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

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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.

2. Effect of depth of fractures on fracture density. Does fracture density include the

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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?

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

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

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.

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4. What happens to the density of ice is 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

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?

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.

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