Response to Anonymous Referee #2

Detailed below are our point-by-point responses to the comments of Anonymous Referee #2. Referee comments are printed in blue font followed by our responses in black.

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The aim of this paper is to compare horizontal strain-rates estimated from surface velocity observations of the Larsen C ice shelf to corresponding predictions coming from classical analytical expressions (Weertman 1957, Thomas 1973), in order to invert both a field of damage parameter D, and a pattern of backstress. This idea is quite interesting and possibly promising. It elaborates from a previous paper by the same authors (Borstad et al., GRL 2012), which considered an inversion of damage only, on Larsen B.

The paper is generally well written and the subject, obviously, deserves attention. I have however one major comment/concern regarding the inversion procedure, i.e. the backbone of the manuscript. This might due to my misunderstanding, or to a more serious concern. In any case, it calls for a significant improvement of sections 3.3 and 4.4, with more details on the inversion procedure.

1) This concern can be formulated as follows: How can the authors be sure that their inversion procedure gives a unique solution for both the damage field and the backstress pattern ? In others words: the effect of damage is to enhance flow rates, whereas backstresses have a reverse effect. Considers e.g. a situation where the observed strain-rates are moderately larger than those predicted by the analytical model. This might be explained by either no backstress and a moderate damage, or by a strong damage and significant backstress, or by any solution in between. How the inversion procedure solves this ? In section 3.3., the damage D, the backstress, and the inverse rigidity Bi are all connected through the definition of Bi, through equation (16) (which is actually simply the definition of Bi !), and through equation (15). So, how Bi can be ob- tained independently from an inverse method (line 18 of p3580) ? And consequently, line 21, how the rigidity field is given ? So, at least, a significant clarification is needed with more details on the inversion procedure in sections 3.3 and 4.4.

We believe that the misunderstanding here is primarily the result of a confusing description in the manuscript, particularly in Section 3.3. Essentially what we are doing is partitioning an inverse calculation for ice rigidity into solutions for damage and backstress. These solutions are indeed independent and unique, provided that the bulk temperature of the ice is specified (whether measured or calculated). We have rewritten Section 3.3 to make this more clear and explicit. We walk more explicitly through the steps of our procedure, starting from an inverse solution for ice rigidity (B_i) using established numerical methods (described in Section 4.4), followed by a masking of this rigidity field based on the specified temperature of the ice (now explicitly shown in an equation with a new notation introduced for clarity), and concluding with the analytical equations for damage and backstress.

Others comments/concerns:

2) In section 4.1, it would be useful to recall how the strain-rates are calculated from surface velocities. I guess that this calculation is based on the hypothesis of a constant vertical profile for strain-rates. This is classical for a shelf. However, is it still reasonable as approaching the grounding line ? In addition, the presence of crevasses and rifts might complicate the problem. Consider e.g. surface crevasses: they will likely have a stronger softening effect on the upper part of the shelf (the reverse for bottom crevasses), and therefore breaking the hypothesis of vertical homogeneity of the strain-rates. This comment is also related to the interactive comment of J. Bassis about stress profiles. This should be taken into account and commented by the authors. In (Borstad, GRL, 2012), the same authors compare observed and modeled surface velocities for the inversion. This is most likely more robust, as it does not relies on a strong hypothesis on the strain-rate vertical profiles. At least, the authors should discuss this point in more details, argue more thoroughly, estimate associated errors, ect..

We believe that part of the confusion here is related to the previous comment regarding the inversion procedure and the calculation of damage and backstress from the inversion results. Our clarification of this procedure should partially alleviate these concerns. Our inversion routine for ice rigidity does indeed rely on a comparison between observed and modeled horizontal surface velocities. From there, the damage and backstress calculations are analytical solutions that use the inversion solution as an input.

The assumption that velocities and strain rates do not vary with depth is indeed implicit in both the inversion routine and the analytical model, and we now point this out in the manuscript. From surface observations it is not possible to discern whether any vertical dependence in strain rates is introduced by damage or fractures within the ice column. Our definition of damage in this context (which we now make more clear and explicit) is that damage is the influence of fractures on deformation visible at the surface. This definition is appropriate for a model designed to be used with remote sensing data which are predominantly limited to the surface of an ice shelf. While they may be important, three-dimensional effects of fractures cannot be addressed within the scope of this framework.

The assumption of hydrostatic equilibrium for the ice shelf is not likely satisfied within several ice thicknesses of the grounding line. Furthermore, the analytical model only accounts for longitudinal stress, whereas near the grounding line vertical shearing may be important as well. It is standard practice to apply a two dimensional SSA model right up to the grounding line for a model considering only an ice shelf. However, we now explicitly state that our results near the grounding line should be viewed with caution in light of these concerns.

