The Cryosphere Discuss., 7, C2356–C2367, 2013
www.the-cryosphere-discuss.net/7/C2356/2013/

Interactive comment on "Fracture-induced softening for large-scale ice dynamics" *by* T. Albrecht and A. Levermann

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Received and published: 8 November 2013

General comments

This paper describes a model formulation for representing the softening influence of fractures on large scale (e.g. ice-shelf scale) viscous dynamics. This builds upon earlier work by the authors (*Albrecht and Levermann*, 2012) characterizing the surface density of fractures using a scalar field variable. In this work, the fracture density is used to soften the the ice by feeding back with the depth-averaged ice viscosity through one of two enhancement factors. The model is able to represent sharp gradients in velocity across adjacent flow units separated by fractures or across shear margins of an ice shelf better than a "standard" model with uniform material properties. This work is part

of a growing body of literature on model formulations to account for the role of fractures in glacier and ice shelf evolution, and the work outlined in the manuscript does show promise toward contributing to this sector of the community.

I have only one potentially serious concern about the model formulation, which relates to the specific formulation of the fracture density source term and the associated feedback mechanism with the flow enhancement factor. Aside from that, in general I feel that this manuscript covers too much material in too little detail. I applaud the broad scope of the modeling work applied to numerous ice shelves, but for many particular ice shelves I feel that more questions are raised than answered, especially given that only a single transect of velocity is analysed in most cases. Given my concerns about the model formulation outlined below, I think more might be gained by carefully dissecting the results and model sensitivities for a single ice shelf before moving on to a broad survey of results over many ice shelves (though I note that this concern could be addressed by a simple change in the title and scope of the paper to something along the lines of "fracture-induced softening for ice shelf shear margins"). Otherwise the remainder of my concerns can be addressed with a careful rewrite, paying attention to using more direct and descriptive language and clarifying the graphical presentation of the results. I urge the authors not to be discouraged by the length of my review, as the comments are intended to be constructive and contribute toward improvement of the manuscript.

Specific comments

I'm concerned about the coupled forms of the fracture density source term (Eq. 2) and the softening feedback through the enhancement factor (Eq. 6). This concern arises from two observations of the model formulation and results. The first is the fact that such low values of the enhancement factor E_{SSA} (0.05-1) are needed to tune the model to match observed velocity data when fracture-induced softening is activated. The second is the fact that there are actually two enhancement factors, one that is uniform across the shelf (E_{SSA}) that is used as a tuning parameter and the second through which fracture-induced softening acts. The former concern suggests that the fracture density source term may be inappropriate, and too much "fracture" is created by this term with the result being that an anomalously low enhancement factor is needed to offset this anomalously high production of fracture density. Rather than invoking a duplicate enhancement factor (E_{SSA}) I would suggest that the source term in Eq. 2 should be investigated for problems or possibly reformulated. I would start by comparing it to a more sophisticated source term, such as that proposed and validated by *Pralong and Funk,* (2005), that uses a multiaxial stress or strain rate criterion (rather than a simple uniaxial strain rate) as well as a power-law dependence on the term $(1-\phi)$ rather than a simple proportionality. Whatever the form of the source term, I think more could be learned physically about the initiation and evolution of fracture density by properly calibrating the source term rather than assuming a simple form and then invoking an independent tuning factor to get the model to fit the observational data.

It appears that the formulation of the enhancement factor feedback mechanism is based on physically sound reasoning. *Borstad et al.,* (2012) analytically related the classic enhancement factor E to damage D as $E=(1-D)^{-n}$, which is a similar form as Eq. 6 in absence of the E_{SSA} term. According to this relationship, increasing damage/fracture density leads to increasing flow enhancement. However, the introduction of a leading coefficient $E_{SSA}<1$ in Eq. 6 counteracts this softening feedback. In fact, for E_{SSA}=0.05 as in Figure 8, the overall enhancement factor E_A is less than one for a fracture density below about 0.6 according to Eq. 6. An enhancement factor less than one indicates stiffening of the flow, whereas $E_A>1$ indicates softening enhancement. Thus the bizarre result here is that the flow of Byrd Inlet is modeled using an overall enhancement factor that *stiffens* the flow even when a moderate level of fracture density is present. This suggests a problem with the model formulation, as an enhancement factor so much smaller than one should not be needed to capture at least the bulk flow features in unfractured areas (a couple examples: *Ma et al.* (2012) found that enhancement factors of $E\sim 0.6-0.7$ are appropriate for accounting for anisotropy of

