Response to referee2 (J. Bassis): 'Ice shelf fracture parameterization in an ice sheet model' by Sainan Sun et al.

1 J. Bassis (Referee)

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1.1 Overview

This study incorporates a damage based parameterization of fracture in the BISICLES ice sheet model and uses this to assess the influence damage has on grounding line position. The BISICLES model is a sophisticated ice sheet model which includes mesh refinement. The goal of the present study is to examine the influence of damage on grounding line position using a MISMIP style setup. The authors introduce a damage formulation in which damage is determined based on the Nye crevasse depth formulation. This has the advantage that, unlike most damage evolution laws that are heuristically based on sparse laboratory or field measurements, damage evolution has a physical component based on some elementary physics. Moreover, because crevasse depth models are popular methods of simulating the advance and retreat of outlet glaciers, the formulation has the potential to provide a unifying theme linking the behavior of outlet glaciers and ice shelves. The distinction between these two regimes is that damage in ice shelves is dominated by advection whereas damage in glaciers tends to grow rapidly near the calving front. In general, I think that this study is interesting and merits publication. However, I have a few major points that the authors should consider addressing in addition to several more minor nit-picky comments. The first sequence of questions relates to the physical formulation of the model where as the second relates to the overall structure of the manuscript and some difficulties I had working my way through it. Overall, however, the manuscript will be a valuable contribution to the field once these questions have been satisfactorily addressed.
I would like to thank Dr. Bassis for this thorough review and I will try to give a response.

1.2 Major issues:

1.2.1 Approach to damage:

The first comment that I have relates to the formulation of damage and advection of damage within the model. I like the general idea of the model and this

feels unseemly to point out in a review proposed something similar several years ago (Bassis and Ma, 2015 Evolution of basal crevasses links ice shelf stability to ocean forcing, 10.1016/j.epsl.2014.11.003). There are, however, several key differences between the formulation proposed in that paper and in this one.
The work of Bassis and Ma, (2015) showed the instability effect of crevasses by strain rate weakening and the evolution of initially narrow crevasses. The penetration of crevasses depends on the the stress field and influenced by the basal melting or freezing in the crevasses. The work is definitely related to our study and we have cited it, noting in particular that some of its physics is missing from our model.

In our model, we assumed that initial crevasse depths used to seed damage are determined by the Nye zero stress model, analogous to the model presented here. However, we used a perturbation analysis to examine how the crevasses evolve and in particular whether they deepen, widen or close. As we show in that paper, the evolution of the *ratio* of crevasse depth to ice thickness (a pseudo damage variable analogous to the one introduced in the present study) is controlled by three factors. The first factor is simply kinematic. If crevasses are passive tracers in the flow field then they will deform with the flow field and their depth (or height) will decrease in exactly the same proportion as the ice thickness. A consequence is that the ratio of crevasse penetration to ice thickness remains *constant*. It is unclear to me how the kinematic distortion is accounted for here. From Equation (11) and (10) it looks like crevasse depths are inherited from upstream without accounting for the distortion associated with ice flow. This could be problematic.
Eq. (11)

$$\nabla \cdot (\mathbf{u} d_{tr}) = (\nabla \cdot \mathbf{u}) d_{tr} + (\nabla d_{tr}) \cdot \mathbf{u} \tag{1}$$

does include this purely kinematic factor (the second term above) because $(\nabla d_{tr}) \cdot \mathbf{u}$ is generally non-zero (with u being only the horizontal velocity). We added a note to the manuscript "The vertically averaged damage can be reduced through meteoric or marine ice accumulation, where ice thickens without an increase in d_{tr} . It can also be reduced or increased through the stretching and compression represented by $(\nabla \cdot \mathbf{u}) d_{tr}$, in such way that the ratio d_{tr}/h remains constant"

As we further show, the ambient stress field within the ice shelf will also result in crevasse growth or closure. In fact, crevasses are likely to widen, but penetrate a smaller portion of the ice thickness unless the tensile stress opening crevasses is larger than the stress for a freely spreading ice tongue. Again, this is based on a linear stability analysis and depends on the wavelength of the perturbation and is limited to the early stages of growth.
The crevasses in our model don't have a width, and it seems that our vertically integrated model does not include this partcular effect. We have pointed that out in the manuscript.

Finally, we also show that the ratio of crevasse penetration to ice thickness will depend on the basal melt/refreezing regime of the ice shelf (for basal crevasses) or the surface mass balance (for surface crevasses). This again follows from

kinematic considerations that depend on whether the melt/refreeze rates within crevasses is larger or smaller than the large-scale melt rate allowing the ocean to excavate crevasses or fill crevasses with marine ice. Again, it is unclear to me how the model proposed here accounts for these factors. To be clear we applied the formulation using observed ice shelf velocity and thickness fields as opposed to integrating it within an ice sheet model so our approach is not entirely transferable.
The right side of eq. (11) defines the effect of surface mass balance and melting. Snowing at the surface can heal the crevasse, as can melt at the base (since the crevassed layer is eroded before the layer above). The melt rates are the same with in and out of the crevasses. We would need to do some further devleopment to improve on this.

