The effect of hydrology and crevasse wall contact on calving

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RC1: 'Comment on tc-2022-37', Anonymous Referee #1, 08 May 2022

This article presents a linear elastic fracture mechanics (LEFM) approach to estimate the penetration depth of water-filled crevasses in an ice shelf. The key novelty is that the authors consider the introduction of crevasse generates elastic stress in the ice shelf, which is otherwise at equilibrium due to viscous stress. The proposed model is an improvement over the van der Veen (1998a,b) and Lai et al. (2020) in that it considers crack wall contact using the discontinuity boundary element method. With regard to water in crevasses, the paper considers both fixed water table and fixed water volume injected, which leads to different propagation conditions. The article is generally well written from Section 3 onwards, but Section 1 and 2 have a few typos and confusing sentences, which can be easily fixed. The conclusion of the paper is long and a bit hard to follow. Overall, I found the article is a good contribution and I recommend it for publication with minor revisions.

Our response to the referee is in red and italicized.

Detailed Comments:

1. The introduction can be improved, as I found a few typos and grammatical errors. Also, it does not acknowledge a lot of prior work on this topic. For example, the article cites Lipovsky (2020) for numerical approaches for LEFM, but it was previously introduced in an article by Jimenez and Duddu (2018). https://www.cambridge.org/core/journals/journal-of-glaciology/article/on-the-evaluation-of-the-stress-intensity-factor-in-calving-models-using-linear-elastic-fracture-mechanics/0378315BDB37E88E37B1B07F6BC60426

   Some more literature reviewed and cited.

2. Replace the usage of the word "torque" with "moment". In physics, the turning effect of a force is generally termed as torque, but in mechanics torque stands for torsional moment, whereas the seawater pressure on an ice shelf causes a bending moment.

   In our understanding, the definition of 'torque' suggested by the referee is not universal to continuum mechanics, but is indeed typical usage in continuum mechanics as practiced by mechanical engineers, see https://en.wikipedia.org/wiki/Torque

   Counterexamples using 'torque' in the same sense as we do within glaciology can be foundin e.g. Lipovsky, B.: Ice Shelf Rift Propagation: Stability, Three-dimensional Effects, and the Role of Marginal Weakening, The Cryosphere, 14, 1673–1683, 2020.

Scambos, T., Fricker, H.A., Liu, C.-C., Bohlander, J., Fastook, J., Sargent, A., Massom, R., and Wu, A.-M.: Ice shelf disintegration by plate bending and hydro-fracture: Satellite observations and model results of the 2008 Wilkins Ice Shelf break-ups, Earth Planet. Sci. Lett., 280, 51–60, 2009.

Evatt, G. and A.C. Fowler, A.C., Cauldron subsidence and subglacial floods, Ann. Glaciol., 45, 163–168, 2007

For consistency with this earlier work, we intend to keep using 'torque' as in the original submission.

3. The model considered here is not a Maxwell model, as mentioned on page 3, line 75. In a Maxwell-type, the viscous stress must be equal to the elastic stress. The strains are additively split. I believe the assumption of this paper is a compressible Kelvin-type model. The introduction of the crack within an otherwise viscous ice shelf at equilibrium leads to elastic stress perturbations. These elastic stress vanish on the boundary far away from the crack. This is better clarified elsewhere in the paper, but not in the model description early on.

We disagree. The model we use is the appropriate limit of an elastically compressible Maxwell model for the case of an imposed change in stress that occurs over a time scale much longer than the inertial time scale (as discussed in the paper) and much shorter than a single Maxwell time. That change in stress obviously occurs here due to the change in boundary conditions introduced by propagation of the crack. As the referee states, elastic and viscous stresses are the same and strain is additive. The point is that, if there is an abrupt change in stress field relative to a pre-existing stress, then the viscous strain is negligible at the time scale under consideration (much less than a single Maxwell time). As a result the change in stress *can simply be related to elastic strain through an elastic rheology. Meanwhile, if the pre-existing stress (labelled the "pre-stress" in the paper) was slowly varying prior to propagation to the crack, then that pre-stress is related to the velocity field that existed just prior to the propagation of the crack by a simple viscous rheology.*

