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Interactive comment on "Creep deformation and buttressing capacity of damaged ice shelves: theory and application to Larsen C ice shelf" *by* C. P. Borstad et al.

Anonymous Referee #2

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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 obtained 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.

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..

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).

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.

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

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