an ice shelf, whereas *Scambos et al.* (2000) used $E=3-8$ for Larsen B). My concern is that the term E_{SSA} may have been introduced post-hoc as a simple way to "tune" the model rather than diving more into the physics underlying the source term, where I suspect the real problem lies.

Aside from that, the discussion of fracturing as a kind of "self-amplified" process (Sections 3 and 5.1, and sprinkled elsewhere throughout the manuscript) is a bit confusing, but I take it you are describing fracturing as setting up some kind of positive feedback process, whereby an initial fracture causes an increase in the effective stress, which causes additional fracturing, and so on in a runaway feedback. I'm not sure this conclusion is supported by your results, nor does it seem consistent with observations. For individual crevasses, this logic would seem to imply that once a crevasse forms, it will continue to grow or propagate, though I'm not sure there are any observations of crevasse depths that continually increase along a longitudinal transect on an ice shelf. Also consider the recent results of *Walker et al.* (2013) who demonstrated that the majority of rifts studied in 13 different ice shelves did not propagate at all over the last decade; they were simply "dormant." If it is common for fractures to initiate, propagate for a short time, and then become dormant, then I hardly think a positive feedback mechanism is operating (or at least the question of what would interrupt such a feedback becomes pertinent).

Finally, can you rule out the role of temperature in accommodating the strong shear across the boundaries between flow units or shear margins that you analyse? If marine ice is present at the base of the shelf in any of these areas, then the ice column will be warmer and will thus deform more readily. *Jansen et al.* (2013) demonstrated that accounting for this warm layer of ice in an ice shelf model can produce the strong shearing across flow units observed in velocity data. It's not clear how (or if) you're treating the ice temperature in your model, which could be a significant limitation of your results. If fracture density is the only model parameter that can vary spatially, then you're implicitly lumping the influence of temperature in with your fracture-induced softening. Therefore it's possible that the temperature alone might explain the sharp gradients in across-flow velocity in some areas where you explain them by fracture-induced softening.

Figures

Many of the figures have components that are difficult to interpret, and could use clarification. In many instances, this could be accommodated by using explicit legends rather than describing each plot figure in the caption (I found myself manually labeling many of the figures to keep the different components straight). Some specific comments on the figures:

Figure 1: The solid and dashed lines need to be labeled more clearly, perhaps starting with a title for the legend. The values used for E_{SSA} (0.6, 0.8 and 0.1) do not cover the range of values used in the paper (0.05-1). For values of $E_{SSA} < 1$, the viscosity of the fractured ice is actually *stiffer* (viscosity ratio greater than one) than for the model without fracture coupling. This is a problematic result.

Figure 2: The contour colors on panels a-c, indicated on the colorbar, are difficult to distinguish. Since no softening or healing is applied in panel d, can you quantify the amount of fracture density "lost" to diffusion along a flowline? It would be useful to know how much of your "signal" you are losing as you advect it.

Figure 3: This is kind of a confusing plot. Perhaps a side-by-side comparison of your new advection scheme compared to a standard first-order upwinding scheme would make more sense?

Figures 4 and 5: The plot panels need more labeling or a legend, as the colors for the different curves are not labeled nor explained in the caption. A conceptual graphic to accompany this plot would be helpful.

Figure 7: The colors and different curves in panel b are confusing. For panel c, is this

one curve or multiple curves? I'm not sure that the schematic for the potential feedback is appropriate, as it can be adequately explained in the text (same for Figure 13).

Figures 9-13: A spatial map showing the misfit between standard-observed and softened-observed cases might be helpful, as the information in the observed vs. computed speed panels can be conveyed without the figure by simply stating the rmse for each case. How is the FESOM melting-factor described in the captions used in the model? For Figure 12, it's not clear which arrows correspond to which plots.