3) The damage fields of figure 4 exhibit a strange characteristic: in average, the damage D is decreasing as one goes downstream along a flow line. This is rather counter-intuitive, as one would expect D to accumulate through time (or to remain more or less constant). The only possible explanation is a sort of “damage reversal” related to healing of crevasses. This is suggested by the authors in case of rifts, introducing the role of mélange. This damage reversal might be indeed locally a possibility, especially if we have local compressive stresses, but its systematic character is highly surprising. This could indicate a partly incorrect inversion of D (see comment above), with the maps of figure 4 resulting from a combination of effects (D, and others effects).

We agree that this behavior may appear counterintuitive, but the decrease in damage moving downstream is consistent with inverse method results for ice rigidity for Larsen C [*Khazendar et al.*, 2011] as well as Larsen B [*Khazendar et al.*, 2007; *Vieli et al.*, 2007] that show soft ice in the lee of promontories that becomes stiffer toward the ice front. Similar results were also found in direct inversions for damage on Larsen B [*Borstad et al.*, 2012]. Therefore the pattern appears to be a robust feature of the rheology of Larsen B and C.

We do feel that this comment is a valid concern and warrants explanation, however, especially because longitudinal traces of fractures are visible all the way to the ice front in many parts of the ice shelf. In our revised manuscript, we have added several paragraphs of discussion in Section 6.3 that may explain why damage would decrease with distance from the grounding line. Possibilities include an incorrect temperature specification for the ice shelf, creep blunting of initially sharp crevasses, or the need for a revised definition of effective stress.

4) As noted by the authors in section 3, damage mechanics assumes that fractures that soften the bulk material are small compare to the mesoscale considered, and “diluted”, meaning that they do not interact. Is this condition respected here with crevasses and rifts ? Probably not, especially for rifts and crevasses apparent on the images. Consequently, the interpretation of the damage pattern around rifts (figure 7) has to taken with caution: the pattern might indeed indicate softening from cracks, but other effects, such as stress screening by large fractures, might also be present (see e.g. for rift 3 on figure 7). In other words: the procedure used here possibly gives a pattern of “strain-rate enhancement”, but its interpretation as a damage effect in the classical sense has to be taken with caution.

Actually, damage mechanics does not require an assumption that fractures are small compared to the scale over which the problem is discretized. In Section 3 (p. 3575, lines 1–25), we outline reasons why damage mechanics is an advantageous framework for accounting for the role of fractures on ice shelf flow. We do indeed highlight the fact that most fractures are small compared to the mesoscale discretization of the model, but we also highlight (p. 3576, lines 5–11) that damage mechanics is also advantageous for modeling large scale fractures, especially for heterogeneous materials. For example, damage mechanics has been applied to successfully replicate experimental tensile fracture experiments for dense snow [*Borstad and McClung*, 2011] as well as surface crevasse penetration in glaciers [*Duddu et al.*, 2013]. In both studies, the damage model simulates macroscopic fractures that propagate over the entire model domain, and many analogous examples exist across a wide range of engineering materials [*Bažant and Jirásek*, 2002]. In other words, damage mechanics is a versatile framework for modeling fractures large and small.

We agree that the first two paragraphs of Section 3 taken alone might give the impression that damage mechanics is only applicable for modeling small-scale and diffuse fractures, and we wish to dispel this misconception (especially if we inadvertently reinforced it). In our revised manuscript we have re-written the introduction to Section 3 to clarify that damage mechanics can be applied to model the initiation and propagation of macroscopic fractures as well as to "smear" the effects of microscale heterogeneity and microcracking.

Finally, we note that we did discuss (Section 6.2) the possible effects of stress shielding (in addition to mélange stabilization) with respect to the pattern of damage for the rifts in Figure 7. This discussion is consistent with our definition of damage, which is precisely inferred by the influence of fractures on enhanced strain rate. There is no discrepancy here with any "classical" definition of damage, since the physical interpretation of damage varies depending on the choice of equivalence schemes.

5) Most likely, figure 2 and figure 3 have been switched (the captions do not correspond).

Thank you for catching this. The captions are in the correct order but somehow the figures got swapped. We'll make sure this error is corrected upon resubmisison.

6) Line 21 of p 3587: "f>1" instead of "f<1" ?

Yes, this should have been $f > 1$, and the error has been corrected.

References

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