I'm also somewhat confused by the model used. This might be because symbols are introduced without definitions making it harder to follow the logic. For example, I have not been able to find a definition of dtr. Similarly, I'm not sure I understand the right hand side of Equation 11. This seems to account for surface/basal mass balance, but it is introduced without explanatory text to help the reader understand the physics and assumptions.
We have now defined the variable d_{tr}, "One more modification is needed to reflect the transport of damage by ice flow. At any one time and place we would have two fields, the $d_l(x, y, t)$ computed above, and a field of transported crevasse depths $d_{tr}(x, y, t)$ which would have originated at $(x', y', t' < t)$ and been carried downstream, stretched, compressed, and so on." We have expanded the description of eq 11 to note that e.g basal melt is assumed to erode the crevassed lower layer so that verically integrated damge is reduced.

There is also another subtle issue with the damage model proposed. In Bassis and Ma (2015) we examined how individual crevasses would evolve using a perturbation analysis. The physical interpretation of damage here is more subtle. For example, suppose crevasses penetrate half of the ice thickness (or more generally X percent of the ice thickness) across a channel along the margin. Does that imply a channel cut into the ice shelf where the ice thickness is reduced by half? Does this also reduce the driving stress? Or are the crevasses assumed to be narrow so that they have little effect on the large-scale driving stress. In this case, the damage would then need to account for the fact that you have intact ice between crevasses, resulting in *lower* damage on a large-scale. Or perhaps crevasses are assumed to be filled with ice/melange? All of this is speculation and it would be helpful to have a cartoon or physical description of the process that readers can refer to.
In effect we are assuming that crevasses are filled with soft ice.
We have added a diagram, (fig 1) and some more text: "Notice that damage affects only the deviatoric stress (as in Jouvet et al, 2011 and Krug et al, 2014) and does not affect the gravitational driving stress. We might expect such a modification if we had instead modified the full Cauchy stress (as in Pralong and Funk, 2005, Bassis and Ma, 2015, and Mobasha et al 2016), but have assumed that damage has no impact with respect to isotropic compression or vertical shear, so that the usual hydrostatic vertical stress balance, and the usual

vertical integral of the resulting horizontal pressure gradient holds. This is analogous to assuming that the crevasses are filled with an inviscid material having the same density as ice.”

1.2.2 Organization

I also struggled to understand the main hypothesizes tested. In the introduction we are told that the authors perform numerical experiments to address how including damage influences the evolution of the ice sheet and how the geometry of the damage field affects the dynamic response to ocean forcing. Later, at the beginning of Section 3 we are told that the goal is to address three question, including “If similar grounding line steady states can be realized with or without the damage model”; “If the ‘hidden’ damage inherent in the difference between A and A’ is revealed in the response of the ice stream to thinning of the ice shelf” and; “If it is necessary to evolve the damage model in time or if one can get away with constructing a damage field at the start of a calculation and then merely hold it constant throughout the simulation.” I don’t object to any of the questions, but it would be helpful to have the main objectives of the study introduced together at the beginning. Perhaps the later three questions can be motivated as more specific versions of the initial questions? In fact, I’m not sure that all questions have been completely addressed–especially if the hidden damage is revealed by perturbation experiments. Perhaps I missed something. Nonetheless,these five motivational questions would ideally also be mentioned in the abstract along with the resolution to the questions posed.
Our aim is to constuct a model that is amenable to large scale calculations, and to decide whether its impact on the ice flow justifies the further devleopment of such a model, or whether even simpler prescriptions (e.g, a rule of thumb for reducing A in the ice shelf) might be just as good. We have modified the manuscript in several places, including the abstract, to make this more obviuous.

In a similar vein, one of the questions that authors seek to address is whether there is an equivalence between the rheology of damaged ice and ice with an adjusted rate factor A. The answer to this question seems obvious, especially when comparing Equations 5 and 6. We see that so long as we define $A' = A(1 - D)^3$ there is an exact correspondence. That this question can be addressed by a simple mathematical definition makes me suspicious that the authors are examining a more subtle question, but if so it would help to provide more signposts for readers to help bring us along.
We meant some simple rule of thumb e.g $A' = A/8$ everywhere, rather than $A' = A(1 - D)^3$, which would require knowledge of $D(x, y, z)$ (or in our case $d(x, y)$) We did not make a good job of explaining this and have made a number of modifications to the text. One of the outcomes is that we do seem able to emulate the damage model with a simple prescription, at least in terms of the ice flow.

