (e.g., beta distribution, logitnormal distribution, truncated normal distribution, etc.) Mathematically, at least these distributions are more appropriate and have many of the same quantitative properties as the filtered Gaussian distribution.