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.

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.

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 <u>compressive</u> 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 <u>tensile</u> versus <u>compressive</u> 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.

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 so-called 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 <u>after</u> Von Mises first proposed the criterion based primarily on theoretical arguments. Similarly, Hayhurst clearly recognized that failure can only depend on the <u>invariants</u> 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 tri-axial 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.

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.

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.

Page 2, assorted comments

"an intensified" → "intensified"

"evaluated to" → estimated?

Page 3:
"In the sake" → for the sake

"a deep understanding" → "a deep<u>er</u> (or improved) understanding"

"basal crevasse opening" →"basal crevasse propagation"

"combined to an empirical criterion for calving" → "combined <u>with an</u> 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.

"computational cost is important" → important to what? I suggest "their high computation cost limits their effectiveness"

"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 macro-scale crevasses, and their effects on the viscous behaviour of the ice while keeping a continuu approach" → give references.

Page 4:

"the critical fracture propagation in the vicinity of the calving front" → why is critical fracture propagation only permitted near the calving front?

"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.

Page 7, line 5ish: The authors argue that there is <u>no</u> evidence of a fracture process zone for ice. However, it is well known that in LEFM sharp starter cracks have a one-over-square-root 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.

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?

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 <u>not</u> plastic and hence referring to it as viscoplastic would seem to be misleading here and may risk confusing readers.

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.

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.

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!

Page 9, line 20ish: Remove "anyway", it is unnecessary verbiage.

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)?

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.

Page 15: "we implemented an horizontal interpolation" → ?? do the authors mean "a horizontal interpolation scheme"? Grammar is off in this sentence.

Page 15: "Adittionnally" → additionally

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 . . ."

Page 18, line 15ish: Covering how many kilometers? Need to be specific.

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?

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?

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.

Page 22, "frontal melting and calving procedure are activated." → "frontal melting and the calving procedure are activated."

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.

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.

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.