Line-by-line Comments

- p. 4502, Lines 5-6: was your objective really to better *understand* the role of fractures? The objective implicitly presented in the manuscript was to *represent* the role of fractures in a large scale model and compare the results to observations. You didn't conclude with any new understanding about the role of fractures, so you might consider changing the stated objective here.
- p. 4502, Lines 12-13: this is a confusing sentence
- p. 4502, Line 16: how does the model account for climate-induced effects on fracturing? Or do you mean that it is expandable to possibly account for such effects?
- The terminology of "fracture-coupled processes," which is used in many places, is a bit awkward and confusing.
- p. 4503, Lines 7-9: The references at the end of this sentence do not support the assertion that fractures play a fundamental role in ice streams and ice shelves.
- p. 4503, Line 13: what do you mean by "expand"? Do the fractures grow longer or wider?

- p. 4503, Lines 14-15: The description is confusing, as you're already talking about fractures that are advecting with the flow. How can the stresses change to activate fracture formation if the fractures are already present?
- p. 4503, Lines 17-18: Define "effective direction" and provide reference(s) to support the assertion at the end of the sentence.
- p. 4504, Lines 10-13: This sentence, including the references at the end, applies *after* the collapse of an ice shelf, not to a fracture-weakened ice shelf.
- p. 4504, Lines 27-28: "exemplarily investigated" is awkward and another example of the overuse of verbose language when more concise language would be more clear ("investigated" alone would be sufficient here)
- Introduction: since what you're doing is closely similar to continuum damage mechanics, it would be worth discussing the different approaches to representing fractures (e.g. previous studies using damage mechanics and fracture mechanics to represent fractures in ice shelves) in the Introduction to better frame the context of the study.
- p. 4505, Line 9: if the characterization of fracture density only applies to subgrid-scale fractures, does that imply that rifts cannot be handled by the model? If not, isn't this a significant limitation of the model, since rifts are likely more important than either surface or basal crevasses in many places?
- p. 4505, Line 16: I would remove the word "probability" as you are not applying a probabilistic framework for fracture initiation (same for Line 20).
- Equation 4: the physical justification described for fracture healing applies primarily to surface crevasses. What about basal crevasses, which presumably have a larger influence on the stress regime?

- p. 4506, Lines 11-12: tell the reader what these senstivities are, at least briefly, rather than making them chase down the details in another paper.
- p. 4506, Lines 16-18: What simplifications are you making from standard continuum damage mechanics? Be explicit, as this can shed light on any limitations (or possible advantages) of your approach.
- p. 4507, Line 1: what are these "distinct dynamic characteristics"? How distinct are they? Be more specific and targeted with your language.
- p. 4507, Lines 3-6 and Equation 5: here you're making it sound like you're applying continuum damage mechanics, yet this is not the form of the viscosity that would result from the strain equivalence principle because you've manually inserted an extra enhancement factor (Equation 6). Furthermore, the *strain rate* is unmodified by the equivalence mapping, but the stress balance equations are modified.
- Equation 6: I doubt it is coincidental that your enhancement factor formulation takes the form of $E_A \propto [1-\phi]^{-n}$, which is precisely the analytical form derived by *Borstad et al.* (2012), so you should probably reference this study here.
- p. 4508 Lines 1-2: Confusing sentence. What is the discontinuity, and how is this "ambiguous"?
- p. 4508, Line 8: Does not enhancement apply to all modes of deformation, not only shear? You seem to mention only shear enhancement in the manuscript. Is there a reason for this?
- p. 4508, Line 10: The description of hitting the initiation threshold here is confusing, as you're describing fractures that are already present and being advected with the shelf.

