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The Cryosphere Discussions

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

T. Albrecht and A. Levermann

torsten.albrecht@pik-potsdam.de

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Response to the interactive comment by Referee #1: Jeremy Bassis

Our point-by-point responses to the comments by Referee Jeremy Bassis are detailed below. Referee comments are printed in blue font followed by our responses in black.

Overview:

This study describes an effort to include a prognostic simulation of the effect of

fractures on the dynamics of ice shelves. The authors re-visit a damage mechanics-like theory of the bulk effect of fracture on flow and heuristic means of evolving the damage parameter. The authors then use this framework to show that fractures can very effectively decouple sections of ice shelves from their embayments and pinning points. Moreover, the fracture evolution framework proposed appears to yield reasonable estimates of the velocity that previous researchers could only replicate by tuning arbitrary enhancement factors in their model. Using this framework, the authors then show that small changes in parameters can lead to large changes in the dynamic response of ice shelves.

Overall, I think this is a very well thought out and written study that makes novel contributions to a challenging problem. In particular, I'm impressed with how well the model appears to reproduce some of the decoupling of ice flow from highly fractured regions. This can have a dramatic effect on the ice dynamics and previous studies have been forced to replicate this behavior with parameter tuning. I do have a few questions about the theoretical formulation. Some of these may be addressed by future work and are not meant to impinge on the quality of work presented here. Others seek clarification on some of the statements in the manuscript. I encourage the authors to consider these questions, but leave it at their discretion the extent to which they wish to alter the manuscript in response to these relatively minor points.

1. Physical meaning of fracture density and relationship with damage. In section 2 the authors re-introduce the concept of a fracture density that they first introduced in a previous paper. The authors, however, use the terms damage and fracture density interchangeably. Should readers interpret fracture density ϕ as a mere relabeling of damage (at which point why not stick with the more commonly used term damage). Or should we view fracture density as something different, perhaps less general than damage. This is not a mere question of semiotics because, at least for the scalar form used here, damage can be related physically to the ratio of the

volume of voids in a control element to the total volume of the element, something that in principal (if not practice) can be measured. However, the form used here relies on depth-integration of both the dynamics and damage variable and, except when stress is independent of depth, one loses the simple interpretation of damage in a depth-integrated model. But the terminology fracture density does not imply to me that we are encouraged to interpret fracture density as a depth-averaged effective damage variable. It would be useful to clarify the terminology and explain how it relates to previous studies and especially how it is similar to or different from damage, as defined in previous studies.

We are very grateful for the referee's positive evaluation and try to address his points appropriately. And we agree that some clarification is needed on the terminology. We have chosen the name "fracture density" to express a simplification of the damage formalism (less general), to reduce the number of parameters, that need calibration, e.g. with laboratory experiments. We rather want to investigate the first-order dynamic effects of such a reduced formulation. But nevertheless it is a scalar damage variable in the classical sense. We are yet not able to consider explicitly the vertical extent of fractures and hence stick to a depth-averaged fracture-density, which allows for comparison with the areal fracture density inferred from ice shelf surface observations. However, we plan to expand the concept towards three dimensions in future.

2. Effect of depth of fractures on fracture density. Does fracture density include the depth of fractures or is it just the horizontal extent of fractures? Imagine a situation in which you have a series of very closely spaced, but shallow surface crevasses. Now compare this to the case where you have a single deep and very wide basal crevasse. The total volume of the fractures may be equal, but the number of basal crevasses is much less than the number of surface crevasses leading to smaller number density. I would (perhaps naively) imagine that the bottom crevasse would

have a more pronounced effect on the dynamics of the ice shelf than the shallow crevasses. How does this fit into the modeling framework and how would you define the fracture density for each case?

