The effect of hydrology and crevasse wall contact on calving

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RC2: 'Comment on tc-2022-37', Bradley Lipovsky, 18 May 2022

Dear Editorial Staff and Authors,

This manuscript by Zarrinderakht and co-authors was simply wonderful to read. It constitutes a significant advance in the field of glacier fracture mechanics and is obviously a stepping stone towards bigger things. I enthusiastically support the publication of this manuscript in the Cryosphere. I do have a few questions and comments that I hope will improve the quality of the manuscript. Many of these draw connections to my own work on glacier fracture mechanics, which isn't to suggest that my work be given any special pedestal, but is rather just to share how I think about some of the physics of these kind of problems. Please feel free to take or leave this work as you see fit.

All the best,

Brad

Our response to the referee is in red and italicized.

Questions and Commetns

1. Why assume the crack propagates slowly (i.e., equation 19)? We know very well that crevasses in ice shelves are seismogenic. See, for example, Aster et al., 2021, who interpreted the unique seismic characteristics of certain impulse ice shelf seismic observations to be caused by crevasses growth. In order for crevasses to generate seismic waves, they must propagate at inertial (or near-inertial) velocities. Furthermore, the full inertial treatment of crevasse growth maintains the form of Equation 18, it just changes the last multiplicative term on the right hand side. This situation was treated by Lipovsky (2018) which to my knowledge is the first-, and prior to the present manuscript the only, study to examine the dynamics of glacier fracture growth (albeit with horizontal propagation, although the authors will appreciate that the math is the same). If the crack does move suddenly then water compressibility may be important (also, see below). The equations necessary to treat compressible pressure gradient flow along hydraulic fractures were given by Lipovsky and Dunham (2015) with application to hydraulic fractures in glaciers.

We agree that a truly complete model of calving should indeed incorporate a full dynamic treatment of crack propagation, with inertial terms retained in the momentum balance equations, and accounting for elastic waves. The text on lines 202 onwards of the original submission was intended to make that clear ("In a general, the computation of K_I during fracture propagation then requires a dynamic model in which inertial terms are not omitted in equation (2).") The point *is however that doing so renders the approach taken in our paper entirely inapplicable: in particular, it does not appear to us as though a simple solution for K_I in terms of a small number of forcing parameters, with crack length(s) as the only dynamic variable(s), would be available: instead, we require a dynamic solution that resolves the displacement field throughout the domain and in time.*

Unfortunately, the Lipovsky (2018) paper does not seem like a viable template for including inertial effects more completely in what we are attempting to do, though we may be mistaken. The way we read the latter paper suggests that wave propagation is computed using a Fourier transform in time and space, thereby avoiding any coupling with a crack whose size changes over time ("The entire geometry is assumed to be translationally invariant in the x-direction", appendix A1 of Lipovsky 2018). In the same vein, the analytical form of eq (14) in Lipovsky suggests the work is restricted to a nascent crack whose length is much smaller than the size of the ice mass, in that case its horizontal extent.

Our interest here is in a crack that propagates through a significant fraction of the ice thickness, and ultimately all the way across. As a result, if crack propagation does occur at seismic time scales, there can be no decoupling between crack propagation and wave propagation in an evolving domain geometry, precluding the use of Fourier transforms in time in any meaningful way (since the spatial domain changes over time!). As described above, it seems to us like a simple model with only crack length as the sole, scalar dynamic variable of time goes out of the window: instad, one would have to account for displacement everywhere in the ice as a dynamic variable of time and *position throughout the calculation. At a single stroke, we have to go from a one-dimenisonal dynamical system (or an n-dimensional one if modelling n interacting cracks with prescribed orientation) to an infinite-dimensional one.*

In the spirit of trying to do something *tractable that stays as close to the underlying physics as we can afford computationally, we have opted to stick with the pseudo-static stress field approximation as described in section 2.2. We had hoped that the text on lines 203 of the original submission onwards (" Solving a time-dependent problem that captures elastic waves renders our just-stated objective of computing fracture propagation for many forcing parameters intractable. Short of solving a full dynamic crack propagation problem, we can use the semi-analytical theory of Freund (1990) ... ") would be sufficient to make our intent (and its limitations!) clear, but we will strengthen the description to make clear that the approximations we make are expedient rather than necessarily accurate.*

2. I realized when reading the caption of Figure 3 ("...even where the crack is closed...." [sic]) that the authors assume hydrostatic pressure for what appear to be closed-off water blobs. Could the pressure be cryostatic? If so, this would provide additional reason to treat the compressibility of the water.

Closed-off water blobs can be treated in two ways. First, we can assume that roughness in the crack surface leads to a hydraulic connection being maintained with the surface, and water pressure being prescribed by the surface drainage system; this is the assumption we make here. The second way is to treat them as hydraulically isolated water bodies. In that case, it is natural to presribe not a pressure as such, but a hydrostatic *pressure distribution within the blob (so there is no pressure gradient and no water flow inside the blob) with the mean pressure to be determined. That mean pressure is then determined by the need to maintain a fixed water volume in the blob. That, however, is our second* *hydraulic scenario, and it requires a prescription of the size of that water volume, which cannot be determined from a water table height. To avoid adding additional, and poorly-constrained physics of the model, we chose to assume a hydraulic connection is maintained in the context of the first hydrology model involving a prescribed water table.*

3. Section 2.1 Model description. Some readers might be interested to know that Lipovsky (2020) also used viscous pre-stresses in LEFM calculations. To my knowledge, this publication introduced these concepts in glaciological research. I gave a different physical explanation of the viscous pre-stresses but the form was mathematically identical to that used in the present manuscript. I do prefer the physical explanation given in the present manuscript, but I'm at least encouraged that the math is the same since I grappled with this for a while.
We now reference Lipovsky (2020) in this context.