- Section 4.1: Some of this material seems like Background instead of Methods, but you also seem to cover some Results here.
- p. 4508, Line 16: According the strength of materials theory, failure of a material occurs when the stress exceeds a threshold, typically associated with the strength of the material. This is not the case for fracture mechanics, however, which was developed when it was observed that materials can fail at nominal stresses *less* than the strength of the material due to the intensification of stresses caused by flaws in the material. You might keep this in mind in describing the failure criteria in this section since you are mixing and matching between strength criteria and fracture mechanics criteria.
- Equation 7: why show the Tresca criterion here if you're using the von Mises criterion?
- p. 4509, Line 11: "...the half-length of *assumed* preexisting edge cracks..."
- p. 4509, Line 20: The fracture toughness is a material property. The stress intensity factor can vary depending on the presence of neighboring fractures, but not the fracture toughness.
- p. 4512, Line 10: unsubstantiated claim, more detail and a reference needed here.
- Section 4.4: Can you discuss the potential variability in the softening influence of your inferred fracture density depending on the nature and location of the fractures? It would appear that the surface expression of a basal crevasse gets "counted" the same as a surface crevasse, even though the basal crevasse might be expected to have a much greater influence on the flow and stress regime since it occupies a much greater fraction of the ice thickness. Furthermore, how are rifts handled? It seems to me that some kind of weighted fracture density calculation might be more appropriate, whereby a rift gets more weight than a basal

crevasse which gets more weight than a surface crevasse. Of course this would assume that you could distinguish the difference between surface crevasses, basal crevasses and rifts in your imagery.

- p. 4514, Lines 7-8: Define "SOR." Also, using the 2007-2009 velocity data as Dirichlet Boundary Conditions for the inlets of Larsen A and B ice shelves hardly seems appropriate given that the tributary glaciers accelerated by 3–8 times following the collapse of these ice shelves. Shouldn't these velocities be scaled down to represent more appropriate values when the ice shelves were present?
- p. 4514, Lines 10-14: this description is confusing. Can you elaborate and clarify?
- p. 4514, Line 24: What do you mean by "ice-free" walls? Are these frictionless boundaries?
- For the ice shelves, are you using an equivalent ice thickness or the actual thickness of the ice shelf? This makes a difference for computing stresses within the shelf, which will impact where fractures are predicted to form (*Kenneally and Hughes,* 2004).
- p. 4515, Lines 9-10: is the healing physical then, or is it due to numerical diffusion? Can you distinguish between the two, or quantify their relative significance?
- p. 4515, Line 19: Is this really hysteresis? I'm not sure I would interpret this result as the system having some kind of "memory."
- p. 4516, Lines 1-2: Which parameters? How are they "roughly" estimated?
- p. 4516, Line 9: Define of quantify how the results are "reasonable"
- p. 4516, Line 18: the orange contour lines very small and difficult to resolve in the figure.

- p. 4517, Line 11: this is a threshold stress, not a fracture toughness (same on next page, Lines 4-5)
- p. 4519, Lines 9-11: Enhancement factors larger than 1 are actually more common than values less than 1 (e.g. *Ma et al.,* 2010), so some kind of explanation or justification is needed here.
- p. 4520, Lines 9-13: This makes it sound like you modeled grounding line retreat. Did you? If so, you should expand on this (probably a lot) here. If you're describing more of a hypothetical feedback scenario, then make this clear.
- p. 4520, Lines 16-18: The ice flow dynamics of glaciers and ice sheets is already nonlinear. Hysteresis is not the same thing as irreversibility.
- p. 4520, Lines 25-27: reference needed at end of sentence.
- p. 4521, Lines 15-16: actually, the tensile strength and fracture toughness of ice are not very sensitive to temperature (*Schulson and Duval,* 2009), even though this claim gets repeated frequently in the glaciological literature.
- p. 4522, Lines 15-16: I don't think you've substantiated this claim.
- p. 4522, Lines 17-19: This is confusing. Are you claiming that you've accounted for all the relevant softening processes you've listed, including microscale processes and damage-induced anisotropy?
- p. 4523, Lines 1-17: This is a nice discussion, but it might be better placed (or repeated) near the beginning of the manuscript (Introduction or Background) to better frame the context of the study.
- p. 4523, Line 21: I'm not sure you accounted for fracture "interactions" explicitly, is this what you meant here?

References

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Interactive comment on The Cryosphere Discuss., 7, 4501, 2013.