Mathematically, an elastically compressible upper-convected Maxwell fluid rheology can be written in the form

$$\frac{(1+\nu)\delta_{ik}\delta_{jl}-\nu\delta_{ij}\delta_{kl}}{E}\overset{\triangledown}{\sigma}_{kl}+\frac{1}{2\eta}\left(\sigma_{ij}-\frac{1}{3}\sigma_{kk}\delta_{ij}\right)=D_{ij}. \tag{1}$$

where η is viscosity, v_i is velocity, $D_{ij}=(\partial v_i/\partial x_j+\partial v_j/\partial x_i)/2$ is strain rate, and the superscript $\triangledown$ denotes the usual upper-convected derivative

$$\overset{\triangledown}{\sigma}_{ij}=\frac{\partial\sigma_{ij}}{\partial t}+v_k\frac{\partial\sigma_{ij}}{\partial x_k}-\frac{\partial v_i}{\partial x_k}\sigma_{kj}-\frac{\partial v_j}{\partial x_k}\sigma_{ik}, \tag{2}$$

In the limit of a an abrupt change in stress, this translates into a large derivative $\partial\sigma_{ij}/\partial t$, so the left-hand side of (1) is dominated by the time derivative, and can be approximated by

$$\frac{(1+\nu)\delta_{ik}\delta_{jl}-\nu\delta_{ij}\delta_{kl}}{E}\frac{\partial\sigma_{kl}}{\partial t}=D_{ij},$$

Integrating from an initial time t_i at which the crack starts propagating (so $\sigma_{ij}(x,t_i)$ is the pre-existig stress before introduction of the crack),

$$\frac{(1+\nu)\delta_{ik}\delta_{jl}-\nu\delta_{ij}\delta_{kl}}{E}\left[\sigma_{ij}(x,t_f)-\sigma_{ij}(x,t_i)\right]=\varepsilon_{ij}(x)=\int_{t_i}^{t_f}D_{ij}(x,t)\partial t.$$

The time integral over the strain rate simply becomes the strain ε_{ij} accumulated over the time period in question, and we can write the

$$\sigma_{ij}(t,x) = \sigma_{ij}^{v} + \sigma_{ij}^{e}.$$

where $\sigma_{ij}^{v} = \sigma_{ij}(t_i, x)$ is the pre-stress (related viscously to the velocity field $v_i(t_i, x)$ that was present before the introduction of the crack) and σ_{ij}^{e} is an effectively elastic stress that satisfies an elastic rheology of the form

$$\frac{(1+\nu)\delta_{ik}\delta_{jl} - \nu\delta_{ij}\delta_{kl}}{E}\sigma_{ij}^{e} = \varepsilon_{ij}.$$

This is consistent with the formulation used in the paper. We have added additional text to this effect to the paper.

4. Line 98, page 4, it is mentioned the stress field defined by (6) cannot be generated by an elastic rheology is not true. This stress field can be obtained with a nearly incompressible elastic rheology. It is really not the elastic or viscous nature but rather the incompressibility assumption that leads to this stress state. Please see Sun et al. (2021) Appendix A for the derivation of the elastic stress field, wherein if you plug in Poisson's ratio of 0.5, you would recover the stress field defined by (6). https://www.sciencedirect.com/science/article/abs/pii/S2352431621000626
   This is true if *you assume that $\nu = 0.5$ is viable, and the medium in question is elastically incompressible. We have altered the text to state that the assumed far field stress is incompatible with an elastic rheology unless ice is assumed to be elastially incompressible with a Poisson's ratio of 0.5. Empirically determined values of ν for ice are however around 0.3.*

5. Line 119 - 120, page 5, seems like a typo, there is no subscript on $[v]_{-}^{+}$ and in equation (9) u should not be bold in $[u]_{-}^{+}$.
   That is true, we have corrected this expression.

6. (11) seems to have some wrong notation. The index j appears three times and this violates Einstein's summation convention.
   We have changed this equation to the correct form of $(\delta_{ij} - n_i n_j)\sigma_{jk} n_k = 0$.

7. Line 139, page 5, I do not understand why it is more natural to prescribe water volume. Isn't it as poorly constrained as the water height in crevasses. Please explain how one would constrain water volume from observations.
   In our view of the dynamics *of crevasse propagation, the relevant question is not how one constraints the forcing parameters* observationally*, though that is an entirely relevant question once you have a successful, self-consistent theory. The relevant question at the the level of model development in our view is how water level is controlled* physically*. In particular, while a crevasse propagates, we need to know whether water level in the crevasse remains constant, or if it changes, what controls water level. The two cases we consider are essentially end-members hydrologies: A fixed water level h_w represents a large water reservoir (such as a porous subsurface aquifer) that easily drains and buffers water level in crevasses against changes. A fixed water volume is what we would expect in a situation where there is no water reservoir and no significant water supply to the crevasse on the time scale of propagation — presumably, on time scales less than a single Maxwell time. We will amend the text of the paper to make this clearer. Reality is, of course, likely to*

lie somewhere between the two, but requires a more refined understanding of near-surface hydrology than we are aiming for here.