1.3 Detailed comments:

The definition of ‘damage’ in Equations (6) and (7) doesn’t follow naturally to me. In the standard approach to continuum damage mechanics one introduces

a mapping from the actual stress σ_{ij} to the effective stress $\tilde{\sigma}_{ij}$ of the form: $\tilde{\sigma}_{ij} = (1 - D)\sigma_{ij}$. Note here that the mapping applies to the Cauchy stress tensor and not merely the deviatoric stress, as implied by Equation (6). It is true that one can define an effective viscosity of the form of Equation (6), but presumably one also must apply a mapping to the pressure term?
Here we have followed others (e.g Krug 2014, Jouvet 2011) in only modifying the part of the stress that maps onto strain-rate. It does seem that a modification to the pressure term would be necessary if we were considering the ice to be weaker under isotropic compresssion, but we have assumed that it is not (i.e, crevasses are either closed but not bonded, or, as the reviewer suggests earlier, filled with incompressible melange.

This leads me to my next question, typically the 'damage' is defined as a decrease in load bearing capacity associated with cross sectional area of microcracks within the ice. Hence, the damage takes on a value between zero and unity. Here damage is defined somewhat differently and damage is effectively unity everywhere there is a crevasse and zero elsewhere. Damage is thus binary instead of continuous. Upon depth integrating one obtains crevasse depth as the effective depth integrated damage variable. This new variable is no longer confined to the interval [0,1) and no longer behaves like a typical damage variable. However, one can define a new variable based on the ratio of crevasse penetration depth (ds+db) to ice thickness H, which then maps the problem make to a more traditional effective damage variable that is again constrained to the interval [0,1). This is what is done in Bassis and Ma (2015) and, as noted before, has several advantages in terms of the ability to account for kinematic distortion and passive advection.

Nonetheless, the authors use the variable "D" to denote what they call damage, which is nebulously defined, but appears to be three-dimensional, dimensionless and is binary taking on values of either 0 or 1. The authors then denote depth integrated damage using the lower-case "d" and this variable mimics crevasse penetration depth, which has units of length. Most of the model description focuses on depth integrated damage "d". However, starting in Section 4, the authors talk exclusively about damage with a capital D (see, e.g., page 7). Similarly, we see damage D in Figures 1,2, 4 and 5. This is acutely confusing because I thought that damage D was three-dimensional and binary and these figures all denote a single map view with a continuous variation in damage. I have a suspicion that the authors are really showing the ratio of crevasse penetration depth to ice thickness and these figures and much of the discussion is mislabeled, but I don't know for sure. Much of notation and discussion could be cleaned up to clear up reader confusion.
The reviwer is entirely correct here. We made this clear in the model description " We construct a vertically integrated damage model by treating the ice sheet as having upper and lower layers of ice entirely fractured by surface and basal crevasses respectively, and an undamaged central layer (Fig, 1). Therefore, the scalar damage variable, $D(x, y, z)$ employed in vertically varying models (Pralong and Funk, 2005; Jouvet et al, 2011; Keller and Hutter, 2014; Krug et al 2015; Bassis and Ma, 2015; Mobasha et al, 2016) takes on either the value

0 (in the central layer) or 1 (in the upper and lower layers). The principal damage variable in our model is $d(x, y) \in [0, h)$, the vertical integral of $D(x, y, z)$, and our closest analogue to the usual D is its vertical average, $\bar{D}(x, y) \in [0, 1)$ "

Page 2, Kachanov (1999) appears to be primarily based on metals and I am not aware of any observations presented therein that relate to ice. As such, I'm not sure that this is the best reference to support the hypotheses that micro-cracks are the ultimate source of crevasses. I think this is likely to be true, but one might thing about citing a more ice-centric study to support this.
We cite Rist et al., (1994) now, which examines the relationship between microcracking and ice strength. The sentence is modified to: "Macro-scale fractures are originate from micro-scale cracks, which appear when viscous strain is too high (Rist et al., 1994)."

Page 2, "calving rate increases with imbalance of forces". In the quasi-static approximation forces are always in balance. An imbalance of forces would result in acceleration and violate the Stokes flow hypothesis. Crucially, crevasses do not require an imbalance of forces to propagate.
We intended to say "imbalance of forces could trigger crevasses propagation", but that's not the only reason. We now modified the sentence to:
"The crevasse depth and propagation depends on the stress field (Nye, 1957) and there are processes that further erode the ice, such as force imbalance and basal melting (Benn et al., 2007)."

Page 2, "The models discussed above have reasonably successfully reproduced the calving rate and fracture distribution on some individual glaciers." Is this really true? With the exception of Pralong and Funk, 2005, I don't think that damage mechanics have successfully predicted the calving rate on individual glaciers. This has always been in the hope of damage mechanics, but most of damage mechanics has so far been conceptually applied (e.g., Duddu and Waisman) or focused on the large-scale softening (e.g., Albrecht and Levermann, Borstad et al.,)
The description is inappropriate here. We delete the sentence.

A complex bestiary of experiments. I had a very hard time keeping track of the bestiary of experiments and variants. The table is helpful and appreciated, but the authors can help readers a bit by reminding readers of key differences in the figure captions and providing a bit more explanation of what we are supposed to see in the figures. In general I prefer more descriptive figure captions that include a short figure title and then some more explanatory text to allow readers to quickly point readers to what the authors want to show.
Agreed. We have added to the figure captions.