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Comment on tc-2022-37

Anonymous Referee #1

Referee comment on "The effect of hydrology and crevasse wall contact on calving" by Maryam Zarrinderakht et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-37-RC1, 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.

Detailed Comments:

- 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
- 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.
- 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.

- 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
- 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]^+_-.
- (11) seems to have some wrong notation. The index j appears three times and this violates Einstein’s summation convention.
- 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.
- 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*
- 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.
- In Eq. (22), the quantity [t] comes out to be negative. Is that correct?
- In Eq. (32) you have the term (s - \eta – z) where z is has a dimension but \eta is nondimensionalized. Is there any typo there?
- 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.
- 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.
- 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.
- 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.