4. Experience hiking around glaciers with water-filled crevasses tells us that crevasses are often up to a meter wide (or more). It is unlikely that this meter of opening is due entirely to elastic stresses, as one can calculate that this would require enormous and unrealistic stresses. The explanation for the opening is instead that the ice surrounding the crevasse has deformed through flow. The crack would have non-zero width in the absence of the elastic tensions. In this case, not all crack closure would result in contact. It is therefore worth noting that —in at least some cases— negative crack opening (i.e., crack closure) does not result in contact, and instead simply results in the crack getting narrower but not having walls that touch. I'm curious now: can the numerical method in this manuscript handle nonzero initial crevasse widths?
Yes, boundary element method is capable that and in general working with any geometry of the crack. *As pointed out, these (older) water filled crevasses have likely attained their shape in part due to viscous deformation of ice after their formation. Part of our ongoing work deals with a coupled model for long-term viscous deformation and (presumably episodic) linear elastic crack propagation, starting with a visocusly defomred geometry. The complications involved are however beyond the scope of this first paper.*

5. If, on the other hand, the crevasse is assumed to be so narrow that the walls could touch, then fluid viscosity should become important [see again LD15]. Maybe these points are already acknowledged in line Line 150, where the reader is cautioned that more complexity in the fluid flow is warranted.
Yes, our intention there was indeed to acknowledge that a more complete hydrofracture model would be a desirable improvement over our work, with propagation speeds that are controlled at least partly by pressure gradients associated with the need to fill the opening crack with fluid. We would love to tackle this in future but the results of the present study still seemed worth reporting as they are.

6. The first sentence of this section seems to imply that the width of the domain is an important parameter in the problem. I don't understand why this would be the case if Rxx is (conceptually at least) treated as a boundary condition at great distances (i.e., +/- infinity). Numerically, shouldn't the simulations be run for a sufficiently large domain width so that the solutions do not depend on this parameter?

The intention is indeed to regard W as wide, emulating an infinite strip of ice. Since actual computations require a finite value of W, it seemed wise to report it for the sake of reproducability. We did test whether our results were sensitive to the choice of W and made sure to pick a value for which they were not.

7. I am confused by the results in Figure 3. The model seems to be treating the case with constant water volume, but yet the water volume clearly changes from figure 3b1 to 3b2. I think this is supposed to mean that some water is stored at the surface. But if water is stored at the surface, then the appropriate tractions ought to be applied at the surface of the glacier. Instead, the surface of the glacier is taken to be traction free (Equation 3). It seems like a rigorous treatment of this situation must either include the surface load or else omit crevasse depths that are too shallow to hold the prescribed water volume.
Yes, excess water is assumed to be stored at the surface as stated in line 145 of the original submission ("Otherwise, if the proscribed volume cannot be accommodated in the crack, then h_w = 0 and the excess is stored at the surface"). The point is that, for a model with small displacements, the water volumes that can be stored in a crack (with an $O(1)$ scaled water volume value β necessarily correspond to a very thin layer of water when spread out over the ice surface, at least if domain width W is comparable to or larger than ice thickness as we do. The equivalent water layer thickness at the ice surface is then comparable to the width of the crack, which is much smaller than the ice thickness. The hydrostatic water pressure generated by this layer is small compared with the water pressure generated by a vertical water column in any crack whose length is much greater than its width. As a result, we ignore the effect of the surface water volume on stress boundary conditions.

8. Figure 4 and 5 are simply wonderful contributions to the literature on glacier fracture mechanics. Thank you for this.
Thank you!

9. Figure 6 / Line 455. See comment above about the surface load due to a lake. As I understand it, the model essentially has the water "coming from nowhere". Maybe the surface loading could resolve the paradox of stability at high prestress. There's an analytical SIF in Tada (2000) that you could compare to, see their section 8.9.
As we point out immediately above, for the water volumes we have in mind here, with $O(1)$ values of β, changes in normal stress at the ice surface will be negigible. We suspect the comment is motivated by the effect of sizeable surface lakes on ice sheets. The volume contained in these seems to us likely to be much larger than a volume that could ever by a crevasse that partially penetrates through the ice; effectively we would have β much larger than any of the values shown in figure 6, and all but the shortest surface cracks necessarily unstable to full propagation through the entire ice thickness. Additional aspects of surface lake hydrofracture however really are not the purpose of the present paper, interesting though it would be to investigate that.

10. Discussion, particularly the "problem" on Line 676: I think this same issue was discussed by Rist et al (2002). Their solution was to introduce back stress from sidewall coupling. Maybe I'm wrong and they were solving a different problem, but either way I would appreciate a clarification.

Back stress is indeed likely to play a role in stabilizing ice shelves. However, near the calving front of an ice shelf, the very stress boundary condition that results from the usual imbalalnce between cryostatic and hydrostatic pressures at the calving front still dictates values of $\tau \approx 0.05$; back stress is a cumulative effect that reduces τ below the "unconfined shelf" value of 0,05 as you move away from the calving front, but it does not help avoid the "problem" near the grounding line itself.

References:

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