We can not yet adequately model the vertical propagation of fractures, however we implicitly assume that fracture growth affects the effective vertical cross section of the ice body through which membrane stresses are transferred, according to the strain equivalence principle (Borstad et al., 2012). Information about the vertical distribution of fractures is not considered in the definition of the observed fracture density (introduced in Albrecht and Levermann (2012)), where only surface features are evaluated, of which actually most are associated with the surface impressions of bottom crevasses or rifts (surface crevasses seem to be small to be seen on the satellite images). Nevertheless, this definition provides a comparison of the pattern of evolving fracture bands, its limitations are now more clearly discussed in the revised manuscript. Regarding the two mentioned cases: The shallow, closely spaced surface crevasses would not be seen at the surface nor would the have a significant contribution to the softening effect, since the reduced cross-sectional area can be neglected. The deep bottom crevasse in contrast, would leave an impression on the surface and would reduce the effective cross section of the ice body, such that its softening effect can be large. In order to account for the variety of different fractures and their effects on the flow, the introduction of a fractal dimension interpretation could be helpful here.

3. Role of the critical stress in promoting fractures. The authors plausibly argue for a critical yield stress above which fractures initiate and examine a few different possibilities. My concern with this formulation in the context of a pseudo-damage type variable is that for a freely floating ice shelf the deviatoric stress τ scales roughly with

ice thickness H

$$\tau = \rho_i \, g \, H \left(1 - \frac{\rho_i}{\rho_w}\right) \quad (1)$$

where ρ_i is the density of ice, ρ_w is the density of sea water and g is the acceleration due to gravity. This implies that the stress increases with ice thickness and one would get deeper crevasses in thick ice tongues. However, when fracture depths are computed based on the various flavors of fracture mechanics, one finds that the ratio of fracture penetration depth to ice thickness is constant. Thick ice shelves have bigger crevasses than thinner ice shelves, but the ice is also thicker so the "damage" remains constant (see, e.g., Bassis and Walker, 2012, for a derivation of this). I wonder if you want a dimensionless criterion for fracture initiation that takes into account the ratio of the Von- Mises stress (or some other criterion) to the hydrostatic confining stress. I proposed something similar purely heuristically in Bassis (2011). I wonder how much this would affect the results and if it might not help explain some of the variability in critical stress thresholds the authors find.

This is a very good idea and we would like to follow on it in future studies. We compared for four cases the respective ratios and detect a clear tendency here:

Byrd: H=700m, σ_{cr}=110kPa, ratio: 0.151
Evans: 1400m, 140kPa, ratio: 0.096
Filchner: 500-800m, 130kPa, ratio: 0.179
Larsen B: 200-500m, 60kPa, ratio: 0.144

However, the main finding of this publication is, that the simplistic coupling of fracture density and ice flow improves the representation of the relevant shear zones. We may build on that and improve and generalize the model in future.

4. What happens to the density of ice if fracture density increases? A fracture

density near one implies an open rift, but an open void in the ice requires a traction boundary between rift walls and the ocean-air-melange filling the rift. In damage mechanics simulations that I've done, we usually remove nodes that are sufficiently damaged. This may be a question for future work, but it seems like you might want to allow for fracture density to alter the density of the ice shelf so that you can simulate a "void" filled with sea-water as opposed to a region of the ice shelf that is heavily fractured and has no cohesive strength, but is otherwise mechanically equivalent to the rest of the ice shelf

The density of the ice in our model remains constant. However, we may think about the J. Bassis' comment on the boundary effects in future studies and other technical aspects of the model.

5. What effect does high fracture density have on the surface topography of the ice shelf? If regions of large damage are straining much faster than regions of low damage, does this create locally dynamically thinned region that are lower than the surrounding undamaged ice?

As pointed out correctly by J. Bassis, strongly straining ice decreases locally the thickness. However, fracture softening leaves a less pronounced signal on the evolution of the ice thickness than on the distribution of the ice velocity, why we prefer to use ice speed for our comparisons. The thinning may induce other effect like the accretion of marine ice, which are not considered so far.

6. Numerics of advection. I'm not an expert in numerics, but I was surprised that the authors are using what looks like a variation of first-order upwind finite differencing. If accurate advection of narrow fracture fields is required, I would have thought that a Gudunov, beam warming or some other advection scheme developed specifically to deal with shocks would be appropriate.

We are aware of the variety of transport schemes and we use some variant of the flux-conservative scheme for the ice thickness evolution (and also shock-like front migration). However, the diffusion direction of concern for the fracture density is transversally to the main flow in two dimensions. And regarding this aspect, we have not found any comparable scheme in literature, which does not mean, that it may have been written down somewhere.

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