8. Line 159, page 6, please use text roman i for the subscript for ice density, so that it does not mix up with the subscript index i.
Done.

9. Line 205, page 8, The authors state that instead of solving a full dynamic crack problem, they can use the semi-analytical theory of Freund (1990). It is not clear to me why Freund's approach is needed. Please explain why the simple stability criterion for steady state crack used in Lai et al. (2020) is not adequate for analysis.
It is correct that, for a single crack, a simple stability criterion of "the crack propagates if $K_I > K_{I,c}$" is satisfied. This is acknowledged on line 187 onwards of the original submission: "If there is only a single crack, its dynamics under changes in parameters are likely to be simple: any such change that reduces the static K_I below K_{Ic} would leave the crack length unchanged, while increases in static K_I above K_{Ic} would cause lengthening of the crack until its tip once more attains the static value of K_{Ic}." The formulation here is developed specifically with a view to being able to model multiple competing cracks (as alluded to in line 192 onwards of the original submission, "In the case of multiple cracks, however, not all cracks need to propagate simultaneously or at the same speed, and it is then unclear which cracks should be lengthened until they reach K_{Ic}"), We intend to treat that case in a follow-up paper currently in preparation. Once there are competing cracks that might lengthen simultaneously, it becomes important to know how fast they propagate relative to each other, (Put another way, in a one-dimensional dynamical system, all I need in order to be able to understand the qualitative, long-term behaviour of the system is (apart from some constraints on smoothness) to know when the right-hand side is positive, negative, and zero. That is no longer true in higher dimensions.)
In order not to have to repeat the basic statement of the model and nondimensionalization in detail, we would like to keep the formulation as is, allowing for that generalization to be done more efficiently.

10. In Eq. (22), the quantity $[t]$ comes out to be negative. Is that correct?
No. $[t]$ is always positive since $K'(0)$ is negative. $K'(0)$ is the second term in the Taylor expanding of the universal function $K(\dot{d})$. Please see the Universal function, $K(\dot{d})$, versus crack tip velocity in Chapter 6, Freund (1990).

11. In Eq. (32) you have the term $(s - \eta \breve{} z)$ where z is has a dimension but η is nondimensionalized. Is there any typo there?
In line 248 we state that from now on we are omitting the asterisks from the nondimensionalized variables. Both s and z are nondimensionalized in this equation.

12. I found the results section to be a bit hard to read. I felt like a lot of minor details were discussed which at time made me lose the big picture. I think the paper can be condensed a lot in this section.
We suspect our goal differs somewhat from what the referee has in mind. Given that the basic idea behind the model was previously developed in van der Veen's (1998) papers as well as Lai et al (2020), our intention was not primarily a "big-picture" article but a detailed and comprehensive analysis. We trust that such an approach still has its place in the

field and in the pages of The Cryosphere*, even if it places greater demands on the reader (who we assume will typically be someone more deeply invested in the study of calving).*

13. Throughout the paper, I found minor typographical errors that are a few too many, but I did not want to list them here. Please proofread the entire article before submitting the final version.
    Our apologies. We have done our best to re-proofread the paper and eliminate more (if possibly not all!) of the remaining typos.

14. In section 5, two calving laws were introduced one for basal crevasses and another for surface crevasses. A major critique is that unless these calving laws are incorporated in an ice sheet model and validated with observational data, we do not know if it is good. However, this maybe beyond the scope of this article.
    Indeed, our goal for the time being was to construct and — more significantly — analyze a prognostic model for calving based on simple but self-consistent physical ingredients that are compatible with incorporation into a large-scale ice sheet model. Calibration, validation and testing are beyond the scope of the present work, but would be an important step.

15. The conclusion of this paper is really long and I found it difficult to read. It will be good if it can be broken up into subsections to improve readability.
    We made some changes and some subsections are added. The intention here was to be comprehensive.