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Interactive comment on "Iceberg calving of Thwaites Glacier, West Antarctica: Full-Stokes modeling combined with linear elastic fracture mechanics" *by* Hongju Yu et al.

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This study seeks to simulate surface and basal crevasse propagation using a variety of viscous models in an attempt to better understand the processes responsible for iceberg calving. The authors find that extra bending stresses near the grounding line that are resolved in full Stokes models are needed to accurately simulate the penetration depth of basal and surface crevasses. Moreover, the authors find that simulated crevasses are qualitatively similar to observed crevasses. Overall, I think this study is both interesting and novel and highly appropriate for publication in Cryosphere Discussion. I do, however, have two major points of concern along with a few more specific comments.

The first comment relates to the overarching goal of this study, which is presumably to better simulate iceberg calving. Here, I would like to see the authors make more detailed comparisons between observations and model predictions at the macro and local scale. For instance, the authors have strong conclusions about the role of different models in simulating crevasse penetration depths in Thwaites Glacier. But I had a hard time deciphering how well any of the models is able to reproduce the current ice tongue length of Thwaites. If the full Stokes model is able to accurately simulate the length of Thwaites Glacier tongue then this should be noted and celebrated. If the models have large discrepancies than, in the interest of honesty, this should also be acknowledged and discussed in light of (possible) missing processes.

Similarly, I would like to see the authors compare their simulated crevasse shapes with observed crevasse shapes (to the extent available). I realize that the radar probably doesn't fully resolve crevasse shapes, but one might be able to examine (at least to first order) penetration height and aspect ratios. Any such comparison would strengthen quantitative links between the model and observations, especially given the authors hints that such comparisons are positive.

My second comment focuses on the methodology and, more specifically, the description of the methodology. To start, it is not obvious to me why stresses computed using a viscous model can be used to drive an elastic fracture mechanics model. Here the authors could assist readers by summarizing the tenets of the theory that they are using to map viscous to elastic stresses or by computing elastic stresses and comparing them to viscous stresses. My uncertainty in the method goes a bit deeper here, both in the physical model posed and the numerical implementation. I will start by describing the questions I have about the physical model before outlining some more numerical concerns. None of these issues are fatal, but point towards a need for a more thorough description of the model assumptions and implementation.

Physical crevasse model description:

1. I think Equation (20) is wrong. If σ_{xx} is the longitudinal stress then why is an additional cryostatic stress added? I'm also not sure that it is permissible in an elastic model to merely add a water pressure to the walls either without considering the effect on the stress field in the elastic body, but more on that later. Note also that Van der Veen does his computations using resistive stresses, not longitudinal or deviatoric stresses. The typo in Equation (20) makes it unclear to me that this translation has been done appropriately.

2. The assumption of elasticity and water (or air) filling crevasses tells us exactly the shape of the crevasse. Perhaps this is already taken into account, but I had a hard time figuring out the initial condition. Formally, we know that for an elastic material, the width of the crack is related to the pressure on the walls through an integral relationship (see, e.g., Equation 2.1 in Self-Similar Solutions for Elastohydrodynamic Cavity Flow by D. A. Spence and P. Sharp Proceedings of the Royal Society). Water pressure only enters into this equation through the expression for the water pressure on the crack walls. For a narrow aspect ratio crack (width to height), we can make the lubrication theory approximation, whence an equation for crack width (or rate of change for crack width) can be found. See for example, Lister (1990, Buoyancy-driven fluid fracture : similarity solutions for the horizontal and vertical propagation of fluid-filled cracks). Of course the water can also freeze on the walls, but I don't think the authors are dealing with thermodynamics.

3. Building on the previous point, the crevasse aspect ratio can be estimated by examining the ratio of pressure opening a crevasse to Young's modulus. For a 1 MPa stress opening a crevasse (an overestimate) and a typical 1-10 GPa Young's modulus I find that the aspect ratio of the crevasse is 0.0001-0.001. This translates into maximum crevasse widths (initially) that are of the order of $\sim$1 cm. For more typical stresses of the order of 100 kPa, I find initial widths of the order of a few mm. As a crude guess, one could take elliptical crevasses and evolve these, which is what I'm guessing the authors have done here, but this would seem to be incompatible with the 5 m resolu-

tion near the crevasses! More significantly, it shows that the crevasses the authors are considering (>200 m width) are **incompatible** with linear elastic fracture mechanics. Outside of the initial condition, I'm not convinced that LEFM is compatible with the fractures simulated. Of course, one might argue that new fractures are initiated at the tips of old fractures and the new fractures are elastic, but this needs to be made explicit.

4. These issues aside, even in the most simple LEFM implementations, we start with a starter crack. What size starter cracks is assumed?

Physical model implementation:

1. How are the crevasse implemented in ISSM? From the Figures I get the impression that crevasses are merely tracers and nodes are not actually removed when propagating crevasses. This needs to be better explained

2. If crevasse are implemented as tracers then they have no effect on the stress/strain rate field and this is probably fine for narrow crevasses, but is increasingly problematic for wide crevasses. We simulated crevasse evolution in Bassis and Ma (2015, 10.1016/j.epsl.2014.11.003) using an analytic calculation perturbation theory and found similar results to those shown, but found that the interaction between crevasses and the background strain rate was crucial to crevasse growth. This is particularly true for wide crevasses, which can result in accelerated flow into crevasses "healing" them. (We also found that large basal melt rates might erode basal crevasses, but this is more speculative.) More details are needed to assist readers to understand how the crevasses are initiated and evolved numerically. If this is a purely Lagrangian method, then this needs to be explained. If nodes are removed then this also should be explained. (There are thermodynamic and numerical issues associated with node removal, so these should be discussed if node removal is done.)

Technical comments:

Page 2, Line 10: The Nye zero stress model doesn't necessarily underestimate the stress-concentration at the tip of crevasses. In fact, as Weertman and others have shown, the Nye zero stress model corresponds to the LEFM problem of closely spaced crevasses. Moreover, even for isolated crevasses, ice behaves like a viscous fluid over long time scales. Hence, it is unclear to me that crevasses should have a stress concentration that is equivalent to that in an elastic plate. In other words, it is unclear to me why the Nye zero stress model isn't the more physically accurate model over long time scales and/or for closely spaced crevasses.

Page 2, Line 25: Here the authors should comment on the appropriateness of a flowline model that neglects lateral drag. What width are the authors using for the flowline (or are they assuming the width is infinite)?

Page 3, Line 5: I don't understand the need for a damping term in the full Stokes equations. The buoyancy term on the bottom acts like a spring and, I thought, the solution, then includes the additional degree of freedom zb?

Equation 14: What is the physical reason for a linear sliding law? I thought prior research by Ian Joughin for Pine Island suggested a Weertman or plastic sliding law was most appropriate and would have (naively) thought that similar laws would be most appropriate for Thwaites as well? I apologize if I'm mistaken about this.

Page 8, line 20. So only crevasses wider than 200 m are considered, but these crevasses would already appear to violate the assumptions of the LEFM?

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